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The Inefficacy of Constitutional Torture Prohibitions

Adam S. Chilton[*] *& Mila Versteeg*[†]

December 17, 2014

Abstract

The prohibition of torture is one of the most emblematic norms of the modern human rights movement, and its prevalence in national constitution has increased steeply in the past three decades. Yet little is known about whether constitutional torture prohibitions actually reduce torture. In this paper, we explore the relationship between constitutional torture prohibitions and torture by utilizing new data that corrects for biases in previous measures of torture, and a recently developed method that mitigates selection bias by incorporating information on countries' constitutional commitments into our research design. Using this new data and method, as well as more conventional data sources and methods, we do not find any evidence that constitutional torture prohibitions have reduced rates of torture in a statistically significant or substantively meaningful way.

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JEL Classifications: K00, K3, C1
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1. Introduction

The prohibition of torture is one of the most emblematic norms of the modern human rights movement (Waldron 2010). It is one of the few whose prohibition under international law is absolute even in times of emergency (see CAT, art. 2.2), that binds all states as a matter of customary international law (Third Restatement 1987, ¶ 702) and which has gained the status of a "jus cogens" norm (Henkin 2009). Torture is absolutely prohibited, not just by international human rights law (CAT, art. 2; UDHR, art. 5), but also by international humanitarian law (*see, e.g.,* Geneva Convention 1, art. 50) and by international criminal law (Rome Statute, art. 7.1.f). Moral philosophers have invariably condemned torture, widely regarding it as a "moral and legal abomination" (Waldron 2010, 4), and describing it as a "crime morally worse than killing" (Shue 2004, 47), "the sort of evil that arouses human rights passions and drives human rights campaigns" (Waldron 2010, 4, 253; Scarry 1987) or an act that is on the "never list" of "forbiddens" in international politics (Elshtain 2004, 77).

Despite being near-universally condemned, torture remains common in many countries, including in democracies with otherwise strong human rights records (Goderis and Versteeg 2012; Davenport, Moore and Armstrong 2007; Conrad and Moore 2010; Wantchekon and Healy 1999; Ron 1997; Rejali 2007). Of the 107 democracies in existence in 2011, 40 frequently tortured their citizens, and another 41 engaged in occasional torture. Only 26 democracies refrained from torture altogether (Cingranelli and Richards 2012). Even in countries known for proclaiming the importance of civil and political rights, reports of torture have surfaced; places such as Guantanamo Bay, Belmarsh Prison, and Abu Ghraib have made headlines around the world. Most recently, the world has been shocked by the release of the U.S. Senate's "Torture Report," that describes the gruesome details of the C.I.A.'s secret enhanced interrogation program carried out in secret prisons in foreign countries (U.S. Senate 2014). Although philosophers have condemned torture as a "barbaric remains of the middle ages" (Ishay 2008, 88), torture appears to be common practice around the world till this day.

The strong international commitments to ban torture have done little to stop such practices. A body of empirical research has shown that the Convention Against Torture

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and Other Forms of Cruel, Inhuman and Degrading Treatment (CAT) has not reduced torture. As of June 2014, 155 countries had ratified the CAT, but the treaty appears not to have reduced the actual prevalence of torture in the countries that have joined it. Some empirical studies find that the incidence of torture is unaffected by CAT ratification (Lupu 2013b; Powell and Staton 2009), while others find that CAT ratification is actually associated with increased torture (Hill 2010; Hafner-Burton and Tsutsui 2005; Neumayer 2005; Hathaway 2004; Holleyer and Rosendorff 2011; Vreeland 2008). Also the international norms regarding torture (measured as the proportion of states that ratified CAT) have not reduced torture (Gillian and Nesbitt 2009). Although many hoped the CAT would bring an end to torture, the balance of evidence produced by this literature suggests that it has failed to even marginally reduce the incidence of torture.

While a dozen papers have explored the CAT's impact on torture, much less is known about the impact of constitutional torture prohibitions on the incidence of torture. This oversight is not confined to the study of torture alone: we know remarkably little about the ability of constitutions to actually prevent human rights abuses. While at least sixty published papers empirically explore the impact of international human rights law, only a handful of studies explore the impact of constitutional rights.[3] Moreover, the existing studies on constitutional rights are often based on small samples, do little to address endogeneity, and have yielded mixed results (Chilton and Versteeg 2014). At the same time, one recent study suggests that the predictive value of constitutional protections might be larger than that of most of the usual-suspect determinants of state repression, and concludes that the comparative constitutional design literature "deserves more attention" in future studies of state repression (Hill and Jones 2014).

One theoretical grounds, there is reason to believe that constitutional prohibitions might constrain torture, even where domestic democratic institutions and international constraints fail to. Unlike treaties, constitutional prohibitions are typically guarded by a judicial body equipped with the power of judicial review, are entrenched against easy revision, and are included in a document that contains the state's basic "operating manual," making the prohibitions hard to ignore altogether. Constitutions, it is thought,

[3] According to our knowledge, the following is an exhaustive list of empirical studies that have explored this question: Pritchard 1986; Davenport 1996; Cross 1999; Keith 2002; Keith, Tate, and Poe 2009; Fox and Flores 2009; Melton 2013; Chilton and Versteeg 2014.

make it harder for popular majorities to renege on the constitution's promises, and therefore have the potential to actually reduce torture in practice.

Although the question whether constitutional torture prohibitions have reduced incidents of torture is an important one, two obstacles have made it all but impossible to test empirically. First, all the available data sources on incidents of torture have been shown to be systematically biased because reporting standards have varied over time and between countries (Farris 2014). Second, there is inherently selection bias because the decision to enshrine a right against torture is systematically related to torture practices (Chilton and Versteeg 2014). Although both problems are substantial, we attempt to overcome both by using new data on state repression that corrects for reporting biases and a recently developed identification strategy that mitigates selection bias by incorporating previously unobserved information on countries' general preferences for constitutional rights into the research design.

In terms of data, we rely on a new dataset recently developed by Farris (2014) that corrects for biases in other sources of data measuring human rights practices. Farris' data do so by using a Bayesian measurement model that accounts for changing standards of accountability. Fariss' model incorporates data from 13 different data sources. His model specifically includes "standards-based" data sources (like the CIRI torture data that measures states level of torture on a 3-point scale) and "events-based" data sources (like whether a country experienced a genocide in a given year). By incorporating these measures into a dynamic measurement model, Fariss is able to generate "unbiased estimates of repression" (Fariss 2014, 297). In addition to using Farris' new "Latent Repression" data, we also rely on five other previously used indicators that explicitly measure torture.

We deal with selection bias by employing a new identification strategy that builds information on pre-existing constitutional rights preferences into the research design. Specifically, we use data on rights included in every constitution between 1946 and 2012 to calculate countries' yearly constitutional "ideal points", and then match both on the probability of a country's constitutionally prohibiting torture and on a set of standard observables (Lupu 2013a; Lupu 2013b; Chilton and Versteeg 2014). Using this data and method, we find no evidence that constitutional torture prohibitions reduce the prevalence of torture. These findings remain the same also when we use more conventional methods. In fact, we fail to find a statistically significant relationship between constitutional torture prohibitions and lower rates of torture in all 65 regression models we report based on six different torture indicators, as well as dozens more unreported robustness checks.

The remainder of this paper is organized as follows. Part 2 explains why constitutional incorporation of torture prohibition might reduce the incidence of torture, but also suggests two reasons for why such provisions might fail in practice. Part 3 describes our data and our main empirical strategy. Part 4 reports our core findings and presents a number of robustness checks that support our results. Part 5 concludes.

2. The Impact of Constitutional Torture Prohibitions

Scholars have only recently taken up the question what, if anything, prevents torture (Conrad and Moore 2010). By now, we know that the Convention Against Torture has not actually halted torture, even though many countries have ratified it (Lupu 2013; Powell and Staton 2009; Hathaway 2004; Holleyer and Rosendorff 2011; Vreeland 2008). Another notable finding from the existing literature is that democratic constraints, such as democratic elections, freedom of expression, or the existence of veto players, fail to reduce torture when democratic governments are faced with terror threats or violent dissent (Davenport, Moore and Amstrong 2007; Conrad and Moore 2010). This finding stands in sharp contrast with the literature on government repression, which has consistently found that democracy reduces general state repression (e.g. Davenport 1999; Davenport 2007; Davenport and Armstrong 2004; Poe and Tate 1994; Richards 1999; Richards and Gelleny 2007; Bueno de Mesquita et al., 2005; Hafner-Burton, Hyde and Jablonski, 2014; Dallin and Breslauer, 1970; Gartner and Regan, 1996), an insight sometimes referred to as the “democratic civil peace” (Hegre et al. 200). When it comes to torture, democratic safeguards turn out to be less effective. Wantchekon and Healy (1999) show theoretically that all governments, including democratic governments, have an incentive to torture when they believe that doing so yields useful information. A number of studies show empirically that democratic governments do torture in the face of terrorism (Goderis and Versteeg 2012; Conrad et al. 2014) and that democratic safeguards, such as elections and freedom of expression, do little to reduce torture in the face of violent dissent (Davenport, Moore and Amstrong 2007; Conrad and Moore 2010). It is only the absent of dissent or terror threats that some democratic institutions reduce torture somewhat (Conrad and Moore 2010).

One question that has not yet been systematically answered is whether constitutional torture prohibitions reduce torture. This oversight is surprising considering that constitutions' enforcement mechanisms are typically stronger than that of CAT, which consists of monitoring by a treaty body whose reports and decisions only have the status of soft law. What is more, constitutions offer constraints that are counter-majoritarian in nature and might thus be more successful in reducing torture than ordinary democratic safeguards.

Despite their theoretical importance, only two studies empirically address constitutions' ability to reduce torture, and both do so only passing, as part of a larger project on constitutional constraints (Melton 2013; Keith, Tate and Poe 2009). Neither one of these studies offers an in-depth exploration of torture prohibition's effect on de facto torture.[4] The lack of attention for constitutions in the political science literature on the determinants of torture stands in stark contrast with the legal literature. Law professors have spilled a substantial amount of ink over the question whether constitutional constraints do (or should) halt torture, although they typically lack the tools to test their claims (see, e.g., Levinson 2002; Cole and Dempsey 2006). Our study bridges these two bodies of literature and provides a first comprehensive systematic exploration of the ability of constitutional torture prohibitions to actually reduce torture in practice.

2.1 Constitutions' Constraining Power

Constitutional torture prohibitions have proliferated in recent decades. According to our data, 84 percent of the world's constitutions prohibit torture today, compared with 39 percent in 1946. England's Bill of Rights Act of 1689 was the first human rights

[4] Our study differs from these existing studies in two ways: (1) we focus specifically on torture prohibitions, and not a range of rights; (2) we use a wider range of data to measure rates of torture; and (3) we use a number of different empirical strategies to test the effect of constitutional torture prohibitions.

document to state that "excessive bail ought not to be required, nor excessive fines imposed, nor cruel or unusual punishment inflicted." The American founders likewise prohibited cruel and unusual punishment in the Eighth Amendment to the U.S. Constitution. Such constitutional torture prohibitions might have been adopted for a variety of reasons. They can be the product of moments of "higher law-making" in which individuals transcend their ordinary self-interest and commit to higher values that serve the common good (Ackerman 1991). However, they might also have been added for less genuine reasons to deceive both domestic and international groups (Law and Versteeg 2014) or to signal conformity to international norms. Indeed, banning torture has been regarded a sign of modernity and civilization (Waldron 2010), and by constitutionally abolishing torture, states can signal that they are a modern civilized nation (Meyer et al. 1997), even when they have not actually internalized these norms (Goodman and Jinks 2004).

Regardless of why they are adopted, conventional constitutional theory suggests that torture prohibitions could have real consequences for future democratic politics. Constitutions are commonly regarded as pre-commitment devices that make it harder for democratic majorities to renege on the constitution's promises in the future (Elster 1984). Indeed, one of the foremost purposes of constitutions is to protect minorities from the majority, or to prevent the so-called "tyranny of the majority," and to constrain popular majorities based on the constitution's higher values.

Constitutional theorists have pointed at different mechanisms that raise the costs for popular majorities of reneging on their constitutional promises. First, the constitution is usually declared to be the supreme authority of any legal system and is typically harder to amend than ordinary legislation. In order to amend the constitution, an ordinary democratic majority will not suffice: a super-majority is typically required. Second, the constitution's rules and principles are commonly justiciable, meaning that courts can strike down democratic legislation that contradicts the constitution. In that sense, courts can act as counter-majoritiarian constraints that hold popular majorities to their constitutional pre-commitments (Conrad, Hill and Moore 2014). What is more, by striking down legislation and hearing cases, courts can act as a "fire alarm" that alerts political opposition groups to rights violations (Hirschl 2000).

