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Essays on the Influence of International Agreements

A dissertation presented
by
Adam Stuart Chilton
to
The Department of Government

In partial fulfillment of the requirements
for the degree of
Doctor of Philosophy
in the subject of
Political Science

Harvard University
Cambridge, Massachusetts

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Dissertation Advisor: Professor Beth A. Simmons Adam Stuart Chilton

Essays on the Influence of International Agreements

Abstract

Since World War II, states have negotiated a staggering number of bilateral and multilateral international agreements. Despite that fact, scholars of international relations and international law have only recently begun to take the idea that these agreements can have important influences on domestic policies and international affairs seriously. This dissertation is comprised of five essays that all try to do exactly that, and hopefully in the process, help improve our understanding of the influence of bilateral and multilateral international agreements on state behavior.

The first three essays examine compliance with the laws of war and international human rights treaties. Chapter 2 shows that prior ratification of treaties on the laws of war is a strong predictor that a country will be less likely to kill civilians during intrastate wars, and suggest that there may be a causal relationship between ratification and lower levels of mass violence against civilians for transitioning democracies. Chapter 3 conducts a randomized survey experiment to test whether information on the status of international law changes public opinion on violations of the laws of war, and produces results showing that international law does change public opinion—especially when the other side has committed to following the laws of war. Chapter 4 uses a randomized experiment to test the theory that domestic politics drives compliance with human rights treaties, and demonstrates that whether the United States has previously ratified

international human rights treaties has the potential to change public opinion on purely domestic policies.

The final two essays examine the United States' policies in two areas of international economic law. Chapter 5 (with Rachel Brewster) explores the United States' compliance with adverse WTO decisions, and argues that the largest determinant of if, and when, America complies is whether Congress is required to act to provide the remedy. Finally, Chapter 6 uses a range of evidence to argue that the United States' Bilateral Investment Treaty program has not been primarily motivated by a desire to provide protections for American investors abroad, but instead it has been a tool to improve relationships with developing states.

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This dissertation is dedicated to my parents,
for teaching me to be curious about the world,
and making sacrifices so I could explore it.

Chapter 1

Introduction

Since World War II, states have negotiated a staggering number of bilateral and multilateral international agreements. Despite that fact, scholars of international relations and international law have only recently begun to take the idea that these agreements can have important influences on domestic policies and international affairs seriously. This dissertation is comprised of five essays that all try to do exactly that; and hopefully in the process, help improve our understanding of the influence of bilateral and multilateral international agreements on state behavior.

Chapter 2 explores compliance with the laws of war during civil wars. Despite the energy that has been expended developing and promoting treaties aimed at protecting civilians during conflicts, only two published studies have attempted to empirically examine whether countries have complied with the laws of war. The two studies both focused on interstate wars and produced conflicting results. As a result, we have little insight into whether the laws of war are helping to protect civilians during interstate war, and no insight into whether they do so during intrastate wars. In the first attempt to empirically examine the subject, I have compiled a dataset on civil wars that have occurred since Additional Protocol II to the Geneva Conventions—which governs non-international wars—went into effect to study if a country's ratification of this agreement has any impact on its conduct during intrastate conflicts. My results suggest both that countries that ratify Additional Protocol II are less likely to intentionally kill civilians during civil wars, and that ratification may alter the behavior during intrastate wars of countries that have had mixed experiences with democracy. These results offer hope that the international project to build a robust legal regime to regulate conflict may be helping to protect civilians from the horrors of war.

Chapter 3 takes a different approach to examine compliance with the laws of war by conducting a random experiment embedded within a survey. Observational studies examining whether ratification of treaties on the laws of war reduces the chance that states will target civilians during war have produced conflicting results. To try and bring new evidence to this debate, I have conducted a survey experiment to determine if respondents who are told their government ratified law of war treaties feel more negatively about learning their government's actions lead to civilian deaths. The goal of the project is to help determine if a theoretical mechanism that is often hypothesized as driving compliance with international law—domestic political pressures—could possibly do so in the law of war context. My results show that information on the status of international law does lower support for actions that would violate the laws of war. Moreover, this result is strongest when respondents are told that the opposition force has committed to following international law as well. These results suggest that ratification of treaties on the laws of war has the potential to change the behavior of democracies during conflicts by altering public opinion on the acceptability of taking actions that would result in excessive civilian casualties.

Chapter 4 uses a randomized experiment to test theories of compliance with human rights treaties based on domestic politics. Scholars of international relations have long speculated that commitments to international human rights treaties are unlikely to have an effect on domestic human rights practices. Recent research, however, has suggested that ratification of these agreements may change public policies in democracies by changing the domestic political support for reform. Although observational studies have shown that democracies are in fact more likely to change their human rights practices as a result of treaty commitments, these studies have not been able to directly test the causal mechanisms speculated to cause the reforms.

In an effort to do so, I have embedded a random experiment within a survey in the first effort to explore whether information on the status of international law changes opinion on a purely domestic policy issue: subjecting prisoners to solitary confinement. My results show that although generic appeals to human rights do not have a statistically significant impact on public opinion, references to prior treaty commitments do. These results thus provide the first experimental evidence supporting domestic politics theories of compliance with international human rights law.

Chapter 5 explores how whether the United States complies with WTO decisions is involved by institutional actors. In studies of compliance with international law, the focus is usually on the "demand side" – that is, how to increase the pressure on the state to comply. Less attention has been paid, however, to the consequences of the "supply side" – who within the state is responsible for the compliance. In what we believe is the first attempt to systematically address this issue, our study explores how the United States government alters national policy in response to the cases initiated within the WTO dispute settlement procedure. To do so, we have compiled the first data set of the policy actions taken by the U.S. in response to other states' requests for DSU consultations. After including variables that control for important characteristics of the state filing the request and the importance of the affected industry, our results go beyond previous research to show that who within the government must supply compliance is the largest determinant of both whether and when the United States government complies with WTO rulings.

Chapter 6 considers the politics of the United States' Bilateral Investment Treaty Program. Why does the United States sign Bilateral Investment Treaties (BITs)? Although this question has received little attention, the scholars that have addressed it have unanimously asserted that the U.S. pursues BITs to Improve the protections provided to American Capital invested abroad. Given this belief, scholars have evaluated U.S. BITs on their ability to protect and promote investment. This view of the U.S. BITs program is incorrect. I argue that the U.S. has used BITs as a foreign policy tool to improve relationships with strategically important countries in the developing world, and as a result, the program should be evaluated based on whether it has produced political benefits. Using a dataset of all U.S. dyads from 1981 to 2009 built for this project, I test this theory in two ways. First, I empirically test the factors that have influenced with who the U.S. has signed BITs. The results of this analysis show that investment concerns cannot explain American BITs, but that the political important of the country does. Second, I use a new method of causal analysis—Life History matching—to evaluate the political benefits the United States has received from signing BITs with developing states. My analysis shows that having a BIT with American makes a country more likely to vote similarly to the U.S. at the United Nations and may allow the U.S. to deploy troops on their soil, but not to have been part of the Iraq War Coalition. This project thus provides the first evidence both that the U.S. BITs program has been motivated by political and not investment considerations, and that it may have produced at least modest foreign policy dividends.

Chapter 2

Do Laws Ameliorate the Horrors of War? Compliance with the Laws of War During Intrastate Conflicts

2.1 Introduction

International Humanitarian Law (IHL)—better known as the laws of war—has evolved over centuries in an effort to try and limit carnage during war to protect both combatants and civilians. Since the end of World War II, the importance and prominence of the IHL project has grown as diplomats and activists have tried to create international laws to tie the hands of governments and insurgents during war. The result has been an increasingly dense web of legal treaties and customs that govern this sphere of international law. Despite the admirable and lofty goals of IHL, there has been very little empirical work done to examine whether the laws of war make a difference on the actual conduct of war. In other words, we do not know whether the considerable diplomatic efforts that have been put into producing the laws of war have helped to save countless civilians or simply produced mountains of paper.

In fact, scholars of international relations have long been skeptical of whether international law can change the preferences and behavior of states at all, let alone affect their conduct during conflicts.[1] Yet despite the prevalence of this skepticism, the only two published studies that have tried to quantitatively examine whether the laws of war help to influence conduct during conflict produced contradictory results. The first study, conducted by Valentino, Huth, & Croco produced results showing that even democratic states ignore the laws of war during interstate conflicts and instead focus solely on achieving strategic military objectives.[2] In contrast, a second study by James Morrow suggested that, although autocratic states are unlikely to comply with the laws of war during interstate conflicts, democratic states actually are likely to do so.[3]

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[1] See, e.g., Morgenthau 2006.

[2] Valentino et al. 2006.

It difficult to draw too many inferences on the success of efforts to promote the legalization of war based on these studies, not only because they reached opposite results, but also because of the conflicts their studies analyzed. Valentino et al. examined conflicts that began after 1900,[4] and Morrow examined conflicts that started after 1899.[5] Although it is true that there were treaties that governed conflicts at the turn of the twentieth century, it was not until in the second half of the century that these efforts truly took off.[6] It is thus likely unfair to measure the success of the efforts to promote IHL based on conflicts that occurred prior to the more concerted efforts to promote the laws of war after World War II. Additionally, both of these studies solely examined interstate wars. The shortcoming of that approach is that interstate wars have comprised an increasingly small share of global conflicts in the post-war period.[7] As a result, the approach of examining only interstate wars to study whether the development of the laws of war have help to protect civilians has serious limitations.

One way to make progress on this important question, however, is to examine whether the laws of war have had an influence on the conduct of civil wars. Although the original focus of the Geneva Conventions of 1949 was on interstate-armed conflicts, Additional Protocol II to the Geneva Conventions (AP II) was created in 1977 to provide a more comprehensive set of protections to the potential criticisms of wars occurring within the boundary of a state.[8] Despite the fact that improving compliance with the laws of war was one of the most pressing concerns facing the framers of AP II,[9] there have not been any empirical efforts to study whether signatories of the treaty have been compliant. There are several reasons, however, why analyzing compliance with AP II is a promising way to study compliance with the laws of war more broadly. First, as previously noted, intrastate wars have been more prevalent than interstate wars since 1945. Second, although essentially all states are now subject to the Geneva Conventions of 1949 and other treaties that regulate interstate wars, there has been a great deal of variance in whether the states that have had civil wars previously ratified AP II. Third, whether states comply with the laws of war during civil wars is both a new test, and likely a harder test, for the study of compliance with international law more broadly.

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[3] Morrow 2007.

[4] Valentino et al. 2006, 350.

[5] Morrow 2007, 562.

[6] See, e.g., Roberts and Guelff 2000, 4 (noting it was “the second half of the nineteenth century when the laws of war began to be codified in multilateral treaties”). See also Myuot and del Rosario 1994, 7.

