Reputation and the Effect of International Law
on Preferences and Beliefs

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Comments welcome!

Abstract: How does international law affect preferences and beliefs about foreign policy? I investigate this question by offering the first-ever experimental analysis of treaty commitments. The experiments, embedded in interviews with U.S. voters and British policymakers, reveal three patterns. First, international law changes preferences and expectations. Individuals are far more likely to oppose policies that would violate international law than to oppose otherwise identical policies that would not trammel upon the law. Moreover many observers, including expert policymakers, anticipate that signatories to treaties will behave differently from non-signatories. Second, these effects arise, at least in part, via a reputational mechanism. By publicizing international commitments and embedding them in a legal framework, treaties raise the reputational ante, making it more costly to renege. Third, the effect of international law is additive, not absolute. If the material or moral case for violating international law is sufficiently strong, large proportions of voters and policymakers will advocate breaking the law and will expect foreign leaders to do the same. Thus, the experiments reported here reveal both the power and the limits of international law.

How does international law affect preferences and beliefs about foreign policy? Few questions are as central to the study and practice of global politics. According to the United Nations, more than 50,000 bilateral and multilateral treaties are currently in force. Although these treaties are legally binding, their credibility remains a matter of academic and practical debate. Without a higher power that compels sovereign states to respect international law, it is not obvious when—if at all—treaties and other legal commitments affect the incentives and expectations of key actors in world affairs.

Moreover, efforts to estimate the effects of international law have been hampered by methodological roadblocks. As Simmons (1998: 89) points out, "Several studies have tried to demonstrate a correlation between legal standards and state behavior, sometimes employing large databases and statistical techniques, but most are unconvincing in demonstrating causation, or even in providing an explanatory link between the actions taken and the existence of agreements or normative considerations." Scholars increasingly acknowledge that problems of endogeneity and measurement make it difficult to draw reliable inferences about the consequences of international law.

In this paper, I advance our understanding of international law by supplementing the analysis of observational data with experiments involving randomized treatment and control. A pure field experiment, in which the researcher forces some governments to sign treaties and others to abstain, is clearly out of the question, but survey-based experiments are both feasible and informative. By embedding experiments in interviews with voters and policymakers, it becomes possible to estimate the effect of international law while avoiding problems of endogeneity and measurement that have impeded previous research.

My analysis, based on a unique set of interviews with U.S. voters and British policymakers, supports three conclusions. First, international law changes preferences and expectations. Individuals are far more likely to oppose policies that would violate international law than to oppose *otherwise identical* policies that would not trammel upon the law. Moreover many observers, including expert policymakers, anticipate that signatories to treaties will behave differently from non-signatories. Second, these effects arise, at least in part, via a reputational mechanism. By publicizing international commitments and embedding them in a legal framework, treaties raise the reputational ante, making it more costly to renege. Third, the effect of international law is additive, not absolute. If the material or moral case for violating international law is sufficiently strong, large proportions of voters and policymakers will advocate breaking the law and will expect foreign leaders to do the same. Thus, the experiments reported here reveal both the power and the limits of international law.

1. International Law in Theory

There is much debate among international relations theorists about the consequences of international law. On one side of the debate, proponents of "realism" argue that international law has little or no independent effect on foreign policy. Leaders, it is claimed, pursue the national interest—broadly defined to include military security and economic prosperity—without regard for international law. Although diplomats and interest groups sometimes argue about whether a given action would transgress international law, such arguments have no significant impact on preferences and decisions (for classic statement, see Mearsheimer 1994, 2001; Morgenthau 2006).

International law lacks force, it is said, because the legislative, judicial, and executive functions are fundamentally decentralized. First, each nation in world affairs is its own lawgiver. Today as in the past, international law arises almost entirely from treaties and other interstate agreements. Under the principle of sovereignty, no state can be bound by an international agreement without consenting to its terms. Moreover, states always have the option of withdrawing consent, thereby reversing legal commitments they took aboard at earlier times. Some scholars maintain that international obligations stem not only from explicit agreements, but also from consistent patterns of state practice that give rise to "customary law." Such customary law does not bind states that "persistently object" to the custom, however. At root, international law is what each state chooses for itself.

This decentralized legislative process saps international law of its strength. Countries sign treaties they are predisposed to follow, it is argued, and remain parties only insofar as the treaties serve their interests. To the skeptic, then, the existing body of international law reflects, rather than changes, the underlying preferences of leaders and citizens (Downs, Rocke, and Barsoom 1996; von Stein 2005).

Second, each nation in world affairs is its own judge, and can interpret the law to serve its own purposes. Although international courts may help adjudicate disputes, the principle of sovereignty stands as a roadblock by immunizing countries from being taken to court against their will. Countries can commit to the compulsory jurisdiction of a tribunal such as the International Court of Justice, but they often express reservations and retain the right to withdraw, as the United States did following the famous Nicaragua case of 1984.

Finally, each nation in international affairs is its own sheriff, who must enforce the law for itself or organize a sympathetic posse. Unlike in domestic politics, there is no international

authority to coerce countries into accepting the decisions of international courts. As Morgenthau (2006, 301) wrote in his classic study of *Politics Among Nations,* "There can be no more primitive and no weaker system of law enforcement than this."

It is, therefore, understandable why so many scholars conclude that international law has little, if any, effect on foreign policy. Treaties and other legal agreements may help countries coordinate their policies, but they have little effect on preferences when cooperation is more controversial. By implication, treaties also have no effect on policy expectations. If treaties are mere scraps of paper that can be shredded or disregarded at no cost, even countries with no desire to abide could nonetheless opt to sign. The act of signing a treaty, in this realist framework, would reveal no information about preferences and capabilities.

Against this skeptical view, institutionalist scholars argue that international law can be profoundly consequential (e.g. Keohane 1984; Fortna 2003; Guzman 2008). When leaders sign a treaty or other legal agreement, it allegedly becomes more costly to take actions the law forbids and less costly to pursue policies the law condones. Treaties, in other words, tie the hands of current and future leaders by increasing the cost of reneging.

How, exactly, could international law alter incentives and expectations? In this paper I emphasize the role of reputation, which recent research has shown to be a foundation for credibility and cooperation in military and economic affairs (see, e.g., Sartori 2005; Tomz 2007b). By embedding agreements in international law, countries "ante up a greater reputational stake" than if they had made the same commitment without invoking the law (Simmons and Hopkins 2005, 623; See also Guzman 2008).

Through legalization, a country stakes its reputation in three ways. First, it publicizes the commitment. Unlike secret threats and promises conveyed behind closed doors, legal

commitments are typically made in plain sight of domestic and foreign observers. Through signing ceremonies and ratification procedures, countries reveal the commitment to outside observers, and thereby widen the set of parties that can monitor compliance. Moreover, legal commitments leave paper trails that reduce the potential for deniability. In short, legal commitments expose countries to potential reputational costs by making commitments visible to the rest of the world.

