What Stops the Torture?

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States whose agents engage in torture in a given year have a 93% chance of continuing to torture in the following year. What leads governments to stop the use of torture? We focus on the principal–agent relationship between the executive and the individuals responsible for supervising and interrogating state prisoners. We argue that some liberal democratic institutions change the probability that leaders support the creation of institutions that discourage jailers and interrogators from engaging in torture, thus increasing the probability of a state terminating its use of torture. These relationships are strongly conditioned by the presence of violent dissent; states rarely terminate the use of torture when they face a threat. Once campaigns of violent dissent stop, however, states with popular suffrage and a free press are considerably more likely to terminate their use of torture. Also given the end of violent dissent, the greater the number of veto points in government, the lower the likelihood that a state terminates its use of torture.

During the final quarter of the twentieth century, freedom from torture was the most widely violated human right to the physical integrity of the person (Cingranelli and Richards 1999, 522). In any given year during that period, an average of 78% of governments, including those with democratic institutions, used torture against at least one person under their control (Cingranelli and Richards 2004).[1] Furthermore, states that have used torture in the past are unlikely to terminate use of the practice; 93% of the countries whose agents engaged in at least one act of torture in a given year also engaged in torture in the following year. Given that the majority of governments use torture, what causes some of them to stop?

Although liberal democratic institutions limit state coercion and repression (Davenport 2007a), democratic institutions only constrain the government's use of torture when the state is not faced with violent dissent (Davenport, Moore, and Armstrong 2007). Furthermore, the majority of states that use torture do so with remarkable consistency. The Cuban government, for example, engages in torture for the entire duration of our sample.

Yet some governments stop their use of torture altogether in a given year. Countries ranging from Thailand and Mali to Greece and the United Kingdom stop torturing at various points from 1980 to 2000. Why is this the case? More specifically, what state characteristics are systematically related to the likelihood that a government permitting torture puts a stop to the practice?

To our knowledge, researchers have yet to examine whether countries with liberal democratic institutions are more likely than countries without them to stop the practice of torture. Furthermore, although there is suggestive discussion about the relationship between a country's judicial system and its likelihood of engaging in torture, we do not yet know whether countries with certain types of justice systems have a lower probability of terminating the use of torture. Similarly, some systematic comparative research explores whether the presence of a strong civil society (Simmons 2009) or "naming and shaming" campaigns by nongovernmental organizations (NGOs) influence human rights abuse (Franklin 2008; Hafner-Burton 2008), but we are unaware of work that asks whether such campaigns stop torture.

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[1]We define torture below. Note that reports of torture represent an undercount of *actual* torture, in part because government leaders face incentives to hide torturous practices.

Our study explores the extent to which political institutions change executive incentives to order and/or permit torture. Although torture is illegal in virtually all countries, we argue that the threat to tenure in office for breaking laws against torture varies across political institutions. When the state is challenged by groups using violent tactics, both the executive and her jailers/interrogators share a preference to use any means they believe effective to produce compliance and/or intelligence. But liberal democratic institutions increase the likelihood that allegations of torture surface. In those cases where such allegations become public, liberal democratic institutions also increase the likelihood that the executive is held responsible and has her tenure in office threatened as a result. We assume that executives make decisions based on expectations of likely outcomes, and thus decide whether to (1) order torture, (2) turn a blind eye to torture, or (3) take actions to prevent torture based on their expectation about how those choices affect their tenure in office. Using a principal–agent approach, we argue that the likelihood that a state whose jailers and interrogators have been using torture stops the practice is a positive function of the executive's incentive to enact policies that prohibit subordinates from exercising discretion and using torture to gain compliance, confessions, or intelligence from prisoners under their control.

More specifically, we argue that liberal democratic institutions—in the form of popular suffrage, which we call Voice,[2] and Freedom of Expression—produce institutional incentives that increase the probability that executives, legislatures, and/or judiciaries will terminate the use of torture by their agents. But not all liberal democratic institutions encourage the termination of torture. Following Tsebelis (2002), we contend that the greater the number of veto points—an indication of diffuse power, one dimension of liberal democracy—the more likely it is that the status quo of torture will persist. Thus, while we expect Voice and Freedom of Expression to be associated with the termination of torture, we anticipate that Veto will be associated with its continued use.[3] But we argue that all of the variables that we study—liberal democratic institutions, inquisitorial criminal justice system, campaigns by international NGOs, and the strength of civil society—will have little, if any, impact when the state is challenged by dissidents who use violence to press their claims.

The remainder of this study proceeds as follows. In the first section we define torture, discuss its history of use as a tool of statecraft, and review existing work about its termination. In the second section we present our agency argument about torture termination, and in the third we develop hypotheses. In the fourth and fifth sections of the article, we demonstrate empirically that very few states stop using torture. Following a description of our sample and variables, we test our hypotheses using two discrete time survival models. We conclude with a discussion of our results, as well as policy recommendations for international organizations interested in the eradication of torture.

A *Brief* History of Torture

The United Nations' Convention Against Torture (CAT) defines torture as

> any act by which severe pain or suffering, whether physical or mental, is intentionally inflicted on a person for such purposes as obtaining from him or a third person information or a confession, punishing him for an act he or a third person has committed or is suspected of having committed, or intimidating or coercing him or a third person, or for any reason based on discrimination of any kind, when such pain or suffering is inflicted by or at the instigation of or with the consent or acquiescence of a public official or other person acting in an official capacity. (United Nations 1984, 85)

Torture was initially developed as a systematic tool of the state in criminal proceedings and for several centuries was a widely practiced judicial technique for establishing the veracity of testimony, as well as determining guilt and innocence (Evans and Morgan 1998). Techniques that scarred the body were not only acceptable, they were required—one needed evidence of the torture to establish that it had been carried out. The nineteenth and twentieth centuries bore witness to the decline of torture as an accepted procedure. The success of the legal movement to produce the CAT demonstrates the extent to which international opinion about torture has changed: today virtually every country in the world has outlawed torture as a legitimate practice.

Yet, as Evans and Morgan (1998, 13–60), Einolf (2007), Rejali (2007), and Ron (1997) observe, torture has not disappeared. When democratic countries began to outlaw the use of torture, police departments in France, the United Kingdom, and the United States innovated new methods of inducing pain without generating scars. The

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[2]Our use of Voice follows Davenport (2007b) and should not be confused with the usage of the term in Hirschman (1970).

[3]As we discuss below, a number of studies have found that judicial independence is positively associated with government respect for physical integrity rights.

"successful" techniques "were nonlethal, portable, left few marks on the body, and were labor saving, painful, and flexible to use" (Rejali 2007, 423).[4] Torture did not disappear in France, the United Kingdom, and the United States; it just evolved and became less pervasive in discourse (Rejali 2007; Ron 1997). Scarless torture diffused beyond the major democracies in response to the success of Amnesty International's antitorture campaigns in the 1970s such that these techniques are now observed throughout the world (Rejali 2007). This history puts in a new context the fact that states signed the CAT beginning in 1984: the agreement likely gained acceptance because techniques to minimize the chances of an executive being held accountable had become well known.

The goal of "clean" techniques is plausible deniability by state executives. One cannot *plausibly deny* the use of scarring techniques in judicial proceedings. "Clean" techniques, on the other hand, permit state agents to shift debate about their treatment of prisoners from blatant lying to a "he said, she said" context in which uncertainty exists. This uncertainty generates plausible deniability and produces an incentive for state agents to use "clean" techniques to obtain compliance, confessions, or intelligence from prisoners—even though others (e.g., judges, government officials, reporters, the public) might judge this type of torture as illegal. It is important to emphasize that these techniques do not make the use of torture impossible to detect. While these techniques make plausible deniability possible, states are not always able to hide torture. Indeed, Amnesty International received credible reports of "torture or ill-treatment by agents of the state in over 150 countries" from 1997 to mid-2000 (2003, 1). Furthermore, the global shift from scarring to "clean" techniques has occurred at the same time that the use of torture as a tool of statecraft has risen.[5] If "clean" techniques made the use of torture invisible, this would not be the case.

