

Do Norms Reduce Torture?

Michael J. Gilligan^{*} and Nathaniel H. Nesbitt

Abstract

One of the most important developments in international political and legal theory over the last fifteen years has been the assertion that norms affect state behavior. Scholars have claimed that states are constrained by norms of appropriate behavior and furthermore that norms actually change (“reconstitute”) states’ understandings of their interests thereby leading states to adapt their behavior in accordance with these new understandings. We test the proposition that norms alter state behavior with respect to the expanding international norm against torture from 1985 through 2003. Unfortunately we find no evidence that the spreading of the international norm against torture, measured by the percentage of countries in the world that have acceded to the UN Convention Against Torture, has led to any reduction in torture according to a variety of measures.

^{*} Corresponding Author. Department of Politics New York University, 19 W. 4th NY, NY 10012. Email: mg5@nyu.edu.

Introduction

This paper tests the claim that human rights norms affect state behavior. Specifically, we test whether or not the proliferation of an international anti-torture norm has led to a reduction in the use of torture by states. Unfortunately, we were unable to find any evidence that the norm, operationalized by the percentage of the countries in the world that are members of the UN Convention, had any effect in reducing reported use of torture by states. If anything, our results suggest that torture has gotten worse over the period of the study despite the fact that membership in the UN Convention against Torture (the CAT) has grown throughout the period.

One of the most important developments in international political and legal theory over the last fifteen years has been the assertion that norms affect state behavior.¹ Scholars have claimed that states are constrained by norms of appropriate behavior and furthermore that norms actually change (or in the jargon of the profession, “reconstitute”) states’ understandings of their own interests thereby leading states to change their behavior as they pursue these new interests. The argument for the importance of norms has been made with respect to areas as varied as national security policy,² environmental policy,³ health policy (Sikkink 1986) and arms control (Price 1998), but nowhere has the claim been more forcefully made than with regard to human rights.⁴ Despite some

¹ Nadelman 1990, Diehl and Goertz 1992, Klotz 1995, Finnemore 1996, Florini 1996a 1996b, Katzenstein, ed. 1996, Raymond 1997, Checkel 1998, and Finnemore and Sikkink 1998 are a few examples of important contributions.

² See the various papers in the volume edited by Katzenstein 1996.

³ Young and Levy 1998 provide one example. Downs 2000, a critic of the argument, provides a useful overview of the effect of norms with respect to environmental agreements.

⁴ Examples include Klotz 1995, Sikkink 1998, Risse, Ropp and Sikkink, eds. 1999, Clark 2001, Khagram, Riker and Sikkink, eds. 2002 to name just a few.

chipping away at it since September 11, one of the strongest prohibitory norms under the rubric of human rights is that against torture (Osiel 2001, Levinson 2002).⁵

To date, these theoretical advancements, as important as they are for our understanding of international politics, have generally not been subjected to rigorous empirical tests. As Simmons (2006, 6) has pointed out “...the emphasis [in the norms literature] is on the spread of norms rather than observed changes in behavior. Explaining behavioral outcomes is largely left to more positivist theoretical traditions.” Furthermore when these scholars have offered evidence for the importance of norms (Klotz 1995, Price 1998 and so on) the dominant methodology has been to provide a small number of nonrandomly selected case studies.⁶

This methodological approach casts something of a shadow over the evidence put forward to support the claim that norms affect states behavior for at least two reasons. First this methodology often implicitly assumes that the processes under study are deterministic. However social scientific claims are typically probabilistic. They imply that *on average* a certain behavior should or should not be observed. The reason that we typically apply statistical tests to our models is to make more convincing our claims that the relationships we have found are not due to random chance. A second issue is that the

⁵ Even in the post-September 11 era, it is interesting to note that the Bush Administration’s defense of interrogation practices that are, to say the least, questionable has *not* been to claim that an exception to the prohibition of torture in extreme circumstance is warranted but instead to claim that those practices do not constitute torture and are thereby compliant with the norm. This defense by the Administration is precisely the kind of behavior Finnemore and Sikkink described when they wrote: “[H]ow do we know a norm when we see one? We can only have indirect evidence of norms ... However because norms ... embody a quality of “oughtness” and shared moral assessment, norms prompt justification for action.”

⁶ Herman and Shannon (2001) is an important exception to this statement. They provide evidence from a survey experiment that indicates that elites are constrained by norms, in their case norms against the use of force, but that the effect of norms is conditioned by the respondents’ material interest and perceptions of the political context.

cases provided as evidence rarely examine failures.⁷ The claim is that the presence of a certain prohibitory norm should lead to less of the prohibited behavior. Thinking in terms of a two-by-two table (see Figure 1) the claim implies that cases should be clustered in the upper right and lower left cells. Unfortunately the evidence supplied so far has sought only to establish that there is a multiplicity of cases in the upper right cell. They have not established a multiplicity of cases in the lower left cell nor have they established a statistical paucity of cases in the upper left and lower right cells.

In short the approach of empirical scholars in this literature has been to point to the gaggle of “black swans” that proves that not all swans are white. By showing that there are cases where norms affect state behavior they are able to prove the falsity of the empirical claim that norms *never* matter. However no serious social scientist would make such an extreme claim. In order to meet the typical standards of social science, evidence in support of the claim that norms affect state behavior should show that there is less bad behavior (as defined by the norm) when norms are present and more bad behavior where norms are absent and that these differences (if any) cannot be accounted for by random error. Most scholars are familiar with the adage “correlation does not imply causality.” Unfortunately the evidence provided for norms-based arguments has not even established correlation because of these problems. Our purpose in this paper is to fill this gap in the literature.

Our paper contributes to both the theoretical literature on norms and the policy literature on human rights, specifically the prohibition of torture. With regard to the former the paper provides a test of the claims that norms alter state behavior. With regard

⁷ See Legro (1997) who goes on to provide evidence of some cases of norm failure. Price (2003) raises the same criticism with respect to the related issue of “transnational civil society.”

to the latter our analysis questions if the time and effort spent spreading norms through the creation of new international law is effective and if not whether other possible strategies for limiting torture (perhaps through domestic political mechanisms in the countries where torture is prevalent) should be explored.

The paper is organized as follows. In the following section we discuss in more detail the various claims about how norms affect state behavior and of how we measure the spread of the anti-torture norm in such a way as to conform with those theoretical claims. We then turn to a discussion of torture and how we measure the prevalence of torture in a given country-year. In the third section we present our statistical results using a variety of estimation techniques and measures of the prevalence of torture. The fourth section offers a discussion of some of the problems that may plague our statistical analysis with a special focus on measurement of the prevalence of torture. We offer a test that, while not a definitive vindication of our results, does bracket the possible extent to which measurement error may be the reason for our negative findings. Section five concludes.