Second, legalization raises the reputational ante by expressing commitments in a particularly solemn form. Treaties are conventionally viewed as "the most solemn pledge a state can make" and, for this reason, a "maximal pledge of reputation" (Guzman 2008, 59; see also Lipson 1991; Simmons and Hopkins 2005). Countries that break casual commitments may be viewed suspiciously, but those who renege on treaties will be viewed with special skepticism. If a country does not uphold its most solemn commitments, can it really be trusted to honor promises or threats of any kind?

Third, legalization heightens the reputational costs of reneging by encouraging conceptual linkages across issues. By legalizing a commitment, a country invites outsiders to regard the commitment as one element in a legal set, and to treat compliance on that particular issue as evidence of law-abidingness more generally. This is, I believe, what scholars and policymakers have in mind when they refer to a country's "reputation for compliance with international law" or "reputation for compliance with legal obligations" (Guzman 2008, 33, 115). By connecting otherwise disparate issues, legalization fosters "reputation spillovers" that multiply the penalty for breaking commitments.

Although this paper emphasizes the potential reputational effects of international law, legalization may create other incentives for cooperation. Embedding commitments in a legal

framework could, for example, enhance the prospects for retaliation, increase the risk of a lawsuit in domestic courts, or raise concerns about morality and appropriateness that would be less salient for non-legal commitments. Future research should explore these potentially complementary ways in which international law could add value in an anarchical world.

In summary, international relations theorists are divided about the effects of international law. Authors in the realist tradition maintain that international law has little if any effect on preferences and expectations about foreign policy. Institutionalists, on the other hand, propose that legalization can contribute to cooperation, above and beyond what states could achieve through repeated interaction and issue linkage in a lawless world. Building on the work of Guzman (2008) and others, I have suggested one pathway by which international law could strengthen the incentive to cooperate: by raising the reputational cost of reneging. In the remainder of the paper, I describe the empirical obstacles to studying the effects of international law, propose an experimental method for shedding new light on the debate, and present a series of experiments that help reveal the effect of international law and the reputational mechanism underpinning it.

2. The Limits of Existing Evidence

Due to limitations of existing data, it has been extremely difficult to make progress in understanding the effects of international law. We currently do not know the conditions under which international law matters, or when the act of signing a legal agreement is most likely to demonstrate credibility. Previous research has relied entirely on historical records to estimate the effect of international law. Have countries that signed environmental protection treaties polluted less than countries that did not sign? Has respect for human rights, arms control, and free trade

been greater among countries that entered treaties on these topics than among countries that did not?

If treaties arose from a purely random process, the use of historical data would be unproblematic. In reality, though, countries choose whether and on what terms to enter treaties. Thus, it is hard to know whether the historical correlation between treaties and behavior is a consequence of the treaty itself, or is due to cross-national or inter-temporal differences in the baseline propensity to take the kinds of actions the treaty requires. Most countries honor their treaties most of the time, but this does not prove that treaties shape foreign policy. According to skeptics, treaties reflect but do not change the pre-existing interests of states.

We can bring the existing debate—and the roadblock of endogeneity—into sharper relief by drawing on Rubin's (1974) counterfactual account of causality. Suppose we are interested in Y_i, a measure of country i's behavior on a given issue at a particular time. The impact of a treaty on Y_i is $\delta_i = Y_{i1} - Y_{i0}$, where Y_{i1} represents the way i would behave if party to the treaty, and Y_{i0} signifies how the same country would behave if not party to the treaty. The quantity δ_i tells what difference, if any, the treaty makes.

Unfortunately, the causal effect δ_i is unobservable. We might imagine how country i would behave in both the treaty and the no-treaty conditions, but we cannot observe both Y_{i1} and Y_{i0} for the same i at the same time. No country can be observed in the treatment and control regimes simultaneously, a fact that Holland (1986) called "the fundamental problem of causal inference."

Although the individual-level effect δ_i is beyond reach, scholars have tried to infer *the average* causal effect, δ, by comparing the observed Y for countries that signed the treaty with

the observed Y for countries that did not. In this context, the average effect of the treaty is estimated as $\hat{\delta} = (Y_1 \mid T = 1) - (Y_0 \mid T = 0)$, where the indicator T takes a value of 1 when the country has signed the treaty and 0 otherwise.

This standard estimator is equal to the true effect δ plus two potential sources of bias. The first source of bias, "different baseline propensities," arises when signatories and non-signatories differ in their fundamental tendency to do Y, even in the absence of a treaty. The second source of bias, "different treatment effects," arises when the treaty would produce stronger (or weaker) effects on the group that actually signed than on the group that did not.

With a bit of algebra, we can show that

$$\hat{\delta} = \delta + \left[(Y_0 \mid T = 1) - (Y_0 \mid T = 0)\right] + \alpha\left[(\delta \mid T = 1) - (\delta \mid T = 0)\right],$$

where α is the proportion of the sample that did not join the treaty, $\delta \mid T = 1$ is the average effect on those who actually signed, and $\delta \mid T = 0$ is the average effect the treaty would have exerted on the remaining countries, if contrary to fact they had chosen to sign. The first term in brackets gives the bias from different baseline propensities; the second gives the bias from heterogeneous treatment effects.

Expressing $\hat{\delta}$ in this way helps clarify the claims and the limitations of the existing literature. Researchers have found that countries often comply with treaties, and in some cases the observed level of Y differs systematically between signatories and non-signatories. To the skeptic, these estimated effects are artifacts of different baselines: those who signed were more inclined to do Y in the first place.

Skeptics essentially argue that that δ is zero but our estimate $\hat{\delta} \neq 0$ because the conditional mean $(Y_0 \mid T{=}1)$ exceeds the conditional mean $(Y_0 \mid T{=}0)$. If we could eliminate baseline differences between the two groups, the skeptic argues, the apparent effect of the treaty would

disappear. To convince the skeptic that treaties matter, and to obtain unbiased estimates of the causal effect more generally, it is important to remove any baseline differences. I argue below that experiments can achieve this goal by design, whereas observational studies can do so only with difficulty.

Against the skeptics, legalists argue that a country can alter its interests and behavior by signing a treaty. Moreover, sophisticated legalists hypothesize that treaties affect different countries to different degrees. For example, Hathaway (2005) and Raustiala and Slaughter (2002) suggest that the relationship between international law and state behavior depends on domestic institutions such as the judiciary, the media, political parties, and interest groups. Others claim that sensitivity to treaties varies with the rule of law or the degree of democracy (e.g. von Stein 2006). These and other domestic institutions vary considerably across states. As a consequence, δ_i should differ from one i to the next.

Heterogeneity in δ_i raises the potential for a second source of bias: a correlation between signing and susceptibility to treatment. If leaders are rational, they will weigh the anticipated effects of the treaty when deciding whether to sign. This rational behavior introduces a systematic relationship between signatory status (T_i) and the treatment effect (δ_i). Using the previous notation, $(\delta \mid T = 1) \neq (\delta \mid T = 0)$ in the presence of self-selection. This heterogeneity is, of course, interesting in its own right and an important subject of study. Unless the heterogeneity is controlled, however, $\hat{\delta}$ will be a biased estimate of the average treatment effect for the population as a whole.