The literature on the termination of torture is quite small. The major works (Amnesty International 2003; Delaplace and Pollard 2006; Evans and Morgan 1998) are all written, in part, by activists; social scientists have yet to engage the question of torture termination. Miller argues that in countries where torture has been "largely eliminated, or at least significantly reduced . . . reeducation and training [and] stringent accountability mechanisms" have been required (2008, sect. 4). Evans and Morgan (1998) emphasize the importance of monitoring. Amnesty International (2003) and Delaplace

and Pollard (2006) call for training programs: "States should develop, implement, and follow-up training programmes for prison staff, which includes training on human rights . . . All law enforcement must be accountable to independent judicial and disciplinary authorities" (Delaplace and Pollard 2006, 225).

Evans and Morgan (1998) find that the European Convention for the Prevention of Torture has been mildly successful, and both Amnesty International (2003) and Delaplace and Pollard (2006) provide brief case studies to explain how several largely democratic countries have significantly reduced the use of torture. These case studies make it clear that the executive, the legislature, and/or the judiciary might be responsible for initiating reforms, that public opinion is sometimes mobilized, and that domestic and/or international NGOs sometimes play an important role in the process. What these case studies fail to provide is systematic comparison that permits inference about broad patterns. Nevertheless, these works provide grist for broadly comparative, large-N research. They support our theoretical approach, which focuses on agency relations between executives, legislatures, and judiciaries and the jailers and interrogators who decide whether to torture when conducting their official duties.

An Agency Analysis of Torture Termination

Because state executives *delegate* the supervision and interrogation of detainees and prisoners to jailers and interrogators, this relationship can be usefully viewed through the lens of a principal–agent model. Agency models are used to explain situations in which power is delegated from one person or institution to another. Miller defines a principal–agent relationship as one in which "the agent has an informational advantage over the principal and takes actions that impact both players' payoffs. The principal has the formal authority, but in [principal–agent relationships], the attention is on a particular form of formal authority: the authority [of the principal] to impose incentives on the agent" (2005, 203–4). With regard to torture, the state executive is the principal, and the jailers and interrogators are the agents. We use the following stylized account to develop hypotheses about the termination of torture.

Executive Orders (Not) to Torture

Executives employ jailers and interrogators to supervise the daily activities of those locked away, extract confessions from criminal suspects, and obtain political information from detainees believed to be a threat to

[4]Scarring techniques are familiar, but "clean" techniques warrant an account. In the interest of space, we refer the reader to Rejali (2007).

[5]The proportion of states that tortured at least one person per year rose from 75% in 1981 to 90% in 1996 (Cingranelli and Richards 1999, 524).

the state.[6] Executives might delegate the supervision of inmates and procurement of information to jailers and interrogators in one of two ways. Some executives will explicitly tell their agents to use torture to maintain control and extract confessions. If agents are told explicitly to torture, it is likely that many will comply. Because torture is illegal, however, some executives, legislators, and judges who order or permit it run the risk that their tenure in office will be threatened.[7] These executives are unlikely to explicitly advocate the use of torture and may even adopt policies to deter its use. In these countries, jailers and interrogators have discretion over how to supervise prisoners and conduct interrogations but are not explicitly told to engage in torture and may even be told not to use torture.

Why do we anticipate that agents will turn to torture to gain the compliance of people under their control? The creation of hierarchies where one group of people is given authority over another group tends to produce abuse of the latter by the former. The Stanford Prison Experiment infamously demonstrated that people do not need special training to abuse and torture people under their control (Zimbardo 2007). Miller puts it this way: "for the most part military, police, and correctional institutions are *qua* institutions very receptive to the practice of torture" (Miller 2008, sect. 4). The point is that the institutional setting, not the moral character of the jailers and interrogators, plays a major role in producing the use of torture. This implies that the absence of explicit policy to *prevent* abuse will produce at least some interrogators who use torture to acquire compliance and/or information from prisoners.

Why can't state leaders trust that merely instructing their agents to stop torturing will serve to change their behavior? Put differently, when should we anticipate that agents' preferences will differ from those of the executive? Consider first a situation in which an executive orders jailers and interrogators to torture. Unless the agent is concerned that the executive will deny complicity if the torture becomes public, the agent and the executive's preferences appear to be aligned: both want

compliance/information, and both believe that torture is at least sometimes effective for producing the desired outcome. As long as the agent's belief that she will be caught is fairly low, there should not be much of a principal–agent problem here. When the executive rules out the use of torture as a means to produce compliance and information from interrogations, more serious principal–agent problems can arise: the agent has intimate knowledge of the techniques she uses to gain compliance and information, and the executive must rely largely on the agent's reports to obtain his own information about those techniques. If the agent believes that torture is never effective for producing compliance and/or information, then there will be no principal–agent problem. Similarly, if the agent believes that she is likely to be caught for torturing prisoners or detainees, there will be no principal–agent problem. If, however, the agent believes that torture is sometimes effective *and* that the probability of being caught is relatively small, then a principal–agent problem is likely to emerge when the executive prefers that the agent refrain from using torture.

We assume that jailers and/or interrogators have a range of beliefs with respect to (1) the expected effectiveness of torture and (2) the likelihood of being held accountable for using torture. We further assume that the average belief among agents of the state is that torture is sometimes effective, and further, that the average agent believes that the executive's reliance on her and her colleagues as a source of information about her practices leaves her at a relatively low risk of being caught (i.e., provides her with a fairly high degree of discretion to supervise and interrogate as she deems appropriate). As a result, even if agents are told not to torture, some will still use torture as a cost-effective means by which to gain compliance or information.[8]

Executive Attempts (Not) to Prevent Torture

In this study, we are interested in the extent to which institutions incentivize executives, legislatures, and/or judiciaries to take actions that lead interrogators and jailers to revise their beliefs about the likelihood of being caught and punished such that few remain willing to employ torture techniques. In liberal democracies the presence of other institutionalized players (e.g., the press or the

[6]Although we lack data about the cross-national distribution of state motives to torture, some suggestive evidence about the people who are torture victims is available: "In more than 70 countries the victims included political prisoners, but ordinary criminals and criminal suspects had reportedly been victims of torture or ill-treatment in over 130 countries" (Amnesty International 2003, 1).

[7]Making a similar argument that focuses on differing electoral institutions across democracies, Cingranelli and Filippov argue that "democratic institutions could positively influence the protection of human rights by supplying very specific *electoral* incentives" (2010, 7; emphasis original).

[8]Many people believe that torture is unusual and that all that is needed to prevent it is to hire "decent, moral" people. But one need only assume instrumental decision making to account for the use of torture: no assumption that the interrogator lacks common moral values is necessary (Wantchekon and Healy 1999). Walzer (1973) makes a similar case with reference to elected executives' need to provide national security.

judiciary) may encourage executives to reduce the use of torture by increasing its costs.[9] Executives facing risk to their tenure in office—and thus wanting to control the techniques used by jailers and interrogators—can enact ex ante or ex post controls on supervision and interrogation processes. Ex ante controls include the implementation of institutions and training programs to prevent human rights violations. Specific training programs that identify appropriate interrogation techniques and teach practitioners the precise definition of torture are required if torture is to be systematically rooted out of prison and interrogation systems (Amnesty International 2003; Delaplace and Pollard 2006). Ex post controls include monitoring the work of jailers and interrogators. Monitoring does not mean that executives have to hover over their agents to ensure compliance; rather, ex post rules serve as monitors to encourage compliance within the agency hierarchy (Alchian and Demsetz 1972).