Norms

We adhere to the consensus definition of a norm as “a standard of appropriate behavior for actors with a given identity” (Finnemore and Sikkink 1998, 819).⁸ This definition better captures the focus in this literature on the causal effect of deontological norms. We purposefully avoid behavioral definitions of norms such as Thomson’s (1993) recommended definition as “normal state practices” because such definitions lead easily

⁸ See also Sikkink (1998). In our view the definition quoted above offers a slight improvement to Katzenstein’s (1996, 5) “collective expectations for the proper behavior of actors with a given identity” because the word “standard” captures better the deontological character of norms that constructivists stress.

to tautology—it would be virtually impossible to reject the hypothesis that norms affect state behavior when they are defined as the way states most commonly behave.⁹ We also refrain from using Axelrod’s (1986) definition, which like Thompson’s is behavioral but adds the feature that actors “are often punished when seen not to be acting this way [that is, in accordance with the norm].” The problem of potential tautology aside, the addition of punishment makes the concept of “norm” indistinguishable from “cooperative equilibrium” as discussed by game-theoretic approaches to international cooperation and effectively jettisons the deontological aspect of norms that scholars in this literature are most interested in, namely that states are effected by the moral impact of norms even when no punishments are threatened.

In testing if norms affect state behavior we utilize the theory of Finnemore and Sikkink (1998) who in turn apply the ideas of Sunstein (1996). In this formulation a norm is first established and adhered to by a few progressive-thinking states (norm entrepreneurs). Once these initial adherents have signed on to the norm it then becomes in the interests of more moderate members to adopt it, creating a snowball effect. As more states adopt a norm, even states that were initially less enthusiastic about the norm determine that it is in their interests to adopt it. Once a “critical mass” of states has signed on to the norm a “tipping point” is reached after which most states will adhere to the norm, a phenomenon they refer to as a “norm cascade.”¹⁰

Scholars have also hypothesized that the creation of international legal instruments (such as the convention we study in this paper) can have a strong positive impact on the establishment of norms and that the more widely accepted a norm is the

⁹ The same point has been made by Finnemore and Sikkink (1998, 592) and Legro (1997).

¹⁰ Also see Sikkink 1998 and Khagram et al. 2002, 14-15.

stronger it is. Risse, Ropp and Sikkink contend that "... international law and international organizations are ... the primary vehicle for stating community norms and for collective legitimation." Similarly, Khagram et al. (2002), building on the theory of Finnemore and Sikkink (1998), write "The entry of a treaty into force can often be used as an indicator of a norm reaching a threshold or tipping point. Widespread and rapid treaty ratification can be a signal of an international norms cascade." They go on to suggest that: "... as a working operational definition we suggest that approximately one-quarter to one-third of the actors must support and accept new standards of behavior before we can speak of the existence of a new norm."

By extension then the number of signatories of human rights treaties is an important indicator of the strength of a norm. The more countries that have signed on to the norm the more likely a "critical mass" will have been achieved. States are more likely to adhere to norms that a greater proportion of other states regard as legitimate. We agree with the scholars cited above that the most outward sign of whether a state considers a norm to be a legitimate standard of state behavior is whether or not that state has signed the international convention that codifies that norm. Thus the number of signatories of a convention that codifies a certain norm is the best measure of the number of states that consider that norm legitimate.¹¹

Figure 2 indicates these processes at work with regard to the UN Convention against Torture. The Convention opened for signature in February 1985. By the end of the year it had 41 signatures, and it entered into force on June 26, 1987, thirty days after

¹¹ Numbers aside great power antinomy toward a particular norm may weaken the norm regardless of how many other countries have adopted it however in the case of the anti-torture norm this issue is not of great concern. The UN Convention against Torture is one of the few UN human rights conventions that the United States has actually signed, and all other great powers have signed as well.

the ratification of the twentieth member. As shown in Figure 2, the treaty met the one-quarter to one third threshold within the first year it was open for signature. The treaty continued to collect adherents until now over 77 percent of the countries in the world have signed, ratified or acceded to it.

Torture

Despite the putative prohibition of torture in international law the practice persists. According to recent data from Amnesty International, “torture or ill-treatment” took place in 132 states in 2003 including many liberal democracies. In Haiti, Judith Roi and three others were reportedly beaten with iron bars while in police custody. In Lebanon, Husayn Ahmad al-Qarahani testified that he and other detainees were beaten and hung by the wrists, bound behind the back. In Germany, an intense debate about the acceptability of torture in certain cases arose after a report that a police captain ordered the use of force on a detainee. In that country, six police officers were convicted of beating to death Stephan Neisius, a detainee.¹² While torture continues to occur around the world, the level of torture varies substantially across regions and countries (Mitchell & McCormick 1988). In other words, some governments torture their populations more than others. This study provides insight into why this is so by addressing the question of the impact of international norms on state behavior. Despite these grim episodes (and many others) our hope was to establish that as the norm against torture has spread, torture has become significantly less prevalent.

As with most measures of human rights practice we expected there to be substantial measurement error in any single indicator of a state’s use of torture. Therefore

¹² Anecdotes taken from Amnesty International Report 2004.

we thought it prudent to construct a scale which captured the underlying but ultimately unobserved prevalence of torture in each country-year in our data set. To that end we completed factor analysis on multiple indicators that we have collected. The first is a five point scale developed by Hathaway (2002) and which we have extended forward in time as indicated in the data appendix. In this measure a score of one indicates that no torture is occurring in a given country-year and a score of five indicates that the use of torture is widespread in that country-year. The second measure we use is a three-point scale developed by Cignarelli and Richards (2005). In this scale a score of zero corresponds to widespread use of torture by the respective state in a given year and a score of 2 corresponds to no torture occurring.

For our first factor scoring we used the two measures described in the previous paragraph to construct an underlying factor score. For our second factor score we included the two scales mentioned above and in addition we included two other measures—the coding of the State Department and Amnesty International reports from the Political Terror Scale (PTS). The Political Terror Scale is a measure that varies from one to five, with one being no use of terror by the state against its citizens and five corresponding to extensive use of terror by the state in a given year. Political Terror as specified by Gibney and Dalton (1996) is not an exact match for torture because it includes other factors such as political imprisonment and therefore our second factor score is perhaps capturing broader characteristics of the polity other than just torture. Once we obtained these underlying factor scores we rescaled them so that they varied over the zero-one interval.