To draw valid inferences from non-experimental data, we need statistical correctives that allow us to approximate the attributes of a genuine experiment. Some researchers address this problem with control variables: they model foreign policy as a function of treaties and controls

that correct for differences between signatories and non-signatories (e.g. Simmons 2000). The goal is to make signatories and non-signatories comparable after conditioning on the X's, such that any systematic difference in behavior would reflect the causal effect of the treaty, rather than distinct baselines or different sensitivities to treatment.[1]

The success of this approach depends on a comprehensive set of controls, however. To solve the bias problem with control variables, the researcher must condition on all variables that correlate with the outcome and membership in the treaty. This can be quite a challenge, made more severe by informational asymmetries in international relations. Governments have private information, which they withhold not only from other countries but (presumably) from academic researchers, as well. When governments have pertinent but private information about their baseline interests or their sensitivity to treatment, the set of control variables is likely to be incomplete and estimates of the treaty's effect will be biased.

Some of these problems can be minimized through the use of panel data. If countries are tracked over a number of years, the insertion of fixed effects for countries or dyads can help correct for unobserved heterogeneity. Hathaway (2002), Goldstein, Rivers and Tomz (2007), Simmons (2004), and Tomz, Goldstein, and Rivers (2007) have used this approach to study the effects of treaties on human rights and international trade. For the strategy to succeed completely, though, the determinants of state policy must be additive and time-invariant. If the decision to enter a treaty and the choice of Y depend on common variables that change over time, omitting those variables will lead to biased estimates of δ, even in the presence of fixed effects (Besley and Case 2000).

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[1] Heckman-type selection models can be viewed variants of the same strategy: using a function of one or more control variables to address the problem of endogeneity. For applications of this approach to IMF agreements, see Przeworski and Vreeland (2000) and Vreeland (2003). Propensity-score matching has the same goal of making assignment of treatment ignorable, after matching on a set of X's. For an application to international law, see Simmons and Hopkins (2005).

There is a second statistical option. Instead of using controls that are correlated with both the treatment and the outcome, one could seek an instrument that affects the outcome only indirectly, via the treatment variable. With a technique such as two-stage least squares regression, the instrument can be used to obtain consistent estimates of the treatment effect. This approach makes sense in theory but has serious problems in practice: it has been nearly impossible to find valid instruments—ones that correlate strongly with the presence or absence of a treaty but have no independent bearing on foreign policy.

In summary, problems of endogeneity and measurement make it difficult to infer the effects of treaties. Several studies have tried to address this problem via control variables, but many questions about causality remain. The next section explains how experiments can contribute to our understanding of international law by isolating the causal effects of treaties on policy preferences. Later in the paper, I use experiments to gauge the influence of treaties on policy expectations and explore the mechanisms underlying these effects.

3. An Experiment-Based Analysis of Preferences

The key empirical innovation in this paper is to supplement observational studies by embedding experiments in interviews with citizens and elites. The experiments take two forms. In the between-subject experiments, some interviewees hear about a hypothetical or historical foreign policy situation in which leaders have signed a treaty. Others consider exactly the same situation, *sans* any treaty. By comparing the views of participants in the treatment condition (treaty) versus the control condition (no treaty), we can isolate the effect of treaties on policy preferences and beliefs.

The within-subject approach is similar, except that participants are exposed to both treatment and control. They are first asked about a foreign policy issue in which international law is not mentioned. They are then provided with information about international legal commitments and re-queried on the same issue. Any change in preference must, on average, be due to the introduction of international law.

As a first step toward implementing the between-subject approach, I designed an experiment about trade treaties. The experiment aimed to quantify the effect of such treaties in the context of many other foreign policy considerations, such as humanitarianism and economic interests. This represents a relatively hard test. If trade treaties matter even in the face of competing concerns, they are likely to be influential in less stringent settings.

The experiment, administered over the internet to a nationally representative sample of 1,000 U.S. adults in July 2005,[2] began as follows: "The next question is about foreign policy. Some leaders want the United States to prohibit trade with the country of Burma. They say we should neither buy products from Burma nor sell products to Burma. Experts who have studied this proposal agree on several points. Please consider each point carefully, and then tell us what you think."

I then presented each respondent with two or more of the following points.

> Human rights: "The proposal would help human rights. In Burma, the government kills
> political opponents and does not allow free speech. By stopping trade with Burma, we
> can pressure the government to start respecting basic rights."

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[2] The experiment was fielded by Knowledge Networks. Interviews were conducted over the internet and, for those without prior internet access, a WebTV console that was supplied at no charge by the polling firm. The sample closely matched the demographic characteristics of the U.S. population, as presented by the U.S. Census Bureau and the Bureau of Labor Statistics in the Current Population Survey for July 2005.

U.S. economy: "The proposal would help the U.S. economy. Many Americans are getting laid off because of competition from Burma. If we stop trading with Burma, there will be more jobs and higher wages in the United States."

Burmese economy: "The proposal would hurt the Burmese economy. Burma sells $300 million in products to the United States each year. If we stop trading with Burma, people in that country will lose their jobs, and poverty will rise."

International law: "The proposal would violate international law. The United States has signed treaties that make it illegal to limit trade with Burma. If we stop trading with Burma, we will be breaking international law."

Each respondent received a random assortment of "pro" arguments that built the case for ending commercial relations with Burma. One-third of participants were told that severing trade with Burma would help promote human rights and protect the U.S. economy. Another third received the claim about human rights without any mention of possible benefits for the U.S. economy. The remaining third received the opposite mix, which mentioned the U.S. economy but not human rights. On the "con" side, all respondents in the sample were told that the proposal would hurt the Burmese economy. In addition, one-third of participants read that the plan would violate international law by contravening trade agreements the United States had signed.[3]

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[3] One-third of respondents received an additional, neutral argument: "The proposal would change our trade relations. The United States trades with many countries. If we stop trading with Burma, we will no longer suffer the costs (if any) nor will we get the benefits (if any) of trade with that particular country." This neutral argument is not a focus of the analysis described here.

In total, then, there were six experimental groups: three configurations of pro arguments, each coupled with a claim about international law in some cases but not in others.[4] To guard against order effects, I randomized not only the group assignments but also the sequence of arguments within each group.

After presenting these arguments, I asked citizens to express their foreign policy preferences. "How good or bad an idea is it for the United States to prohibit trade with Burma?" The response options were: extremely good, moderately good, slightly good, neither good nor bad, slightly bad, moderately bad, and extremely bad. By analyzing the responses, one can infer how the presence or absence of international legal agreements affected policy preferences.