[7] See Moir 2002, 1 (noting that “[s]ince 1945, the vast majority of armed conflicts have been internal rather than international in character”). See also Perna 2006, 99.

For these reasons, I have built a dataset of intrastate conflicts covered under AP II that occurred between 1989 and 2010. Using this dataset of 38 countries and 279 conflict years, I have conducted a series of statistical tests to estimate whether ratification of AP II made a country less likely to intentionally kill civilians during civil wars. These results suggest that ratification of AP II is a statistically significant predictor of fewer incidents of intentional violence against civilians. To measure whether ratification actually affects behavior, instead of just being a sign that a country already did not plan to target civilians, I have also conducted a series of instrumental variable regressions that suggest committing to AP II is likely to influence the decisions of countries with mixed histories with democracy. In other words, my results suggest that countries that have ratified AP II are less likely to kill civilians, and for countries that are partial or transitioning democracies, the fact that they choose to ratify AP II may actually have a causally significant impact on behavior.

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[8] Protocol Additional to the Geneva Conventions of 1949 (Protocol II), opened for signature Dec. 12, 1977, reprinted in 16 INT'L LEGAL MATERIALS 1442 (1977).

[9] Aldrich 1981, 765.

This paper thus makes three important contributions. First, this paper helps to expand the incredibly small body of literature that has sought to apply rigorous quantitative methods to the study of the laws of war.[10] Despite the fact that this is an area where the stakes of international law are perhaps the highest,[11] relatively little quantitative work has been done addressing compliance with the laws of war, and it is my hope that this paper will make a meaningful contribution to this neglected area of research. Second, this paper presents the first evidence that it is at least plausible that promotion of AP II has helped to protect civilians during conflicts. Although it is difficult to definitively determine causation, it is still important to know that countries engaged in intrastate wars that have ratified the treaty have committed fewer intentional acts of one-sided violence against civilians. This finding directly challenges the conventional wisdom that countries are likely to ignore international law during war.[12] Third, the results of this paper lend support to emerging theories of compliance with international law based on domestic politics.[13] Previous research has suggested that the ratification of human rights treaties is the most likely to alter behavior in the case of partial or transitioning democracies,[14] and this paper suggests that the same may be true with law of war treaties.

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[10] For a discussion of the empirical research on international law focusing on war, peace, and security, see Beth Simmons 2010, 280-83. For a more general discussion of the growing trend of empirical work in international legal scholarship, see Shaffer and Ginsburg 2012. See also Hathaway 2004.

[11] See Armstrong et al. 2012, 147. (noting while discussing compliance with international law that "[o]f all the areas of state activity, war involves the highest stakes"). See also Simmons 2010, 281 ("Agreements that constrain military operations in the heat of battle present the most significant challenges for international treaties.").

[12] See, e.g., Mearsheimer 1995.

This paper proceeds in four parts. Part 2.2 provides a very brief discussion on previous research on the laws of war, and then discusses the merits of studying civil wars, before generating hypotheses on the factors that should influence compliance with AP II. Part 2.3 describes the dataset built for this project. Part 2.4 presents the results from three different empirical approaches that were used to estimate whether ratification of AP II predicts or influences compliance with the laws of war. Part 2.5 concludes.

2.2 The Laws of War & Compliance with International Law

Since the development of multinational treaties governing conflicts after World War II, one of the most important concerns of international humanitarian lawyers has been improving compliance with the laws of war.[15] Despite that fact, there has not been a single empirical study that has focused on whether their efforts have produced dividends.[16] This part lays the groundwork for my effort to answer that question by first outlining existing scholarship on compliance with international law as it relates to the laws of war, then providing a very brief background on the regulation of civil wars and AP II, and finally discussing theoretical expectations that can be derived from prior research on compliance with international law.

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[13] It is worth noting that substantial progress has been made in developing these kinds of theories. See, e.g., Koh 1998; Koh 1995.

[14] See generally Simmons 2009. For more discussion on this topic, see infra Part 4.2.3.

[15] See Aldrich 1981, 765.

[16] But see Jo and Thomson 2012 (analyzing the compliance of state and non-state actors in granting the International Committee of the Red Cross access to detention centers during civil conflicts).

2.2.1 Research on Compliance and the Laws of War

The laws of war are a body of treaty and customary law that seeks to limit the humanitarian costs of conflicts. Specifically, the "stated goal of [the laws of war] is minimizing humanitarian suffering of both combatants and civilians during the conduct of hostilities."[17] Although the laws of war have developed over centuries, the obligations and restrictions placed upon states have increased dramatically in the second half of the twentieth century as an increasingly dense web of treaties and agreements have developed to try and regulate armed conflicts.[18] The rules currently in place include a mix of absolute and relative restrictions on conduct. For example, intentional killing of civilians is strictly forbidden, but attacks that may cause incidental civilian deaths are only prohibited if the loss of innocent life is excessive relative to the military advantage of the actions.[19] Taken together, this body of law represents an attempt to balance the importance of protecting lives and property during conflicts while still recognizing the realities of modern warfare.

Although there has been considerable diplomatic effort put into developing and promoting this body of law, scholars of international relations have been skeptical over whether it has had any influence on the way that states conduct war. According to one set of recent commentators, "it is widely believed, especially by realist scholars of [International Relations] that when it comes to war, states pay no heed to international law."[20] Of course, given the general skepticism that exists among many scholars in international relations about the ability of international law to alter preferences or decisions in any context,[21] it should be unsurprising that there would be doubts over the likelihood of compliance when state security and survival is on the line (as it often is during war). Despite these doubts, the empirical evidence on whether the laws of war alter state behavior is decidedly mixed. As previously mentioned, the two studies that have attempted to quantitatively assess whether states have complied with the laws of war produced opposite results.

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[17] Blum 2010, 7.

[18] See Roberts and Guelff 2000; Perna 2006.

[19] See Blum 2010, 9 (citing Additional Protocol Relating to the Protection of Victims of International Armed Conflicts arts. 37, 51, June 8, 1977, 1125 U.N.T.S. 3.).

The first study, by Valentino et al., found that ratification of treaties on the laws of war did not have a statistically significant impact on the number of civilians killed during conflicts.[22] To reach this result, Velentino et al. compiled a data set of all interstate wars from 1900 to 2003. They then coded whether combatants had ratified a succession of treaties on the laws of war prior to the start of the conflict. After doing so, they ran a series of regressions estimating the impact of ratification on both intentional and total civilian casualties during the conflicts. Despite estimating a range of models with various controls, their research did not produce any statistically significant relationship between treaty ratification and civilian casualties.

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[20] Armstrong et al. 2012, 147.

[21] See, e.g., Mearsheimer 2001; Mearsheimer 1995; Morgenthau 2006.

[22] Valentino et al. 2006.

The results produced by Valentino et al. are consistent with other research that has examined whether regime type has an impact on civilian casualties during war. In a series of projects, Alexander Downes has argued that democratic states are actually *more* likely to target civilians during warfare.[23] To reach this conclusion, Downes looked at a similar list of interstate conflicts from 1900 to 2003 as Valentino et al. But instead of using an OLS model to estimate intentional or total civilian deaths like Valentino et al., Downes employed a binary model to estimate whether regime type had any effect on if a "mass killing" of civilians occurred at any point during a conflict. Although Downes did not specifically include ratification of laws of war as a variable in his studies, his results have significant implications for the question of whether states comply with the laws of war because "mass killings" are unambiguously prohibited in IHL and it has been theorized that the laws of war are more likely to alter the conduct of democratic states than other types of regimes. Downes work thus provides additional evidence suggesting that democracies are less likely to follow the principles of IHL and protect civilians during war.

The second study to directly examine whether states comply with the laws of war, by James Morrow, produced results opposite to those discerned by Valentino et al.[24] Morrow's study also focused exclusively on interstate wars, but examined a different a slightly different set of conflicts that occurred between 1901 and 1993. Morrow's study also differed from Valentino et al.'s by using an ordered dependent variable for compliance, examining a wider range of issues than just civilian deaths, and including interaction terms for the ratification behavior of both states in the conflicts. Morrow's results suggest that, although ratification of the laws of war does not have an effect on non-democracies, it does change the behavior of democratic states. Moreover, Morrow's results suggest that this relationship is especially strong in cases where both states have ratified the laws of war. Morrow theorizes that this result is because mutual ratification of legally binding treaties signals a willingness to enforce agreements through reciprocity.

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[23] Downes 2008; Downes 2007.

[24] See Morrow 2007.

The results produced by Morrow are consistent with a growing body of research that has focused on how domestic politics can drive compliance with international law.[25] Perhaps the most prominent example of this literature is Beth Simmons' recent book on compliance with international human rights treaties.[26] Simmons argues that, even when there are not external enforcement mechanisms, prior ratification of formal international legal agreements become powerful rhetorical and political tools for domestic advocacy groups. According to Simmons, these domestic groups are able to take advantage of the fact of ratification to pressure governments to protect human rights even in cases where the government otherwise would not be inclined to do so. Simmons tests her theory on compliance with six different human rights agreements. After utilizing instrumental variable regression to help deal with the fact that ratification is endogenous to other factors that could lead to compliance, Simmons' results show that ratification is a significant indicator of compliance with human rights regimes for democratic—and partially democratic—states. Although Simmons' research focuses solely on compliance with human rights treaties, it would be reasonable to think that domestic politics could also drive compliance with the laws of war in states that are partially democratic—just as Morrow's research suggests.

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[25] See, e.g., Dai 2007; Dai 2005.

[26] See Simmons 2009.

2.2.2 Additional Protocol II and the Advantages of Studying Civil Wars

Despite the seeming impasse created by the previous efforts to research whether the laws of war help to protect civilians, one previously unexplored way to gain new insight into this question is to examine civil wars. Although the customary international law governing conduct during war has developed over centuries,[27] it was not until the mid-nineteenth century that efforts to codify the laws of war began to gain traction.[28] During these initial efforts, however, states were apprehensive about including regulation of internal armed conflict.[29] Given these apprehensions, intrastate wars were not regulated by a multinational treaty until the Geneva Conventions of 1949.[30] Despite the importance of this development, only one part of the treaty—common Article 3—addressed the humanitarian obligations of countries that were engaged in internal armed conflicts.[31] Moreover, not only was this the only part of the Geneva Conventions of 1949 that addressed intrastate wars, but also the provisions contained in common Article 3 were still extremely limited.[32]

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[27] For a discussion of sources and views on the sources of law governing internal conflicts before the eighteenth century, see Perna 2006, 1-27.

[28] See Myuot and del Rosario 1994, 7; see also Roberts and Guelff 2000, 4 (noting it was "the second half of the nineteenth century when the laws of war began to be codified in multilateral treaties").

[29] See Moir 2002, 21 ("States were strongly opposed to any compulsory international regulation of internal armed conflict.").