Unfortunately, we are unaware of cross-national information about the presence of ex ante and ex post methods of torture prevention. Because we cannot observe the presence of institutions and training programs that discourage inappropriate control and interrogation techniques, our study identifies the liberal democratic institutions, criminal justice systems, and NGO activities most likely to lead to the adoption of such programs. Institutions including popular suffrage and a free press increase the costs of torture to executives working within such institutions. The presence of these institutions increases the motivation of democratic leaders to limit their subordinates' discretion to use torture when controlling and/or obtaining information from detainees.

There is a considerable literature that explores the extent to which liberal democratic institutions limit the state's use of repression (Davenport 2007a). Although this work finds that liberal democratic institutions are associated with lower levels of coercion and violations of human rights, torture is unique from other types of violations: not only do all states torture with remarkable regularity (Cingranelli and Richards 1999), but also democracies have innovated a new regime of "clean" torture that does not leave obvious marks (Rejali 2007; Ron 1997). Furthermore, when the state is faced with violent dissent, democratic institutions have only a limited impact on the likelihood that the state uses torture (Davenport, Moore, and Armstrong 2007). To the best of our knowledge, no research has yet examined whether countries with liberal

democratic institutions are more likely than countries without them to terminate the use of torture.

We distinguish between three liberal democratic institutions: Voice, Freedom of Expression, and Veto. We argue that both Voice and Freedom of Expression are associated with *increases* in the likelihood that an executive creates institutions and puts in place programs to train prison guards and interrogators to eschew torture. More generally, popular suffrage and guarantees of freedom of expression provide opportunities for other actors to check the actions of the state. In systems where institutions do not empower other actors to challenge state actions, executives need not fear disclosure of state agents' use of torture. Autocratic states, for example, are likely to use torture as a standard tool to exercise and retain power (Rejali 1994; Vreeland 2008). Conversely, in countries with institutions that permit members of society to call attention to the use of torture, executives, legislatures, and/or judiciaries face greater incentives to create ex ante and ex post institutions to prevent their agents from engaging in torture. Other liberal democratic institutions— specifically Veto, the distribution of authority over policy across actors other than the executive—may not result in an increased likelihood of the government terminating torture, especially when other actors in government face incentives to ignore executive directives.

Voice involves the citizens' ability to select those who hold political office. We are specifically interested in the extent to which citizens have input to policymaking through the election ballot box. As such, we have interest in people's ability to vote in executive and/or legislative elections *as well as* the extent to which a competitive party system ensures that effective choices are available to voters. Electoral systems not meeting these criteria do not increase the extent to which citizens are able to participate in the political process. The institutions associated with increased Voice provide the populace with an opportunity to respond collectively to the actions of their governments. Electoral institutions that provide people with effective choices and allow them to express their opinions lead executives to weigh public reaction to government policy. Because torture is both morally abhorrent and illegal, nontrivial portions of the public are likely to decry its use. As a result, we expect that executives in countries with high levels of Voice are more likely to create institutions to prevent torture than executives in countries without free and fair elections. This line of reasoning leads to our first hypothesis:

Voice Hypothesis: *The presence of competitive elections within a state is positively associated with the likelihood of that state terminating torture.*

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[9]One could argue that democratic leaders should realize they will be held accountable for the use of torture and will therefore order their subordinates not to torture. Such an argument assumes away the principal–agent problem and is at odds with the empirical evidence: most democracies torture at least some of the time.

Freedom of Expression captures the extent to which individual citizens and groups can express their views without sanction. We are particularly interested in the freedom of the press to exist without government censor. A press that is free from government intrusion and regulation acts as another actor with an incentive to check the power of state leaders. As a result, executives in countries with a free press will be more likely than those in countries without a free press to enact programs training their agents to refrain from using torture. Otherwise, they face consequences in the media's publication of their support of torture. Our second hypothesis comes from this discussion:

Freedom of Expression Hypothesis: *Higher levels of freedom of expression within a state are positively associated with the likelihood of that state terminating torture.*

Veto concerns the extent to which the executive's authority can be checked by other institutional actors in government (Tsebelis 2002). We are interested in the extent to which the legislature contains members of parties not in executive government, the extent to which an independent judiciary is able to check the legislature and the executive,[10] and the extent to which there exist subnational levels of government. More specifically, we are concerned with a basic institutional separation of powers (formal veto points) and a political separation of power based on the partisan composition of different branches of government (partisan veto points).

Our expectations for Veto are different than those for Voice and Freedom of Expression. Much of the literature on government repression and the presence of institutional veto players hypothesizes that the dispersion of veto throughout government creates political actors who have the capacity to speak out against segments of the government that support coercion and repression (e.g., Davenport 2007b). That work argues that executives are less likely to turn a blind eye to incidents of coercion and torture when they know there are other actors in government who can call them out for (tacitly) endorsing the use of torture. But this literature makes either an implicit or explicit assumption that institutional actors aside from the executive will seek to *prevent* the executive from engaging in government torture. It seems to us equally plausible that other institutional actors within government may *benefit* from the use of torture. Recent literature argues that this is often the case (e.g., consider transitions from authoritarian rule); when a state leader is unable to

control the bureaucracy, even the most well-intentioned executive attempts to stop the torture will be unsuccessful (Rejali 2007, 48–49). If this is the case, other actors within government may stymie executive preference for terminating torture. Indeed, the primary implication Tsebelis (2002) draws from his theory is that the larger the number of veto players, the more difficult it is to change any given status quo policy; in our study the status quo is the use of torture. Rather than make an assumption about the preferences of other institutional actors, we posit a third hypothesis that is consistent with that of Tsebelis (2002):

Veto Hypothesis: *Larger numbers of institutional and partisan veto players in a state are negatively associated with the likelihood of that state terminating torture.*

This hypothesis about Veto seemingly runs counter to a widely reported hypothesis and finding that judicial independence—the existence of a judiciary with institutional safeguards that permit it to check the executive and legislature—is strongly, positively associated with greater respect for physical integrity rights (e.g., Apodaca 2004; Cross 1999; Keith 2002; Keith, Tate, and Poe 2009; Powell and Staton 2009). One right to the physical integrity of the person is freedom from torture. Most discussions of the impact of constraints on the executive implicitly reference this more specific argument, and these studies typically find support for the hypothesis that executive constraints are associated with greater respect for physical integrity rights (e.g., Bueno de Mesquita et al. 2005; Davenport 2007b; Davenport and Armstrong 2004). Our hypothesis is distinct because it focuses on the Tsebelis (2002) account about the impact of veto points on policy stability, which anticipates that changing any policy—including torture—will be more difficult the more veto points there are in a system. The fact that we focus on torture spells is important: there is a status quo in our study—the use of torture. That is why we turn to Tsebelis (2002). To summarize, it is conceivable that independent judiciaries are associated with respect for all physical integrity rights, including freedom from torture. But it might alternatively be the case that torture is different from other physical integrity rights and that judicial independence is either not associated with, or is negatively associated with, freedom from torture. Our data suggest that the latter is the case.[11]

We now turn our attention from institutions to the threat produced by violent challenges to state policy or the government itself, and the impact of such threat on

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[10] For a recent study of the impact of the judiciary on human rights, see Tate and Keith (2009).