Before computing the effect of such agreements, it seemed prudent to confirm that the treatment and control groups were balanced on baseline covariates that could affect foreign policy preferences. I estimated a logistic regression in which the dependent variable was the dichotomous treatment (international law mentioned=1, not mentioned=0) and asked whether any demographic or contextual variables predicted membership in the treatment group. Not one of the many variables in the model—ideology, party identification, gender, age, race, education, income, and a variety of other demographic factors—had a statistically significant effect on the probability of being in the treatment group. Based on a likelihood ratio test, we cannot reject the hypothesis that the relationship between the treatment and *all* baseline variables was zero.[5]

Having established that the treatment was random, I proceeded to estimate the effect of international law. Of respondents who took a side (all respondents except those who answered

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[4] Sample sizes were as follows: (Human Rights, Burmese Economy), N = 213; (Human Rights, Burmese Economy, International Law), N = 103; (U.S. Economy, Burmese Economy), N = 253; (U.S. Economy, Burmese Economy, International Law), N = 114; (Human Rights, U.S. Economy, Burmese Economy), N = 229; (Human Rights, U.S. Economy, Burmese Economy, International Law), N = 88.

[5] The likelihood ratio test statistic, 22.26, was distributed chi-squared with 23 degrees of freedom. If all coefficients were zero, we would observe a test statistic that large roughly half the time. I conducted many other parametric and nonparametric tests, none of which suggested significant imbalance between treatment and control.

"neither good nor bad"), I computed the share who thought it would be a bad idea to sever commercial relations with Burma. To the extent that international law matters, this share should be significantly higher when the law is mentioned than when it is not.

In all cases, I obtained estimates and credible intervals via Bayesian simulation. Specifically, I modeled the proportion of people who opposed the policy proposal as a binomial distribution with a noninformative Jeffreys prior, Beta(0.5,0.5). When legal commitments exist, the posterior proportion π_1|data is distributed as Beta(b_1+0.5, $N_1 - b_1$ +0.5), where the subscript 1 indicates the treatment regime, N_1 is the number of respondents that received the treatment, and b_1 is the number of people that thought it would be bad to cut trade with Burma.

Without international law, the proportion of naysayers is π_0|data and its posterior distribution is Beta(b_0+0.5, $N_0 - b_0$ +.5), where the subscript 0 signifies the control regime. By drawing random variates from these independent beta distributions, we can obtain the full posterior distribution (and therefore point estimates and credible intervals) of $\delta = \pi_1 - \pi_0$, the effect of international law.[6]

Table 1 shows that international law powerfully affects the preferences of citizens. When no legal agreements were mentioned, approximately 27 percent of respondents who took a side deemed it bad to prohibit trade with Burma. This percentage jumped 17 points when respondents were told that the policy would violate international law. The 95 percent credible interval around this effect ranged from 10 to 25, so we can be quite sure that the treaty moved policy preferences. In summary, the data in Table 1 provide strong behavioral microfoundations for the view that international law changes peoples' preferences about foreign policy.

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[6] Other statistical methods, including ordered probit analysis and comparison of means using the full seven-point scale from extremely bad to extremely good, lead to the same conclusions.

[TABLE 1 ABOUT HERE]

At the same time, the evidence in Table 1 underscores the limits of international law. When told that a trade embargo against Burma would violate international law, only 44 percent of respondents who took a side felt that the embargo remained a bad idea. The remaining 56 percent, a majority, concluded that the illegal embargo would be a good idea. International law has a powerful ability to sway voters, but economic and moral considerations trump the law for a significant portion of the electorate.

Table 2 divides the sample into three parts, based on non-legal considerations that respondents weighed. The table supports three inferences. First, in this study of international trade, economic arguments swayed citizens more effectively than appeals to human rights. When experts concluded that the trade barriers would improve human rights, 37 percent of respondents who took a side nonetheless opposed the measure (Table 2, column 1). Citizens who heard about the U.S. economy, rather than human rights, were significantly more likely to support the proposal.

[TABLE 2 ABOUT HERE]

Second, appeals to human rights and economic interest reinforce each other. Citizens do not regard these considerations as equivalent, and they are more willing to support a foreign policy that would serve both objectives than a policy that would enhance only one. In fact, only 15 percent of citizens who expressed an opinion actually disapproved when told that we could create U.S. jobs and improve human rights by eliminating trade with Burma.

Third, the table confirms that international law can change foreign policy preferences, even in the face of powerful counterarguments. Consider the first row of Table 2, which pertains to people who heard that the policy initiative would help human rights. For those citizens, the

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mere mention of international law converted naysayers from a minority (37 percent) to a majority (54 percent). The estimated effect of international law in this situation was 17 percentage points, with an unambiguously positive credible interval. The second row of the table summarizes opinion among citizens who heard that the proposal would help the U.S. economy. Here, international law exerted a smaller but still discernable effect, leading to a 10-percentage-point swing in policy preferences. This estimate, though somewhat imprecise, was nonetheless greater than zero with probability 0.94.

Surprisingly, international law remained potent even when counter-arguments were extremely strong (Table 2, row 3). The proposal to create jobs and improve human rights was highly popular among members of the control group, but the same proposal elicited scorn from 37 percent of citizens in the treatment condition. Under this scenario, the presence of legal commitments more than doubled the share of citizens who opposed the foreign policy. The overall effect was 22 points, with a 95-percent credible interval from 9 to 35. Thus, contrary to the skeptics, it appears that international law can affect preferences, even when the decision to follow international law conflicts with economic self-interest and humanitarian concerns.

Further analysis suggests that the political effects of international law diffuse widely throughout the population, affecting conservatives as well as liberals and crossing other demographic divides. Table 3 presents a breakdown of estimates by demographic group. Regardless of political ideology, party identification, gender, education or income, international law substantially changes preferences about foreign policy. The effect sizes in the table range from 12 to 25 percentage points, and all are greater than zero with a probability of at least 0.999. Moreover, although some demographic groups appear more sensitive to international law than other groups, we cannot affirm these differences with a high level of confidence. With the data at

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hand, we cannot reject the proposition that international law has similar effects on all demographic groups.

[TABLE 3 ABOUT HERE]

4. Alternative Experimental Designs to Study Preferences

I now describe two alternative designs, which shed additional light on the effect of international law on policy preferences.

Addition versus Substitution: The previous experiment about Burma can be characterized as an "additive" design. The control group received a mix of pro and con arguments, while the treatment group received the same mix *plus* the claim that cutting trade with Burma would violate international law.

This design has both advantages and drawbacks. On the plus side, it captures the intuition that international law gives political groups an extra argument to invoke when lobbying for or against foreign policy proposals. On the minus side, the treatment and control groups differ not only in the presence or absence of international law, but also in the number of pro and con arguments. It is not clear whether the apparent effect of international law is due to the law itself, to an imbalance con considerations, or both.

To find out, I devised a follow-up experiment that involved both addition and substitution. All participants were told that cutting trade with Burma would help human rights and the U.S. economy but hurt the Burmese people. I then divided the sample into six groups. Group 1 received no additional arguments, whereas Group 2 was told, as in the previous experiment, that the proposal would break international law. By including groups 1 and 2, I replicated the "addition" experiment that had been conducted two years earlier.