[30] Id. at 19 ("Before the mid-twentieth century, however, no international agreement applied to anything other than purely international conflicts . . . , and although customary law provided that the rules of international armed conflict could apply to internal conflicts through the recognition of belligerency, that doctrine was rapidly becoming obsolete.").

The shortcomings of common Article 3 became increasingly clear as intrastate wars constituted a larger percentage of conflicts than interstate wars in the later half of the twentieth century.[33] In fact, research suggests that perhaps eighty percent of the victims of armed conflicts during this period were killed during intrastate wars, and that the majority of those killed were civilians.[34] As it became clear that common Article 3 alone provided insufficient restrictions on intrastate wars,[35] the International Committee of the Red Cross began to study the possibility of improving this area of international law in the 1960s and 1970s.[36] It was against this backdrop that Additional Protocol II to the Geneva Conventions was negotiated.[37]

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[31] Id. at 30 (common Article 3 was "the first legal regulation of internal armed conflict to be contained in an international instrument").

[32] See Roberts and Guelff 2000, 481 (noting common Article 3 "binds parties to observe a limited number of fundamental humanitarian principles in 'armed conflicts not of an international character'").

[33] See Perna 2006, 99; see also Moir 2002, 1 (noting that " [s]ince 1945, the vast majority of armed conflicts have been internal rather than international in character").

[34] See id.; see also Fearon and Laitin 2003, 75 ("A conservative estimate of the total dead as a direct result of [civil wars between 1945 and 1999] is 16.2 million, five times the interstate toll.").

[35] See Perna 2006, 99.

[36] See Junod 1983, 31.

[37] See generally id. at 30.

After a series of consultations and conferences in the 1970s,[38] Additional Protocol II was opened for signature in 1977; it then went into effect in December of 1978.[39] The three aims of AP II were to improve and clarify the laws governing intrastate armed conflicts by (1) filling in existing gaps in the classes of individuals protected by humanitarian law during internal conflicts; (2) developing clear criteria for when internal conflicts would be governed by international law; and (3) clearly establishing that common Article 3 would retain its own separate existence.[40] To do so, the treaty both set forth criteria for which conflicts should fall under the ambit of AP II,[41] and also extended clear protections to civilians, detainees, and medical personnel.[42]

Even though AP II represents the most significant effort to provide protections to civilians and combatants during intrastate wars, there have been concerns that the treaty is not having its intended effect. Firstly, in many of the internal armed conflicts since 1977, the parties to the conflict have not consented to be bound by AP II.[43] Additionally, states that have consented to be bound by AP II have often argued that the treaty does not apply during internal hostilities that have erupted.[44] Moreover, there has been concern that even if states were to commit themselves by ratifying AP II, it is not clear that this ratification would effectively change the behavior of those states during conflict.[45] In fact, the shortcomings of AP II have even led one prominent commentator on the topic to lament that "[i]t is as if the efforts of so many since the end of the Second World War to ease the suffering during hostilities, and indeed to prevent such conflicts from arising, have counted for nothing."[46]

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[38] For a discussion of the history of AP II, see Perna 2006, 99 – 107; see also Junod 1983, 30- 34.

[39] See Roberts and Guelff 2000, 483.

[40] See Moir 2002, 91 (noting these are "widely regarded" as the three aims of AP II).

[41] Protocol II, Part I.

[42] Protocol II, Part II-IV; see also Moir 2002, 274.

[43] See Moir 2002, 120 (citing Angola, Namibia, Mozambique, Somalia, Afghanistan, Sri Lanka, Haiti, and Nicaragua as examples of countries that have been engaged in internal wars without previously ratifying AP II).

Although it is undeniably true that AP II has not ended all suffering during civil wars, it is impossible to know without rigorous analysis whether states that have ratified the treaty were less likely to engage in the massing killing of civilians. Moreover, there are strong reasons to attempt that analysis. First, civil wars have made up an increasingly large share of all conflicts since 1945, and thus it is particularly important to know whether AP II has helped to reduce violence. Second, since there have been more intrastate wars than interstate wars in the later half of the twentieth century and there has been greater variation in the ratification of AP II than the Geneva Conventions more broadly, examining civil wars allows for superior causal inference. Third, examining compliance with AP II provides an important new, and difficult, test of whether international law can help to change the behavior of states.

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[44] Cf. id. at 274 n.220 ("El Salvador and the Philippines are still the only examples where both government and insurgent parties to a non-international armed conflict have accepted its application.").

[45] Id. at 120 (arguing that "[e]xamples are numerous of violations even of its more basic provisions").

[46] Id. at 131.

2.2.3 Theoretical Expectations

Despite the conventional wisdom held by international relations scholars that states do not pay attention to the laws of war during conflict,[47] the conflicting research in the interstate war context indicates that it is still an open question whether countries that have ratified AP II are less likely to intentionally kill civilians during conflicts. Developing theoretical predictions about the potential impact of the ratification of AP II thus requires looking more broadly to literature on treaty ratification and compliance. Although there is still considerable debate among scholars of international relations and international law on the impact of ratifying treaties without international enforcement mechanisms, it is possible to make two predictions. First, although countries ratify international agreements for a wide range of reasons,[48] it would be reasonable to assume that countries that voluntarily choose to commit themselves to adhere to international standards are more likely to comply than those that do not. After all, previous explanations for high rates of compliance with international law center on the fact that countries selectively enter into international agreements that they find agreeable.[49] As a consequence, it is reasonable to hypothesize that countries that ratify AP II are less likely to target civilians during war.

Of course, just because a country complies with the treaties that it ratifies does not mean that this action has changed the country's behavior.[50] Therefore, whether ratification of AP II alters the behavior of countries is an important question. Recent empirical research suggests that, in the context of human right treaties, ratification is more likely to impact the behavior of certain types of countries. Specifically, Beth Simmons has provided a variety of evidence to suggest that countries that are partial or transitioning democracies—as opposed to stable autocracies or stable democracies—are the most likely to have their behavior altered by the ratification of international treaties.[51] This is because stable democracies are likely to already comply with the human rights norms espoused in international treaties, and stable autocracies are unlikely to be swayed by the domestic political mechanisms that could push a country to comply with its treaty obligations.[52] Countries with a mixed history of democracy, on the other hand, are the most likely to alter their behavior as a result of ratification because there are domestic political actors in these countries that can use the fact that their country has ratified an agreement as a powerful rhetorical tool to help sway the policies of their governments.[53]

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[47] See Armstrong et al. 2012, 147.

[48] See generally Hathaway 2007.

[49] See, e.g., Downs et al. 1996.

[50] See Hathaway 2002, 1939.

It is an open question, however, whether partial democracies would be less likely to target civilians during as a consequence of ratifying treaties on the laws of war. As previously noted, there is body of evidence showing that democracies are just as likely to kill civilians during interstate conflicts as non-democracies.[54] Moreover, there has also been some research suggesting that there is an inverted U shape relationship between political regimes and violent suppressions—that is, stable autocracies and stable democracies are less likely to violently suppress their own citizens than countries "in the middle."[55] That said, although partially democratic states may be drawn to violence generally, research has consistently shown that partially democratic states are less likely than autocracies to engage in mass killings of civilians during intrastate conflicts.[56] The argument is that partially democratic states have to be responsive to their civilian populations, and that if they were to begin targeting civilians (even those aligned with the opposition) that they would pay a political consequence because other civilians would no cease to support the regime.[57]

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[51] Simmons 2009, 153 ("Where we are likely to see the most significant treaty effects—at least with respect to civil and political rights—is in the less stable, transitioning [countries].").

[52] See id. at 152-53.

[53] The idea that domestic politics could drive compliance with international law is not unique to the human rights context. For example, Dai (2007, 2005) has argued that this occurs with environmental regulations, and Pelc (2012) has argued that domestic politics are a large driver of compliance with the WTO.

[54] Downes 2008; Downes 2007.

It thus stands to reason that if ratification of AP II were to make a difference in the behavior of any countries, it would be partially democratic states were having previously ratified the agreement might increase the political costs of targeting civilians. As a result, the second prediction of this paper is that the ratification of AP II will have the largest effect on countries with mixed experience with democracy.

2.3 Data

Because this is the first attempt to empirically examine whether the ratification of treaties on the laws of war helps to protect civilians during intrastate conflicts, the most significant aspect of this research has been creating a dataset that allows this question to be operationalized and studied. Doing so has thus required a number of important substantive and practical decisions about which data sources provide the best route to empirically examining the role that AP II has played in the conduct of civil wars. This Part describes the dataset that has been compiled to study this important topic and proceeds in three sections. First, I outline the universe of civil wars that were included in the dataset. Second, I explain the dependent variable that was collected to determine if states are complying with the laws of war. Third, I discuss the explanatory and control variables that have been collected to control for alternative explanations of compliance.

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[55] See Fein 1995. For a list of articles finding similar results, see Vreeland 2008, 401.

[56] See Eck and Hultmann 2007; Valeninto, Huth, and Balch-Lindsa y 2004; Harf 2003.

[57] Hultman 2012.

2.3.1 Universe of Cases

Although it may seem like a straightforward task to compile a list of civil wars that have occurred since civil wars became meaningfully regulated when AP II opened for signature in 1977, there is considerable disagreement on this topic. Recent studies on civil wars use a range of definitions to define civil conflicts, and thus have produced very divergent lists of civil wars.[58] It is perhaps unsurprising then that there are several prominent civil war datasets that have been used by empirical researchers studying the causes, conduct, and consequences of civil war in the last decade.[59] As a result, deciding which civil war dataset to use as the basis for this study is an important part of producing a convincing examination of whether states comply with the laws of war.

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[58] For an excellent discussion of the difficulties of defining and empirically studying civil wars, see Sambanis 2004. Sambanis specifically focuses on the coding decisions made by the Correlates of War project as a way to discuss the definitional complexity of defining and coding civil wars.

[59] See, e.g., Sarkees and Wayman 2010 (updating the popular Correlates of War intrastate dataset); Lyall 2010 (introducing what Lyall has referred to as the Correlates of Insurgencies dataset); Sambanis 2004 (outlining a dataset of conflicts from 1945-1999); Fearon and Laitin 2003 (providing a seminal dataset on civil war onset from 1945 to 1999).

Ultimately, I elected to base my empirical analysis on the Armed Conflict Dataset that is compiled by the Uppsala Conflict Data Program (UCDP) and the Peace Research Institute (PRIO).[60] This dataset—commonly referred to as the UCDP/PRIO dataset—codes all instances of armed conflict that have occurred between 1946 and 2010.[61] Using the UCDP/PRIO dataset has three distinct advantages. First, the dataset is updated annually, which makes it possible to include conflicts that occurred as late as 2010 in my sample. Second, the criteria used to define armed conflicts used by this dataset are compatible with the datasets that are used to provide other key variables for the study.[62] Third, the dataset has emerged as the most widely used dataset on civil wars in the last few years, and thus is the most defensible to other researchers in the field.[63] For these reasons, I elected to use this data as the basis for the universe of civil wars included in the study.