Third, the constitution helps a state to coordinate upon a set of conventions that clarify the rules by which different actors must play the political game (Hardin 2013). Just as drivers must drive on the same side of the road to avoid crashes, a government must agree on how to appoint its officials or where to locate its capital city to prevent political chaos (Hardin 2013). Even when political actors do not value a particular constitutional rule, they value the constitution as a bundle of rules, which facilitate better government coordination. Because of the constitution's coordination benefits, it becomes more costly to change or disobey the constitution's rules, even the less unpopular ones. As Hardin (2013) points out, Americans ultimately acquiesced in the Bush v. Gore Supreme Court Decision exactly because they did not want to undermine the constitution, even though a majority of people had voted for Gore. Ignoring the prohibition of torture might likewise undermine the constitution's coordination benefits as a whole, and deprive a country of its basic operating manual. In sum, through mechanisms such as entrenchment, judicial review and coordination, constitutional promises become harder to break than non-constitutional ones. While different constitutional theorists emphasize different mechanisms of constraint, they share a common belief that constitutions make it harder for popular majorities to deviate from the constitution's promises ex post.

If constitutions indeed raise the costs of reneging on the constitution's promises, we would expect that countries whose constitutions prohibit torture are less likely to torture than countries whose constitutions do not include such prohibitions. Where countries want to respond to terror threats with torture, the aforementioned constitutional mechanisms may make it harder for the government to do so. In sum, because their constitutions provide a set of mechanisms that raise the cost of torture, states with constitutional torture prohibitions might torture less than states without such prohibitions.

2.2 The Limits of Constitutional Constraints

It is also possible, however, that conventional constitutional theory might be too optimistic, especially when it comes to torture prohibitions. In some cases, constitutional commitments might be adopted under false pretenses, in which case they are nothing but empty promises (Law and Versteeg 2013). Especially autocratic nations commonly pay lip service to international norms, without intending to implement them. These are what Beth Simmons calls "false positives," that is, states that commit to human rights on paper without an intention to respect these rights in practice (Simmons 2009, 18). The phenomenon of false positives is well-documented and presents a general challenge for constitutional rights enforcement, especially in authoritarian regimes.

Yet, there is reason to believe that constitutional torture prohibitions might be more prone to failure than other constitutional rights, even in democratic regimes that adopted these prohibitions with good intentions. Indeed, Law and Versteeg (2013, 915) find that torture prohibitions have the lowest compliance rate of the fifteen constitutional rights they study: a mere 12.3 percent of countries that prohibit torture in their constitution actually refrain from torture. By contrast, no less than 71 percent of countries that protect religious freedom actually respect it in practice; and every single country that constitutionally prohibits the death penalty refrains from imposing the death penalty in practice (Law and Versteeg 2013, 915). Moreover, in our own previous work, we find that constitutional rights that establish organizations with the incentives and means to protect the rights that establish them (most notably, trade unions and political parties) do appear to be associated with improved rights practices (Chilton and Versteeg 2014). By contrast, we find no evidence that rights that are practiced on an individual basis (such as the freedom of expression or movement) change government behavior (Chilton and Versteeg 2014). The small empirical literature on constitutional rights effectiveness thus suggests that the impact of a right might depend on its nature.

There are at least two features of constitutional torture prohibitions that make it more prone to abuse than other constitutional rights, even in countries that generally uphold their constitutional commitments. First, it is often possible for governments to torture covertly, which allows them to circumvent the constitutional mechanisms that are supposed to raise the costs of non-compliance. As Davenport, Moore and Amstrong (2007, 2) note "democratic governments have an opportunity to cheat behind closed doors where the public and other actors are not watching." Yonatan Lupu (2013b) has argued that torture's secretive nature hampers the judicial enforcement of torture prohibitions enshrined in international human rights treaties. Even in states with independent judiciaries, torturers often go unpunished simply because their transgressions are not observed (Lupu 2013b). When government agents cover their tracks, it is difficult for torture victims to bring a case in court. Moreover, courts lack the ability to continuously monitor government action. To use McCubbins and Schwartz' (1984) famous analogy, courts can act as a fire alarm, but not as a police patrol. Where cases do not reach courts, courts cannot serve their role as fire alarm. What is more, because legislatures and executives know there exists a possibility of judicial enforcement, they might be incentivized to erase evidence of torture (Lupu 2013b).

In a related argument, Darius Rejali (2007) argues that governments manage to get away with torture by using clean or "stealth" torture techniques. Unlike "dirty" or "scarring" torture (such as beatings, branding, dog attacks, hanging by limbs and sexual assaults), these stealth techniques do not leave any scars on the body (Davenport, Moore and Amstrong 2007, 3-4). According to Rejali, clean torture techniques are increasingly common in democracies: "where free elections have gone, where monitoring agencies have set up shop, and journalist have taken to the streets and airwaves, they have been followed by electric prods and electoshockers, tortures by water and ice, drugs of sinister variety, sonic devices . . . ; the modern torturer knows how to beat a suspect senseless without leaving a mark" (Rejali 2007, 3). Rejali claims that for victims without physical scars, it is harder to present evidence to courts and also harder to gather popular support and sympathy for their suffering. Both Lupu's and Rejali's arguments suggest that, because torture can be done in secret and/or without leaving physical scars, it might be relatively easy for governments to circumvent constitutional bans on torture. Not only are courts unable to act, covert torture also does not undermine the coordination value of the constitution, as there is little evidence that the constitution is ignored in the first place.

A second reason why constitutional prohibitions of torture might fail in practice is that the use of torture actually enjoys fairly widespread popular support, which can potentially undermine even those constraints that are designed to be counter-majoritarian in nature. As Waldron (2010, 6) acknowledges, there is a "great deal of enthusiasm for torture." Opinion poll research suggests that popular support for terrorism increases in times of emergency, such as in the U.S. and its allies after 9/11 (Davis and Silver 2004). One recent poll by the Huffington post found that 47 percent of Americans believe that torture of suspected terrorists with possible information on a terror attack was "always" or at least "sometimes" justified, while only 25 percent held that torture was never justified (Swanson 2012; but see Gronke et al. 2010). Reports from other countries vary. One study finds that a majority of people in India, Thailand, South Korea, Turkey, Iran and Russia believe that torture should be allowed in some circumstances, while a majority of people in twelve other countries believes that torture should never be allowed (Kull et al., 2008). A similar study conducted by Amnesty International finds that a majority of people in China, India, Kenya, Nigeria, Pakistan and Indonesia support torture under some circumstances, while those who support torture are in the minority in 15 other countries. On average, across 21 countries, the Amnesty survey found that 36 percent of all people think that torture should be allowed under some circumstances (Amnesty International 2014). While the numbers differ by country, and likely also vary over time as perceptions of security change (Davis and Silver 2004), they suggest that a substantial portion of people support torture at least some of the time.

Where constitutional rights lack popular support, there is reason to believe that the mechanisms that are supposed to make it harder to renege on the constitution's promises are less effective in practice. Although constitutions are designed to be counter-majoritarian, and supposed to raise the costs of reneging on the constitution's promises, all the constitutional mechanisms that are supposed to do so are ultimately fallible (Levinson 2011). Indeed, this was precisely James Madison's concern when he described the bill of rights as nothing but "parchment barriers" (Madison, 1788). According to Madison, constitutions can prevent the problem of "faction," that is, minorities taking advantage of the majority. But Madison was worried that constitutional rights would not be able to systematically protect minorities from majorities.

Where popular majorities systematically support certain kinds of rights violations, it will be harder for the courts to prevent such violations. While courts can occasionally enforce the constitution against democratic majorities, they will quickly lose their power when they do so too often and/or are too far out of step with popular preferences. Indeed, a body of literature (sometimes referred to as the "regime politics school") suggests that courts are ultimately political actors, embedded in a social and political context, and are not oblivious to the political circumstances surrounding them (Dahl 1957; Epstein et al. 2001; Kegan 2013; Kapiszweski et al. 2013). Where courts are too far out of step with popular majorities, they invite backlash. For example, when the U.S. Supreme Court kept striking down social welfare legislation during the Lochner era, President Roosenvelt responded with a court-packing plan that would alter the composition of the court and bring the court in line with popular majorities. In a remarkably similar episode in India, the Indian parliament responded to a set of counter-majoritarian social welfare decisions by adopting numerous constitutional amendments that overruled the courts' constitutional interpretations (Van der Walt 1999; Mate 2013).

There is reason to believe that courts might be particularly sensitive to political realities in the face of security threats (Posner 2006). In those times, popular support for torture increases and courts do not want to be blamed for the next terror attacks. Although civil libertarian scholars have argued courts should enforce the constitution also in times of emergency, others have argued that courts should defer to the executive (Posner 2006; Posner and Vermeule 2008, 4). Issacharoff and Pildes (2004) observe that, as a practical matter, the latter view has prevailed in the U.S., and that courts do not generally step up against the executive in times of emergency, but merely make sure that the President seeks congressional authorization.[5]

Not only do courts fail to impose counter-majoritarian constraints if support for torture is high enough, under those circumstances the nation may also be able to overcome to cost of re-coordination, and coordinate upon a different set of rules. It could do so de jure, by passing a constitutional amendment or it could do so de facto, by settling upon a convention that deviates from the stated rule. One such convention could be that in times of emergency, some torture is permissible, even where the text of the constitution prohibits it. For example, according to some scholars, nations 'step outside' the constitutional framework in times of emergency, because the constitution is "not a suicide pact"[6] (Gross 2004). As Judge Posner puts it, "only the most doctrinaire civil libertarians (not that there aren't plenty of them) deny that if the stakes are high enough, torture is permissible" (Posner 2004, 295).

These distinctive features of constitutional torture prohibitions (that is, the possibility to torture covertly and/or without leaving scars and the popularity of torture

[5] This process based approach is set out in *Youngstown Sheet & Tube Co. v. Sawyer*, 343 U.S. 579 (1952).
[6] *Kennedy v. Mendoza Martinez*, 372 U.S. 144,160 (1963).

under some circumstances) are mutually reinforcing. Because torture can be done without leaving scars and without generating too much attention, the public might support it more. As Rejali (2007, 2) puts it "a victim with scars to show to the media will get sympathy or at least attention, but victims without scars do not have much to authorize their complaints to a skeptical public." Conversely, popular support for torture likely declines when the public is confronted with images of suffering and humiliation, especially when the fear of terrorism subdues. Indeed the release of a series of infamous pictures that depicted the abuse of Iraqi prisoners by the U.S. military in Abu Ghraib prison was met with swift and strong public condemnation. For example, the front page of *The Economist* (which had supported George W. Bush in the 2000 election) depicted a picture of a hooded prisoner along with the message "Resign, Rumsfeld."

In sum, torture prohibitions' distinct nature might make it more prone to abuse than other rights protected in national constitutions. Although constitutions provide a set of mechanisms that have the potential to reduce torture in theory, the ability of governments to torture covertly and to gather public support for their actions may render these constitutional mechanisms ineffective in practice. These theories, however, have not yet been systematically and rigorously tested using empirical research methods.

3. Research Design

3.1. Constitutions Data

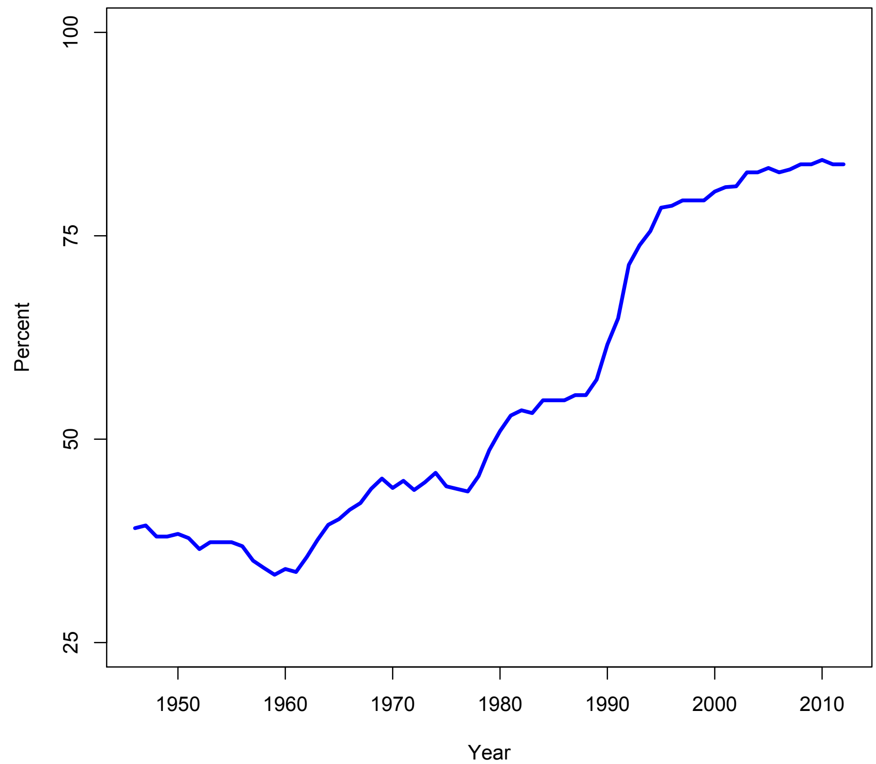
To analyze whether constitutional torture prohibitions are effective, we analyzed all national constitutions written between 1946 and 2013 using a dataset collected by Versteeg, and first introduced in Goderis and Versteeg (2015) and Law and Versteeg (2011). To further ensure the reliability of our data, we cross-checked the prohibition of torture coding by Versteeg against the coding by the Comparative Constitutions Project, and resolved any disagreements between the two.