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I assigned the remaining groups to arguments that made it possible to estimate substitution effects. Group 3 received a weak international law argument, which raised the spectre of illegality but also introduced significant doubt. Participants in this control group were told: "Stopping trade with Burma might violate international law. The United States has signed treaties that might make it illegal to limit trade with Burma. But there is much debate among experts about what the treaties really mean." By comparing the force of this argument, versus the claim that stopping trade with burma *would* violate international law (i.e., by comparing Group 2 versus Group 3), one can estimate the effect of moving along the legality spectrum while holding the number of con arguments constant.

Group 4 received a redundant con argument, which was different in language but similar in spirit to the contention that the policy would hurt the Burmese economy. Respondents were told that "Stopping trade with Burma would increase unemployment in Burma. Burmese workers make products that are exported to the United States. If we stop trading with Burma, some people in that country will lose their jobs." Formally, Groups 2 and 4 contained the same number of con arguments. Consequently, any difference between treatment and control can be attributed to international law itself.

Group 5 received a weak con argument, that U.S. policy would trigger a diplomatic protest. "Stopping trade with Burma might cause Burma withdraw its ambassador. Burma does not want the United States to impose economic sanctions. If we stop trading with Burma, Burma might withdraw its ambassador from the United States." Finally, Group 6 was offered a strong con argument, that "Stopping trade with Burma might cause China to retaliate. China is an ally of Burma. If we stop trading with Burma, China might take diplomatic or economic action

against the United States." By comparing Group 2 with Groups 5 and 6, one can gauge the force of international law relative to other substantive arguments about trade sanctions.

This follow-up design was administered to a representative sample of 5,800 U.S. adults by Polimetrix, an internet-based polling company, in October 2007. Table 4 gives the percentage-point increase in opposition to trade sanctions as a consequence of adding or substituting a strong law argument. The first row of the table replicates the additive design. Starting from a baseline in which all participants receive two pro arguments (human rights and the U.S. economy) and one con argument (the Burmese economy), adding the claim that the policy would violate international law caused opposition to rise by 10 percentage points, with a 95 percent credible interval from 6 to 14 percentage points. Interestingly, this effect is only half as large as observed in the previous experiment, perhaps because the worsening U.S. economy and widespread news coverage of Burmese human rights violations made citizens more willing to override international law for economic and humanitarian reasons.

[TABLE 4 ABOUT HERE]

The key lesson from Table 4, however, is that the substitution effects of international law were nearly as large as the addition effects. Opposition to trade sanctions was significantly higher (by 7-9 percentage points) when respondents were told that the policy would violate international law than when they received a weak law argument, a redundant con argument, or a weak con argument. In fact, international law was even more potent, by about 5 percentage points, than the apparently strong con argument that an embargo against Burma might cause China to retaliate against the United States. Overall, this follow-up experiment confirms that international law has not only additive but also substitutive effects, which do not depend on the balance of pro and con arguments.

Within-Subject Analysis: The previous experiments involved between-subject analysis: some participants were treated with the international law argument, whereas others were not. We can gain additional insight from within-subject tests, which involve three steps: (1) measure the preferences of citizens without mentioning international law; (2) inform participants that the policy proposal would violate in international law; and then (3) re-query participants to see whether information about international law changed their preferences.

To find evidence of this sort, I conducted a comprehensive search through nearly half a million survey questions asked in the United States over the last 65 years by more than 150 survey organizations. Six questions, dealing with military intervention and missile defense, approximated a within-subject test of the effect of international law.

In 1988, a random sample of 1,000 registered U.S. voters was asked whether the government should send "an undercover U.S. military force to destroy the crops and production facilities in the countries where illegal drugs are produced." Participants initially received no indication of whether such action would run afoul of international law. Nearly 42 percent of respondents favored this proposal to combat the drug problem.

Participants who supported the proposal were then challenged with the following statement: "You said you favored sending a U.S. military force to destroy crops and production facilities in countries where illegal drugs are produced. Would you still favor this action if many countries considered it a violation of international law?" Only half (52%) of the respondents who received this followup continued to favor the covert military option. The remainder shifted to a position of ambivalence (13%) or opposition (35%). Thus, international law caused roughly half of the previous advocates of military intervention to change their minds about the wisdom of the

proposal. This within-subject response pattern, summarized in Table 5, corroborates the

between-subject studies by demonstrating that international law changes policy preferences.

[TABLE 5 ABOUT HERE]

Additional data come from within-subject tests about the deployment of an anti-ballistic

missile shield. Debate over this topic spanned from the 1980s to the present, giving several

decades to observe potential effects.

In 1985, a poll of more than 1,500 U.S. adults found that approximately 41 percent

approved of plans to develop space-based weapons. The approvers were then given a follow-up

statement, which amounted to an international law treatment. "Currently the U.S. and the Soviet

Union have an anti-ballistic missile treaty that prohibits both nations from developing certain

weapons. Suppose the U.S. had to violate or abandon that treaty in order to develop the space-

based weapons (Star Wars, or Strategic Defense Initiative). Would you still favor the

development of those space-based weapons?" Only 63 percent continued to endorse SDI; the

reminder expressed ambivalence (5%) or switched sides (32%). Overall, international law caused

more than one-third of people who would otherwise have advocated SDI to abandon their

position. Other polls on the same issue from the same time decade lead to the same conclusion

(see Table 5).

International law had an even more powerful effect at the turn of the century. In the year

2000, about 59 percent of respondents favored having the U.S. continue to try to build a missile

defense shield against nuclear attack. When asked if their policy preference would change if

building such a system "meant that the United States would have to break the arms control treaty

we now have with Russia," only 48 percent maintained their support for Star Wars. A study one

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year later reached nearly the same conclusion: of the 61 percent that originally advocated star wars, less than half clung to the view when told that it would require violating a treaty.[7]

In summary, both between-subject and within-subject tests show that international law constrains the foreign policy preferences of voters. Do U.S. policymakers—the President, members of Congress, and the bureaucracy—feel similarly constrained? One cannot know for sure without administering a similar set of experiments to leaders in Washington, DC. Nonetheless, three factors suggest that the findings may hold not only for voters, but also for policymakers. First, in a democracy, mechanisms of electoral control and accountability increase the likelihood that the preferences of leaders will match the preferences of the electorate. Second, it is not obvious why politicians would be less sensitive than citizens to issues of international law. Politicians differ from the average voter in certain respects, including education and income. Table 3 shows, however, that the impact of international law is at least at large for people with college degrees and high incomes, as for people with less education and wealth. Third, data in the next two sections show that British policymakers regard international law as highly consequential. It seems likely that many U.S. leaders would approach international law in the same way.