Descriptive statistics for these scores, which we call “torture score 1” and “torture score 2” respectively are presented in Table 1 for the full sample and for Canada and Afghanistan. Canada and Afghanistan provide two interesting, contrasting examples of the score. Canada of course has a very good human rights record and Afghanistan has a dismal one. This is evidenced in the scale by a considerably higher mean, minimum and maximum value for Afghanistan than for Canada. Descriptive statistics for the scales from which our scores were constructed are also presented in Table 1 for the full sample, Canada and Afghanistan.

Control Variables

To guard against omitted variable bias we also included a variety of control variables. Because our main interest in these variables is only as controls we dispense with the theoretical justification for including them in the interests of brevity and instead refer the reader to the sources cited. Population size, level of development, level of democracy and whether the country in question is at war (either international or civil) have been found to have an impact on levels of human rights violations or “repression” variously defined. (Henderson 1991; Henderson 1993; Poe & Tate 1994; Mitchell & McCormick 1988; Blanton 2000). Therefore we also control for these factors. British colonial heritage has consistently been shown to have a negative impact on human rights violations, however we cannot control for that variable because we include country-specific fixed effects. Following the scholars indicated above we also control for

economic growth, population growth, the extent to which the regime is controlled by the military and ethnic fractionalization.¹³

One final control variable is a dummy variable that is equal to one if a country is a signatory of the UN anti-torture convention in a given year and zero otherwise. We recognize that this control variable may be “endogenous.” Much has been written about how nonrandom assignment obscures the inferences that one can draw from such measures (Downs, Rocke, and Barsoom 1996). Because the “treatment” (in this case membership in the convention) is not assigned randomly but instead is selected by the members, the researcher cannot infer from simple regression techniques whether different behaviors by treaty members are due to the treaty or other characteristics of the countries that signed the convention. If treaty membership is endogenous (and the endogeneity is sufficiently severe) our estimates will be biased. Fortunately our main interest is not in the effect of the membership in the treaty itself but in the effect of the norm, which affords us more estimation options as discussed below.¹⁴ Note also that similar self-selection problems do not plague our measure of norms because norms as we measure them are a *systemic* variable, not a country-level variable. It is the running total of the number of signatories and is not a variable that is under the control of an individual country.

Results

¹³ We also tested the effects of ethnic fractionalization (Walker and Poe 2002; Lee et al. 2004) and judicial independence (Slaughter 1993, 1998) but neither was significant and both reduced our sample size considerably so we do not report those results here in the interests of brevity.

¹⁴ Regardless, the treaty variable did not indicate that signatories of the treaty had significantly different records on torture than non-members, a finding that corroborates Hathaway’s (2002) results.

Table 3 presents the estimates using the two different versions of the continuous torture scale that we developed using factor analysis. We estimated the models using time-series cross sectional regression controlling for fixed effects. Using this dependent variable we did not find that countries with larger populations were more likely to engage in torture *ceteris paribus*, unlike others before us (Henderson 1993, Poe and Tate 1994, Poe Tate and Keith 1999) although this result changed in later specifications (see Tables 4 and 5). We were able to find no significant impact of population growth, a finding that is consistent with Poe and Tate (1994) and Poe, Tate and Keith (1999). We were also able to find fairly robust evidence that countries plagued with civil war practiced torture more prevalently than those that were not. International war, however, did not produce any significant increase in the torture scale. Consistent with Poe and Tate and Poe, Tate and Keith, economic development, in our cases measured by log of GDP per capita, was a strong predictor of a reduction in the use of torture, and GDP growth produced spottier results in that it was significant using Torture score 2 but not Torture score 1. In both cases, however, it was in the predicted direction. We also generally corroborated the finding of those previous studies that democracies clearly engaged in less torture than dictatorships, even though we used a different measure of democracy than they did. The variable was borderline significant using torture score 1 and was fully significant using torture score 2. The effect of democracy was further substantiated in most of the other specifications we estimated (see Tables 4 and 5). Finally our results are consistent with those of Hathaway (2002) in that we were able to find no evidence that states that have signed the UN convention against torture were any less prone to engage in torture than those that did not. Of course our main interest is the effect of the expanding anti-torture

norm on state behavior. The results could not be clearer: there is no evidence that as more states have joined the CAT, states use of torture has abated. Indeed if anything the results suggest that the levels of torture have *increased*.

As a robustness check we also estimated our model on the two categorical measures that we used in the factor analysis described above—our temporally-augmented data on Hathaway’s five-point scale and the three point CIRI Torture scale provided by Cignarelli and Richards. In their scale, countries where torture was more prevalent receive a smaller score so we altered the scale accordingly to make it consistent with our other measures. We estimated the model using ordered probit, clustering standard errors by country. The results from this specification are presented in Table 4. They are generally consistent with the results we obtained using the continuous measures.

Consistent with our predecessors (Henderson 1993, Poe and Tate 1994, Poe Tate and Keith 1999) large populations, civil war and autocratic regimes were associated with more torture and large GDP per capita was associated with less torture. Whether or not the country has signed the UN Convention had no impact on the dependent variable as before. The results of the ordered probit analysis also agree with our earlier finding that the spreading of the anti-torture norm is, if anything, associated with an increase in torture.

As a final robustness check we dichotomized the CIRI torture variable and our temporally-augmented version of the Hathaway’s scale and tested if the effect of the expanding norms was different on several sub samples as described below. We recoded the CIRI torture variable as a one if the original coding was zero and zero otherwise. We recoded the Hathaway variable to one when the original values were four or five and zero

otherwise. In other words, states that torture more according to these scales are coded with a “1” and those that torture less are coded with a “0.” The estimates on the full sample are presented in the first two columns of Table 5. As in our earlier specifications large populations, civil war and autocratic regimes were associated with more torture and large GDP per capita was associated with less torture. Whether or not the country has signed the UN Convention had no impact on the dependent variable as before. These results also confirm our earlier finding that the spreading of the anti-torture norm is, if anything, associated with an increase in torture.

Next we test if the norm has an effect on what might be called “transition probabilities” that is, the probability that a country that practices torture is more likely to transition to a country that does not practice torture or that a country that currently does not practice torture is less likely to transition to a country that does practice torture as a result of the norm. At time $\tau-1$ states either practice torture or they do not. States transition into a type (either the same or the alternate one) at time τ . For example a state that practices torture at time $\tau-1$ may transition into a state that practices torture with probability p_{tt} and it may transition into a state that does not practice torture with probability $1-p_{tt}$. Similarly a state that practices torture at time $\tau-1$ may, in time τ , transition into a state that practices torture with probability p_{nt} and a state that does not practice torture with probability $1-p_{nt}$. To estimate the probabilities p_{tt} and p_{nt} we simply estimate the probit model for each of the two sub samples, one for countries that did not practice torture at time $\tau-1$ and again for countries that did practice torture at time $\tau-1$.