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[60] This data is from the UCDP/PRIO Armed Conflict Dataset, 1946-2010, version 4-2011. See Themnér and Wallensteen 2011. For a description of the original dataset, see Gleditsch et al. 2002.

[61] For an armed conflict to be included in this dataset it must meet four criteria. See Themnér and Wallensteen 2011, 532. Specifically, a conflict must be "[1] a contested incompatibility that concerns government or territory or both where [2] the use of armed forces between two parties [3] results in at least 25 battle-related deaths in a year. [4] Of these two parties, at least one has to be the government of a state." Id. Version 4-2011 of this dataset provides information on 2022 conflict-years from 1946 to 2010 that meet this definition.

[62] See infra text accompanying notes 68 & 75.

[63] A partial list of the wide range of publications using this dataset is available at <http://www.prio.no/CSCW/Datasets/Replication-Data-List/> (last visited February 13, 2012).

After selecting the UCDP/PRIO dataset, I further subset the data in four ways. First, I restricted the observations to conflicts that were categorized “internal armed conflicts” that occurred between the government of a country and at least one opposition group.[64] The critical distinction here is that “internationalized” internal conflicts, where there is intervention from other states on one side, are excluded. I did this because the presence of other countries in the conflict might make it “international” in character, complicating whether AP II would apply. Second, conflicts were further limited to those that occurred between 1989 and 2010. This is both because I wanted to limit the study to the time after countries could have at least theoretically signed AP II, and also because of data limitations on the dependent variable make studying conflicts between 1977 and 1989 impossible.[65] Because recent scholarship has argued that the Cold War had a significant impact on the conduct of civil wars by changing the technology and resources available to insurgents,[66] only examining conflicts that occurred between 1989 and 2010 has the added advantage of helping to control for that possibility. Third, conflicts were excluded if a rebel group did not control territory during the conflict. This is because the scope of AP II is limited to cases where the “dissident armed forces” exercise “control over a part of [the country’s] territory.”[67] To limit the dataset in this way, I used data on non-state actors compiled by David Cunningham, Kristian Gleditsch, & Idean Salehyan that codes whether an armed group controlled territory during the conflict.[68] All countries where at least one of the rebel groups fighting the state did not control territory were thus excluded from the dataset. Fourth, only one observation was included for each country per year. In other words, if a country fought two armed insurrections in the same year, this was collapsed into a single observation. This decision was made because information for the dependent variable was not broken down by multiple conflicts for each country in a given year.

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[64] An “internal armed conflict” is defined as a conflict that “occurs between the government of a state and one or more internal opposition group(s) without intervention form other states.” See UCDP/PRIO ARMED CONFLICT DATASET CODEBOOK 9 (2011), *available at* <http://www.pcr.uu.se/research/ucdp/datasets/ucdp_prio_armed_conflict_dataset/> (last visited April 1, 2012).

[65] See infra the text accompanying note 75.

[66] Kalyvas and Balcells 2010.

[67] Protocol II, art. 1. For a brief discussion of this requirement, see Junod 1983, 37.

After these four restrictions were put in place, an observation was included in the dataset for each year, or part thereof, that a country was engaged in an intrastate war that had at least 25 battle-related deaths.[69] The result is that the dataset includes observations from 38 different countries. Moreover, there are 279 conflict-years between 1989 and 2010 between these 38 countries. Table 2.1 includes a list of the countries included in the dataset with the number conflict years in parentheses. The countries are divided by whether the country had previously ratified AP II.[70] It is worth noting that 6 of the 38 countries had conflict-years before and after ratifying AP II, and thus are included in both columns of Table 2.1. This fact will be leveraged in the analysis presented in Part 2.4.2.

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[68] This data is from the Non-State Actor Data, version 3.3. See Cunningham, et al. 2009. It is worth noting that one limitation of this dataset is that it only has one observation for each conflict, which means that it only codes whether a rebel group controlled territory "during the conflict" but not for an individual year.

[69] See supra text accompanying note 61. The Comoros also had one year of conflict in 1997 that met the previous criteria, but was excluded from the dataset due to inability to obtain data on the independent variables for the country.

[70] See infra text accompanying note 80.

Table 2.1: Countries with Civil Wars Included in the Dataset (N = 279)

Ratified AP II Prior (N = 110)	AP II Not Ratified (N = 169)
Bosnia-Herzegovina ('93-'95) (3)	Afghanistan ('90-'91, '93-'00) (10)
Colombia ('96-'10) (15)*	Angola ('90-'95) (6)
Cote d'Ivoire ('02-'04) (3)	Azerbaijan ('94) (1)
Croatia ('95) (1)	Colombia ('89-'95) (7)*
Dem. Repub. of Congo ('06-'08) (3)	Djibouti ('91) (1) *
Djibouti ('92-'94) (3)*	Ethiopia ('89-'92, '94) (5)*
El Salvador ('89-'91) (3)	Georgia ('92-'93) (2)*
Ethiopia ('95, '98-'09) (13)*	Haiti ('04) (1)
Georgia ('04) (1)*	India ('90-'91, '94-'10) (20)
Laos ('89-'89) (2)	Indonesia ('90-'91, '99-'05) (9)
Liberia ('89-'90, '03) (3)	Iraq ('89-'92, '95-'96) (6)
Niger ('91-'92, '97, '07-'08) (5)	Israel ('00-'07) (8)
Philippines ('89-'95, '97, '99-'10) (20)	Mexico ('94) (1)
Russia ('90-'91, '94-'96, '99-'07) (14)	Moldova ('92) (1)
Senegal ('90, '92-'93, '95, '97-'98, '00-'01, '03) (9)	Morocco ('89) (1)
	Mozambique ('91-'92) (2)
Sierra Leone ('91-'96) (6)	Myanmar ('89-'10) (21)
Sudan ('07-'10) (4)*	Nepal ('96-'06) (11)
Uganda ('92) (1)*	Pakistan ('07) (1)
Yemen ('94) (1)	Papua New Guinea ('89-'90, '92-'96) (7)
	Serbia ('91, '98) (2)
	Somalia ('89-'96) (8)
	Sri Lanka ('89-'01, '03, '05-'09) (19)
	Sudan ('89-'06) (17)*
	Uganda ('89-'91) (3)*

Note: The years of conflict and number of conflict-years for each country included in the dataset are in parentheses. Countries marked * appear on both lists because they had years of conflict before and after ratification.

2.3.2 Dependent Variables

After establishing the universe of cases for the study, the next important task was determining the optimal dependent variable. Since the goal of this project is to determine whether countries are compliant with international law, the dependent variable must measure whether a country violated Additional Protocol II. Although AP II regulates a range of conduct during intrastate wars, the rationale for creating the protocol was to provide "protection to victims of domestic armed conflicts."[71] It is thus reasonable to argue that the most important prohibitions in AP II are those that provide for the "[p]rotection of the civilian population" provided for in Article 13.[72] Article 13 specifically provides that the "civilian population as such, as well as individual civilians, shall not be the object of attack."[73] Given this clear language, one kind of conduct that is clearly prohibited by AP II is the intentional targeting and killing of civilian populations.

Despite the importance of protecting civilians during armed conflicts, the data on civilian deaths that has been available have had significant limitations until recently.[74] This would have seriously hampered any earlier efforts to rigorously test whether countries that signed AP II were less likely to kill civilians during civil wars. Fortunately, there is a new dataset by the Uppsala Conflict Data Program (UCDP) on "one-sided violence" that measures whether either a state or non-state actor intentionally attacked civilians in any year between 1989 and 2010.[75] The criterion used by this dataset is that a country is deemed to have committed an act of one-sided violence in a year if "the use of armed force by the government of a state . . . results in at least 25 deaths per year."[76] Moreover, this data is excellent for studying whether a state complied with the requirements of AP II because the dataset was limited to "only those fatalities that are caused by the intentional and direct use of violence."[77] This data was compiled by combing news reports from five major international news bureaus from 1989 to 2010 with case specific data sources and analysis.[78]

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[71] Muyot and Rosario 1994, 16.

[72] Protocol II, art. 13.

[73] Id.

[74] See Eck and Hultman 2007, 234. Eck and Hultman argue that there have previously been only limited academic efforts to collet data on violence against civilians. These efforts have had serious limitations because they have been "limited to genocide or mass killings, interstate wars, or rely only on a proxy for violence." *Id.* (citations omitted).

[75] This data is from the UCDP One-Sided Violence Dataset, 1989-2010, version 1.3-2011. See Sundberg 2009. For information on the original dataset, see Eck and Hultman 2007.

Table 2.2: Instances of One-Sided Violence

	Ratified AP II Prior	AP II Not Ratified
Instances of Violence	26	76
No Instances	84	93
Compliance Rate	**24%**	**45%**

For each year that a country was included in the dataset, the dependent variable was coded as either a 0 or 1 for whether an act of one-sided violence was committed by the state in that year. Table 2.2 breaks down the instances of one-sided violence by whether the country had ratified AP II prior to the year of conflict. As the table shows, without controlling for any other variables, the rates of instances of one-sided violence for states that had previously ratified AP II were roughly half that of those states that had not previously ratified AP II. Specifically, countries that had ratified AP II prior to the year of conflict committed acts of one-sided violence in 24% of conflict years, whereas countries that that not previously ratified AP II committed acts of one-sided violence in 45% of conflict years.

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[76] See Eck and Hultman 2007, 235.

[77] Id.

[78] For a detailed account of how the data was collected and coded for this dataset, see Eck and Hultman 2007, 236-37.

2.3.3 Independent Variables

The final step in building the dataset to measure compliance with the laws of war during intrastate conflicts was deciding which independent variables to include. The most important independent variable is of course whether or not a country has ratified AP II. For this variable, I simply coded whether a country had ratified AP II in the year prior to the year they were in conflict.[79] This data is available from the International Committee of the Red Cross.[80] It is worth noting that countries are able to file reservations or declarations at the time of ratification, but the only country included in the dataset that chose to do so was the Russian Federation, which simply filed a declaration highlighting the county's role in creating the treaty.[81]

After coding this important variable, the more difficult decision was determining what other controls to include in the dataset to capture other factors that might influence a country's behavior during civil war. Given the varied and growing literature of civil wars, there is unsurprisingly a wide range of variables that scholars have included in their models to predict the onset, duration, and conduct of civil wars. That said, despite this variance there is at least growing consensus on several variables that are of particular importance to control for during civil wars.[82]

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[79] For example, if a country ratified AP II in July of 1994, they were coded as having previously ratified AP II starting in 1995.