5. An Experiment-Based Analysis of Expectations

Having found, through experiments, that international law exerts a substantial effect on policy preferences, I collaborated with Caroline Andersen, Lauren Falcao, and Merrit Kennedy on a follow-up study about the impact of treaties on expectations. The experiment, which

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[7] One might wonder whether respondents felt social pressure to respect international law. This is, of course, possible. But it is equally possible that respondents felt social pressure to demonstrate consistency, by not letting counter-arguments by the interviewer swing their opinions one way or the other. Moreover, if social desirability effects exist in the survey data, they may be just as present in the real world. After all, interest groups, NGOs, and politicians moral pressure to persuade voters and policymakers about various courses of action.

concerned the Nuclear Nonproliferation Treaty, was administered through face-to-face interviews with members of the British House of Commons in June-August 2006. All 646 members of the Commons were invited by email to participate in this study. A total of 75 agreed to be interviewed. The first eight interviews were devoted to pretesting and refining the questionnaire. The analysis below focuses on the 67 members of parliament who answered the questionnaire in its final form.

Each British MPs was told: "There's much concern these days about the spread of nuclear weapons. I'm going to describe a country that may or may not be pursuing nuclear weapons. For scientific validity, the description involves a general type of country, rather than a specific country in the news today. Some parts of the description may strike you as very important; other parts may seem much less important. When I have finished the description, I will ask—in your best judgment given the limited information available—how likely or unlikely you think it is that the country is pursuing nuclear weapons."

Each MP then received background information, akin to an intelligence report. The treatment group, comprising roughly half the sample, was presented with the following facts:

- The country borders on an unfriendly nation that has nuclear weapons and has threatened to use them in a future war.

- The country has repeatedly said that it does not want nuclear weapons.

- The country has signed the Nuclear Non-Proliferation Treaty, thereby pledging "not to receive, manufacture, or otherwise acquire nuclear weapons."

- Recent satellite images show that the country has started enriching uranium, which could be used for either civilian or military purposes.

- The country has a stagnant economy.

The control group received an identical set of facts but was told that "The country has not signed the Nuclear Non-Proliferation Treaty."

By design, the experiment did not call undue attention to the NPT. The country's status as a signatory was included as just one of many facts, and mention of the NPT was inserted in the middle of the list, rather than at the beginning or the end, where its placement might have been more salient. Moreover, participants were told that some of the facts might be unimportant, to reduce if not remove any implication that the country's status in the NPT should be a key consideration.

After reading these points aloud, the interviewers passed the MP a card that contained the same information in written form, thereby giving the MP a chance to review the facts. The interviewers then asked: "Would you say that it is very likely that the country is pursuing nuclear weapons, that it's somewhat likely, that there's a 50-50 chance, that it's somewhat unlikely, or that it's very unlikely?" Finally, the interviewers encouraged each MP to elaborate on the reasons behind their assessment.

MPs who answered these questions were fairly representative of the House of Commons as a whole. For instance, the party affiliations and tenures of participants in our study were very close to the House benchmarks.[8] The sample did include a disproportionate share of males, and a larger proportion of MPs that had served on foreign affairs committees or had, in their official biobiographies, expressed an interest in foreign countries.[9] Given that we had asked MPs to participate in a study about "how British leaders think about important foreign policy issues," it

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[8] At the time of the study, the partisan composition of the House of commons was 55 percent Labour, 31 percent Conservative, 10 percent Liberal Democrat, and 5 percent Other. In our sample, the analogous figures were 51, 33, 15, and 1 percent. Likewise, the median years of experience in the House of Commons was 10 years, the same as in our sample, and the mean was 12 years in the Commons, versus 11 in our sample.

[9] The sample was 87 percent male, versus 80 percent in the house of commons. Approximately 72 percent of the sample had, in their official biographies, expressed an interest in foreign affairs, and 30 percent had served on foreign affairs committees. The analogous figures for the House of Commons as a whole were 62 percent and 20 percent, respectively.

is likely that some foreign-affairs-minded leaders self-selected into the study. This kind of sample may be especially relevant for understanding the foreign policy, since those who expressed an interest in foreign affairs or had served on such committees would probably be most influential in shaping British foreign policy. Through a battery of parametric and nonparametric tests, I confirmed that treatment and control groups were balanced according to demographic and political variables.

Data from this experiment show that even the oft-maligned Nuclear Nonproliferation Treaty has a powerful effect on the expectations of policymakers (see Table 6). When told about a country that had not signed the NPT, a sizeable majority (approximately 61 percent) of British MPs deemed it either somewhat likely or very likely that the country was pursuing nuclear weapons. In contrast, only 35 percent of MPs thought it likely that a signatory was following the nuclear path. The difference between these two estimates is roughly 25 percent, implying that the mere signing of a treaty would have changed the policy expectations of roughly one-fourth of the British parliament. Another way to express the power of international law is via the relative risk statistic, 1.8, which means that, in this experiment, the perceived likelihood of pursuing nuclear weapons was approximately 80 percent higher when the country had not signed the NPT than when it was party to the treaty.

[TABLE 6 ABOUT HERE]

Analysis of open-ended comments from British MPs supports the same conclusion. Roughly one-fourth of MPs in the treatment group explained that they judged proliferation less likely *because* the country had signed the NPT. Even more impressively, 39 percent of MPs in the control group said they were swayed in their estimates by the country's failure to sign the NPT. Apparently, countries that signed the treaty reassured British policymakers, whereas

countries that failed to sign aroused suspicion. Both patterns are consistent with the idea that treaties constrain, such that decisions to sign (or not) convey information about preferences and abilities.

As with the experiments about international trade, though, the experiment about nuclear proliferation demonstrated not only the power but also the limits of international law. More than one-third of British MPs deemed it likely that the country was pursuing nuclear weapons, even though it had signed the Nuclear Nonproliferation treaty. Moreover, six participants in the study explicitly noted that the treaty had no effect. One parliamentarian, for example, described the treaty as "irrelevant," and another emphasized that signing or not signing the treaty would "not actually make any difference." But in the eyes of a significant portion of British policymakers, treaties transform expectations about the behavior of other countries.

6. Mechanisms: Reputation and International Law

The previous sections presented individual-level evidence that treaties change preferences and expectations. This section offers a preliminary empirical look at the mechanisms behind these effects. I hypothesized that, in an anarchical world, international law could affect preferences and expectations by raising the reputational cost of reneging. Treatymaking could generate this effect by publicizing commitments, by using language that coveys solemnity of purpose, and by creating conceptual links among otherwise disparate commitments.

As a step toward exploring these mechanisms, I analyzed how British MPs responded to two hypothetical scenarios. The first scenario, devoid of any legal overtones, tested the effects of publicity. "Suppose two countries make similar military threats. One country delivers its threat in a private meeting with the foreign president. The other country delivers the threat in a public

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speech at the United Nations. If the threats are alike in other ways, which country is less likely to be bluffing and thus more likely to follow through, or are they about equally likely to follow through?"

The second scenario was designed to quantify the added effect, if any, of legalizing a public commitment. MPs were asked to comment on the following: "At an international summit, two countries publicly promised to defend their allies. One announced its intention orally, the other signed a treaty. Which country would be more likely to defend its ally, or would they be equally likely to do so?"