The general coding rules for coding constitutions are documented in Law and Versteeg (2011) as well as in Elkins et al. (2009). Generally, the coding of torture prohibitions was a fairly straightforward task as most constitutions prohibit torture in similar terms. Nonetheless, we had to make a few judgment calls. First, where constitutions prohibit "cruel and unusual" or "cruel, inhuman and degrading" punishment but did not explicitly use the word torture, we coded this as a prohibition of torture. This decision is consistent with controlling interpretations of international and domestic law. The Convention Against Torture prohibits cruel, inhuman and degrading punishment as well as torture, and the CAT Committee has made clear that it is not possible to draw the line between the two (CAT Committee General Comment No. 2. *See also* ICCPR Committee General Comment No. 20). Moreover, the Eighth Amendment of the U.S. Constitution, which prohibits "cruel and unusual" punishment, has been interpreted to include a ban on torture.[7] Our coding thus builds on the assumption that a prohibition of cruel and/or unusual and degrading treatment entails a prohibition on torture. Second, we coded the prohibition to torture those deprived of their liberty as a full prohibition of torture. While such prohibitions protect only a sub-group of all potential torture victims, those imprisoned or detained are arguably the most common target group for torture. Third, we decided to treat prohibitions of corporal punishment as prohibitions of torture. Even though these prohibitions are narrower in scope than torture prohibitions, they do fall within the definition of torture, which includes "any act by which severe pain or suffering, whether physical or mental, is intentionally inflicted on a person for such purposes as . . . punishing him for an act he or a third person has committed or is suspected of having committed[.]" (CAT, art. 1).

Using this data, Figure 1 depicts the prevalence of the prohibition of torture over time. It shows that the prevalence of torture prohibitions increased particularly steeply in the past few decades. In 1946, 39 percent of all constitutions prohibited torture. Three decades later, by 1976, this number stood at 44 percent—an increase of only 5 percentage points. In the next three decades, the prevalence of torture prohibitions almost doubled. Today, no less than 84 percent of all countries prohibit torture in their constitution.

[7] *In re Kemmler,* 123 U.S. 436, 447 (1890) ("[p]unishments are cruel when they involve torture or a lingering death").

Figure 1: The Prevalence of Constitutional Torture Prohibitions Over Time



3.2 De Facto Torture Data

To capture de facto incidents of torture, we use the new *Latent Repression* variable developed by the political scientist Christopher Fariss (Fariss 2014; Schnakenberg and Fariss 2014). As Fariss (2014) documents, previously datasets that measure incidents of torture—include the CIRI project, Hathaway's torture data, and the ITT torture data—are biased because reporting standards vary over time and across countries. Fariss has found a way to address these problems by developing a dynamic model that incorporates data from 13 sources. Specifically, Fariss corrects for reporting biases in other data sources by using a Bayesian measurement model that accounts for changing standards of accountability. His model specifically includes "standards-based" data sources (like the CIRI torture data that measures states level of repression on a 3-point scale) and "events-based" data sources (like whether a country experienced a genocide in a given year). By incorporating these measures into a dynamic measurement model, Fariss is able to generate "unbiased estimates of repression" (Fariss 2014, 297).

The average level of repression for all observations in the dataset is set at 0, and the score that a country receives is the number of standard deviations of Latent Repression that the country is away from 0. For example, North Korea has a score of roughly -2.0 in 2010, which means that the level of Repression in North Korea that year is roughly 2 standard deviations worse than the average for all countries in the world in the years 1949 to 2010.

One limitation of this measure is that it is broader than just torture, and also includes other abuses of physical integrity rights, such as extra-judicial killings, disappearances, and genocide. The data arguably broadens the definition of torture to include those cases where physical harm results in death, and includes physical harm not inflicted for the purpose of obtaining a confession, punishment, or discrimination (Article 1 CAT).[8] At the same time, the data only takes into account harm inflicted by government officials and is largely limited to rights abuses that cause physical harm, thus excluding political rights violations of freedom of expression, freedom of movement, freedom of association, etc.[9] Moreover, three of the eight standard-based measures in the Fariss data explicitly capture torture, while another two include torture, thus making torture a disproportionately important component on the index. Indeed, in his article introducing the data, Fariss himself uses this data to test the effectiveness of the Convention Against Torture (CAT).[10]

Another possible downside of this data is that it does not take into account the possibility that different countries might have different interpretations of what constitutes torture. For example, the Bush Administration maintained that waterboarding does not constitute torture, even though international human rights organizations (including those that provide the input for our dependent variables) treated it as such. Our approach is to hold all countries to the same standard. First, most countries themselves use remarkably

[8] According to the CAT, article 1: “torture" means 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. It does not include pain or suffering arising only from, inherent in or incidental to lawful sanctions.

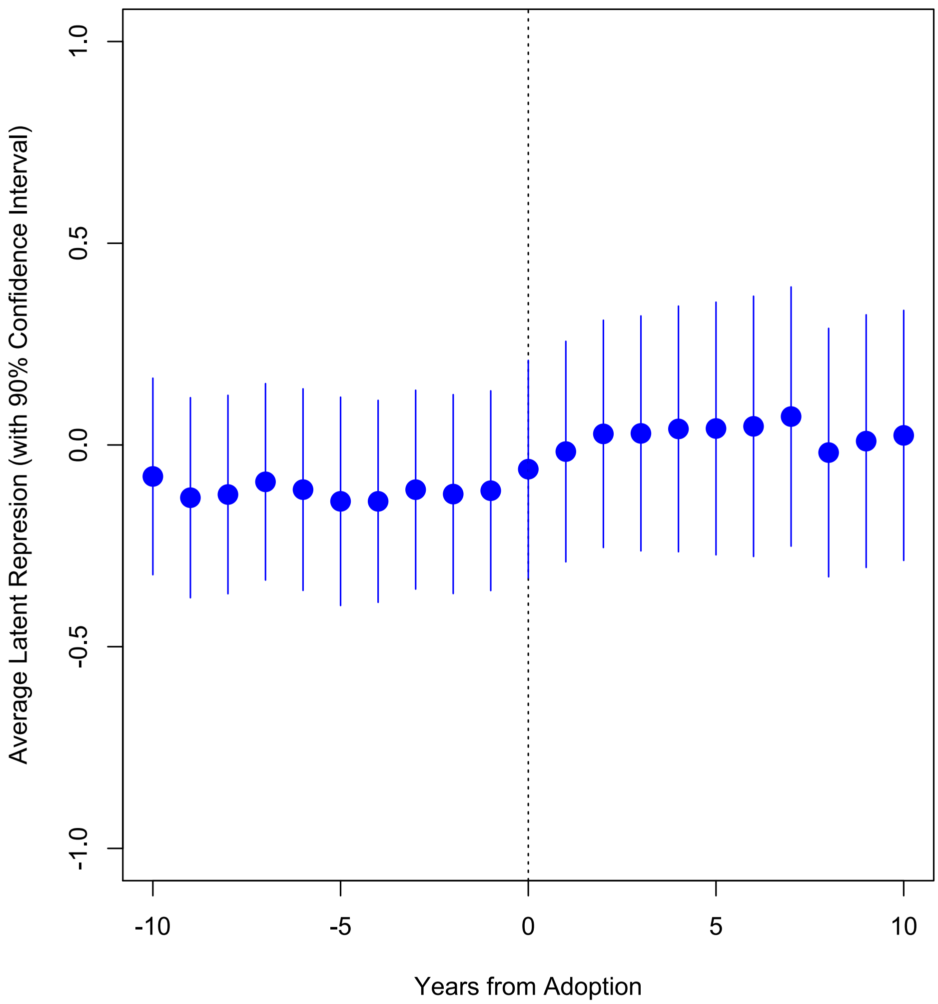
[9] One of the thirteen measures captures unjust imprisonment, which is arguably beyond the scope of torture entirely.

[10] In addition to using Latent Repression as our dependent variable, in Section 4.3 we also explore the robustness of our results to using five alternative variables that explicitly capture torture that have previously been used in the empirical literature on torture.

similar language in their torture prohibitions, and have often adopted their constitutional prohibitions with the intention to incorporate the international norm against torture (Versteeg 2015). Moreover, it seems conceptually unsound to suggest that a regime that engages in torture could escape international scrutiny simply by adopting a restricted definition of torture that condones its own behavior (Law and Versteeg 2013, 878). As Law and Versteeg (2013, 878) note: "one cannot measure government respect for rights using a yardstick that itself varies in size depending upon the extent to which the government respects rights." Moreover, it has long been suggested that torture tends to be an extra-legal affair. Even though there have been recent attempts to create a legal space for torture, as in the torture memos (Cole 2009), torture usually happens outside the law. As William Blackstone famously observed, the use of the rack in Tudor times was used "as an engine of the state," but "not of law" (Waldron 2010, 248). Indeed, despite the earlier interpretations by the Bush Administration, President Obama has now accepted that waterboarding indeed is torture and therefore illegal. Finally, holding all countries to the same standard of human rights has been *universally* adopted by the empirical human rights literature (Hathaway 2002; Simmons 2009; Hill 2010; Lupu 2013a; Lupu 2013b), and the very limited literature that has empirically tested the effectiveness of constitutional rights (Melton 2013; Chilton and Versteeg 2014). Thus, we work from the assumption that constitutional prohibitions of torture set the same standard everywhere.

A cursory exploration of the data suggests that the relationship between constitutional torture prohibition and de facto torture is tenuous at best. Figure 2 depicts the average score for our dependent variables that capture de facto torture—*Latent Repression*—for countries that added a prohibition against torture in the 10 years before and the 10 years after adding the constitutional right. The data reveals a small increase in respect for the prohibition of torture after adding the prohibition. Although this small increase may be due to the effectiveness of the constitutional prohibition, however, the raw data does not reveal a clear improvement in respect for the prohibition of torture. The following sections will explain how we will test the relationship between constitutional torture prohibitions and levels of torture more rigorously.

Figure 2: Average *Latent Repression* Before and After Constitutional Prohibition



3.2. Empirical Approach

Assessing the impact of constitutional right is not an easy task, in large part because constitutional rights are not assigned randomly (Chilton and Versteeg 2014). In the case of torture prohibitions, constitution-makers likely decide on whether to include a prohibition of torture at least in part based on the state's views and preferences on torture. Thus, states that refrain from torture in practice are more likely to constitutionally prohibit torture than states that engage in torture. The result is a selection problem that could bias any attempts to empirically analyze of the effectiveness of constitutional rights without at least mitigating these selection problems (Chilton and Versteeg 2014).

While there have been few attempts to develop methods to address selection effects in the comparative constitutional law literature, a body of research has developed in response to the related problem of trying to understand the effectiveness of international human rights treaties (Neumayer 2005; Simmons 2009; Hill 2010; Lupu 2013b). In order to account for the fact that ratification of human rights treaties is endogenous to state practice, scholars studying the effectiveness of human rights treaties have used a number of increasingly sophisticated techniques. Although a number of methods have been used—like Heckman selection models and instrumental variable regression—one method that is increasingly popular among scholars studying compliance with international agreements is matching (Simmons and Hopkins 2005; Hill 2010; Lupu 2013a; Lupu 2013b; Nielsen and Simmons 2014). Matching attempts to account for significant differences between groups that researchers are interested in studying by pairing observations that are as similar as possible except that one has received a particular treatment (i.e. adopted a constitutional torture prohibition) while the other has not (Ho et al. 2007). The intuition is then that if the observations are similar along all relevant dimensions except that one constitutionally prohibits torture while the other does not, then observed differences in the dependent variable can be attributed to the constitutional prohibition of torture.