[80] For the International Committee of the Red Cross' list of state parties to AP II, *see* <http://www.icrc.org/ihl.nsf/WebSign?ReadForm&id=475&ps=P> (last visited April 1, 2012).

[81] Id.

[82] See generally Hegre and Sambanis 2006; see also Cunningham and Lemke 2011 (drawing on the work of Herge and Sambanis to argue for which variables to include in their study of sub-

Using this research for guidance, I have thus decided to use six additional control variables, which have been the most widely used and theoretically supported by scholars examining civil wars. First, the most important control variable to include is a measure of the country's regime type. For this, I use the country's Polity Score—a measure of whether a country is autocratic or democratic on a scale of -10 to 10. This variable is based on the "polity2" variable from the Polity IV project.[83] Following Cunningham & Lemke, a squared version of each country's polity score is also included in several of the models estimated.[84] Second, I include a measure of the country's population since there is strong evidence that it influences civil war onset and conduct.[85] The log of this variable is used, and the data is from the United Nation's Statistics Division.[86] Third, since the wealth of a country influences intrastate war, each country's Gross Domestic Product Per Capita is included.[87] This data is also logged and also from the United Nations Statistics Division.[88] Fourth, although there has been

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state violence). This paper's approach for deciding which variables to include follows the one outlined by Cunningham and Lemke.

[83] See Marshall and Jaggers 2011, *available at* <http://www.systemicpeace.org/inscr/inscr.htm> (last visited April 1, 2012).

[84] But see Vreeland 2008 (arguing that the decision on how to code of regimes during violent conflicts impacts statistical results).

[85] See, e.g., Herge and Sambanis 2006.

[86] *United Nations Statistics Division, National Accounts Main Aggregates Database: Exchange Rates and Population* (2011), *available at* <http://unstats.un.org/unsd/snaama/dnlList.asp> (last visited April 1, 2012).

[87] See, e.g., Thyne 2009.

[88] *United Nations Statistics Division, National Accounts Main Aggregates Database: Per Capita GDP at Current Prices in US Dollars* (2011), *available at* <http://unstats.un.org/unsd/snaama/dnlList.asp> (last visited April 1, 2012).

considerable debate on whether ethnic resentment has an influence on civil war, the persistence of the debate leads ethnic fractionalization to be consistently included in models of civil war conduct.[89] Following this trend, I have included the ethnic fractionalization variable, as well as its squared term, from Fearon & Laitin's dataset.[90] Fifth, there is also evidence that countries with rough terrain have longer and more violent civil wars.[91] To control for the impact of rough terrain, I have included the log of Fearon & Latin's measure of how mountainous each country is on a 1-5 scale.[92] Finally, a variable for whether a country has oil is included because it has been theorized that the presence of this national resource influences civil war conduct and duration.[93]

The mean values for these independent variables, broken down by whether the country had ratified AP II, are reported in Table 2.3. As the table shows, the variables are fairly balanced, with the exception of the two Polity measures. Perhaps unsurprisingly, countries that have ratified AP II have a higher polity score than countries that have not. Additionally, countries that have ratified AP II have lower Polity^2 values, which indicates that countries that have ratified AP II have less extreme values for this variable. Otherwise, the values for the other independent variables are remarkably similar across both sets of countries.

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[89] Compare Lars-Erik Cederman and Girardin 2007, with Fearon, et al. 2007.

[90] Fearon and Laitin 2003.

[91] Cf. Kalyvas and Balcells 2010.

[92] Fearon and Laitin 2003.

[93] For a discussion of research on this topic, see Adam Glynn 2011.

Table 2.3: Descriptive Statistics (mean values)

	Ratified AP II Prior	AP II Not Ratified
Polity	2.8	0.2
Polity^2	32.8	45.0
Population (ln)	17.1	17.3
GDPPC (ln)	6.7	6.3
Ethnic Frac.	0.6	0.6
Ethnic Frac.^{2}	0.4	0.4
Rough Terrain	2.5	2.8
OIL	0.1	0.1

2.4 Results

After compiling this dataset, I performed a number of statistical tests to try and estimate whether ratification of AP II predicts a lower rate of one-sided violence during civil wars. First, I performed a series of logistic regressions on the complete country year dataset to estimate the effect of ratification of AP II. Under this approach, all 279 years of conflict are used as observations. Second, I performed a series of logistic regressions on the countries that had civil war before and after ratifying AP II. Focusing specifically on six countries that ratified AP II between periods of violence in this way helps present clearer evidence on whether ratification of the treaty might alter behavior. Third, to try and move from predictive analysis to causal analysis, I performed a series of instrumental variable regressions on the complete dataset. For this analysis, three variables were used as instruments to estimate the likelihood that a country would ratify AP II, which then in a second stage of the regression was used to predict the impact of ratification on one-sided violence. The advantage of using this approach is that it endogenizes treaty ratification, which thus increases the chances that reductions in violence predicted by AP II are caused by the influence of the treaty and not the conditions that gave led to ratification.

Fourth, I performed a series of robustness checks to help lend credibility to the results produced using the previous three empirical approaches.

2.4.1 Country Year Approach

The first approach that I used to estimate the impact of AP II ratification on whether civilians were targeted during civil wars was to perform a series of logit regressions on the complete country year dataset. For this, each year a country in the dataset was in conflict is an observation; which resulted in 279 observations from 38 countries. The logit regressions then simply measure whether a government committed at least one act of one-sided violence that killed more than 25 civilians during that conflict year.

Figure 2.1 presents the results of four different models that were estimated using this approach.[94] These results are presented graphically.[95] The results shown are the simulated first difference as each variable moves from its minimum to maximum value. For each model, each line represents the point estimate and confidence interval for an individual variable included within the regression.[96] Point estimates to the right of zero means that the variable is associated with a lower probability of one-sided violence. Statistically significant variables are presented with solid lines, and variables that did not achieve significance at the 0.05 level are presented as dotted lines.

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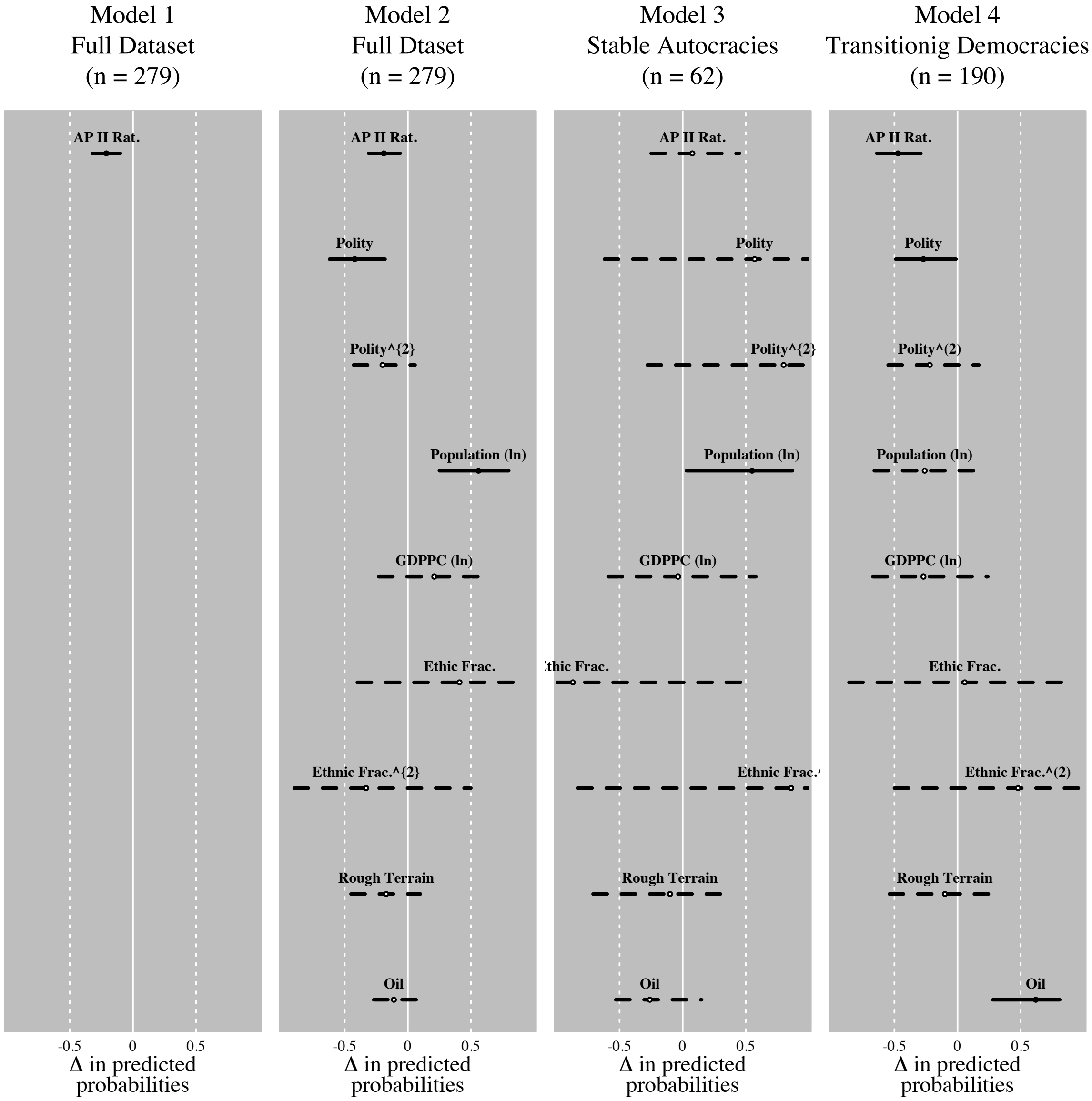
[94] All statistical tests presented in this paper were conducted using "Zelig" for R. See Imai, King, and Lau 2008.

[95] The regressions ran in Figures 2.1, 2.2, and 2.3 are presented in conventional tables in Appendix 1.1, 1.2, and 1.2 respectively.

[96] For explanations of the value of presenting regression results as graphs instead of tables, see Kastellec and Leoni 2007; King, Tomz, and Wittenberg 2000.

Figure 2.1: Logit Regression on Likelihood of One-Sided Violence



In Figure 2.1, the first model only estimates the impact of ratification of AP II on the likelihood of one-sided violence in a given year. The second model then incorporates all of the variables discussed in Part 2.3.3. The third model only looks at countries that were stable autocracies between WWII and 2010, and the fourth model only looks at countries that had a least some experience with democracy during the same period.[97]

The results demonstrate a statistically significant relationship between the ratification of AP II and the occurrence of one-sided violence against civilians. In three of the four models estimated, countries that ratified AP II were less likely to intentionally kill civilians during intrastate conflicts. In fact, the only model estimated that did not find this relationship was for countries that were stable autocracies during the entire post-war period. That said, the ratification of AP II had the greatest effect for countries that were democracies for only part of this period.