[11] Space constraints prohibit us from addressing this matter in more detail here, but we address it in detail in an online appendix available at the authors' websites or http://tinyurl.com/whmdata.

the executive's incentive to prohibit the use of torture by agents of the state. Following Walzer (1973), Davenport (1995, 2007a), and Wantchekon and Healy (1999), we argue that states facing violent dissent will be systematically less likely to put in place the reforms needed to stop torture, even in the presence of democratic institutions. When a state is faced with violent dissent, Voice and Freedom of Expression are less likely to incentivize executives to place limits on interrogators' ability to extract information via torture. With respect to Veto, however, it is difficult to say whether the staus quo (i.e., continuing to torture) would be more privileged given violent dissent. We thus restrict our hypothesis about the conditional impact of violent dissent to the relationship of Voice and Freedom of Expression upon the likelihood of terminating torture.

Violent Dissent Hypothesis: *The relationships between Voice and torture termination, and Freedom of Expression and torture termination, are attenuated by violent dissent.*

We also explore the impact of several additional variables that may affect the termination of torture. First, inquisitorial criminal justice systems emphasize confessions in criminal prosecutions, and some criminology scholars contend that the premium placed on confessions in this type of criminal justice system pressures police to torture criminal suspects (e.g., Bradley 1999). For this reason we control for whether the state has an inquisitorial legal system because such systems should be less likely to stop torturing. Second, the constructivist school in political science and world society school in sociology emphasize the importance of activists and organizations in generating norms that pressure states to comply with their treaty obligations (Clark 2001; Meyer and Scott 1983; Risse 1999). Hafner-Burton and Tsutsui (2005) find support for the impact of the link between domestic society and global civil society via NGOs. This work leads us to anticipate that countries with greater numbers of NGOs operating *within* the country will be more likely to stop the use of torture. Activists and human rights organizations on the ground can influence government behavior in two ways—by actually changing the preferences of government leaders against torture, or by influencing individuals to call for more accountability within the supervision and interrogation processes of the state (e.g., Delaplace and Pollard 2006, 234–36). As such, we include a measure of the strength of civil society—qua the size of the domestic NGO community—to determine whether it is associated with an increased probability of stopping torture. Third, Franklin (2008) and Hafner-Burton (2008) test the possibility that the "naming and shaming" behav-

ior of international human rights organizations might increase the likelihood that a government stops its use of torture. If international NGOs rebuke states for the use of torture, executives may be more likely to implement interrogator training programs than executives in states that have not faced sanction. Based on this work, we include a measure of international NGO activity to see whether being called out for human rights violations makes states more likely to stop the torture.

Another potential influence is suggested by research that shows that states are more likely to terminate war following leadership change (Goemans 2000). Anticipated leadership changes might be associated with policy change more generally, and if so, changes in programs to deter the government use of torture. Because leadership changes in democracies are different from those in autocracies, we are more interested in a leader's *expected* likelihood of removal from office than actual leadership change. We suspect leaders are more likely to calculate that gains from torture outweigh potential costs as their expected tenure in office decreases. Finally, we include two covariates associated with the probability of stopping torture: population and national income. These variables tend to have a statistically significant impact in models of coercion (e.g., Davenport 1995, 2007b; Poe and Tate 1994; Ziegenhagen 1986). Because they are likely correlated with other covariates, we include them to guard against spurious inferences (King, Keohane, and Verba 1994).

Empirical Indicators

The unit of observation for our study is the country–year, and the spatio-temporal domain covers all countries with populations over one million from 1981 through 1999.[12] Although one could extend the temporal domain through 2003, we chose not to largely because of the occurrence

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[12]One might object that if we use the country–year as our unit of observation, our theory would need to account for the difference between federal and nonfederal systems. Such a concern would observe that the national-level executive is not responsible for torture that occurs in a city, as opposed to federal, prison. This is true. To adopt the country–year as our unit of observation, we must assume that the Voice, Veto, and Freedom of Expression institutions that exist at the federal level are mirrored at the regional/state and local levels. To the extent that federal and local institutions have highly similar values on these variables, executives at the regional/state and local levels of federal systems will face the same incentives to (1) order torture, (2) turn a blind eye to torture, or (3) take actions to prevent torture as does the national executive. We believe this is a reasonable simplifying assumption that permits us to conduct analyses despite an absence of information on whether torture coded in the CIRI (2004) database was committed by national, regional/state, or local jailers/interrogators.

of Al Qaeda's 9/11 attacks on the United States and the global crackdown of governments in response to concerns about terror. It strikes us as plausible that evidence will show something akin to an intercept shift (i.e., a discrete change) in states' propensity to torture in the wake of 9/11. Empirical inquiry of this period is warranted, but several factors lead us to put that off for future work. First, because no cross-national studies yet exist, we do not have an empirical baseline for the propensity of states to stop torture spells. Given that the 2000s decade might prove to be an outlier, we focus on the final 20 years of the twentieth century to establish what general patterns existed during and after the Cold War. Second, global responses to the 9/11 attacks and the revelation of abuses at Abu Ghraib, Guantanamo Bay, and other U.S. detention centers may have led to a backlash against the increased propensity to torture that we suspect occurred post-9/11. As we show below, violent attacks on governments have a surprisingly strong impact on the likelihood that state agents torture. Rather than expose our inferences to such potentially considerable changes in states' propensity to terminate spells of torture, we restrict this study to the 1981–99 period.

We study torture spells, a sequence of years in which agents of the state committed at least one reported act of torture. Although we focus on spells because our study seeks to identify correlates of torture termination, this focus arguably has an additional research design benefit. Because not all instances of torture are recorded, the measure we use in our models is an undercount of the number of actual counts of torture within a country. First, this undercount provides us with a *conservative* estimate of the effects of our independent variables on torture; truncation in the dependent variable biases inferences of the size of the effect in the direction of null findings (King, Keohane, and Verba 1994, 130). Second, the undercount is likely to be stronger in some countries than others. For example, countries with a free press are likely to have a smaller downward bias than countries without press freedom. By focusing on the termination of torture rather than its level, our design mitigates this issue: the low threshold and the focus on change work like fixed effects intercepts—the level of bias is likely to be largely constant over time for each country. This would not be the case if countries without press freedom tended to produce no torture spells. Visual inspection of such cases, as well as the high incidence of torture in the global sample, demonstrates, however, that this is not the case.[13]

We measure *Torture* using data from the CIRI Human Rights Database (Cingranelli and Richards 2004). The CIRI data are based upon Amnesty International and U.S. State Department reports and include both incidents of scarring torture and "clean" torture. We dichotomize CIRI's trichotomous ordered variable of torture to create a dichotomous indicator of torture coded "1" if a government does *not* torture in a given year and "0" otherwise. Why not use CIRI's trichotomous measure? CIRI distinguishes cases where between 1 and 49 cases were reported in a year from those in which 50 or more cases were reported. Although we recognize that thresholds such as this are inevitably arbitrary, it still strikes us as unlikely to provide a meaningful breakpoint by which to distinguish cases. To probe whether the 50 threshold has traction within our study, we estimated multivariate models using the trichotomous coding and established that none of the institutional, criminal justice system, NGO behavior, or other variables we study are *robustly* related to the ordered difference across the threshold. Given our interest in the termination of torture and our dubiousness about the threshold, those results support our decision to study the termination of torture spells.

To measure *Violent Dissent* we use a dichotomous measure coded "1" if a country experiences at least one act of guerrilla war (Banks 2001) or is coded by the Correlates of War (COW) project as experiencing a civil war in a given year (Sarkees 2000). We intentionally select a measure with a minimal threshold because we are interested in determining whether the presence of at least *some* violence is sufficient to change the probability that a state uses torture. As we describe in the online appendix, however, the results reported below are robust to the use of the Armed Conflict Data (Gleditsch et al. 2002) to measure civil war.