Although matching methods can help to improve causal inference by producing comparable samples, a shortcoming of the method is that it relies on pairing observations exclusively on observable variables. As a result, it is always possible that are unobserved variables that influence both the treatment and outcome, thereby biasing the results produced through matching procedures. In other words, matching does not solve the problem of omitted variable bias. In the human rights context, a major concern has been that there are unobserved differences in state's preferences for treaty commitments that are related to human rights practices. Such preferences for treaty commitments are harder to observe than a country's level of democracy or economic welfare, for example.

In order to address this problem, Lupu (2013a) recently developed a methodology to measure state's preferences for treaty commitments. Lupu's method is based on using software that was developed to explain the ideological preferences of legislators—the W-NOMINATE algorithm (Poole and Rosenthal 1997). The W-NOMINATE program takes the members of a legislative body—say Congress—in a given term, and codes their votes on every issues as either 0 or 1. Using this matrix of legislatures and votes, the algorithm plots legislators in n-dimensional space based on how closely they voted with other members. In other words, the algorithm would place legislators that never vote the same on legislation as the opposite poles of the possible policy space. This approach produces an "ideal point"—the coordinates that a specific legislator would occupy in n-dimensional space—for every member of the legislative body being studied.

The W-NOMINATE algorithm has been used to study a range of settings beyond Congress, including voting by countries in the United Nations (Voeten 2001) or national constitutions (Law and Versteeg 2011). The innovation of Lupu (2013a) is to apply it to countries' decision to ratify treaties. Lupu treats countries like members of a legislature, and every year like a term of the legislature. In each year, Lupu codes whether countries have adopted international treaties as 0 and 1, and then uses this coding to produce each country's "treaty" ideal point. Using this information, it is then possible to calculate how likely it is that a country would have ratified a particular treaty. To do so, the assumption is that the closer a state's ideal point is to the specific point estimated for a particular treaty, the more likely it is that a state will ratify that treaty (Lupu 2013a; Lupu 2013b).[11] Based on these calculations of how likely a country would have been to ratify a specific treaty, Lupu then uses propensity score matching to pair countries that have similar treaty ideal points, but where only one state has occupied a treaty of interest.[12] Using standard regression techniques, Lupu then attempts to estimate the decision to ratify a specific treaty on human rights practices.[13]

Although Lupu's method was developed to test whether treaty commitments improved the protection of rights, Chilton and Versteeg (2014) have argued that is also a promising method to test the effectiveness of constitutional rights. Instead of considering state's decisions to ratify treaties as a proxy for their affinity for rights, Chilton and

[11] Although this paper does not have the space to fully explain this method, Lupu (2013a) provides a longer explanation for the method, as well as a long justification for its use. Most notably, Lupu (2013a) uses Monte Carlo simulations to demonstrate that this method is better at predicting whether a state would have ratified a given treaty than statistical models using conventional observable variables

[12] In addition to using the same method to calculate countries constitutional ideal points as Lupu (2013a; 2013b), we use the same method to match countries. This is discussed in Section 3.3.2.

[13] We additionally use the same post-matching regression techniques as Lupu (2013a; 2013b). This is discussed in Section 3.3.3.

Versteeg look at states' decisions to adopt 87 different rights in their constitution, and argue that this captures their preferences for constitutional rights commitments. Intuitively, their method compares countries that have very similar patterns of constitutional rights adoption, but one prohibits torture while the other does not.

3.3 Implementation

3.3.1 Ideal Point Estimation

Following Lupu (2013a; 2013b; 2015) and our own previous work on constitutional rights (Chilton and Versteeg 2014), our analysis involves a three-stage process. In the first stage, we begin by estimating every country's constitutional ideal point for every year between 1946 and 2012. Following Chilton and Versteeg (2014), we perform ideal point estimation for a set of 87 rights,[14] estimating a two-dimensional model using the W-NOMINATE algorithm for the R programming language (Poole et al. 2011). This analysis yields annual constitutional ideal points along two dimensions for 186 countries for every year between 1946 to 2012.

With these ideal points, we next estimate the probability that a country would have included a prohibition of torture by calculating the distance between the country's ideal point and the ideal point of the prohibition of torture. Doing so produces a yearly estimate of the probability between 0 and 1 that a country would have a constitutional torture prohibition.

3.3.2 Matching

The second stage of our analysis involves the matching of country-year observations that include a prohibition of torture country-year observations that do not include a prohibition of torture, using the probabilities calculated in the first stage of analysis. We also include a number of other observable variables in our matching equations that have been previously shown to influence rights practices (Poe and Tate 1994; Poe, Tate, and Keith 1999; Hill 2010).[15] Specifically, in addition to the

[14] Information on these 87 rights is reported in Appendix 1.
[15] This decision is a departure from Lupu (2013a) and Lupu (2013b). Lupu 2013a and Lupu 2013b only matched on the estimated probability that a country would have ratified a particular treaty, but then

probabilities generated through the ideal point analysis, we match on a number of variables that have been identified as "the usual suspects" (Hill and Jones 2014, 661) or "the standard model" (Keith, Tate and Poe. 2009, 648) of repression in the existing literature, and that are also used by Lupu (2013a; 2013b) and Chilton and Versteeg (2013). The rationale for inclusion of these variables is extensively discussed in earlier studies (Poe and Tate 1994; Poe, Tate and Keith 1999). First, we match on level of democracy using the polity2 variable Polity IV dataset ("Polity"). As discussed, previous studies have consistently found a link between democracy and respect for human rights, although this finding does not always hold for torture. Second, we match on the natural log of a country's GDP per capita, taken from the World Development Indicators, because more wealthy countries tend to be less repressive (Poe and Tate 1994). Third, because most data counts the total number of rights violations, larger countries tend to commit more violations (Poe and Tate 1994). We therefore match on the natural log of a country's population size, taken from the World Development Indicators. Fourth, because engagement in civil and international conflict tends to increase human rights violations (Poe and Tate 1994; Hill and Jones 2004), we also match on two variables that capture whether a country is engaged in international war and whether it is engaged in civil war, both taken from the Correlates of War database.

In addition to these "usual suspects" (Hill and Jones 2014, 661) we match on two variables that are arguably particularly important for constitutional rights: judicial independence and the number of international Non-Governmental Organizations (INGO's). Independent judiciaries are important in enforcing rights de facto (Lupu 2013b), but may also affect whether constitutional torture prohibitions are adopted in the first place (Cross 1999). To capture judicial independence, we use a variable from the CIRI dataset, that rates judicial independence on a three-point scale. We include INGO's because these organizations have the ability to both prevent torture in practice through naming and shaming and activism, but might also be able to pressure for the constitutional inclusion of a torture prohibition by spreading global norms (Hafner-Burton and Tsutsui, 2005).

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included a number of other variables in his post-matching regressions. In more recent work, however, Lupu has also included observable variables in his initial matching procedure. See Lupu (2015).

Finally, to address the concern that countries with better rights records are more likely to adopt constitutional rights, we also match on the lagged dependent variable (t-1). Matching on the lagged dependent variable also reduces concerns over reversed causality (that is, the concern that a relationship between de jure and de facto torture results from countries strong de facto rights practices adopting de jure rights, rather than vice-versa), because it allows us to compare countries that had rather similar human rights records in the year preceding the adoption of the constitutional torture prohibitions (Lupu 2013a; Lupu 2013b; Lupu 2015). In our robustness analysis, we experiment with matching without lagged dependent variable, and matching on two lagged dependent variables.

We next matched out data using propensity score matching.[16] Despite the fact that many different matching methods have been developed (Honaker, King, and Blackwell 2011), we believe that using propensity score matching is most appropriate because it has both been advocated by Lupu (2013a) and has been the primary method used in the international law literature (Simmons and Hopkins 2005; Hill 2010). This is also the method we used in our previous work (Chilton and Versteeg 2014).

Table 1: Matching Results

	Full Sample	Matched Sample
Sample Size	7,959	2,332
Treatment Units	4,796	1,178
Control Units	3,163	1,178
Mean Distance – Treatment Group	0.814	0.544
Mean Distance – Control Group	0.282	0.477
Improvement in Balance	87.46%	

Using this approach, we created a matched dataset. As Table 1 shows, matching dramatically improves the balance of the sample. In fact, the balance improves by 87 percent. This is important because the goal of matching is to produce a dataset where the observations that have received a given treatment and the control observations have as similar values for all observed variables as possible (Ho et al. 2007).

[16] Specifically, we use nearest neighbor matching with a caliper of 0.5 to ensure that the matched pairs improve the balance within the sample. This approach is consistent with Chilton and Versteeg (2014).

3.3.3 Multivariate Regression Analysis

In the third and final stage of our analysis, we use the matched dataset to test the effect of constitutional torture prohibitions on de facto torture. Because our matched samples are not perfectly balanced, we use multivariate regression analysis to analyze the matched datasets (Ho et al., 2007; Hill 2010; Lupu 2013a; Lupu 2013b; Nielsen and Simmons 2014). Our regression model includes all the variables that we matched on (see Section 3.3.2), as well as a set of year fixed-effects. We address any potential serial correlation by calculating robust standard errors clustered at the country level (Lupu 2013a; Lupu 2013b; Chilton and Versteeg 2014). We use an OLS model, because the Farris data is continuous in nature.

Although our de jure and de facto torture data has relatively few missing values, there were more missing data among our control variables. Dropping these observations via list-wise deletion, however, risks biasing our results because the missing observations are likely non-random (Honaker and King 2010). To account for this source of bias, we imputed values for the missing observations using the Amelia II package for R (Honaker, King, and Blackwell 2011).[17] Doing so is consistent with the practice of other recent scholarship in the human rights literature (Hill 2010; Lupu 2013a; Lupu 2013b) as well as our own previous work (Chilton and Versteeg 2014).[18] In our robustness analysis, we will also show our results without imputed missing observations.

4. Estimating the Effect of Constitutional Torture Prohibitions on Torture

4.1 Primary Results

Using the data and methods described above, we find no evidence that constitutional torture prohibitions reduce torture. Table 2, column 1, presents the results

[17] Following Lupu (2013a) and Lupu (2013b), we imputed the missing values for all control variables using the full dataset of country years instead of simply the matched sample. This decision was made because including all country years improves the accuracy of the imputation.

[18] While using the Amelia II package, we only imputed missing values for our seven control variables. We also included a polynomial term to allow for the possibility of trends in these variables over time ("polytime = 2"). Finally, we followed the standard procedure of imputing five datasets, but used the average values of the imputed datasets for our matching procedure because it is only possible to use a single value during the matching process.

of our baseline regression model. This model uses the method described in Section 3.3.3 which, as previously noted, was developed by Lupu (2013a, 2013b, 2015) to test the effectiveness of human rights treaties. The coefficient for the key explanatory variable in this model—*Torture Prohibition*—is substantively small, and far from statistically significant (p = 0.90). In other words, this model does not provide evidence that constitutional prohibitions have reduced evidence of torture.

Table 2: Primary Results

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	0.001	-0.177	-0.005	-0.137	-0.028
	(0.009)	(0.133)	(0.007)	(0.121)	(0.125)
Probability of Prohibition	-0.006	-0.145	-0.010	-0.108	0.074
	(0.011)	(0.175)	(0.008)	(0.131)	(0.133)
Polity	0.004***	0.032***	0.003***	0.034***	0.018**
	(0.001)	(0.010)	(0.001)	(0.007)	(0.008)
GDP per capita (ln)	0.006	0.098*	0.006**	0.145***	0.177***
	(0.005)	(0.054)	(0.002)	(0.041)	(0.049)
Population size (ln)	-0.003	-0.382***	-0.006***	-0.340***	-0.094*
	(0.003)	(0.038)	(0.002)	(0.031)	(0.056)
Interstate war	-0.038	-0.225	-0.041**	-0.316*	-0.114
	(0.025)	(0.276)	(0.021)	(0.174)	(0.156)
Civil war	-0.070***	-1.147***	-0.058***	-0.994***	-0.553***
	(0.017)	(0.159)	(0.012)	(0.091)	(0.102)
Judicial independence	-0.024***	0.210***	-0.013***	0.230***	0.100**
	(0.008)	(0.080)	(0.005)	(0.053)	(0.044)
INGOs	-0.000	0.001***	-0.000	0.000***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Mean $_{t-1}$	0.987***		0.989***		
	(0.005)		(0.003)		
Observations	2,356	2,342	8,302	8,491	8,491
R-squared	0.991	0.651	0.990	0.595	0.818

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

The finding that constitutional torture prohibitions do not reduce torture contradicts the intuition of many constitutional lawyers who have placed substantial faith in constitutions' ability to constrain government, which suggests that we have to be careful in drawing such a conclusion. It is therefore important to explore the possibility that our results are driven by type-II errors, that is, a failure to reject the null-hypothesis that constitutional torture prohibitions do not make a difference because of an overly restrictive model. To explore this possibility, we estimate a range of models that are arguably less restricted than our baseline model.