These results thus make two important contributions to our understanding of compliance with international law. First, countries that ratified AP II were less likely to intentionally kill civilians. This was true even controlling for a number of critical variables that explain conduct during civil wars. These results do not demonstrate that ratification changes behavior, but continue to lend support to the view that countries ratify treaties when they are willing to comply with the terms.**[98]** Second, these results also lend support to previous research that has shown that stable autocracies may not be likely to comply with their treaty commitments, while countries that have some experience with democracy are more likely to comply with the international legal commitments they make to respect human rights. Although this theory has been prominently developed by Beth Simmons in the context of international human rights treaties, it has not yet been extended to treaties that govern armed conflict.

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[97] Both this approach and the definitions used to categorize countries as either "Stable Autocracies" or "Transitioning Democracies" are taken from Beth Simmons. See Simmons 2009, 385. Stable Autocracies are countries that never scored above a 5 on the Polity IV Democracy variable between 1945 and 2010. Transitioning Democracies are countries that have scored above a 5 during the same period, but were not Stable Democracies that remained above 8 for the entire period. For a list of countries in each category, see Appendix 1.4.

[98] For an excellent discussion of why countries ratify human rights treaties, see Simmons 2009, 57-111. Simmons argues that "[it] makes sense . . . to assume that treaty commitments are not completely disingenuous: *Most* governments ratify treaties because they support them and anticipate that they will be able and willing to comply with them under most circumstances." Id. at 65.

2.4.2 Restricted Dataset

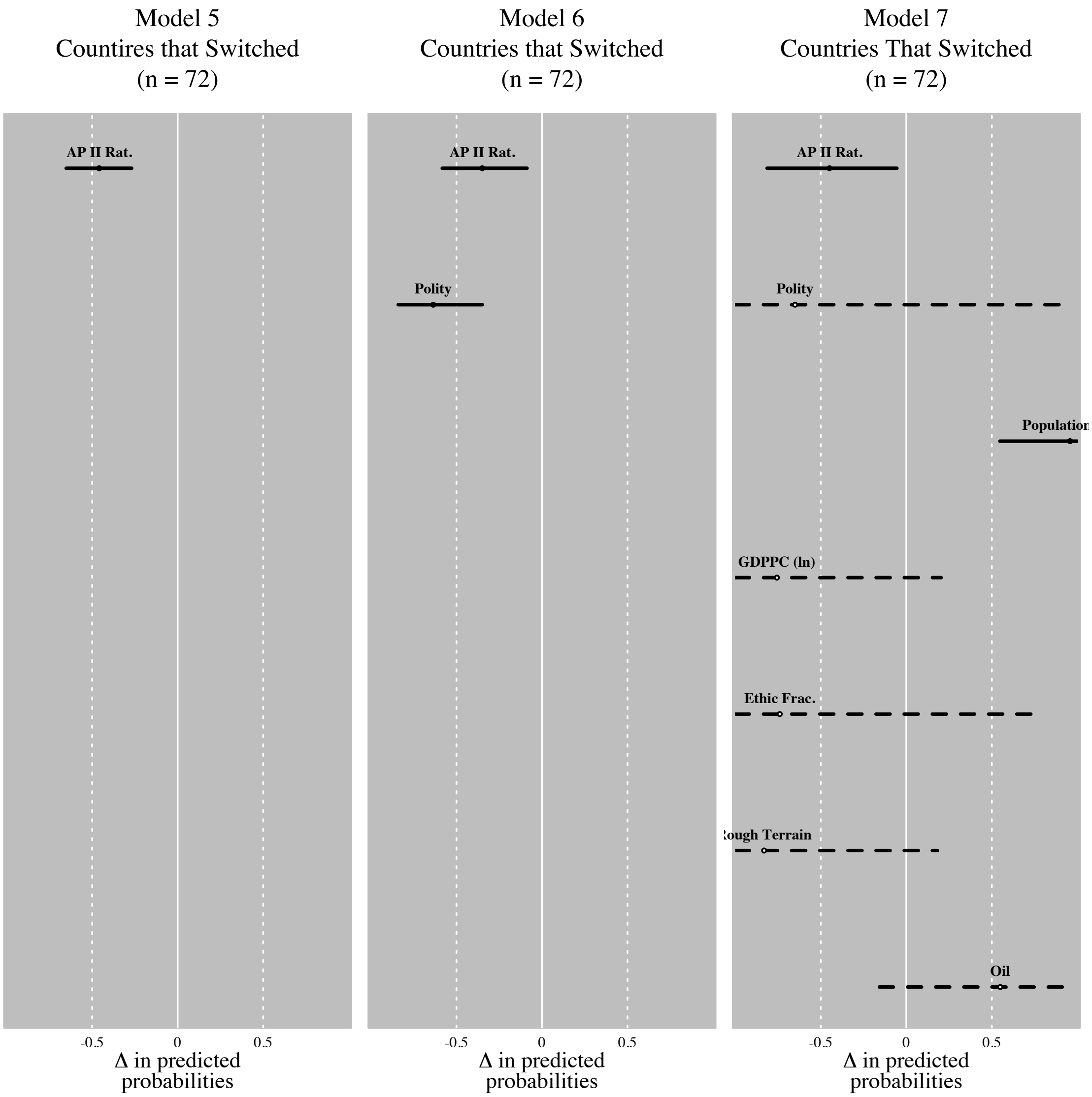
In addition to using the complete dataset to estimate the impact of ratification of AP II on the likelihood that governments will refrain from intentionally killing civilians, I have also performed a number of tests on a restricted version of the dataset. Specifically, I restricted the observations to those from countries that experienced conflict years in the dataset before and after the ratification of AP II. In six countries—Columbia, Djibouti, Ethiopia, Georgia, Sudan, and Uganda—there was a conflict that merited inclusion in the dataset, the government then ratified AP II, and post-ratification that conflict or a new one raged in the country. The observations included in this analysis are presented in Table 2.4.

Table 2.4: Countries with Conflict Before and After AP II Ratification

	Conflict Years Pre-Rat.	AP II Ratification Date	Conflict Years Post-Rat.
Colombia	89-95	8/14/95	96-10
Djibouti	91	4/8/91	92-94
Ethiopia	89-92, 94	2/8/94	95, 98-09
Georgia	92-93	9/14/93	04
Sudan	89-02, 04-06	7/13/06	07-10
Uganda	89-91	3/12/91	92

Looking at these observations alone provides a test of whether a country that ratified AP II may have changed its behavior after doing so. It is important to note that at this point I am not claiming that ratification of AP II itself changed the government's behavior, but simply that looking at these observations can give insight into whether ratification was associated with a change in behavior. It is of course possible that some other change, like a new government, both resulted in the ratification of AP II and the change in behavior without the treaty itself exerting any causal influence.

Figure 2.2: Logit Regression on Likelihood of One-Sided Violence



Using a similar approach to the previous section, I estimated a series of logit regressions on this restricted dataset. Figure 2.2 reports the results of three of these regressions graphically. The first model only includes the AP II variable. The second model also includes the country's polity score. The third model is further expanded to include all of the independent variables, with the exception of the squared terms for polity and ethnic fractionalization, discussed in 2.3.3. Unfortunately, given the small sample size, it was not possible to estimate any models including the squared terms for polity and ethnic fractionalization, or to subset the sample by stable autocracies and transitioning democracies like in Part 2.4.1.

As the results in Figure 2.2 show, once again ratifying AP II resulted in countries being less likely to commit acts of one-sided violence. In other words, countries that had years of conflict before and after ratifying AP II were less likely to intentionally kill civilians in the years after ratifying AP II. This was true in all three models estimated after controlling for other possible explanations for violence. This suggests that countries were more likely to respect the rights of civilians after ratifying AP II. Although this does not prove that ratification caused the change in behavior, the evidence it presents is consistent with the theory that ratification of AP II should alter the behavior of states by raising the costs of committing attacks against civilians. For example, the Colombian government committed acts of one-sided violence in two of the seven years of civil war prior to ratifying AP II, but not in a single one of the fifteen years of civil war after ratifying AP II. This is thus significant because, by showing that countries committed more acts of one-sided violence before ratifying AP II than after, it is at least possible that the expansion of the treaty regime has helped to protect civilians during civil wars.

2.4.3 Instrumental Variable Regression

As a third empirical approach, I performed a number of instrumental variable (IV) regressions.[99] One problem that has consistently plagued research on international law is that it is difficult to know whether observed compliance with a treaty is due to the treaty itself having a causal impact on behavior, or whether the observed compliance is simply due to the fact that countries negotiate and then ratify treaties with terms they are likely to agree with.[100] In her recent book on compliance with international law, Beth Simmons used an IV regression approach to try and estimate the impact of ratification on compliance with a number of international human rights treaties.[101] Simmons' approach was to use a two-stage regression model that used three variables as instruments for whether a country ratified a particular treaty.[102] These variables were whether the country had a British common law legal tradition, the country's ratification procedures, and the density of ratification in the country's region.[103] The advantage of using these three variables as instruments is that they are significantly associated with the ratification of human rights treaties, but there is not a theoretical reason to believe that the variables themselves directly impact a country's human rights practices. Using this approach thus endogenizes treaty commitment, which increases the probability that the impact of ratification on instances of violence can be explained by the commitment itself, and not by the conditions that gave rise to ratification.[104] In other words, it moves closer towards a causal analysis of whether ratification impacted a country's behavior during civil wars.

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[99] It is worth noting that Morrow (2007) uses instrumental variables in his study of compliance with the laws of war during civil wars. That said, Morrow develops an instrument for the opposition side's compliance so that he can measure the influence of reciprocity. He does not, however, take any steps to address the selection issues caused ratification of the treaty.

[100] See Simmons 2010, 275 ("Almost all studies of the influence of treaties on state behavior encounter serious issues of endogeneity and selection, both with respect to the provisions of the treaty and with respect to ratification. Because treaties exist and are written for specific purposes, it is hard to know how much causal weight to attribute to the treaty versus the underlying purpose.").

[101] Simmons 2009.

[102] Id. at 172.

[103] Id.

Following Simmons' lead, I have used the same three variables as instruments for the ratification of AP II. These variables are also good instruments to use in this case because they are statistically significant predictors of the ratification of AP II, but they are not theorized as causes of violence towards civilians. The variable for common law was coded based on data from the Global Development Network Growth Database.[105] Each country was coded as 1 if its legal system developed from the British legal tradition. The variable for ratification procedure is taken from a four-category scale developed by Simmons.[106] On this scale, countries are given the lowest value if their executive or cabinet is able to ratify a treaty and the highest value if it takes either two legislative bodies or a supermajority of one body to ratify a treaty.[107] Finally, the regional ratification density variable is the percent of other countries in the region that had ratified the AP II in the year prior to an observation. This was calculated based on World Bank Regions—Africa, East Asia and Pacific, Europe and Central Asia, Latin American and Caribbean, Middle East & North Africa, and South Asia—and the country in question was excluded regional ratification rate. In other words, if India was in a conflict in 1992, the value for this variable was based on the percent of the other countries in South Asia in 1991 that had ratified AP II.