This procedure addresses the potential problem that the norm has a different effect on states that commit torture than it does on states that do not. It is possible that these two

subsamples should not be pooled because the data generating process that produces torture (or not) is truly different in those states that engage in torture than in those states that do not. Specifically it pools samples of countries where torture is not practiced with those where it is. There may be reason to believe that torture is produced by a markedly different data generating process in each of the two sets of countries.

The results of these specifications are presented in columns three through six of Table 5. Some of the results in these specifications were the same as in our previous analysis. For example, large populations and civil war appear to be associated with an increase probability of transition to a type of state that practices torture in both the samples that practiced torture at $\tau-1$ and those that did not. GDP per capita reduced the probability of transition to a type of state that practices torture in both the samples that practiced torture at $\tau-1$ and those that did not although the effect was stronger in the latter sub sample than the former, and indeed the effect was not even statistically significant in the former sample for the dichotomous torture variable that we created from Hathaway's scale. However these specifications did produce some changes in results compared to the earlier specifications. GDP growth lead to a reduction in probability of transition to a state that practices torture only in the subsample that did not practice torture in the previous period. Thus there is some indication that the data generating process for countries that practice torture and those that do not is somewhat different. Also somewhat different from previous specification was the effect of regime type, which was not consistently significant across the four specifications, although it was always in the expected direction. Finally, the effect of our measure of the norm was quite similar to

previous specifications. Again, if anything, as the number of signatories grew states in both subsamples became *more* likely to transition to the type of state the commits torture.

Finally we come back to the issue of the endogenous treaty signatory variable. If that variable is endogenous its coefficient will be biased and by extension so will the coefficients on all other variables in the specification. Typically the way one would handle this problem is to perform a Heckman regression which first models the selection process and then uses estimates from that model to control for the nonrandom assignment of the treaty variable. Unfortunately we could not come up with a plausible instrument—a variable correlated with a country’s decision to sign the treaty but not correlated with its human rights record. Therefore we take a different tack. For our purposes we do not require a precise estimate of the effect of the treaty variable, only of the effect of the norm on state behavior. We split our sample between those countries that signed the treaty and those that did not and estimate the effect of the norm in each case.

These results answer a slightly different question than the previous ones. They answer the question “Does the norm affect state behavior in the predicted way among signatories of the treaty?” and “Does the norm affect state behavior in the predicted way among non-signatories?” In other words, *conditional on* whether (or not) a country has signed the treaty does the norm affect its behavior? These are more limited questions, but they avoid the endogeneity issue and as shown by the results in Table 6 they point in the same direction. The estimated effects of the norm are remarkably similar in both subsamples. When torture scale 1 is used the coefficients are almost identical—0.35 for the subsample that signed the treaty and 0.30 for the sample that did not. When torture scale 2 is used the coefficients are somewhat farther apart—0.165 for the subsample that

signed the treaty and, again, 0.30 for the sample that did not, but they are still quite close and point in the same substantive direction. As mentioned above we regard “torture scale 1” to be the more accurate representation because it does not include other factors like political imprisonments and disappearances. In short the norm did not have the hypothesized effect in either subsample, and in fact the results indicate that torture was becoming worse while the norm spread. The results in this table do not suffer from endogeneity and they all point in the same direction—the norm is not reducing torture.

In summary the specifications in all four tables point to the clear finding that there is no evidence—not even weak evidence—that the spreading of the anti-torture norms, operationalized by the percentage of states that are members of the CAT—had any impact on reducing torture *ceteris paribus*. Indeed if anything the results indicate that torture practices became worse as more countries signed the conventions. A case for the validity of these results is bolstered by the fact that the other results in the tables—those concerning the effect of population size, regime type and economic development—made theoretical sense and were consistent with the findings of other quantitative research on human rights practice. What might explain these disappointing results? One clear suspect is measurement of the dependent variable. Might the results be due to the possibility that the expanding anti-torture norm was encouraging victims to come forward and NGOs to engage in more vigorous scrutiny of state practices? If so, it may be the case that our measures of torture were increasing due to better reporting even while the practice of torture was actually *decreasing*. It is to a lengthier discussion of this potential problem that we now turn.

Measurement

The most serious threat to validity of our findings is the issue of measurement of human rights practices—a topic that has garnered the attention of that literature (Goodman and Jinks 2003, Jabine and Claude 1992 and the articles within).¹⁵ We have attempted to deal with the problem of “noisy” data by using multiple indicators of torture, including combining them via factor analysis.¹⁶ However, the problem is not simply that measures of human rights practices are “noisy” because our finding cannot be driven by random error in our measure of torture. If our results had shown that the spreading of the norm had a negative effect on the prevalence of torture but that the effect was statistically insignificant then we possibly could chalk the results up to measurement error, but our results indicate that torture is actually becoming *worse* as the norm spreads, and this result was statistically significant in general. Random measurement error in the dependent variable can account for insignificant results but it cannot generate coefficients that are so biased as to point in the wrong direction.

The main concern of scholars who are skeptical about quantitative measures of human rights practice is *systematic* bias in the standard measures of human rights in ways that are correlated with the spreading of the anti-torture norm. There are two possible explanations for our findings. One is that the norm had no impact on states’ policies and that torture was not reduced by the putative spreading of the norm. The other is that the amount of actual torture did in fact decline but that the *reported* cases of torture actually rose. The latter may be a plausible outcome for at least two related reasons. First, along

¹⁵ See the articles in the volume edited by Jabine and Claude (1992)

¹⁶ A recent paper by Hathaway and Ho (2004) also uses multiple indicators employing sophisticated Bayesian techniques to address this problem on a common measure of human rights practice (the Political Terror Scale described above). They show that the effects of important variables like regime type are substantially underestimated when random measurement error is not taken into account.

with the spreading of the norm is the spreading of an international bureaucracy and NGO community to monitor that the norm is being observed. Second whereas victims of torture were perhaps more prone to suffer in silence previously now they may be more likely to come forward because they have learned that torture is a criminal activity and that there is an international framework for redressing it.