We have three measures of democratic institutions to correspond with the theoretical arguments outlined above. First, we employ a binary conceptualization of *Voice* (Alvarez et al. 1996; Cheibub and Gandhi 2004). Our scale is reversed so that a "1" indicates a country with free elections and a "0" indicates a country without free elections. States must meet two criteria to be coded as having free elections: (1) the executive must be selected through election, and (2) there must be ex ante uncertainty about who will win the election (Alvarez et al. 1996). Next, *Freedom of Expression* is measured using data from CIRI on state restrictions on free speech (Cingranelli and Richards 2004). We reversed the trichotomous scale such that "2" indicates no restrictions, "1" indicates some restrictions,

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[13]We would like to thank an anonymous reviewer for noting this aspect of our design. Information on the empirical relationship between domestic freedom of expression and torture is in-

cluded in the online appendix available at the authors' websites or http://tinyurl.com/whmdata.

and "0" indicates severe restrictions on free speech. To measure our *Veto* hypothesis, we use the PolConV measure from the Political Constraints Project (Henisz 2002). PolConV measures institutional restrictions on executive behavior. Based on a spatial model, it ranges from 0 to 1 and includes information on five veto points: the executive, two legislative chambers (accounting for partisan differences), the judiciary, and the subnational government's ability to establish monetary policy. Space constraints prohibit us from distinguishing judicial independence from legislative veto here, but we report a number of ancillary analyses in our online appendix (available at the authors' websites or http://tinyurl.com/whmdata).

Inquiries directed to a number of criminologists and political scientists with an interest in crime produced the disappointing knowledge that a cross-national database of criminal justice systems does not yet exist.[14] We thus employ a proxy measure that should be positively correlated with the concept of inquisitorial criminal justice systems we wish to measure. As Bradley (1999) observes, civil law countries usually have inquisitorial criminal justice systems. To measure civil legal systems we employ a dummy variable for *Civil* legal systems that is available in Powell (2006). This variable permits us to distinguish civil legal systems from noncivil legal systems (i.e., Common Law, Islamist, or Mixed systems). This operational decision is quite crude. For example, Keith and Ogundele (2007) report seven different types of legal systems, all of which have some type of civil component. Unfortunately, our dichotomy misses many of the finer distinctions to be made among criminal justice systems.[15] We initially sought to estimate a model that would allow us to draw inferences about additional types of legal systems, but there is insufficient variation in the data to estimate such a model. Rather than ignore the literature on criminal justice systems, we employ the Civil versus Not Civil dummy variable.

We also include several noninstitutional measures in the analysis. As a proxy for the strength of civil society, we use data from Wiik (2002) to measure the number of *NGOs per capita* within a country during a given year. To examine the possibility that states terminate their use

of torture as a result of *Naming and Shaming* in the international community, we include an indicator based upon whether a country is the target of shaming by NGOs (specifically Amnesty International), the Western news media (specifically *Newsweek* and *The Economist*), and the United Nations Commission on Human Rights (UNCHR). It is coded "0" if none of these "naming and shaming" sources targets a country, "1" if one of these sources targets a country, "2" if two of these sources target a country, and "3" if all three "name and shame" a country in a given year.[16] To control for the probability that the Chief Executive will be removed from office, "given the length of tenure in office, the rate of economic growth, and the past rate of executive turnover in each country," we turn to Cheibub, who uses a survival model to estimate the risk of an executive's removal from office (1998, 359–60). Rather than employ Cheibub's measure, which is limited geographically and temporally, we use a replication of *Threat to Leader Tenure* that extends the domain (Young 2008, 49).[17] The final measure ranges from 0 (low probability of losing office) to 1 (high probability of losing office). To measure our control variables, we use the gross domestic product (*GDP*) per capita, *GDP Growth*, and *Population* variables available in the replication data from Fearon and Laitin (2003).

States Facing Violent Dissent

Survival (aka duration, hazard, or event history) models are used to estimate the risk of an "event" occurring at a moment in time (Box-Steffensmeier and Jones 1997, 2004). In our first model, the "event" of interest is the termination of torture. Our data to test the aforementioned hypotheses consist of spells of torture in discrete time format. The data cover the years 1981–99, and there are 3,587 country-years in the data. Of those, 2,341 country-years are "at risk" to stopping torture (i.e., that many cases were engaged in torture in the preceding year). The data include 146 countries, and there are a total of 284 spells of torture (i.e., countries that either had a reported use of torture in 1981 when the data set begins or had a reported use of torture in a year after 1981 after having had at least one year in which no uses of torture were uncovered). Of the countries with at least one reported use of torture,

[14]The United Nations Surveys on Crime Trends and the Operations of Criminal Justice Systems (http://www.uncjin.org/Statistics/WCTS/wcts.html) looks promising, but data of interest to this study are largely missing for non-OECD countries; thus, the likelihood they are missing is correlated with variables of interest to our study. Similarly, the World Factbook of Criminal Justice Systems (http://www.ojp.usdoj.gov/bjs/abstract/wfcj.htm) contains data on only 45 countries, for the year 2002.

[15]Please see the online appendix for a brief discussion of some of the inferential challenges raised by our use of this crude measure (available at the authors' websites or http://tinyurl.com/whmdata).

[16]These data come from Hafner-Burton (2008), which includes detailed description of the coding rules.

[17]Both Young (2008) and Cheibub (1998) estimate the time to losing office for each leader, then predict the hazard rate of losing office for each leader-year, assuming a Weibull distribution for the hazard.

141 of them stopped using torture for at least one year. The termination of torture, coded "1" in our data, is a repeated event; states can stop and restart torture, engaging in multiple "spells." States can also continue to torture for the duration of our temporal domain, with 43 spells lasting the 20-year period (i.e., a country was torturing in 1981 and in every year through 1999). The remaining 100 spells had not ended by 1999 (i.e., are right-censored).

Because we have discrete time-duration data, our dependent variable is binary. As a result, we conduct our multivariate survival analyses using logit (Box-Steffensmeier and Jones 1997, 2004). Models in which the dependent variable is dichotomous or multichotomous produce inefficient estimates if there is temporal dependence within the units. If time dependency is high, the standard errors from a normal probit or logit may be underestimated by more than 50% (Beck, Katz, and Tucker 1998). We control for temporal dependence using a third-order polynomial time counter t, t^2, and t^3, where t counts the period that observation represents in each series (Carter and Signorino 2006).

Our hypothesis about violent dissent suggests a conditional effect of Voice and Freedom of Expression on the termination of torture; as a result, we included in our initial survival model two interaction terms composed of Voice and Freedom of Expression, each multiplied by Violent Dissent. To our surprise, those models would not estimate because the matrices were not positive definite. Closer inspection of our data revealed why: even fewer states terminate their use of torture when they are under the threat of violent dissent than expected—regardless of their institutional composition. Failure to estimate a model is rarely support for a hypothesis, but in this case it provides considerable support for the violent dissent hypothesis. Why? Inspection of the data revealed that only seven spells were terminated when states faced violent dissent![18] This finding explains why we cannot estimate the model with interaction terms: when we parse the data in this manner, there are a number of empty cells and thus no variation to explain.

Although we are unable to run analyses on our conditional hypotheses, we are able to estimate an additive discrete time-duration model that controls for violent dissent as a covariate. Though the model is not a proper

specification given our hypotheses, in the interest of providing information, the results from that logit model are reported in the online appendix. Two results from that model merit note here. First, Violent Dissent has a negative and highly significant effect on the probability that a state terminates its use of torture in a given year. Second, not one of the democratic institutions has a significant effect on the probability of a state terminating its use of torture. In fact, outside of GDP, which is a control variable, only Naming and Shaming has a statistically significant impact. That result is interesting because the sign is in the opposite direction of the hypothesized effect. We suspect that this is likely best explained by the stickiness of the torture spells: since 93% of the cases that torture in year t also torture in year $t + 1$, there are lots of countries to "name and shame." If greater effort is directed at the most egregious offenders and those countries are least likely to stop their use of torture, this is the result one would anticipate. We do not interpret this result as evidence that "naming and shaming" campaigns do not work. It does suggest that they do not systematically play a role in the termination of torture. Returning to the nonsignificant variables, these results suggest that if the variables do have an effect on the termination of torture, it must only occur once violent dissent has been eliminated.