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[104] Id. at 215.

[105] *See* Global Development Network Growth Database*, available at* <http://nyudri.org/resources/global-development-network-growth-database/> (last visited April 1, 2012). There were three countries—Bosnia, Croatia, and Yemen—that did not have values for this variable available in this dataset. After additional research, all three were coded as not having a British common law legal system.

[106] This data is available in online appendix 3.2 of Beth Simmons' book, *available at* <http://scholar.harvard.edu/bsimmons/files/APP_3.2_Ratification_rules.pdf> (last visited April 1, 2012). There were two countries without values reported by Simmons: the Ivory Coast and Myanmar. After additional research, the Ivory Coast was given the highest value and Myanmar was coded as the lowest value.

Of course, it is important to consider how these variables perform as instruments.[108] Although it is not possible to directly test the exclusion restriction required of instrumental variables, it is possible to examine how the instruments do predicting the treatment variable. A table in Appendix 1.5 reports the results of regressions estimating the impact of these variables on the likelihood of ratification of AP II. All three variables are statistically significant predictors of ratification of AP II at the 0.001 level. Moreover, there have not been any previously efforts to theoretically link—that this author is aware of at least—any of these instruments to violence against civilians.

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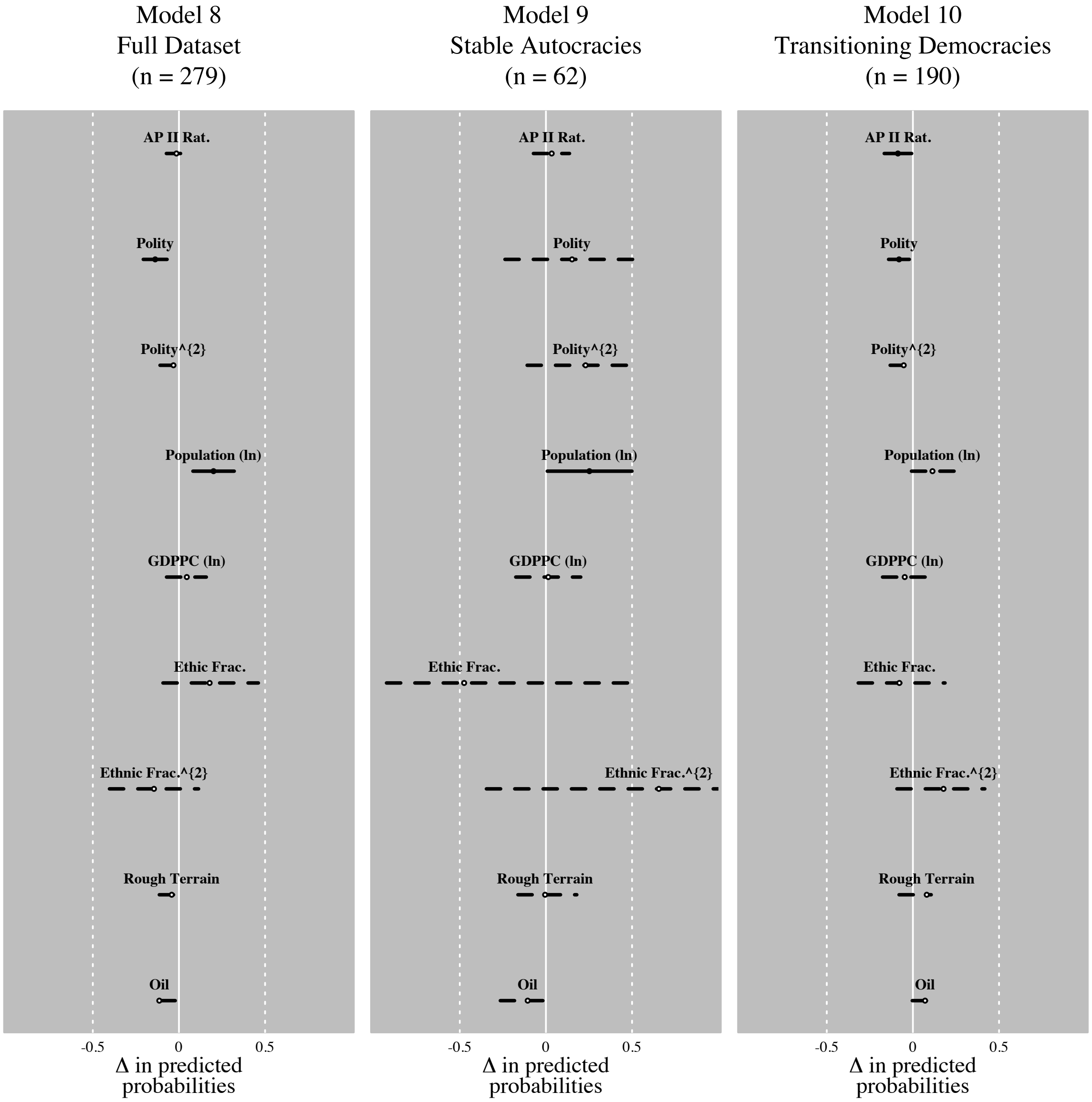
[107] Simmons 2009, 383.

[108] See generally Sovey and Green 2011.

Figure 2.3: IV Regression on Likelihood of One-Sided Violence



The results of the IV regression analysis are presented in Figure 2.3.[109] One again, the results reported are from two-stage regressions with three variables—common law, regional treaty density, and ratification process—used to estimate ratification of AP II, which then is included in an equation that estimates the impact of ratification on one-sided violence. The models reported in this table roughly correspond to those reported in Figure 2.1. The interesting result is that for the majority of models estimated, ratification of AP II does predict a lower likelihood of intentional attacks on civilians, but this likelihood is not statistically significant for the majority of the models estimated. The exception, however, is for countries that are transitioning democracies. In Simmons' analysis of compliance with human rights treaties, she found that ratification was the most likely to influence behavior in transitioning democracies.[110] The results I had using the IV regression approach used by Simmons suggest the exact same phenomenon: ratifying AP II had the largest impact on countries that have had some experience with democracy in the post-war period. In other words, signing a treaty may not change the behavior of all countries, but for those that are partial or transitioning democracies (as opposed to stable democracies or stable autocracies), it might have had an impact on their behavior. These empirical results thus present the first evidence that for certain countries, deciding to ratify AP II could reduce the likelihood that civilians will be targeted in future intrastate wars.

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[109] It is worth noting that the simulated first differences presented in Figure 2.3 were not created with Zelig, which does not have code to simulate results from "twosls" regressions. Instead,

2.4.4 Robustness Checks

To ensure that the results produced in the previous section were not a result of model dependence, I conducted a number of robustness checks. First, one concern might be that a

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these resulted were simulated with code recreating the general approach used by Zelig for instrumental variable regression.

[110] See Simmons 2009, 360 (concluding that "ratified treaties have their strongest effects in countries that are neither stable democracies nor stable autocracies").

number of conflicts included in the dataset were not of a sufficient intensity for AP II to have been applicable.[111] To help control for this possibility, I subset the data to only include conflicts in years after 1,000 battle related deaths had already occurred during the hostilities.[112] This resulted in dropping the number of observations in the overall dataset from 279 to 206. After doing so, all of the regressions from Parts III.A-C. that are reported in Figures 2.2, 2.2, and 2.3 were re-run. The results from doing so were substantively the same, with the ratification of AP II predicting a statistically significant lower likelihood that states would commit acts of one-sided violence.[113]

Second, it would be theoretically possible to question the approach for categorizing regimes into stable autocracies and transitioning democracies adopted from Simmons based on the fact that several of the countries listed as being partially democratic since WWII were not remotely democratic in the years in which they are in conflict. Although using the entire history since WWII to categorize regimes is likely the optimal approach given the questions that have been raised about the coding of variables by the Polity Project for countries during conflict,[114] I still have taken a second approach for categorizing regimes to test the robustness of the result that ratification of AP II is most likely to have a causal impact on behavior for countries with partial histories with democracy. To do so, I coded each country's democracy score in the year prior to each observation, and divided countries with scores below five ("autocratic countries") and countries with five or above ("democratic countries"). The breakdown of countries by regime type using this approach is presented in Appendix 1.6. After doing so, I the estimated models 3 and 4 from Figure 2.1 and models 9 and 10 from Figure 2.3. Doing so produced substantively the same results: ratification of AP II was positively associated with acts of one-sided violence for autocratic countries (but not statistically significantly so), and ratification of AP II had a statistically significantly negative effect on democratic countries' likelihood to commit acts of one-sided violence.

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[111] See, e.g., Moir 2002, 274 (noting that AP II "regulates only the most extreme internal conflicts, leaving the majority regulated by common Article 3 as before").

[112] This was done by limiting observations to those that had a value of "1" for the "CumInt" variable in the UCDP/PRIO Armed Conflicts Dataset.

[113] The only exception is that for Model 10 in Figure 2.3, AP II ratification because has a p-value of 0.09.

[114] See Vreeland 2008.

Third, I have also attempted to isolate the effect that the International Criminal Court (ICC) has on the results. The ICC began operation in 2002, and has the ability to bring criminal charges against leaders from states that are party to the court.[115] Since the ICC has an enforcement mechanism, whereas AP II does not, it is possible that the ICC is at least in part driving countries to not commit intentional acts of violence against civilians. To account for this possibility, I coded whether a country was a party to the ICC in each conflict year. The result was that countries were party to the ICC in 17 of the 279 observations. After this coding, I first ran the regressions from Part 2.4.1 & 2.4.3 having dropped these observations. This made no impact on the statistical results. Second, I ran the regressions for Part 2.4.1 & 2.4.3 with all 279 observations, but included prior ICC ratification as an independent variable. Once again, this did not alter the statistical results. This indicates that, although the establishment of the ICC may be helping to drive down the commission of war crimes, it does not appear to be influencing the statistical results of this study.

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[115] As of April 2012, there are 121 countries that are State Parties to the ICC. <http://www.icc-cpi.int/Menus/ASP/states+parties/> (last accessed April 26, 2012).