More formally we have a measurement error problem that can be expressed in three equations: one for the actual level of torture that occurs and one for the unreported torture that occurs.

$$ActualTorture = \beta_0 + \beta_1 Norm + \beta_2 Regime + X\beta + \varepsilon \quad (1.1)$$

$$UnreportedTorture = \gamma_0 + \gamma_1 Norm + \gamma_2 Regime + Z\gamma + \eta \quad (1.2)$$

The variables “Norm” and “regime” are self-explanatory. X and Z are matrices containing the remaining factors that affect actual and unreported torture respectively, β and γ are their coefficient vectors, and ε and η are random error terms. The equation that we actually estimated in the previous section was based on the reported cases of torture, which is the difference of the two quantities in (1.1) and (1.2):

$$ReportedTorture = ActualTorture - UnreportedTorture = (\beta_0 - \gamma_0) + (\beta_1 - \gamma_1)Norm + (\beta_2 - \gamma_2)Regime + X\beta - Z\gamma + \varepsilon - \eta \quad (1.3)$$

The effect of the norm on torture is expected to be negative as is the effect of the norm on unreported torture. Thus the parameter that we actually estimated $\beta_1 - \gamma_1$ underestimates the reduction of torture as a result of the norm. However in order to generate our results γ_1 would have to be substantially larger than β_1 .¹⁷ In other words the effect of

¹⁷ For example from the coefficients on the norms variable in the specifications in Table 3 and 6 that used torture score 1, the effect of the norm on reporting would have to have been about thirty percent larger than the effect of the norm on reducing torture.

the norm on underreporting of torture has to be much stronger than the effect of the norm on reducing torture outright. We submit that while there is certainly some of this process going on, it is implausible that it could be so severe as to completely mask the effect of norms. The claim would have to be that the measurement bias is so severe as to change the sign on the effect of norms.

Furthermore, the effect of the norm on underreporting would have to be considerably greater than the effect of *dictatorship* on underreporting. It is typically argued that more open societies (like democracies) receive worse records on human rights measures *ceteris paribus* than autocracies do simply because it is easier to monitor practices in them. The effect of regime type (dictatorship) on torture is expected to be positive as its effect on unreported torture (that is, there should be less underreported torture in democracies). Thus the parameter that we actually estimated on our regime variable was $\beta_2 - \gamma_2$, which underestimates the increase in torture as a result of dictatorship. However despite that problem our results consistently showed, as expected, that dictatorships tortured more than democracies. In other words the bias was not so bad as to alter the sign of the coefficient. While it is certainly possible that the effect of the norm on underreporting is even greater than the effect of dictatorship on underreporting, a simpler explanation is that dictatorship has a powerful effect in reducing torture and the norm does not.

A second potential problem with our analysis is our measure of the spread of the norm. Disagreements among scholars about whether norms can be measured and how they should be defined have contributed to the lack of empirical research about the theoretical impact of norms (Raymond 1997). Indeed one of the main contributions of our

paper is that we have put forward a clear and plausible operationalization of the spread of an international norm. Does our measure fail to capture the prevalence of the anti-torture norm? Perhaps it does, but it is difficult to believe that such measurement error explains our results, which are the *opposite* of the anticipated sign. Certainly no one would claim that the norm has gotten *weaker* as more countries have signed the three anti-torture conventions. Furthermore as we discussed above, the idea that treaties spread norms and that norms become stronger as more states pledge adherence to them is one of the central tenets of the norms research program. If our measure does not capture these features of the changing international environment what measure possibly could?

Conclusion

The findings in this paper are a disappointment to anyone who believes that an anti-torture norm can reduce the state practice of torture. We could find no evidence to sustain that belief. The spreading of an international norm against torture has, according to our findings, not limited the practice of torture to any extent. Indeed, reported practice worsened as the Convention against Torture gathered more adherents.

Clearly there are issues with the measures of torture that we used, as there are with any measure of human rights practice. However if the measures were fundamentally flawed, our dictatorship variable should have performed equally badly to our norms measure. It did not. Furthermore, we were able to bracket what the effect of the norm on the increased reporting of torture would have to be in order to account for our results, and found that to explain our results the effect of the norm on reporting would have to be significantly larger than the effect of the norm on state practice.

While disappointing, these results are perhaps not all that surprising given earlier studies of the effects of human rights treaties on state practice, notably Hathaway (2002). If states' adherence to an obvious legal commitment like a treaty does not change its practice, what hope is there that something as amorphous as a norm will? Still, behavior *has* changed conterminously with changing norms in the past. The slave trade has been abolished and colonies are virtually non-existent, consistent with the development of strong prohibitory norms against both of those practices. The question is did the changing norm cause the changing behavior or reflect it while behavior changed for other reasons? One has to be very careful about conflating behavioral norms and deontological norms.

On that very point our results point to one inescapable conclusion: the anti-torture norm is a deontological norm but it is not a behavioral norm.¹⁸ It is clearly a deontological norm because 80 percent of the countries in the world have legally bound themselves to it in some fashion. Indeed the scope of the deontological norm is such that even those who torture often recognize the inappropriateness of their actions.¹⁹ And yet the anti-torture norm is clearly not a behavioral norm because exceptions to it are widespread. In 2003, a year in which 75 percent of the countries of the world were members of the CAT, 69 percent of the countries in the world practiced some form of torture according to Amnesty International Reports. Our results suggest, quite simply, that torture is a practice in which leaders engage even though they know it is wrong.

¹⁸ In the terminology of Diehl and Goertz (1992) is a "type 3" norm but not "type 1" norm.

¹⁹ As Admiral Horacio Mayorga, a torturer during Argentina's "Dirty War," later admitted: "The day we stop condemning torture—although we tortured ... is the day we stop being human." The quote is taken from Rosenberg (1991) and is also quoted in Osiel (2001) and Levinson (2002). Vertbitsky (1996) provides the further example of a Captain Scilingo of the Argentinean Navy, who in Vertbitsky's account appeared to be guilt-ridden for his past involvement in throwing live prisoners out of flying aircraft and into the open sea during the same period. We realize, of course, that one would not have to be a complete cynic to recognize the implications of the self-serving nature of these "conversions" for the point we made above.