States *Not* Facing Violent Dissent

The previous analysis strongly demonstrates that during the final 20 years of the twentieth century, so few states terminated their use of torture when they were faced with violent dissent that no systematic relationships with liberal democratic institutions exist. Should we reject our agency model of the impact of liberal democratic institutions on an executive's likelihood of putting in place programs and institutions that will stop torture? We submit that doing so is premature. Because so few states terminate their use of torture when they are faced with violent dissent, we now test our conditional hypotheses in an alternative fashion. Once the threat of violent dissent has ended, do executives governing within liberal democratic institutions have a greater incentive to stop the torture than those governing outside of such institutions?

To determine the effect of democratic institutions in states where a once-salient threat has dissipated, we reestimated the discrete time-duration model using a different set of spells that accounts for competing risks. In this data set spells begin when a state that faced violent dissent *and* used torture at time $t - 1$ uses torture again at time t but no longer faces violent dissent at time t. Constructing the spells this way allows us to study the following

[18]The cases are Angola (1985), Greece (1990), Mali (1992), Thailand (1983), and the United Kingdom (1982, 1986, 1988). Mali and Thailand terminated the use of torture in the year in which they transitioned from autocracy to democracy. The termination of torture in Greece coincided with a change in executive leadership. That leaves only four cases—Angola (1985) and the United Kingdom (1982, 1986, 1988)—in which executives under violent dissent terminated the use of torture for reasons other than leadership or regime change.

question: once violent dissent stops, do our hypotheses hold? Spells conditioned on the termination of violent dissent can end in one of two ways: with the termination of torture in a given year *or* with the reemergence of violent dissent and continued torture. Consequently, in our competing risk design, states can now fall out of our risk set by (1) terminating their use of torture in a given year or (2) facing another violent threat and continuing to torture. Because our dependent variable is no longer dichotomous, we test our hypotheses using multinomial logit (MNL), continuing to control for time dependence using a third-order polynomial time counter (Carter and Signorino 2006). The dependent variable is coded "1" if a state that no longer faces violent dissent terminates its use of torture in a given year, coded "2" if the threat of violent dissent reemerges within a state, and coded "0" if the country continues to engage in torture absent violent dissent.[19] Both the termination of torture—given the absence of violent dissent—and the reemergence of violent dissent are repeated events; states can thus engage in multiple "spells" of torture.

The new data cover the years 1986–99, and there are 417 country-years in the data. The data include 64 countries, and there are a total of 95 spells of torture given an absence of violent dissent. Many spells were dropped from these data because they either were experiencing violent dissent, or because they never experienced violent dissent—a necessary condition to enter the risk set. Twelve of the 95 spells (13%) end because the state terminates its use of torture for at least one year. The covariates from the aforementioned logit model are only specified to explain why states terminate their use of torture; consequently, we include controls in the MNL model to account for the fact that a spell can now also end with the reemergence of threat. The controls are based upon Hegre and Sambanis's (2006) sensitivity analyses of the empirical civil war literature. *GDP Growth* measures a country's annual percent change in gross domestic product in a given year (Fearon and Laitin 2003). *Military Personnel* measures the number of military personnel within a country in thousands.[20]

Table 1 provides results from the competing risk discrete time duration model. We discuss the substantive

[19] There are no cases in our data where a country stops using torture *and* violent dissent begins again.

[20] Hegre and Sambanis (2006) find several other indicators to be robustly significant in the civil war literature: country population, country wealth, institutional instability, rough terrain, and a dummy variable for undemocratic regions. Country wealth and population are included in our analyses. We exclude institutional instability because it lacks variation in our sample. We also exclude controls for rough terrain and undemocratic regions because they are cross-sectional measures; we are primarily interested in temporal variation.

TABLE 1 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)
	[51.384]	[2.722]
Veto$_t$	−5.468*	−0.677
	(2.989)	(1.460)
	[0.004]	[0.508]
Freedom of Expression$_t$	4.559***	0.799
	(1.407)	(0.675)
	[95.529]	[2.223]
Naming and Shaming$_{t-1}$	−0.380	0.968
	(0.587)	(0.689)
	[0.684]	[2.632]
NGOs per capita$_t$	0.227	0.011
	(0.236)	(0.065)
	[1.255]	[1.011]
Threat to Leader Tenure$_t$	−0.534***	−0.019
	(0.173)	(0.049)
	[0.586]	[0.981]
Civil Law$_t$	−3.274***	−0.388
	(1.172)	(1.339)
	[0.038]	[0.679]
GDP$_t$	−0.151	−1.982**
	(1.206)	(0.866)
	[0.860]	[0.138]
Population$_t$	0.312	−0.900
	(1.238)	(0.824)
	[1.367]	[0.407]
GDP Growth$_t$	25.972***	3.551
	(6.003)	(6.749)
	[$1.90e+11$]	[34.858]
Military Personnel$_t$	0.514	1.035**
	(0.644)	(0.514)
	[1.672]	[2.814]
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.

effects below, but begin by observing that the results provide considerable support for our hypotheses: all three democratic institutions exhibit the expected effects and the estimate for our proxy variable for Inquisitorial Criminal Justice system is signed as expected. Our estimate of the risk to tenure in office also has the expected impact on the likelihood of terminating torture given the absence of violent dissent. Our strength of civil society measure and our Naming and Shaming variable each fail to produce statistically significant estimates. Finally, GDP Growth, which was included as a control for the return of violent dissent, produces a substantively large, statistically significant impact. We do not discuss the impact of the covariates on the return to violent dissent given that the state tortures and there was past violent dissent (listed as "Threat Returns" in Table 1). GDP and Military Personnel produce statistically significant, and substantively sensible, impacts on that likelihood.

To study the substantive effects of our covariates, we examine the relative risk ratios (RRR; reported in square brackets in Table 1) and selected predicted probabilities (based on simulation) of observing a spell ending with the termination of torture. Relative risk ratios range from 0 to ∞ and are the exponentiated values of coefficient estimates. They indicate the change in the risk of observing the outcome of interest relative to the baseline category. A value of 1 indicates no change in the risk of observing the outcome relative to the baseline. Values between 0 and 1 indicate a negative relationship, and values greater than 1 indicate a positive relationship. We use two tables to communicate the predicted probabilities. In Table 2, we report the predicted probability of termination across several different scenarios. Our purpose here is to draw attention to the impact of the different democratic institutions: Voice, Freedom of Expression, and Veto. The results reported in Table 3 focus attention on the impact of our measure of inquisitorial criminal justice systems: states with a civil legal system that are no longer facing violent dissent have a dramatically lower probability of terminating the use of torture than states that do not have a civil legal system.

To illustrate the substantive impact of the three liberal democratic institutional variables in our analysis, consider the relative risk ratios reported in Table 1 for each: Voice = 51.384; Freedom of Expression = 95.529; Veto = 0.004. These are very large substantive effects. A country with competitive elections has a 50% greater risk of terminating torture (compared to continuing to torture) than a country without elections. A one-unit increase in the freedom of the press increases the likelihood that a country terminates its use of torture (relative to continu-

ing to torture) by almost 95%. More explicitly, a country moving from restricted to partly free, or partly free to free is nearly 95% more likely to terminate its use of torture; one that moves from restricted to free is 190% more likely to terminate its use of torture (relative to continuing to torture). Finally, Veto is a continuous measure that empirically does not cover the full range between 0 and 1, thus limiting the usefulness of the relative risk ratio because such a change is out of sample. Nevertheless, a hypothetical change from a state with no veto points to one with the maximum possible number of veto points is 99.6% less likely to terminate the use of torture (relative to continuing to torture).