First, we explore whether our results are the same when we exclude the lagged dependent variable. The baseline model reported in column 1, is arguably an exacting regression specification because it includes a lagged dependent variable. Since a country's previous torture practices likely have strong explanatory power for its current practices, it is possible that there is relatively little variance left to be explained by constitutional torture prohibitions. To explore whether our preferred specification might have been overly restrictive, we re-estimate our base regression model without including a lagged dependent variable in either the matching stage or the post-matching regression. The results, reported in Table 2, column 2, show that while the estimated size of the coefficient is larger than in the baseline model, and becomes negative, the coefficient for *Torture Prohibitions* is still far from statistically significant (p=0.18) Thus, the lack of statistically significant relationship between constitutional torture prohibitions and actual torture does not result from the inclusion of a lagged dependent variable.[19]

Second, we explore whether the results are the same when we do not pre-process our data with matching. Although matching has the advantage of improving the balance between observations that have received a given treatment, the disadvantage is that matching methods disregards a great deal of data. To test whether or results were dependent on the our matching process, we estimated the same specifications as reported in Table 2, columns 1 and 2, but did not but did not pre-process the data with matching. The results, reported in Table 2, columns 3 and 4, reveal that the estimate for *Torture*

[19] We also estimated the same model with two lagged dependent variables, and find that the results are similar the baseline model in column 1.

Prohibitions is still negative, comparable in size, and far from statistical significance. Thus, our findings are not the product of the matching procedure.

Third, we explore whether the results are the same when we estimate a more conventional fixed-effects panel regression. While our preferred approach brings a country's pre-existing preference for constitutional rights into the research design, we do not control for all possible permanent differences between countries. A more conventional fixed-effects model allows us to control for all non-time varying cross-country differences. Table 2, column 5, shows the results from a fixed-effect model. This model includes the same explanatory variables as our baseline model, but excludes the lagged dependent variables and the data was not pre-processed with matching. The results show that the estimate for the *Torture Prohibition* variable remains negative, substantively small, and statistically insignificant.

Of course, failure to reject the null hypothesis—that Constitutional Torture Prohibitions do not reduce incidents of torture—is not the same as evidence for the null hypothesis. That is, just because a result does not achieve statistical significance does not mean that the variable has no effect (or even a negligible effect). It could be the case, for example, that the effect of a given variable is substantively large but fails to achieve statistically significance because the model is imprecisely estimated. Rainey (2014) recent developed an approach to test whether null results are actually the same as are evidence that a given variable has "no effect." The approach requires first defining the smallest effect that could be considered substantively meaningful (denoted as *m*), and then defining a reject region from –*m* to *m*. A variable is then considered to have no effect when the 90% confidence interval for the estimated regression coefficient does not cross –*m* or *m*. This approach has the advantage of being easy to implement in a standard regression framework, and also has already been used to test for null findings in the human rights literature (Nielsen and Simmons 2014).

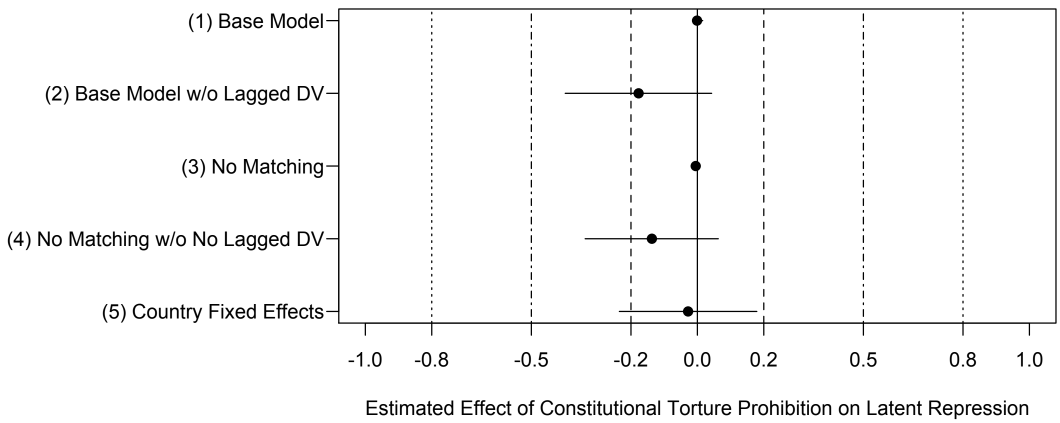
This approach does, however, require researchers to subjectively define the size of *m*. Although this selection is inherently subjective, one advantage of the *Latent Repression* variable created by Fariss (2014) is that the unit of measurement is in standard deviations. In other words, if a variable has a coefficient of 1.0, it means that it is associated with countries having a one standard deviation higher value for *Latent*

Repression compared to the average country between 1949-2010. This not only makes it easy to interpret the effect of variables on *Latent Repression* generally, but also is specifically useful in this case because there are some general guidelines on how big a chance in standard deviation a variable must create to be “meaningful.” The general guideline, from Cohen (1988), is that an effect of 0.2 standard deviations is small, an effect of 0.5 standard deviations is moderate, and an effect of 0.8 standard deviations is large. As a result, we set *m* at 0.2 and then test whether the 90 percent confidence interval for the coefficient for the *Torture Prohibition* variable crosses -0.2 or 0.2.

To put a change of 0.2 into perspective, in 2010 countries latent repression scores ranged from a low of Sudan at -2.4 to a high of Luxembourg at 4.7—a range of just over 7.0 standard deviations. An improvement of 0.2 would not even move Sudan to the level of Repression in North Korea (which had a score of -2.0). Or alternatively, in 2010 the country with a score closest to the overall median is Uzbekistan at -0.01. An improvement of 0.2 would raise Uzbekistan to roughly the level of repression in the Gambia in 2010 (0.20).

Figure 3 shows the estimate and the 90 confidence intervals for the *Torture Prohibition* variable for Models 1-5 from Table 2. As Figure 3 shows, the confidence intervals for the estimated effects does not cross 0.2 for any of the 5 models. It does however, cross -0.2 for Models 2, 3, and 5. This suggests that the Torture Prohibition variable does not have even a “small” positive effect (that is, reduce incidents of torture), but it may have a small negative effect (that is, increase incidents of torture). In other words, using the same empirical method and variables that have been recently developed to test the effectiveness of human rights treaties (Lupu 2013a; Lupu 2013b; Lupu 2015), we find no evidence that constitutional torture prohibitions help to reduce torture. This is despite using the best data and most sophisticated method currently available. Moreover, although there is a possibility that *Torture Prohibitions* result in higher incidents of torture, these results do not reveal any evidence that they help to make things better.

Figure 3: Estimated Effect of Constitutional Torture Prohibitions (baseline results)



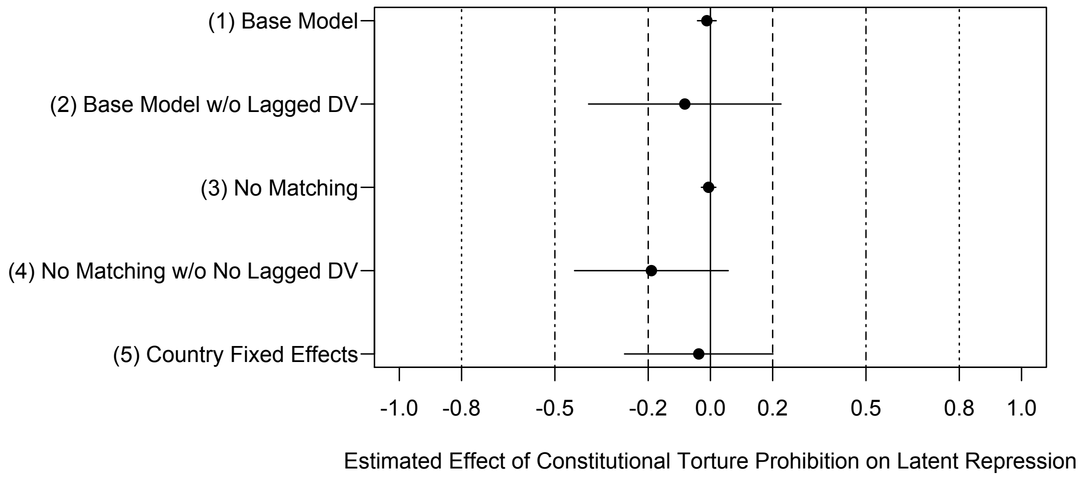
4.2. Further Robustness Checks

In this section, we present a range of further robustness checks. First, we explore whether our results (or lack thereof) are driven by our decision to use imputed data in our primary regression specifications. Imputing values for missing control variables has become standard practice in the human rights literature, because dropping observations because they are missing values for some variables biases the results because the observations' missing values are not random (Hill 2010; Lupu 2013a; Lupu 2013b: Lupu 2015). However, the imputation procedure also has the possibility of affecting our results. To explore if the imputation we use drove our findings, we recreated the analysis reported in Table 2 and Figure 3 without using any imputed data.[20] These results are reported in Figure 4.[21] Unsurprisingly, the confidence intervals are slightly larger given that there is a reduction in our sample size. This approach, however, does not have a substantial impact on our primary results. The coefficients for *Torture Prohibition* only have 90 percent confidence intervals that cross 0.2 in models 2 and 5, and even then they only barely so (model 2 90% CI = -0.39 to 0.23; model 5 90% CI = -0.28 to 0.20).

[20] We also estimate a specification with two lagged dependent variables, but do not report it because the estimate is similar to the estimate produced when using two lagged dependent variables as reported by "(1) Base Model."

[21] Full regression tables for all the figures presented in Section 4.2 are reported in Appendix 2.

Figure 4: Estimated Effect of Torture Prohibitions – Without Imputed Data

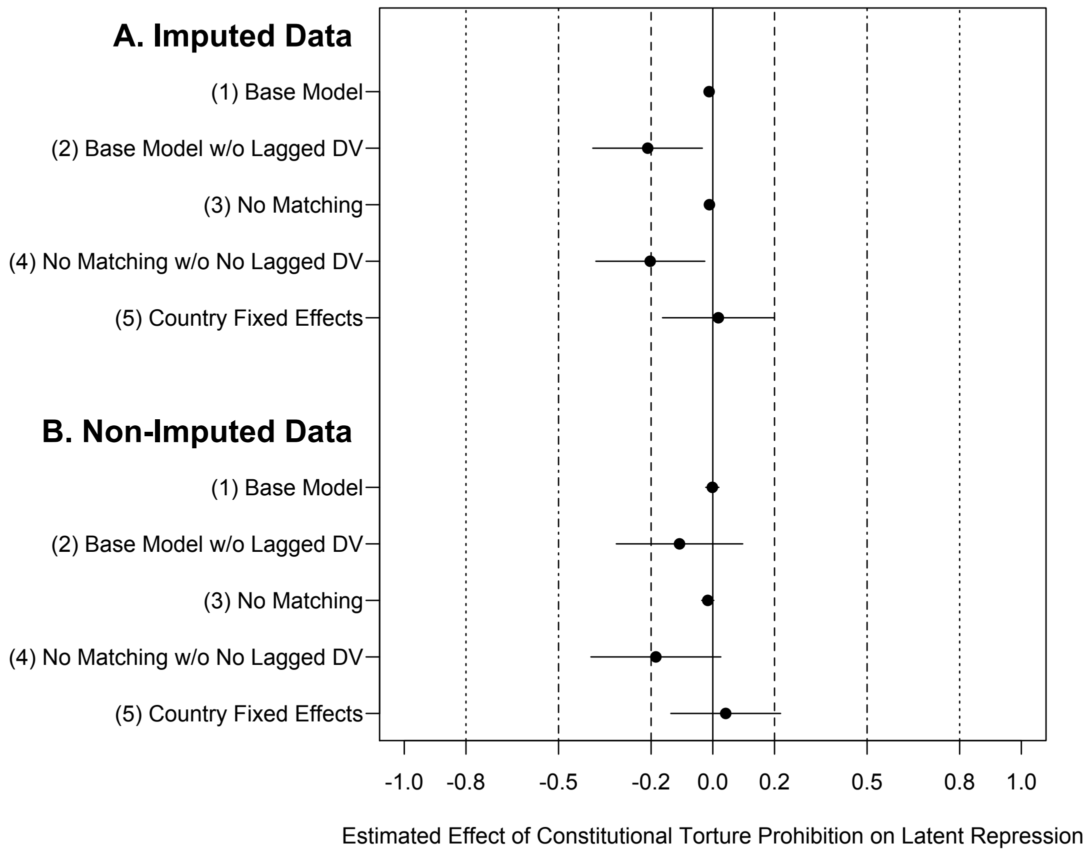


Second, we explore the robustness of our results to eliminating the W-Nominate procedure from our estimation strategy. As we explained in section 3.2, the primary innovation of Lupu (2013a) is to estimate countries' likelihood of joining international treaties buy using those countries' previous ratification decisions to calculate the countries treaty ideal points. This is then used to match countries that were both likely to ratify a specific treaty based on their past ratifications, but where only one has done so. In this paper, we adjust Lupu's approach by using information on previous constitutional rights adoption to calculate countries' "constitutional ideal points" to estimate the likelihood that a country would have included a Torture Prohibition in its constitution in a given year. Although we believe this approach helps to correct for selection effects, we have also estimated all of our models without including this variable in either our matching procedure or regressions. Figure 5 reports the results of this analysis. Panel A recreates the regressions presented in Figure 3 (baseline model), whereas Panel B recreates the analysis presented in Figure 4 (without imputed data), but in both cases without the W-Nominate scores.[22] As Figure 5 shows, not only did not a single model show a statistically significant improvement in the Latent Repression variable (that is, evidence of less torture), only 1 model even had a 90 percent confidence interval that