Appendix

In extending Hathaway's measure forward and backward in time we follow Hathaway in coding the relevant sections of US Department of State *Country Reports on Human Rights*. We adopt her coded torture scores for the years 1985-1999 and thereafter supplement with our own measurements until 2003 using her schema of a 5 point scale. In Hathaway's scale, a torture score of "1" signifies that no allegations or instances of torture occurred during this year. A score of "5" signifies that torture is "prevalent" or "widespread." Hathaway specifies detailed criteria for classifying the Country Reports. When possible, countries are coded according to key words (e.g. country-year is coded as a "3" if there are "numerous" reports of beatings or there is "regular" brutality).²⁰

²⁰ See Hathaway (p. 1969-1972) for detailed description of coding procedure

Table 1: Torture Measures

	Variable	Obs.	Mean	Std. Dev.	Min.	Max.
Full Sample	Torture Score 1	2607	0.529	0.296	0	1
	Torture Score 2	2215	0.478	0.248	0	1
	Hathaway	2747	2.815	1.159	1	5
	CIRI	3585	0.809	0.754	0	2
	PTS State Dept. Codings	4038	2.390	1.174	1	5
	PTS Amnesty International Codings	3301	2.735	1.100	1	5
Canada	Torture Score 1	19	0.133	0.168	0	0.360
	Torture Score 2	12	0.067	0.075	0	0.216
	Hathaway	19	1.368	0.496	1	2
	CIRI	24	1.542	0.509	1	2
	PTS State Dept. Codings	25	1.040	0.200	1	2
	PTS Amnesty International Codings	15	1	0	1	1
Afghanistan	Torture Score 1	14	0.939	0.132	0.640	1
	Torture Score 2	14	0.937	0.078	0.784	1
	Hathaway	18	4.722	0.461	4	5
	CIRI	18	0.111	0.323	0	1
	PTS State Dept. Codings	25	4.880	0.332	4	5
	PTS Amnesty International Codings	25	4.560	0.507	4	5

Table 2: Descriptive Statistics of Control Variables

Variable	Obs.	Mean	Std. Dev.	Min	Max
Civilian/Military Control	3869	1.156	0.445	1	3
Log of Population in millions	4064	1.633	2.007	-3.912	7.161
Population growth, percent	4059	1.779	1.697	-44.408	21.758
Civil War – No=0, Yes=1	4189	0.062	0.241	0	1
Regime—Democracy=0, Autocracy=1	3913	0.530	0.499	0	1
GDP growth (annual %)	3744	2.936	6.432	-51.031	106.280
Log of GDP per capita	3746	7.448	1.556	3.799	10.747
Has country signed CAT? No=0, 1 = Yes	4170	0.256	0.436	0	1
Norm (Percent of Countries that have joined the CAT)	4946	0.432	0.252	0	0.771
International War	4947	0.005	0.072	0	1

Table 3: Fixed effects regression coefficients on factor scores

	(1)	(2)
	Torture score 1	Torture score 2
Lag of the Dependent Variable	0.362***	0.513***
	(0.022)	(0.021)
Civilian/Military control	0.000	-0.001
	(0.010)	(0.007)
Log of Total Population (in millions)	-0.052	-0.037
	(0.065)	(0.047)
Population growth (annual %)	-0.002	0.000
	(0.003)	(0.002)
Civil War – No=0, Yes=1	0.043**	0.075***
	(0.018)	(0.012)
Regime – Democracy=0, Autocracy=1	0.022	0.036***
	(0.015)	(0.011)
GDP growth (annual %)	-0.001	-0.002***
	(0.001)	(0.001)
Log of GDP per capita	-0.073**	-0.105***
	(0.030)	(0.021)
Has country signed CAT? No=0, 1 = Yes	0.003	-0.017
	(0.047)	(0.031)
Norm (Percent of Countries that have joined the CAT)	0.320***	0.227***
	(0.065)	(0.048)
International War	0.001	0.010
	(0.044)	(0.029)
Constant	0.826***	0.964***
	(0.276)	(0.197)
Observations	1883	1559
Number of fixed effect categories	119	118
log likelihood	0.20	0.39
F	956.441	1472.997
R-squared	39.991	81.681

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4: Ordered Probit results for Ordinal Variables

	1 CIRI Torture Scale, 0 - 2	2 Hathaway Torture Scale, 1- 5
Lag of the Dependent Variable	1.094*** (0.055)	1.040*** (0.054)
Civilian/Military control	-0.036 (0.051)	0.004 (0.057)
Log of Total Population (in millions)	0.177*** (0.024)	0.162*** (0.028)
Population growth (annual %)	0.051* (0.029)	0.029 (0.032)
Civil War – No=0, Yes=1	0.336*** (0.130)	0.322*** (0.099)
Regime – Democracy=0, Autocracy=1	0.175** (0.077)	0.238*** (0.064)
GDP growth (annual %)	-0.006 (0.005)	-0.004 (0.005)
Log of GDP per capita	-0.174*** (0.030)	-0.119*** (0.026)
Has country signed CAT? No=0, 1 = Yes	0.057 (0.079)	0.040 (0.065)
Norm (Percent of Countries that have joined the CAT)	0.568*** (0.114)	1.061*** (0.184)
International War	-0.008 (0.281)	-0.185 (0.371)
Observations	2410	1980
Log likelihood	-1740.6540	-1980.6807
Chi-squared	733.989	681.750
Pseudo R-squared	0.318	0.337

Standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5: Binary Dependent Variables

	(1)	(2)	(3)	(4)	(5)	(6)
	Hathaway	CIRI	Hathaway	Hathaway	CIRI	CIRI
			No torture at time $\tau-1$	Torture at time $\tau-1$	No torture at time $\tau-1$	Torture at time $\tau-1$
Lag of the Dependent Variable	1.658***	1.158***				
	(0.105)	(0.080)				
Civilian/Military control	0.003	-0.061	-0.017	0.048	-0.108	-0.006
	(0.072)	(0.062)	(0.125)	(0.145)	(0.097)	(0.104)
Log of Total Population (in millions)	0.213***	0.201***	0.128***	0.366***	0.197***	0.223***
	(0.043)	(0.031)	(0.044)	(0.067)	(0.042)	(0.038)
Population growth (annual %)	0.030	0.049	0.056	-0.022	0.076	0.015
	(0.029)	(0.033)	(0.046)	(0.034)	(0.059)	(0.029)
Civil War – No=0, Yes=1	0.454***	0.449***	0.573***	0.545**	0.601***	0.323*
	(0.143)	(0.137)	(0.168)	(0.222)	(0.224)	(0.184)
Regime – Democracy=0, Autocracy=1	0.251**	0.186**	0.193	0.282*	0.277**	0.046
	(0.100)	(0.091)	(0.134)	(0.151)	(0.140)	(0.123)
GDP growth (annual %)	-0.008	-0.010	-0.012*	0.002	-0.016*	-0.002
	(0.006)	(0.007)	(0.007)	(0.009)	(0.009)	(0.008)
Log of GDP per capita	-0.111***	-0.178***	-0.144***	0.061	-0.198***	-0.087*
	(0.037)	(0.036)	(0.042)	(0.061)	(0.044)	(0.051)
Has country signed CAT? No=0, 1 = Yes	0.015	0.148	0.020	-0.113	0.179	0.074
	(0.102)	(0.097)	(0.135)	(0.160)	(0.148)	(0.125)
Norm (Percent of Countries that have joined the CAT)	1.278***	0.547***	0.503	2.871***	0.553***	0.632**
	(0.295)	(0.155)	(0.401)	(0.610)	(0.209)	(0.285)
International War	0.157	-0.529				-0.134
	(0.273)	(0.347)				(0.531)
Constant	-1.823***	-0.317	-0.980**	-2.584***	-0.208	0.204
	(0.378)	(0.323)	(0.451)	(0.696)	(0.410)	(0.468)
Observations	1980	2544	1478	489	1609	928
Log likelihood	-692.516	-1215.604	-435.813	-231.684	-697.794	-506.493
Chi-squared	447.877	450.949	74.849	64.666	93.261	54.392
Pseudo R-squared	0.389	0.285	0.081	0.170	0.121	0.063

Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 6: Subsamples that have signed the UN Convention against Torture and that have not:
Fixed effects regression coefficients on factor scores

Dependent Variable	Torture Scale 1	Torture Scale 1	Torture Scale 2	Torture Scale 2
Subsample	Signatories	Nonsignatories	Signatories	Nonsignatories
Lag of the Dependent Variable	0.260*** (0.034)	0.406*** (0.029)	0.207*** 0.025	0.251*** 0.022
Civilian/Military control	0.009 (0.015)	0.001 (0.012)	-0.023 0.011	0.010 0.009
Log of Total Population (in millions)	-0.031 (0.110)	-0.070 (0.093)	0.234*** 0.085	-0.073 0.070
Population growth (annual %)	-0.012 (0.017)	-0.001 (0.036)	0.013 0.013	0.002 0.002
Civil War – No=0, Yes=1	0.029 (0.034)	0.038* (0.022)	0.120 0.025***	0.093*** 0.016
Regime: Democracy=0, Autocracy=1	0.071** (0.025)	0.003** (0.195)	0.060*** 0.018	0.054*** 0.015
GDP growth (annual %)	-0.000 (0.001)	-0.001 (0.001)	-0.003*** 0.001	-0.002*** 0.001
Log of GDP per capita	-0.058 (0.056)	-0.081*** (0.037)	-0.092** 0.042	-0.231*** 0.028
Norm (Percent of Countries that have joined the CAT)	0.353*** (0.090)	0.304*** (0.104)	0.166** 0.071	0.302*** 0.079
International War	0.039 (0.060)	0.026 (0.064)	-0.031 0.043	0.038 0.046
Constant	-0.700 (0.541)	0.887 (0.322)	0.311 0.412	1.884*** 0.248
Observations	840	1043	691	943
Number of fixed effects categories	52	74	51	74
F	16.86	27.22	21.37	29.21
R-squared	0.511	0.209	0.500	0.156

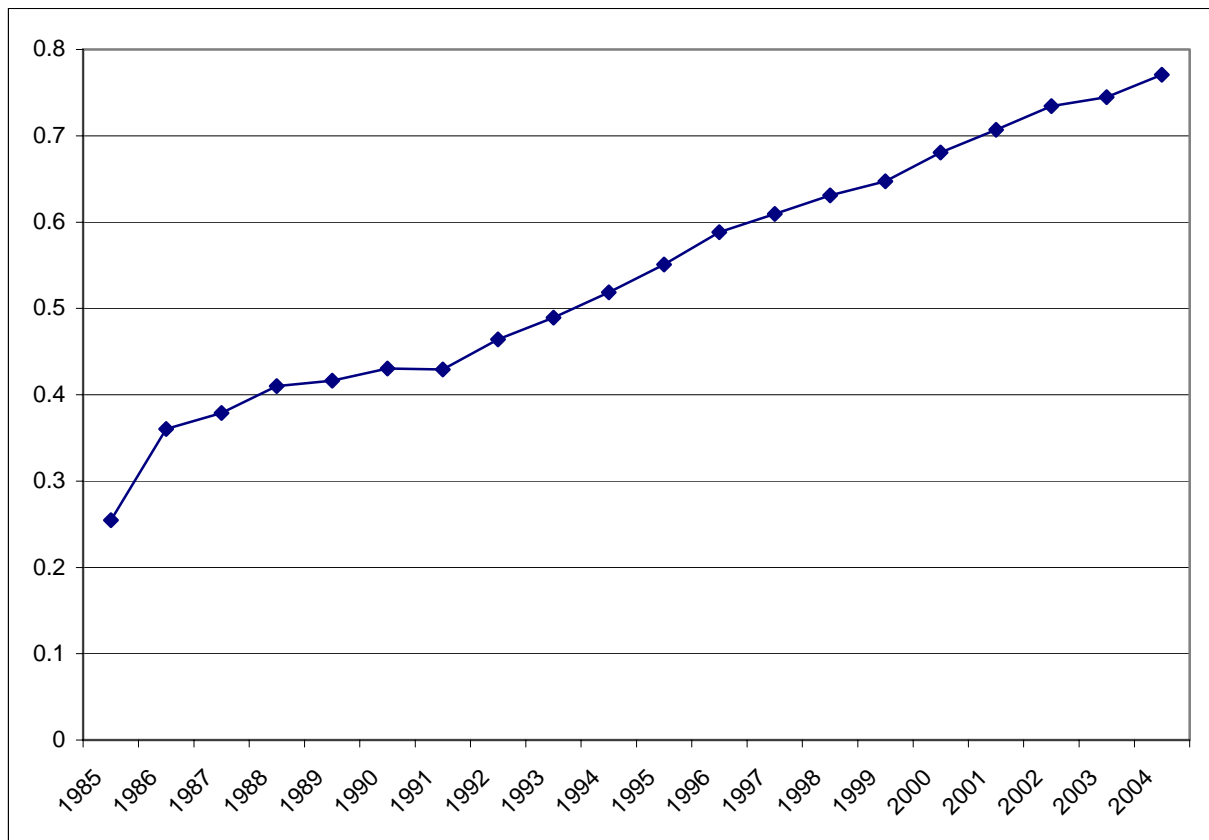
Robust standard errors in parentheses

* significant at 10%; ** significant at 5%; *** significant at 1%

Figure 1: Claims about the Effect of Norms in a 2x2 Table

		Prohibitory Norm?	
		Yes	No
Bad Behavior?	Yes	Contradictory	Confirmatory
	No	Confirmatory	Contradictory

Figure 2: Percent of Countries in the World that are Members* of the UN Convention against Torture



* Members includes those countries that have signed ratified or acceded to the convention.

References

Axelrod, Robert. 1986. An Evolutionary Approach to Norms. *American Political Science Review*. 80: 1095-1111.