We also find support for the expectation that inquisitorial criminal justice systems are more likely to be associated with torture: a country with a civil legal system is 96% less likely to terminate its use of torture (relative to continuing it) than one without such a system. This too is a surprisingly strong impact. Finally, a one-unit increase in Threat to Leadership Tenure produces a 42% decrease in the likelihood of terminating torture (relative to continuing). The variable we use to measure the threat to leadership tenure is a coefficient estimate from a Weibull regression (see Cheibub 1998) and ranges from under one to over 30. As such, we are hard pressed to substantively interpret the impact of a one-unit increase. It is properly signed and whatever the units, a decrease of over 40% given a one-unit change is nontrivial.

While relative risk ratios provide a useful summary of the substantive impact of variables, it is also useful to examine the predicted probabilities of different values of the independent variable. To conserve space rather than present several tables of predicted probabilities, we have produced Table 2, which reports a considerable amount of information in a compact space. Four variables are of particular interest: Voice, Freedom of Expression, Veto, and Criminal Justice System. Voice and Criminal Justice System take only two values, Freedom of Expression can take three values, and Veto is continuous over the 0–1 interval. To structure the table, then, we produced two columns, each of which represents one of the two values from Criminal Justice System. The row entries in each column are composed of the set of values across the remaining three variables. There are 18 rows, each broken into blocks of six which represent the value of Freedom of Expression, which is listed in the first column. The second column indicates the value of Voice: the top three rows of each block represent cases without elections, and the bottom three rows of each block represent cases with elections. Finally, the third column records the value of Veto, which varies over

TABLE 2 Substantive Effects of Selected Variables on the Termination of Torture

Free Expression	Voice	Veto	Civil Law	Not Civil Law
None	No	Minimum	0.00003	0.00020
			(6.80e-10, 0.00011)	(3.67e-8, 0.00119)
		Median	0.00002	0.00015
			(2.88e-12, 0.00004)	(1.97e-10, 0.00072)
		Maximum	0.00002	0.00017
			(3.92e-13, 0.00003)	(2.31e-11, 0.00063)
	Yes	Minimum	0.00056	0.00260
			(1.26e-8, 0.00196)	(1.17e-6, 0.01842)
		Median	0.00022	0.00072
			(4.12e-10, 0.00051)	(3.11e-8, 0.00513)
		Maximum	0.00015	0.0005005
			(6.46e-11, 0.00037)	(4.21e-9, 0.00362)
Some	No	Minimum	0.00015	0.00233
			(9.64e-8, 0.00096)	(4.78e-6, 0.01681)
		Median	0.00007	0.00128
			(2.11e-9, 0.00039)	(1.06e-7, 0.0119)
		Maximum	0.00007	0.00138
			(2.91e-10, 0.00033)	(1.20e-8, 0.01029)
	Yes	Minimum	0.00971	0.07516
			(3.18e-6, 0.08413)	(0.00017, 0.56466)
		Median	0.00199	0.01801
			(1.76e-7, 0.01130)	(0.00001, 0.14405)
		Maximum	0.00120	0.01104
			(3.46e-8, 0.00600)	(2.02e-6, 0.09782)
Full	No	Minimum	0.00711	0.09967
			(0.00002, 0.05280)	(0.00034, 0.65921)
		Median	0.00210	0.03800
			(5.60e-7, 0.01276)	(0.00001, 0.39229)
		Maximum	0.00157	0.02798
			(8.25e-8, 0.01117)	(1.85e-6, 0.28874)
	Yes	Minimum	0.18700	0.50982
			(0.00014, 0.92800)	(0.00293, 0.99658)
		Median	0.05200	0.26288
			(0.00002, 0.46300)	(0.00048, 0.94104)
		Maximum	0.02900	0.16977
			(4.78e-6, 0.28700)	(0.00012, 0.88513)

Notes: Cell values are the simulated predicted probability for the joint-in-sample values depicted across the first three columns given the criminal justice system (holding all other variables at their means). 95% confidence intervals are given in parentheses (lower bound, upper bound).

its joint-in-sample minimum, median, and maximum values,[21] across each set of three rows.

[21]By joint-in-sample values we are referring to the range of each variable given the selected value of the other variables. King and Zeng (2006) present a formal treatment of the convex-hull. Our approach is a less thorough effort to produce counterfactual analysis that is connected to the observed data: rather than calculate the joint-in-sample values for every variable in the model, we cal-

Before turning to comparing across the independent variables, we should note for comparison that the unconditional probability that one of the spells in this data set ends because the state terminates its use of torture is 0.028. Only four sets of values produce double-digit

culate the joint-in-sample values for our three liberal institutional variables and the criminal justice system variable while holding the other variables' values at their mean.

TABLE 3   Substantive Effect of Three Variables on the Termination of Torture

Values of Independent Variables	Difference in Pr(Torture Terminates)
Low Voice, Veto, Expression	0.1871
Civil Law Countries	(0.0001, 0.9284)
Low Voice, Veto, Expression	0.5097
Non-Civil Law Countries	(0.0029, 0.9964)

Notes: Differences above compare changes from the estimates in the table to a baseline predicted probability for a democracy with low veto points (calculated with maximum joint-in-sample values on Voice and Freedom of Expression and minimum joint-in-sample values on Veto). All other variables are held at their means. 95% confidence intervals are given in parentheses (lower bound, upper bound).

predicted probabilities, and they all involve Full Freedom of Expression and Elections (see the final three rows of Table 2). We briefly highlight how each of the independent variables changes the predicted probability given the values of the other independent variables.

We begin by considering the substantive impact of Voice, which has a relative risk ratio (RRR) of 51. A comparison of the sets of values across the rows representing the absence and presence of Voice (i.e., elections) indicates that its positive impact is effectively meaningless in civil law systems, except in countries with a fully free press and the joint-in-sample minimum number of veto points. In that case the predicted probability is 0.187 (see the fourth row, left column of the third block of rows), which represents a large change from the baseline probability of 0.028. Once one moves to the median value of Veto (down one row), however, the positive impact becomes sufficiently small to be considerably less interesting. Compare the predicted probabilities in the column to the right: in criminal justice systems that do not use the inquisitorial approach, Voice does not have a substantively appreciable impact in the absence of Free Expression (the first block of six rows), nor much of one at at middle levels of Free Expression (the second block of six rows). When the press is unrestricted (the third block of rows), elections produce a substantively large positive impact regardless of the level of Veto. The predicted probability in non-Civil Law countries with a free press but no elections ranges from 0.03 to 0.09 (the first three rows in the third block), depending on the level of Veto. The predicted probability ranges from ∼0.17 to ∼0.51—depending on the value of Veto—in countries that hold elections (see the fourth through sixth rows of the third block).

To summarize, the minimalist (or classic) democratic institution—elections—has a nontrivial positive impact

on the likelihood that a country that is no longer facing violent dissent will stop the use of torture. But the substantively large positive effect is largely restricted to countries that do not have a civil law system and also fully respect freedom of the press. While a number of studies have found that elections enhance physical integrity rights (Abouharb and Cingranelli 2006, 2007; Davenport 1998; Zanger 2000), others have reported that elections are an ineffectual institution for ensuring protection from physical abuse (Richards 1999). Like the work by Davenport and Armstrong (2004) and Bueno de Mesquita et al. (2005), our findings suggest that the impact of elections is contextual (Davenport 2007b, chap. 2). When it comes to halting the use of torture, elections generally have a positive, but trivial, impact *except* in countries with a fully free press *and* a noninquisitorial criminal justice system.