[22] For both the baseline model and the model without imputed data, we also estimate a specification without W-Nominate scores, but with two lagged dependent variables. We do not find that this changes the size or significance of the constitutional torture prohibition variable compared to the base models (which uses one lagged dependent variable).

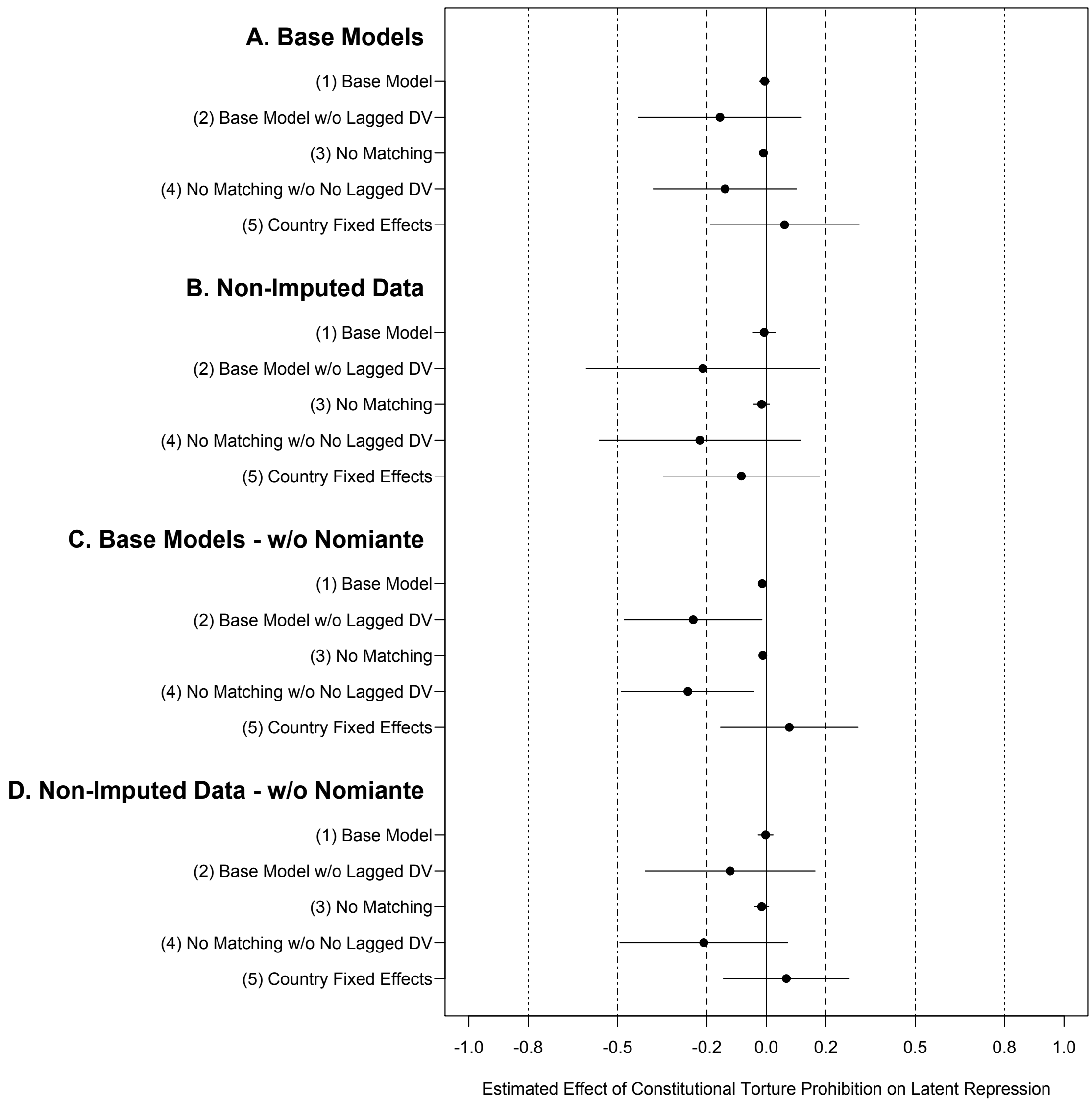
crossed 0.20, and even then, only barely so (Model B.5 90% CI = -0.14 to 0.22). As a result, we do not believe that our null and negligible results are a product of our decision to use Lupu's method for addressing selection effects.

Figure 5: Estimated Effect of Torture Prohibitions – Without W-Nominate Scores



Third, we explore whether our results might be driven by sham constitutions adopted by the authoritarian regimes in our sample. It is well documented that autocratic regimes often adopt constitutions that are nothing but empty promises, and are enacted merely to appease the international community (Law and Versteeg 2013). Indeed, in her analysis on human rights treaties, Beth Simmons omits these "false positives," that is, the states commit to human rights on paper without an intention to respect these rights in practice (Simmons 2009, 18). Simmons also omits "false negatives," that is, states that commit to treaties but that already have the strongest possible rights records and where treaties thus cannot do much to improve practices.

Figure 6: Estimated Effect of Torture Prohibitions – Democracies only



For constitutional torture prohibitions, the false positives have real potential to distort our findings, as there likely exists a group of countries that adopt torture prohibitions without any intention of compliance. Yet, for constitutional torture prohibitions, there is arguably no such thing as "false negatives," as even democracies with strong human rights records have been shown to resort torture. To explore whether our findings are in part of product of the false positives, we re-estimate our analysis when excluding authoritarian regimes that might have adopted these provisions for disingenuous reasons. Specifically, we exclude all countries that have never been democratic.[23]

Using this more limited sample, we re-estimate all the specifications reported in Figure 3 through 5. The results are reported in Figure 6. Panel A reports the baseline models from Figure 3, Panel C reports the models without imputed data from Figure 4, and Panels C and D report the models without W-Nominate scores from Figure 5.[24] The results show that none of these specifications are statistically significant and that they are mostly negative. The *Torture Prohibitions* variable only has a 90 percent confidence interval that crosses 0.20 in three specifications: in Panel A model 5, the estimate coefficient for *Torture Prohibitions* is 0.06 (90% CI = -0.19 to 0.31); in Panel C model 5, the estimate coefficient for *Torture Prohibitions* is 0.08 (90% CI = -0.15 to 0.31); and in Panel D model 5, the estimate coefficient for *Torture Prohibitions* is 0.07 (90% CI = -0.14 to 0.28).[25]

4.3. Alternative Torture Data

We have so far used the Farris data in all our specifications because it takes account of changing standards in torture reporting. One downside of the Farris data, however, is that it captures general government repression, which includes torture, but also includes information on other physical integrity rights, such as extra-judicial killings, disappearances, or genocide.

To explore whether the over-inclusiveness of the Fariss data affects our results, we also experiment with five alternative measures that have been used previously in empirical studies of torture and that explicitly capture incidents of torture. First, we use torture data from the "CIRI" dataset, which was created by Cingranelli and Richard (2012) and covers 195 countries from 1981 to 2011. This data is the most commonly used

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[23] We take the list of countries that have never been democratic from Simmons (2009, 396). Simmons defines a country as having never been democratic if it never scored above 5 on the polity scale during the twentieth century.
[24] Once again, for each of these models, we also estimate a model with two lagged dependent variable, but do not find that this changes the results.
[25] We also estimated the same regressions reported in Figure 6 for countries that Simmons defines as "Transitioning Democracies." The results of this analysis produced results that were substantively the same as the results in Figure 6.

in the empirical human rights literature (Hill 2010; Lupu 2013a; Lupu 2013b; Lupu 2015). It is based on quantitative coding of the annual U.S. State Department and Amnesty International country reports. It captures torture by government officials directed towards citizens only. CIRI grades countries' torture practices on a three-point scale: a score of 0 indicates that torture was practiced frequently in a given year; a score of 1 indicates that torture was practiced occasionally; and a score of 2 indicates that torture did not occur in a given year.[26]

While the CIRI data is by far the most widely used measure of torture in empirical research, it is relatively coarse and perhaps insensitive to small increases or reductions in torture incidents. We therefore also use a second measure that was created by Oona Hathaway, and that measures the prevalence of torture on a five-point scale based on Amnesty International and U.S. State Department Country Reports (Hathaway 2002). Unlike the Fariss and CIRI data, higher values in the Hathaway data are associated with more torture. While the Hathaway measure is more granular than the CIRI data, it is only available for the period from 1987 to 2001.

Neither the CIRI nor the Hathaway data distinguishes between different types of torture. Our theoretical framework, however, suggests that countries with constitutional torture prohibitions might primarily resort to stealth torture, because it does not leave any visible scars, which might allow government officials to escape accountability. It is possible, therefore, that the adoption of constitutional torture prohibitions leads to a reduction in scarring torture, because governments that can be held constitutionally accountable use stealth techniques to obtain information instead (Rejali 2007).

A new dataset by Conrad and Moore (2012) allows us to explore this possibility. Conrad and Moore's "Ill-Treatment and Torture Dataset" (ITT) distinguishes between scarring and stealth torture. The dataset also differs from existing dataset in other ways. An important feature of this data is that, unlike the CIRI and Hathaway data, it is based on Amnesty International data only. For all sovereign countries with a population of at least one million, the dataset compiles information from Amnesty International's country

[26] CIRI defines torture as follows: "the purposeful inflicting of extreme pain, whether mental or physical, by government officials or by private individuals at the instigation of government officials. Torture includes the use of physical and other force by police and prison guards that is cruel, inhuman, or degrading. This also includes deaths in custody due to negligence by government officials." (CIRI short variable descriptions, 2010).

reports, press releases and action alerts for the years from 1995 to 2005 (Conrad and Moore 2012). Notably, because the data is based on Amnesty International data only, the dataset arguably captures the torture reporting behavior by Amnesty International rather than actual levels of torture. The ITT dataset includes an overall ordinal measure of torture that ranges from 0 (no torture allegations) to 5 (systematic torture allegations). The data on stealth and scarring torture, by contrast, is count data: it counts the incidents of scarring and stealth torture reported by Amnesty International in a given year. Even though the data captures Amnesty International reporting behavior rather than true torture, we nonetheless use (1) the aggregate ITT data, (2) the ITT stealth torture data, and (3) the ITT scarring torture data to explore whether constitutional torture prohibitions perhaps lead to a reduction in scarring torture only.

We estimate all five of our primary models (reported in Table 2 and Figure 3) for each of these five alternative dependent variables. Table 3 presents the results for the five primary specifications for each of the five alternative dependent variables.[27] Panels A, B and C present odds ratios from the ordered logit models, while Panels D and E present incident rate ratios from the negative binomial models. To save space, the control variables are not reported.[28] The main impression from this exercise is that constitutional torture prohibitions do not reduce torture. In not a single one of the 25 specifications reported in Table 3 do torture prohibitions appear to reduce rates of torture. Instead, the only two statistically significant results—Panel B Models 2 and 4—found that *Torture Prohibitions* result in higher levels of torture. Overall, this exercise lends strong support to our primary results suggesting that constitutional torture prohibitions do not reduce torture.