Blanton, Shannon Lindsey. 2000. Promoting Human Rights and Democracy in the Developing World: U.S. Rhetoric versus U.S. Arms Exports. *American Journal of Political Science* 44(1): 123-131.

Clark, Ann Marie. 2001. *Diplomacy of Conscience: Amnesty International and Changing Human Rights Norms*. Princeton: Princeton Univ. Press.

Checkel, Jeffrey T. 1998. The Constructivist Turn in International Relations Theory. *World Politics*. 50(2): 324-48.

Cingranelli David L. and David L. Richards. 2005. The Cingranelli-Richards (CIRI) Human Rights Dataset. Available at <http://www.humanrightsdata.org>

Conway Henderson. 1993. "Population Pressures and Political Repression." *Social Science Quarterly* 74:322-333.

Downs, George W. 2000. Constructing Effective Environmental Regimes. *Annu Rev. Polit. Sci.* 3: 25-42.

Downs, George W., David M. Rocke and Peter N. Barsoom. 1996. Is the Good News About Compliance Good News About Cooperation? *International Organization*. 50(3): 379-406.

Finnemore, Martha and Kathryn Sikkink. 1998. International Norm Dynamics and Political Change. *International Organization* 52(4): 887-917.

Finnemore, Martha. 1996. Norms, Culture, and World Politics: Insights from Sociology's Institutionalism. *International Organization*. 50(2): 325-47.

Florini, Ann. 1996. The Evolution of International Norms. *International Studies Quarterly*. 40: 363-89.

Gibney, Mark and Matthew Dalton. 1996. The Political Terror Scale. *Policy Studies and Developing Nations*. 4: 73-84.

Goertz, Gary, and Paul F. Diehl. 1992. Toward a Theory of International Norms: Some Conceptual and Measurement Issues. *Journal of Conflict Resolution* 36(4):634-664.

Goodman, Ryan and Derek Jinks. 2003. Measuring the Effects of Human Rights Treaties. *European Journal of International Law*. 14(1): 171-184.

Hathaway, Oona, "Do Human Rights Treaties Make a Difference?" *The Yale Law Journal* 111(2): 1935-2042.

Hathaway, Oona and Daniel Ho. 2004. *Characterizing Measurement Error in Human Rights*. Manuscript dated Aug. 30, 2004, available at http://www.law.yale.edu/documents/pdf/Faculty/Hathaway_measurement_error.pdf#search=%22measurement%20error%20in%20human%20rights%22.

Henderson, Conway, W. 1991. Conditions Affecting the Use of Political Repression. *The Journal of Conflict Resolution* 35(1): 120-42.

Hermann, Richard K. and Vaughan P. Shannon. 2001. Defending International Norms: The Role of Obligation Material Interest and Perception in Decision Making. *International Organization*. 55(3): 621-54

Howard, Rhoda, E. and Jack Donnelly. 1986. Human Dignity, Human Rights, and Political Regimes. *American Political Science Review* 80(3): 801-18.

Jabine, Thomas B., and Richard P Claude. 1992. *Human Rights and Statistics: Getting the Record Straight*. Philadelphia: University of Pennsylvania Press.

Katzenstein, P.J., ed. 1996. *The Culture of National Security: Norms and Identity in World Politics*. New York: Columbia Univ. Press.

Khagram, Sanjeev et al., eds. 2002. *Restructuring World Politics: Transnational Social Movements, Networks, and Norms*. Minneapolis: Univ. of Minnesota Press.

Klotz, Audie. 1995. Norms Reconstituting Interests: Global Racial Equality and U.S. Sanctions against South Africa. *International Organization*. 49(3): 451-78.

Lee, Chris et al. 2004. Ethnicity and Repression: The Ethnic Composition of Countries and Human Rights Violations. in Sabine C. Carey and Stephen C. Poe, Eds. *Understanding Human Rights Violations: New Systematic Studies*. Aldershot, Hampshire, UK: Ashgate Publishing.

Legro, Jeffrey. 1997. Which Norms Matter? Revisiting the "Failure" of Internationalism. *International Organization*. 51: 31-63.

Levinson, Sanford. 2002. "Precommitment" and "Postcommitment": The Ban on Torture in the Wake of September 11. *Texas Law Review*. 81: 2013-53.

Mitchell, Neil J. and James M. McCormick. 1988. Economic and Political Explanations of Human Rights Violations. *World Politics* 40(2): 476-498.

Nadelman, Ethan A. 1990. Global Prohibition Regimes: The Evolution of Norms in International Society. *International Organization*. 44: 479-526.

- Osiel, Mark J. 2001. *Mass Atrocity, Ordinary Evil and Hannah Arendt: Criminal Consciousness in Argentina's Dirty War*. New Haven: Yale Univ. Press.
- Poe, Steven C. & C. Neal Tate. 1994. Repression of Rights to Personal Integrity to the 1980s: A Global Analysis. *American Political Science Review* 88(3):853-872.
- Price, Richard. 1998. Reversing the Gun Sights: Transnational Civil Society Targets Land Mines. *International Organization*. 52(3): 613-44.
- Price, Richard. 2003. Transnational Civil Society and Advocacy in World Politics. *World Politics*. 55(4): 579-606.
- Raymond, Gregory A. 1997. Problems and Prospects in the Study of International Norms. *Mershon International Studies Review*. 41: 205-245.
- Risse, Thomas, Stephen C. Ropp, and Kathryn Sikkink, eds. 1999. *The Power of Human Rights: International Norms and Domestic Change*. Cambridge: Cambridge Univ. Press.
- Sikkink, Kathryn. 1998. Transnational Politics, International Relations Theory, and Human Rights. *Political Science and Politics*. 31(3): 516-523.
- Simmons, Beth. 2006. *International Human Rights: Law, Politics, and Accountability*. Book manuscript. Harvard University.
- Slaughter, Anne-Marie et al. 1998. International Law and International Relations Theory: A New Generation of Interdisciplinary Scholarship. *The American Journal of International Law* 92(2): 367-397.
- Slaughter, Anne-Marie. 1993. "International Law and International Relations: A Dual Agenda." *The American Journal of International Law*. 87(1): 205-239.
- Sunstein, Cass. 1996. Social Norms and Social Roles. *Columbia Law Review*. 96: 903-68.
- Thomson, Janice. 1993. Norms in international relations: A conceptual analysis. *International Journal of Group Tensions* 23: 67-83.
- Young Oran R. and March Levy. 1998. The Effectiveness of International Environmental Regimes. In Oran Young ed. *The Effectiveness of International Environmental Agreements*. Cambridge MA: MIT Press. 1-32.