Table 7 strongly confirms that publicity and legalization have independent effects, and they combine to render international commitments more credible. The interior cells give the percentage of British MPs who offered each possible combination of responses to the publicity and legalization scenarios, and the margins (right column and bottom row) summarize the aggregate effects of publicity and legalization.

[TABLE 7 ABOUT HERE]

The table demonstrates that publicity in itself contributes to credibility. A slight majority (57 percent) of MPs felt that public commitments delivered in an international forum such as the United Nations were more likely to be carried out than private commitments from one president to another. Another 27 percent contended that publicity would neither increase nor decrease credibility. Finally, a small minority (16 percent) contended that private commitments were more believable than public ones. On balance, then, MPs were about 3.5 times more likely to view publicity as a boon to, rather than a detractor from, credibility (for additional evidence about the reputational effects of publicity in military affairs, see Tomz 2007a).

The evidence shows even more resoundingly that legalization raises the credibility of public commitments. In the judgment of 75 percent of British policymakers, entering a treaty to defend an ally signaled greater commitment than simply announcing one's intention orally. About 22 percent perceived no difference, and only 3 percent viewed the treaty as less convincing than a public proclamation. Among MPs who regarded legalization as consequential, then, positive effects outweighed negative effects by 25 to 1.

To what extent did MPs see both publicity and legalization as consequential, rather than crediting one while doubting the other? The interior cells of Table 7 provide an answer. A plurality (39 percent) assigned positive effects to both mechanisms, and an additional 36 percent felt one was productive while the other was neutral. In contrast, only 18 percent contended that publicity and legalization worked at cross-purposes, and only 6 percent assigned no importance to either mechanism. This evidence is consistent with a reputational theory in which treaties contribute to credibility by publicizing international commitments *and* embedding them in a legal framework.

Additional evidence of the reputational mechanism comes from a follow-up questionnaire, which was administered to 22 members of the House of Commons in February 2008.[10] MPs were asked to "compare the consequences of breaking two types of international commitments: written treaties signed by the leader, versus oral promises made by the leader." One potential consequence involved reputation. "Some people say that, if a country breaks an international commitment, its reputation might suffer. Foreign leaders might become more skeptical of the country's willingness to keep commitments in the future. Which statement

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[10] A total of 100 MPs were invited to participate by taking an on-line questionnaire. In the first week of the field period, 22 completed the survey, representing a response rate of 22 percent.

comes closest to your view about how the two types of actions—breaking a written treaty versus breaking an oral promise—would affect a country's international reputation?"

As Table 8 shows, a majority of British policymakers (59 percent) concluded that breaking a written treaty would cause more reputational damage than violating an oral commitment. This powerfully confirms the hypothesis that, in the judgment of most elected officials, international law raises the reputational cost of reneging on international agreements.

An additional 36 percent of British MPs felt that both types of actions would harm a country's reputation to an equal degree. Their answers, though inconsistent with the idea that international law uniquely raises the reputational ante, are nonetheless consistent with a general theory of cooperation through reputation, in which cooperation is sustained by the fear that breaking any commitment—legal or nonlegal—could make foreign leaders skeptical in the future. Finally, only 5 percent of MPs (1 of 22 in this study) felt that neither action would damage a country's reputation. Overall, these data strongly support the idea that breaking commitments is reputationally costly, especially when commitments are embedded in international law.

[TABLE 8 ABOUT HERE]

7. Conclusions

In at least two ways, the experiments in this paper shed have new light on the effects of international law. First, the experiments have overcome problems of endogeneity and measurement that have hampered previous studies. In the experiments, I assigned treatment and control randomly without reference to background features of the situation or the respondent. As a result, there was no significant correlation between the treaty and baseline propensities or

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sensitivity to treatment. This greatly simplified the problem of inference: I obtained unbiased estimates of the law's effect through tabular analysis of the experimental data. Randomization eliminated the need for scores of regressions with control variables, which must be used in observational studies to balance the treatment and control groups.

Second, the experiments have strengthened the microfoundations of international relations theory by revealing how international law affects preferences and beliefs. Previous researchers have found correlations between treaties and policy outcomes. Some contend that the relationship is spurious, whereas others argue that treaties change cost/benefit calculations. The evidence in this paper supports the second interpretation. Individuals are more reluctant to pursue policies that would violate international law than to pursue otherwise identical policies that are not enshrined in a legal commitment. And individuals—including expert policymakers—anticipate that signatories to treaties will exhibit systematically different behavior from non-signatories. Moreover, preliminary evidence suggests that these effects arise, at least in part, because international law raises the reputational cost of reneging on international commitments.

The experimental method in this paper could be extended to answer a wide range of questions about international agreements. The prevailing research strategy, which relies entirely on observational data, is essentially passive. To study a particular variable, researchers must wait for natural processes to generate the variation they need, in quantities large enough to support statistical analysis or in patterns convenient enough to permit controlled case studies. A passive strategy has significant limitations. Some factors may exhibit minimal variation or be highly collinear with other factors, and some values may occur too rarely to support precise estimates. An experimental approach can overcome these limitations by allowing full control over the explanatory variables.

In particular, subsequent studies could vary not only the presence but also the form of the international agreement, thereby shedding light on the effects of institutional design (Koremenos, Lipson and Snidal 2001). Lipson (1991) hypothesizes that the costs of reneging increase with the precision of the agreement, the formality by which it was conveyed, and level of government that authorized it. Rosendorff and Milner (2001) add that the penalty for deviating from commitments can be lower in the presence of escape clauses. Finally, work on international debt shows that lenders will excuse defaults that occur because of a fundamental change in circumstances or widespread noncompliance by other parties (Tomz 2007b). With experiments, one can test whether citizens and elites take similar contingencies into account when thinking about treaties and other international agreements.

Of course, the experimental approach is not infallible. Indeed, experiments are vulnerable on precisely the dimension where observational data is most compelling: external validity. Voters and elites might behave differently in an interview than in real foreign policy situations. Differences could emerge because respondents know they are subjects of a study, because the interviewer can offer only limited background information, or because emotion plays a different role in interviews than in actual politics.

To some extent, concerns about external validity can be minimized by making the scenario as convincing as possible and replicating the experiments with different question wording and sample frames, to increase confidence in the generality of the results. Ultimately, though, the evidence from experiments should be combined with observational data to obtain a fuller understanding of international law. Every methodology has its limitations. The best way to make progress on complicated topics is to analyze data from multiple sources. The evidence in this paper complements a growing body of high-quality research that others have done with

historical data, by demonstrating that international law has a large and important effect on preferences and beliefs about foreign policy.

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TABLE 1. *Effect of international law on policy preferences*

	Opposition to policy (%)
Violates international law	44 (38 to 51)
Does not violate international law	27 (23 to 31)
Difference (effect of int'l law)	17 (10 to 25)

Note: Table gives the percent of respondents who opposed cutting trade with Burma. Bayesian 95 percent credible intervals appear in parentheses.

Source: Author's calculations from a survey of 1,000 U.S. adults conducted by Knowledge Networks in July 2005.