[27] We use Ordered Logit models in Panels A, B, and C because the CIRI, Hathaway, and ITT base data is categorical (this is consistent with Lupu 2013a; 2013b; 2013c). We use negative-binomial models in Panels D and E because the Scarring and Stealth data is count data (this is consistent with Conrad & Moore 2012). In addition to the models reported in Table 3, we also estimated all these specifications with two lagged dependent variables, but did not find that this changed the results in any meaningful way.

[28] Full regression tables for all five panels in Table 3 are reported in Appendix 3.

Table 3: Alternative Torture Data

	(1) Base Model	(2) Base Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
(A) DV = CIRI Torture Data					
Torture Prohibition	0.702	0.601	0.765	0.636	0.917
	(0.137)	(0.191)	(0.142)	(0.186)	(0.363)
N	1,020	1,030	4,459	4,685	4,685
Model	O-Logit	O-Logit	O-Logit	O-Logit	O-Logit
(B) DV = Hathaway Data					
Torture Prohibition	1.379	2.481**	1.351	2.111*	0.815
	(0.334)	(1.021)	(0.298)	(0.815)	(0.526)
N	502	504	1,926	2,084	2,084
Model	O-Logit	O-Logit	O-Logit	O-Logit	O-Logit
(C) DV = ITT Data					
Torture Prohibition	0.956	1.168	1.086	1.044	0.941
	(0.299)	(0.574)	(0.283)	(0.434)	(0.780)
N	212	212	1,141	1,441	1,441
Model	O-Logit	O-Logit	O-Logit	O-Logit	O-Logit
(D) DV = "Stealth" Torture					
Torture Prohibition	0.812	0.950	0.955	0.977	0.599
	(0.172)	(0.317)	(0.178)	(0.319)	(0.294)
N	268	268	1,476	1,620	1,620
Model	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom
(E) DV = "Scarring" Torture					
Torture Prohibition	0.908	0.883	0.986	0.982	0.561
	(0.199)	(0.263)	(0.200)	(0.291)	(0.306)
N	268	268	1,476	1,620	1,620
Model	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom	Neg Bi-Nom

- All Models include controls for: Polity; GDP per capita (ln); Population size (ln); Interstate war; Civil war; Judicial independence; INGOs.
- All models included year fixed effects.
- Robust standard errors clustered on country in parenthesis.
- * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

5. Conclusion

Our analysis reveals that constitutional torture prohibitions have been ineffective in reducing torture. Despite the fact that a large majority of countries have included a prohibition against torture in their constitution, and torture has been condemned as "the barbaric remains of the middle ages" (Ishay 2004, 88), the use of torture remains widespread up till this day. Our analysis reveals that not only have constitutional torture prohibitions failed to eliminate torture, we did not find any evidence that constitutional bans even marginally reduced its use. Instead, despite using six different data sources and estimating dozens of regression models, we did not find a single model that produced a positive and statistically significant effect for the constitutional prohibition of torture.

Of course, our analysis offers merely an initial exploration of the topic. While it provides strong evidence that constitutional torture prohibitions do not affect torture *in the aggregate*, we have not systematically explored any conditional theories, or the possibility that torture prohibitions matter in some countries only. In recent years, there has been a growing consensus that when empirically exploring the effectiveness of de jure rights, researchers should not just focus on aggregate effects in all countries, but also explore the conditions under which rights do matter (Simmons 2009). One thing we did establish that also in democratic countries, constitutional torture prohibitions failed to constrain. Yet there are many other conditional theories that could be explored, such as whether torture prohibitions' impact depends on the presence of an independent judiciary, domestic veto players, the existence of freedom of expression, and so on. Our paper is merely a first inquiry into the impact of these and future research is needed to delve further into the conditions under which torture prohibitions might matter.

Finally, it is important to note that including constitutional torture prohibitions may have produced benefits that our methods do not detect. This could either be because better data on torture rates would reveal an effect of prohibitions, or that there are benefits to including the protection in constitutions beyond reducing rates of torture (Harel 2014). Based on the existing data, however, our evidence suggests that constitutional torture prohibitions have been ineffective.

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Appendix 1: Rights for Ideal point Estimation

The table below lists the 87 rights that we selected to conduct our ideal point estimation. The figure below depicts the average number of these 87 rights in the world's constitutions for the period 1946-2012.

87 Rights Used in Ideal Point Estimation

Right to freedom of religion	Rights for prisoners
Freedom of press and/or expression	Right to due process
Right to assembly and/ or association	Right to equality
Right to strike and/ or unionize	Right to private property
Right to vote	Right to education
Freedom to form political parties	Right to work
Right to a remedy when rights are violated	Right to health
Right to petition	Right to social security
Right to information about government	Freedom of enterprise
Right to compensation	Right to rest
Right to resist when rights are violated	Right to minimum wage
Right to "petition for amparo"	Right to housing
Right to establish private schools	Right to work for the government
Freedom of education	Right to favorable working conditions
Right to privacy of family life	Intellectual property
Right to protection of one's reputation or honor	Right to sport
Prohibition of death penalty	Right to adequate standard of living
Right to privacy of personal data	Prohibition of child labor
Free development of personality	Prohibition of confiscation
Protection of rights for unborn children	Right to food
Right to bear arms	Right to water
Prohibition of arbitrary arrest and detention	Right to establish a family
Right to privacy of the home	Rights for children
Right to privacy of communication	Special protection of mothers
Freedom of movement	Right to get married
Prohibition of torture	Equality husband and wife within the family
Right to life	Rights for elderly people
Right not to be expelled from home territory	Special protection of women
Prohibition of slavery	Women empowerment in labor relations
Right to personal privacy	Right to maternity leave
Artistic freedom	Right to a healthy environment
Right of access to court (habeas corpus)	Right to culture
Prohibition of ex post facto laws	Protection of minority language
Presumption of innocence	Right to preserve traditional ways
Right to present a defense	Right to asylum
Right to counsel	Special protection of minorities
Right to public trial	Rights for handicapped people
Prohibition of double jeopardy	Schooling right for minorities
Right to remain silent	Rights for consumers
Right to a timely trial	Right for minorities to use indigenous lands
Right to an interpreter	Rights for victims of crimes
Right to fair trial	
Right to appeal to higher court	
Representation right for minorities	
Autonomy for minorities	

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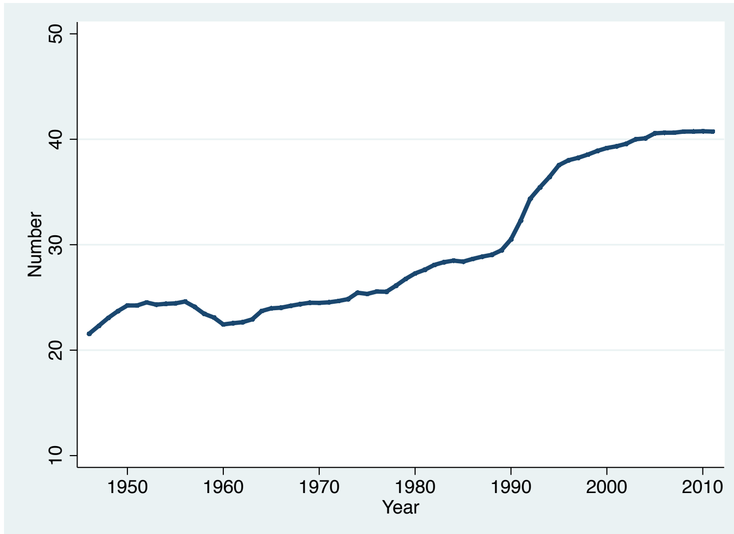

Appendix 2: Regression Results Reported in Figure 4

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.012	-0.082	-0.006	-0.190	-0.038
	(0.018)	(0.186)	(0.014)	(0.149)	(0.144)
Probability of Prohibition	-0.024	0.222	-0.017	0.009	0.117
	(0.021)	(0.227)	(0.015)	(0.160)	(0.140)
Polity	0.005***	0.033**	0.003***	0.028***	0.031***
	(0.002)	(0.015)	(0.001)	(0.010)	(0.007)
GDP per capita (ln)	0.020**	-0.065	0.007	0.068	0.538***
	(0.008)	(0.064)	(0.005)	(0.062)	(0.157)
Population size (ln)	-0.004	-0.469***	-0.013***	-0.352***	-0.243
	(0.007)	(0.057)	(0.004)	(0.048)	(0.618)
Interstate war	-0.009	-0.032	-0.027	-0.347	-0.279
	(0.045)	(0.267)	(0.034)	(0.287)	(0.189)
Civil war	-0.154***	-1.136***	-0.111***	-1.203***	-0.642***
	(0.046)	(0.196)	(0.022)	(0.120)	(0.114)
Judicial independence	-0.020	0.276***	-0.003	0.251***	0.117***
	(0.015)	(0.097)	(0.008)	(0.066)	(0.042)
INGOs	-0.000	0.001***	0.000	0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.955***		0.959***		
	(0.015)		(0.007)		
Observations	660	624	2,316	2,337	2,337
R-squared	0.986	0.716	0.985	0.620	0.915

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 5A

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.012**	-0.211*	-0.011**	-0.203*	0.018
	(0.005)	(0.107)	(0.005)	(0.107)	(0.109)
Polity	0.002***	0.033***	0.003***	0.033***	0.018**
	(0.001)	(0.008)	(0.001)	(0.007)	(0.008)
GDP per capita (ln)	0.007**	0.121***	0.006***	0.147***	0.178***
	(0.003)	(0.045)	(0.002)	(0.041)	(0.049)
Population size (ln)	-0.005***	-0.345***	-0.006***	-0.341***	-0.096*
	(0.002)	(0.035)	(0.002)	(0.031)	(0.056)
Interstate war	0.005	-0.346*	-0.040*	-0.301*	-0.119
	(0.025)	(0.202)	(0.020)	(0.175)	(0.154)
Civil war	-0.059***	-1.012***	-0.057***	-0.991***	-0.554***
	(0.014)	(0.096)	(0.012)	(0.091)	(0.101)
Judicial independence	-0.014***	0.200***	-0.014***	0.226***	0.101**
	(0.005)	(0.054)	(0.005)	(0.053)	(0.044)
INGOs	-0.000	0.001***	-0.000	0.000***	0.000**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.989***		0.989***		
	(0.003)		(0.003)		
Observations	6,280	6,310	8,302	8,491	8,491
R-squared	0.990	0.587	0.990	0.594	0.818

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 5B

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.002	-0.108	-0.016	-0.185	0.042
	(0.013)	(0.124)	(0.012)	(0.127)	(0.107)
Polity	0.004***	0.026**	0.003***	0.028***	0.032***
	(0.001)	(0.011)	(0.001)	(0.009)	(0.007)
GDP per capita (ln)	0.006	0.037	0.007	0.068	0.539***
	(0.005)	(0.066)	(0.005)	(0.062)	(0.158)
Population size (ln)	-0.012**	-0.346***	-0.013***	-0.352***	-0.271
	(0.005)	(0.048)	(0.004)	(0.048)	(0.618)
Interstate war	0.042	-0.353	-0.026	-0.347	-0.278
	(0.045)	(0.286)	(0.035)	(0.287)	(0.188)
Civil war	-0.123***	-1.145***	-0.111***	-1.203***	-0.649***
	(0.027)	(0.130)	(0.022)	(0.120)	(0.115)
Judicial independence	-0.003	0.203***	-0.003	0.252***	0.118***
	(0.010)	(0.067)	(0.008)	(0.066)	(0.042)
INGOs	0.000	0.001***	0.000*	0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.955***		0.959***		
	(0.010)		(0.007)		
Observations	1,606	1,598	2,316	2,337	2,337
R-squared	0.985	0.643	0.985	0.620	0.915

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 6A

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.006	-0.156	-0.010	-0.139	0.061
	(0.010)	(0.165)	(0.008)	(0.145)	(0.151)
Probability of Prohibition	-0.004	-0.216	-0.002	-0.204	0.025
	(0.013)	(0.216)	(0.009)	(0.158)	(0.162)
Polity	0.004***	0.031**	0.002***	0.037***	0.021**
	(0.001)	(0.012)	(0.001)	(0.009)	(0.009)
GDP per capita (ln)	0.009	0.118	0.009***	0.139**	0.224***
	(0.006)	(0.079)	(0.003)	(0.054)	(0.057)
Population size (ln)	-0.003	-0.401***	-0.006**	-0.370***	-0.124**
	(0.004)	(0.048)	(0.002)	(0.037)	(0.062)
Interstate war	-0.023	0.089	0.002	0.028	0.001
	(0.038)	(0.247)	(0.028)	(0.137)	(0.137)
Civil war	-0.047***	-1.039***	-0.046***	-0.972***	-0.423***
	(0.017)	(0.224)	(0.013)	(0.131)	(0.128)
Judicial independence	-0.032***	0.036	-0.014**	0.169***	0.073
	(0.009)	(0.088)	(0.006)	(0.061)	(0.052)
INGOs	-0.000	0.001***	-0.000	0.001***	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.997***		0.992***		
	(0.005)		(0.003)		
Observations	1,692	1,692	6,300	6,440	6,440
R-squared	0.992	0.653	0.991	0.593	0.826