What of Freedom of Association as measured with our press freedom indicator? To get a sense of its impact on predicted probability, compare the sets of cell entries for a country with no freedom of the press to those entries in countries with some freedom and finally in countries with full freedom of the press. Again, note the difference across the Civil Law columns (we discuss that variable below). Despite its large RRR (95.53), we observe that in countries likely to have an inquisitorial criminal justice system, the impact of freedom of expression is positive but not terribly meaningful except in those countries that have elections and a minimal number of veto points (compare the value in the fourth row of the third block, left column, with the other values in the column). Interestingly, however, full press freedom has a rather robust positive effect in countries likely to have a noninquisitorial criminal justice systems (third block, right column): regardless of the level of Veto, the predicted probability when Voice is high ranges from a minimum of ∼0.17 to a maximum of ∼0.51. Given that the unconditional probability that a spell in this data set terminates due to stopping torture is ∼0.03, this is a remarkable result! We argue that Freedom of Expression should be important because it allows reporters to search for examples of government corruption and malfeasance. Executives, legislators, and judges recognize that this is the case and adjust their behavior accordingly. The institution of a free press thus leads to an increased incentive for executives to make efforts to stop torture and address the agency problems that challenge officials who wish to change the status quo.

Let us now turn our attention to Veto. Compare within and then across the three blocks of rows and note that regardless of which three Veto rows one wishes to examine, the probability of torture drops as one moves from the minimum joint-in-sample value to the maximum. That is the pattern our Veto hypothesis leads us

to expect. That said, note that in many of those sets of rows the predicted probability is so low that the decrease in probability as one adds veto points is not of much substantive interest.

We argue that an agency analysis is likely to yield insight to why states might terminate the use of torture among their interrogators and jailers. Tsebelis (2002) argues that veto points are positively associated with status quo policy. In our study, torture is the status quo, and we expect veto points to make it more difficult to change. Although some scholars view checks on executive authority as antidemocratic because they are associated with gridlock that can frustrate the implementation of voting majorities' favored policies, others view these constraints as a positive attribute of liberal democracies because they should reduce the "violence of majority faction" (Hamilton, Madison, and Jay [1788] 2009, # 10). Whether viewed as a constraint on, or an enhancement of, democracy, veto points appear to be difficult to overcome with respect to stopping the use of torture. Although space constraints prohibit us from reporting the results here, we encourage the reader to consult the online appendix where we use *this sample of countries with torture spells and no violent dissent* and replicate the finding that judicial independence is positively associated with respect for physical integrity rights. Yet, as the results above indicate, Veto points are negatively associated with the likelihood of terminating a torture spell, even in a sample where the state no longer faces violent dissent.

As can be gleaned from the discussion above, the predicted probabilities in Table 2 indicate that countries with civil law systems are considerably less likely to terminate the use of torture than countries without civil law systems (compare across the left and right columns). Every entry in the Civil Law column is at least one order of magnitude lower than the corresponding row entry in the Not Civil Law column. To summarize our findings we computed the change in predicted probability across two scenarios and report them in Table 3. These changes in predicted probabilities allow us to examine the impact of Voice and Freedom of Expression *across criminal justice systems* while holding Veto and the other variables constant. We set Veto to its lowest joint-in-sample value and change Voice and Freedom of Expression from their minimum to their maximum values. The first row of the table reports the *change* in predicted probability in a country with a Civil Law system as one shifts from minimum to maximum on Voice and Freedom of Expression. The second row represents a country without a Civil Law system. Moving from a country with neither Elections nor Press Freedom to one with both institutions (holding Veto at its lowest joint-in-sample value) increases the probability of

terminating the use of torture by a relatively small 0.19 in countries with a Civil Law system, and by an eye-popping 0.51 in a country without such a system!

The results reported in Table 3 highlight where activists might concentrate different types of antitorture campaigns. In countries with civil law legal systems, elections, and strong press freedoms, activism ought to be targeted at ensuring that freedom of expression is not eroded and that candidates running for office are pressured to respond to questions about training programs to stop the use of torture. Activists in countries with larger numbers of veto points should direct critical media-friendly campaigns at stakeholders who are likely to frustrate reform efforts. In similar countries with inquisitorial criminal justice systems, activists might want to invest their efforts in producing legal protections for people incarcerated by the state. Our results suggest that until that situation is improved, progress on other dimensions is likely to have a relatively limited effect on the termination of torture.

It is important to stress that all of our results are strictly conditional on the absence of violent dissent; our research design excludes all country-years where torture takes place during a period in which dissidents use violence to challenge the state. They are also relative in that there is a competing risk: dissidents might terminate a spell by returning to the use of violence. This is the most depressing finding from an activist standpoint. The silver lining is that our study says nothing about *nonviolent* dissent. One way in which our results can be interpreted is to take from it a main message of the nonviolent direct action movement: violence begets violence. If dissidents want to live in a world where human rights are respected, then dissidents need to respect the rights of those in government they wish to challenge and/or displace.

Discussion

When we began this study, we expected the negative effect of violent dissent to attenuate any positive effect of democratic institutions on the likelihood that a state terminates its use of torture. The effect of violent dissent is even more severe than we expected; democratic institutions have little effect on the likelihood that states terminate their use of torture when they are faced with a threat. When violent dissent stops, democratic institutions do matter. Increases in Voice and Freedom of Expression increase the likelihood that a state terminates its use of torture. When state power is checked by the people, officials are more likely to implement training programs to stop the torture. One staple of liberal democracy—checks on executive authority—serves to uphold the status quo and

limits the likelihood that states stop the torture. Perhaps most importantly, criminal justice systems that use an inquisitorial approach are considerably less likely to terminate the use of torture, even in the absence of violent dissent.

We wish to make three parting observations. First, that torture is "normal" in the sense that it is commonly used as a tool of statecraft is not yet widely appreciated. Human rights activists are very aware of this fact, but policy makers, politicians, reporters, and the general public in liberal democracies are considerably less informed. Politicians and pundits speak in a highly moralistic, binary language that suggests that because torture is abhorrent it is unusual. While it is indeed abhorrent, it is not unusual. Researchers need to better establish this basic fact so that it is widely appreciated beyond activists and human rights researchers.

Second, along with the considerable institutional literature on international organizations (e.g., Koromenos, Lipson, and Snidal 2001; Milner 2005) and human rights in particular (e.g., Goodliffe and Hawkins 2006; Hawkins 2004; Simmons 2009; Vreeland 2008), as well as the literature on democracy and human rights (e.g., Apodaca 2004; Cingranelli and Filippov 2010; Davenport 2007b; Keith 2002; Landman 2005; Poe and Tate 1994; Richards and Gelleny 2007), this study suggests that studying the impact of domestic political institutions on the incentives of politicians to either ignore or respect the human rights of their citizens has considerable theoretical merit. While our theoretical models are very partial, and our data are rough and certainly less than we would like them to be, there is nonetheless considerable cause for optimism about growth in our understanding. We can confidently say that we know more today than we did even 10 years ago; broad, brush-clearing sorts of efforts have begun to give way to more contextualized analyses that are uncovering systematic patterns that more readily admit to policy implications.

Finally, the international movement to actively stop the use of torture by demanding institutional training, monitoring, and sanctioning programs is relatively new. International NGOs are working with national ones to strengthen the CAT and its more recent Optional Protocol (OPCAT). OPCAT is interesting because it requires state parties to create independent domestic institutions that have training, monitoring, and sanctioning authority. Our study's findings with respect to Veto suggest that this is especially interesting because it will create actors with both the authority (and, hopefully, the budget) to challenge stakeholders who block reform. When one takes into consideration states transitioning from an authoritarian past, our Veto finding and the implementation

of the OPCAT become especially interesting. We will be delighted should this study contribute to research that addresses these issues.

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