TABLE 2. *Effect of international law on preferences, by counter-argument*

Counter arguments	Opposition if illegal (%)	−	Opposition if not illegal (%)	=	Effect of int'l law (%)
Human rights only	54 (43 to 65)		37 (30 to 45)		17 (4 to 30)
U.S. economy only	40 (30 to 51)		30 (23 to 37)		10 (-3 to 23)
Both arguments	37 (26 to 49)		15 (10 to 21)		22 (9 to 35)

Note: Table gives the percent of respondents who opposed cutting trade with Burma. Bayesian 95 percent credible intervals appear in parentheses.

Source: Author's calculations from a survey of 1,000 U.S. adults conducted by Knowledge Networks in July 2005.

TABLE 3. *Effect of international law on preferences, by demographic group*

Demographic group	*Opposition if illegal (%)*	−	*Opposition if not illegal (%)*	=	*Effect of int'l law (%)*	*Difference in effects (%)*
Liberals	53 (41 to 65)		28 (21 to 36)		25 (11 to 40)	10 (-10 to 29)
Conservatives	47 (35 to 58)		31 (25 to 38)		15 (2 to 29)	
Democrats	48 (37 to 59)		27 (21 to 33)		21 (9 to 34)	4 (-14 to 22)
Republicans	43 (32 to 53)		25 (19 to 32)		17 (5 to 30)	
Females	45 (37 to 54)		26 (21 to 31)		19 (9 to 30)	-4 (-11 to 20)
Males	43 (33 to 53)		28 (22 to 34)		15 (4 to 26)	
Some college	47 (38 to 56)		28 (23 to 33)		19 (9 to 29)	5 (-11 to 20)
No college	40 (31 to 50)		26 (20 to 32)		15 (3 to 26)	
High income	46 (36 to 56)		23 (18 to 28)		24 (12 to 35)	12 (-4 to 27)
Low income	43 (34 to 51)		31 (25 to 36)		12 (2 to 22)	

Note: Table gives the percent of respondents who opposed cutting trade with Burma. Bayesian 95 percent credible intervals appear in parentheses.

Source: Author's calculations from a survey of 1,000 U.S. adults conducted by Knowledge Networks in July 2005.

TABLE 4. *Effect of international law on preferences—addition versus substitution*

Experimental design	*Effect of adding or substituting strong law (%)*
Add a strong law argument	10 (6 to 14)
Substitute strong law for a:	
weak law argument	7 (3 to 11)
redundant con argument	8 (4 to 12)
weak con argument	9 (5 to 13)
strong con argument	5 5 (1 to 9)

Note: Table gives the percentage-point increase in opposition to trade sanctions as a consequence of adding or substituting a strong-law argument. Bayesian 95 percent credible intervals appear in parentheses. Opposition was 26 percent (23 to 29) in the presence of a strong law argument, versus 19 percent (17 to 22) with a weak law argument, 18 percent (15 to 21) with a redundant con argument, 17 percent (14 to 20) with a weak con argument, and 21 percent (18 to 24) with a strong con argument. In the absence of any of these extra con arguments, opposition was 15 percent (13 to 18).

Source: Author's calculations from a survey of 5,800 U.S. adults conducted by Polimetrix in October 2007.

TABLE 5. *Effect of international law on preferences: within-subject experiments*

Issue	Year	Pre-treatment (full sample) Favor (%)	N	Post-teatment (subsample that favored previously) Still favor (%)	Now unsure (%)	Now oppose (%)	N
Military intervention	1988	42 (39 to 45)	1,000	52 (47 to 58)	13 (10 to 17)	35 (30 to 40)	418
Missile defense	1985	41 (38 to 43)	1,506	62 (58 to 66)	5 (3 to 7)	33 (30 to 37)	611
Missile defense	1985	55 (53 to 57)	2,041	65 (62 to 68)	6 (5 to 8)	29 (26 to 31)	1,125
Missile defense	1987	54 (51 to 57)	1,001	60 (56 to 65)	12 (10 to 15)	27 (24 to 31)	539
Missile defense	2000	59 (55 to 62)	947	48 (43 to 52)	10 (8 to 13)	42 (38 to 46)	554
Missile defense	2001	61 (58 to 64)	1,105	49 (46 to 53)	9 (7 to 12)	41 (38 to 45)	677

Note: Table gives the percentage of citizens that favored drug interdiction or missile defense without any mention of international law, and it shows how their opinions changed after introducing a consideration about international law. Bayesian 95 percent confidence intervals appear in parentheses.

Source: Author's calculations from polls by CBS News & the New York Times; Chilton Research Services, The Los Angeles Times, Market Opinion Research, and Marttila & Kiley. All data provided by the Roper Center for Public Opinion Research, University of Connecticut.

TABLE 6. *Effect of international law on policy expectations*

	Likely to be pursuing nuclear weapons (%)
Did not sign the NPT	61
	(42 to 77)
Signed the NPT	35
	(21 to 52)
Difference	25
	(1 to 47)
Relative risk	1.8
	(1.0 to 3.0)

Note: Table gives the percent of respondents who deemed it likely that the country was pursuing nuclear weapons. There were 28 valid responses in the control group (did not sign NPT) and 34 valid responses in the treatment group (signed the NPT). Bayesian 95% credible intervals appear in parentheses.

Source: Author's calculations from interviews with members of the British House of Commons conducted by Caroline Anderson, Lauren Falcao, and Merrit Kennedy in June–August 2006.

TABLE 7. *Mechanisms: effects of publicity and legalization on credibility*

Publicizing a commitment	Legalizing a public commitment			
	Reduces credibility	*Has no effect*	*Increases credibility*	*Total*
Reduces credibility	0 (0 – 5)	1 (0 – 8)	15 (7 – 26)	16 (8 – 27)
Has no effect	0 (0 – 5)	6 (2 – 14)	21 (12 – 33)	27 (17 – 39)
Increases credibility	3 (0 – 10)	15 (7 – 26)	39 (27 – 51)	57 (44 – 69)
Total	3 (0 – 10)	22 (0 – 10)	75 (0 – 10)	100

Note: Each cell of the cross-tabulation gives the percentage of MPs, from a total sample of 67, who offered the stated combination of answers to the publicity and legalization questions. 95 percent confidence intervals appear in parentheses.

Source: Author's calculations from interviews with members of the British House of Commons conducted by Caroline Anderson, Lauren Falcao, and Merrit Kennedy in June–August 2006.

TABLE 8. *International law and the reputational cost of reneging*

Effect on reputation	%
Breaking a written treaty would be more harmful	59 (37 to 81)
Breaking an oral promise would be more harmful	0 (0 to 15)
Both actions would be equally harmful	36 (17 to 59)
Neither action would be harmful	5 (0 to 23)

Note: Table gives the percentage of respondents, out of 22 in total, who offered each perspective about the reputational consequences of breaking international commitments. 95 percent confidence intervals appear in parentheses.

Source: Author's calculations from a survey of members of the British House of Commons administered by Stanford University in February 2008.