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 6B

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.008	-0.214	-0.016	-0.224	-0.084
	(0.022)	(0.235)	(0.016)	(0.204)	(0.158)
Probability of Prohibition	-0.003	0.104	0.000	0.021	0.209
	(0.022)	(0.289)	(0.017)	(0.203)	(0.143)
Polity	0.007**	0.052***	0.002	0.038***	0.033***
	(0.003)	(0.019)	(0.001)	(0.010)	(0.007)
GDP per capita (ln)	-0.005	-0.241**	0.005	0.000	0.312
	(0.014)	(0.099)	(0.008)	(0.102)	(0.235)
Population size (ln)	-0.012	-0.547***	-0.015***	-0.422***	-0.944
	(0.009)	(0.072)	(0.005)	(0.066)	(0.607)
Interstate war	0.134	-0.311	0.003	-0.114	-0.040
	(0.093)	(0.358)	(0.057)	(0.216)	(0.100)
Civil war	-0.114***	-1.191***	-0.090***	-1.276***	-0.515***
	(0.041)	(0.234)	(0.023)	(0.160)	(0.129)
Judicial independence	-0.029*	0.177	-0.012	0.208**	0.071
	(0.017)	(0.134)	(0.010)	(0.087)	(0.048)
INGOs	0.000	0.001***	0.000	0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.975***		0.966***		
	(0.017)		(0.008)		
Observations	430	426	1,730	1,743	1,743
R-squared	0.989	0.726	0.987	0.631	0.932

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 6C

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.014**	-0.246*	-0.012*	-0.264*	0.077
	(0.006)	(0.140)	(0.006)	(0.134)	(0.139)
Polity	0.002***	0.036***	0.002***	0.035***	0.021**
	(0.001)	(0.009)	(0.001)	(0.008)	(0.008)
GDP per capita (ln)	0.009***	0.131**	0.009***	0.144***	0.223***
	(0.003)	(0.064)	(0.003)	(0.054)	(0.057)
Population size (ln)	-0.005**	-0.384***	-0.006**	-0.373***	-0.125**
	(0.002)	(0.043)	(0.002)	(0.037)	(0.063)
Interstate war	0.026	-0.041	0.002	0.041	0.000
	(0.038)	(0.179)	(0.028)	(0.140)	(0.137)
Civil war	-0.033**	-0.916***	-0.045***	-0.966***	-0.423***
	(0.016)	(0.141)	(0.013)	(0.131)	(0.127)
Judicial independence	-0.014**	0.105	-0.015***	0.159**	0.073
	(0.006)	(0.064)	(0.006)	(0.062)	(0.052)
INGOs	-0.000	0.001***	-0.000	0.001***	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.993***		0.992***		
	(0.004)		(0.003)		
Observations	4,398	4,398	6,300	6,440	6,440
R-squared	0.992	0.587	0.991	0.591	0.826

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 2: Regression Results Reported in Figure 6D

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.003	-0.122	-0.016	-0.211	0.067
	(0.015)	(0.172)	(0.014)	(0.170)	(0.127)
Polity	0.004**	0.042***	0.002	0.038***	0.035***
	(0.002)	(0.014)	(0.001)	(0.010)	(0.007)
GDP per capita (ln)	0.002	-0.154	0.005	0.000	0.289
	(0.008)	(0.107)	(0.008)	(0.102)	(0.242)
Population size (ln)	-0.013**	-0.473***	-0.015***	-0.421***	-1.004
	(0.006)	(0.061)	(0.005)	(0.065)	(0.617)
Interstate war	0.095	-0.078	0.003	-0.114	-0.043
	(0.083)	(0.353)	(0.057)	(0.216)	(0.101)
Civil war	-0.106***	-1.273***	-0.090***	-1.276***	-0.530***
	(0.027)	(0.163)	(0.023)	(0.161)	(0.131)
Judicial independence	-0.009	0.123	-0.012	0.209**	0.074
	(0.012)	(0.105)	(0.010)	(0.086)	(0.047)
INGOs	0.000	0.001***	0.000	0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Latent Repression $_{t-1}$	0.966***		0.966***		
	(0.012)		(0.008)		
Observations	1,026	1,024	1,730	1,743	1,743
R-squared	0.989	0.684	0.987	0.631	0.932

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 3: Regression Results Reported in Table 3A

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.353*	-0.509	-0.268	-0.452	-0.086
	(0.195)	(0.318)	(0.186)	(0.292)	(0.395)
Probability of Prohibition	-0.310	-0.365	-0.071	-0.044	-0.018
	(0.227)	(0.367)	(0.203)	(0.315)	(0.428)
Polity	0.035**	0.055**	0.026**	0.042***	0.087***
	(0.016)	(0.023)	(0.010)	(0.016)	(0.021)
GDP per capita (ln)	0.111	0.017	0.102*	0.123	0.513**
	(0.080)	(0.143)	(0.058)	(0.093)	(0.222)
Population size (ln)	-0.351***	-0.663***	-0.358***	-0.599***	0.860
	(0.080)	(0.124)	(0.044)	(0.064)	(0.625)
Interstate war	-0.027	0.383	0.027	-0.145	0.078
	(0.594)	(0.589)	(0.373)	(0.519)	(0.591)
Civil war	-0.518	-1.342***	-0.912***	-1.466***	-1.117***
	(0.331)	(0.435)	(0.162)	(0.239)	(0.260)
Judicial independence	0.257*	0.423**	0.261***	0.442***	0.336**
	(0.151)	(0.208)	(0.086)	(0.117)	(0.137)
INGOs	0.000*	0.001***	0.001***	0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
CIRI $_{t-1}$	2.281***		2.222***		
	(0.165)		(0.097)		
Observations	1,020	1,030	4,459	4,685	4,685

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 3: Regression Results Reported in Table 3B

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	0.321	0.909**	0.301	0.747*	-0.205
	(0.242)	(0.411)	(0.220)	(0.386)	(0.646)
Probability of Prohibition	0.307	0.135	0.021	-0.170	-0.911
	(0.259)	(0.461)	(0.246)	(0.429)	(0.763)
Polity	-0.052**	-0.049	-0.018*	-0.048***	-0.038
	(0.023)	(0.035)	(0.010)	(0.017)	(0.024)
GDP per capita (ln)	0.058	0.151	0.057	0.024	-0.650**
	(0.091)	(0.148)	(0.056)	(0.104)	(0.272)
Population size (ln)	0.397***	0.768***	0.406***	0.741***	-0.789
	(0.123)	(0.197)	(0.060)	(0.102)	(1.326)
Interstate war	-0.028	-0.062	0.150	0.254	1.154*
	(0.775)	(0.759)	(0.382)	(0.626)	(0.683)
Civil war	0.181	0.909**	0.542***	1.130***	0.506
	(0.235)	(0.355)	(0.169)	(0.279)	(0.322)
Judicial independence	-0.261	-0.276	-0.247**	-0.382**	-0.432***
	(0.228)	(0.330)	(0.097)	(0.149)	(0.162)
INGOs	-0.001**	-0.001***	-0.001***	-0.001***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Hathaway $_{t-1}$	1.940***		1.876***		
	(0.171)		(0.101)		
Observations	502	504	1,926	2,084	2,084

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 3: Regression Results Reported in Table 3C

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.045	0.155	0.083	0.043	-0.061
	(0.312)	(0.491)	(0.261)	(0.416)	(0.829)
Probability of Prohibition	0.050	0.546	0.197	0.267	0.910*
	(0.391)	(0.719)	(0.264)	(0.423)	(0.547)
Polity	-0.088***	-0.140***	-0.034**	-0.046**	-0.074
	(0.032)	(0.051)	(0.015)	(0.023)	(0.050)
GDP per capita (ln)	-0.304*	-0.531**	-0.031	-0.062	0.283
	(0.183)	(0.240)	(0.064)	(0.112)	(0.306)
Population size (ln)	0.169*	0.510***	0.261***	0.555***	-2.165
	(0.099)	(0.143)	(0.056)	(0.084)	(2.021)
Interstate war	-2.421***	-1.688*	0.246	-0.195	0.263
	(0.794)	(0.870)	(0.556)	(0.579)	(0.424)
Civil war	0.902**	0.530	0.609	0.845**	0.030
	(0.402)	(0.550)	(0.370)	(0.385)	(0.526)
Judicial independence	0.054	0.055	-0.138	-0.239	-0.836***
	(0.255)	(0.380)	(0.124)	(0.186)	(0.290)
INGOs	0.001*	0.001***	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
ITT_{t-1}	0.919***		0.799***		
	(0.134)		(0.058)		
Observations	212	212	1,141	1,441	1,441

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 3: Regression Results Reported in Table 3D

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.208	-0.052	-0.046	-0.023	-0.513
	(0.212)	(0.334)	(0.186)	(0.326)	(0.492)
Probability of Prohibition	-0.190	-0.344	-0.071	-0.376	1.094***
	(0.256)	(0.413)	(0.196)	(0.349)	(0.353)
Polity	-0.065***	-0.098**	-0.039***	-0.058***	-0.068***
	(0.025)	(0.040)	(0.011)	(0.019)	(0.024)
GDP per capita (ln)	-0.154	-0.108	0.048	0.168*	0.004
	(0.109)	(0.206)	(0.065)	(0.094)	(0.187)
Population size (ln)	0.222***	0.469***	0.280***	0.549***	1.230
	(0.067)	(0.132)	(0.057)	(0.072)	(0.851)
Interstate war	-1.047	-1.277	-0.730***	-0.423	-0.177
	(0.654)	(0.997)	(0.224)	(0.344)	(0.204)
Civil war	-0.109	-0.490	-0.124	0.466	0.089
	(0.423)	(0.445)	(0.188)	(0.370)	(0.217)
Judicial independence	-0.198	-0.080	-0.114	-0.102	-0.323*
	(0.193)	(0.339)	(0.117)	(0.156)	(0.173)
INGOs	0.001**	0.001	0.000**	0.000	0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.000)
Stealth $_{t-1}$	0.107***		0.108***		
	(0.024)		(0.020)		
Observations	268	268	1,476	1,620	1,620

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Appendix 3: Regression Results Reported in Table 3E

	(1) Baseline Model	(2) Baseline Model w/o Lagged DV	(3) No Matching	(4) No Matching w/o Lagged DV	(5) Country Fixed Effects
Torture Prohibition	-0.097	-0.125	-0.014	-0.018	-0.579
	(0.219)	(0.298)	(0.203)	(0.297)	(0.546)
Probability of Prohibition	0.308	0.227	0.252	0.271	0.799**
	(0.299)	(0.353)	(0.209)	(0.296)	(0.393)
Polity	-0.053**	-0.088**	-0.021**	-0.023	-0.049*
	(0.025)	(0.035)	(0.010)	(0.015)	(0.027)
GDP per capita (ln)	-0.185	-0.317*	-0.087	-0.055	0.050
	(0.121)	(0.166)	(0.057)	(0.079)	(0.188)
Population size (ln)	0.099	0.293***	0.156***	0.343***	0.934
	(0.079)	(0.095)	(0.047)	(0.057)	(0.805)
Interstate war	-0.639*	-1.166***	-0.279	-0.569**	0.011
	(0.342)	(0.388)	(0.275)	(0.282)	(0.135)
Civil war	-0.020	-0.075	0.024	0.554*	0.142
	(0.321)	(0.494)	(0.150)	(0.283)	(0.255)
Judicial independence	-0.295	-0.284	-0.132	-0.250**	-0.228
	(0.183)	(0.266)	(0.090)	(0.126)	(0.140)
INGOs	0.001***	0.001***	0.000***	0.000***	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Scarring $_{t-1}$	0.078***		0.072***		
	(0.020)		(0.011)		
Observations	268	268	1,476	1,620	1,620

- Robust standard errors in parentheses.
- All models include year fixed effects.
- *** p<0.01, ** p<0.05, * p<0.1

Readers with comments should address them to:

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