Fourth, because the ratification of AP II occurred before the values for many of the variables (e.g. population) were observed, there is a risk of post-treatment bias. In other words, whether a country chose to ratify AP II in 1990 could theoretically have an impact on the values observed for variables like polity score in 1991.[116] To help correct for this possibility, the values for all of the variables used in this paper were collected for each observation in 1977. As this was the year before AP II went into effect, it is not possible that the decision of whether to ratify AP II had an impact on these values. The models reported in Figures 2.1, 2.2, & 2.3, were then estimated again. Just as with the last robustness check, this did not change the finding that ratification of AP II has a statistically significant negative impact on one-sided violence in any of the models reported. To take the analysis using 1977 values for variables one step further, another robustness check was performed on all of the regressions: observations from countries that have dissolved since 1977 (states previously part of the USSR, Yugoslavia, and Yemen) were dropped from the dataset.[117] The reason for doing this is that for states that have dissolved, the 1977 values for variables are dramatically different than the values for the successor states today (i.e. the population of Georgia today is only a fraction of the population of the USSR in 1977). This check did not change the significance of the AP II variable.

Fifth, the models estimated on the observations from countries that had conflict before and after ratifying AP II reported in Part 2.3.2. were rerun with Djibouti dropped from the dataset. This is because Djibouti both ratified AP II and entered into conflict in 1991. Using the coding procedures laid out in Part 2.3, however, resulted in Djibouti being treated as being in conflict for a year before AP II went into effect, and then several years after. Since it might not be accurate to think of Djibouti as a state that had ratified AP II in between periods of conflict, it was worth testing whether including it influenced the results reported in Part 2.4.2. Fortunately, dropping Djibouti from the sample did not have any substantive impact on any of the results reported in Table 6.

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[116] It is worth noting that the variables for Ethnic Fractionalization, Rough Terrain, and Oil were all based on values prior to the negotiating of AP II in 1977.

[117] Specifically, observations from Azerbaijan, Georgia, Moldova, Russia, Bosnia, Croatia, Serbia, and Yemen were dropped.

Sixth, checks were taken to prevent the possibility that the instruments used in the IV regressions reported in Part 2.4.3 actually directly influenced incidents of one-sided violence. To do so, the regressions reported in Parts 2.4.1 & 2.4.2 were estimated again while including the three instruments—British common law legal tradition, the country's ratification procedures, and the regional density of AP II ratification—in each of the models. Doing so did not have an impact on the fact that AP II ratification resulted in a statistical significance lower likelihood of one-sided violence.

2.5 Conclusion

Civil wars have constituted an increasingly large percentage of wars since WWII, and have unfortunately resulted in the majority of civilian deaths from combat. During this same period, the international community has developed a new treaty regime—Additional Protocol II to the Geneva Conventions—to govern the behavior of states during intrastate conflicts. Since the treaty was opened for signature in 1977, over 160 nations have ratified it. Despite the considerable diplomatic efforts that went into the creation and promotion of this treaty, however, until now there have not been any systematic attempts to determine if countries that have ratified AP II are less likely to intentionally kill civilians during war.

To address this question, this study has created a dataset of intrastate conflicts that are covered by the terms of AP II. After controlling for a number of factors, my empirical results suggested that ratification of AP II is associated with a statistically significant lower probability that a government will intentionally commit acts of one-sided violence against civilians during internal war. Moreover, two-staged regression results suggest that this effect is even more pronounced for countries that have been partial or transitioning democracies since WWII.

The results of this paper thus not only suggest that ratification of AP II predicts a lower likelihood of violence against civilians, but also that in certain countries, ratification may meaningfully change behavior. This constitutes a significant challenge to the widely held view that countries ignore international law during times of war, and should give hope to lawyers, activists, and all those concerned with protecting the innocent that their efforts to codify and promote ratification of the laws of war are not in vain. Although in depth case studies and further statistical analysis will likely be needed to develop a clearer picture of how the ratification of AP II impacts behavior, this study has at least shown that states that have ratified AP II are less likely to intentionally kill civilians during civil war. This study thus presents the first evidence that continuing to promote the dissemination of AP II might provide an important legal restraint that will help lead to peace.

Chapter 3

Public Opinion, the Laws of War, & Saving Civilians: An Experimental Study

3.1 Introduction

A considerable effort has been expended since World War II to develop an increasingly dense web of treaties that regulate state conduct during conflicts. The explicit goal of that project has been to soften the edges of war in the hopes that doing so will help to protect civilians from indiscriminate violence. Although there has been considerable debate on the efficacy and wisdom of particular provisions contained within specific treaties, little is known about whether the "laws of war" project is achieving its overall goals. Simply put, we still do not know if the laws of war help to protect civilians.

Despite the importance of this question, scholars have not paid much attention to trying to examine whether the laws of war change the behavior of states during conflict. In fact, to date there have only been two published studies on the topic.[1] The first, by Valentino, Huth, & Croco produced results showing that the laws of war have not even influenced whether democracies choose to target civilians.[2] The second study, by James Morrow, generated results showing that autocratic states are unlikely to comply with the laws of war, but that democratic states often do.[3] Moreover, this result was even stronger when democratic countries faced enemies for which it was likely that the opponent would reciprocate and follow the laws of war.

As a consequence of the contrary evidence produced by Valentino et al. and Morrow, there is not a conclusive answer to the question of whether international law changes the behavior of states during conflicts. This outcome is not simply attributable to limitations within these studies, however, but instead occurs because there are four inherent shortcomings to observational studies that make it unlikely that one would ever be able to produce a definitive answer to this important question: first, there is little variance in the applicability of international law to conflicts; second, there is a very small sample of conflicts to study since International Humanitarian Law ("IHL") has been fully developed; third, scholars are forced to look at aggregate data and not on the individual decision that leaders face when they must decide whether to comply with the laws of war during conflicts; and fourth, there is likely endogeneity between ratification of IHL and the other variables that would predict respect for life during conflicts.

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[1] Chapter two of this dissertation also details the two existent published studies examining the laws of war and addresses their differing conclusions.

[2] Valentino et al. 2006.

[3] Morrow 2007.

Despite the limitations of using an observational research design to study this topic, the question of whether the laws of war help to protect civilians is critically important. As a result, attempts should be made to find new strategies to discern an answer. In one such attempt, I have conducted a survey experiment that examines whether information on the laws of war can change public approval for strategic decisions made during conflicts. My experiment, administered to a sample of U.S. adults, presented subjects with a vignette wherein the American President had to decide whether to halt a bombing campaign where changed circumstances guaranteed that, if the bombing campaign were to continue, it would result in excessive loss of civilian life. When I presented the scenario, I randomly assigned the information that subjects were given on the status of international law. Taking this approach allowed me to directly test the influence that information on the status of the laws of war has on public opinion, while avoiding many of the obstacles presented by observational data. Perhaps more interestingly, this experiment was the first to test compliance with international law while also using a novel research design and new methods that make it possible to test the causal mechanisms through which information on international law changes public opinion.

This experiment produced at least three results that have important theoretical implications for our understanding of compliance with the laws of war. First, information on treaty ratification does lower support for violations of the laws of war, suggesting that democracies may be more likely to comply with treaty obligations because they are responsive to public opinion. Second, being informed that opponents have committed themselves to international law has an even larger effect on lowering support for violations of the laws of war, providing support to previous scholars who suggest compliance is likely to occur when there is the expectation of reciprocity. Third, the use of mediation analysis suggests that the causal mechanism by which information on the status of international law changes public opinion is not by changing respondents' views regarding the possible repressions from non-compliance, but instead occurs by changing Republican respondents' views on the underlying morality of targeting civilians.

The remainder of the paper proceeds in four parts. In Part 3.2, I review the existing empirical literature on compliance with the laws of war, and then discuss the limitations of observational studies that make it difficult to confidently assess the influence of the laws of war on conduct during conflicts. In Part 3.3, I explain the experimental approach that I have taken to address this question. In Part 3.4, I discuss the results of a survey experiment I have conducted on the role of international law in shaping public opinion on the conduct of war. Finally, Part 3.5 summarizes the key findings of the paper and concludes.

3.2 Compliance with the Laws of War

Researchers have struggled to provide a definitive answer to whether the laws of war have helped to protect civilians during conflicts. In this section, I will describe that struggle. First, I will discuss the existing literature that has examined the influence of the laws of war on conduct during conflicts. Second, I will explain why inherent limitations to observational studies make it unlikely that any such study could end the debate on this issue. Third, I argue that using an experimental approach has promise to help shed new light on this important question.

3.2.1 Previous Research on Compliance with the Laws of War

The question of whether states meaningfully comply with international law has been one of the principal topics studied by scholars of international relations and international law in the last decade.[4] This scholarship emerged out of the criticism that early evidence of high rates of compliance with international law[5] was the result of little more than selection effects because states would sign treaties that codified actions that they planned to take regardless, even in the absence of treaties.[6] Despite evidence that international commitments influence behavior during economic transactions[7] and efforts to protect the environment,[8] and increase domestic respect for human rights,[9] many scholars are skeptical that states would comply with international law if state security were on the line.[10] In fact, one prominent legal scholar, Eric Posner, recently argued that the only reason that states ever choose to honor the laws of war has everything to do with fears of reprisal from enemies and nothing to do with the presence of treaties codifying commitments.[11] Far from being aberrant, Posner's comment is merely reflective of a view that is "widely believed, especially by realist scholars of [International Relations], that when it comes to war, states pay no heed to international law."[12]

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[4] See, e.g., Simmons 2009; Dai 2007; Dai 2005; Hathaway 2002.

[5] Chayes and Chayes 1993.

[6] Downs et al. 1996.

[7] Simmons 2000.

[8] Dai 2007; Dai 2005.

It is perhaps surprising that it has become conventional wisdom that states would not comply with international law during conflict given that only two published studies have attempted to empirically address whether ratification of the laws of war helps to protect civilians during conflicts.[13] Moreover, these two studies, which both focused on interstate conflicts, produced conflicting results. The first study, by Benjamin A. Valentino , Paul K. Huth, & Sarah Croco ("Valentino et al."), compiled a dataset of conflicts between 1900 and 2003, and then ran a series of linear regressions to determine that the fact that a state had signed the Geneva Conventions, or that it was a democracy, made no impact on the number of civilians the state killed during conflicts.[14] This study argued that strategic concerns during conflict were the only thing that influenced the decisions of leaders.

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[9] Simmons 2009; Hathaway 2002.

[10] See, e.g., Desch 2003. Mearsheimer 1995.

[11] Posner 2013.

[12] Armstrong 2012, 147.

[13] Simmons 2010, 281-82.

The second study, by James Morrow, examined a similar set of conflicts as the Valentino et al. study.[15] Morrow used data from the Correlates of War project to analyze interstate conflicts between 1899 and 1991. After controlling for a number of factors, Morrow found that, although ratifying international treaties did not impact the behavior of non-democracies, it did (and hopefully still does) alter the behavior of democracies. Morrow argued that this resulted because democracies signal their willingness to comply with their laws of war obligations through ratification, and the agreements are then enforced through reciprocity when both states have signaled their willingness to comply.

3.2.2 The Limits of Observational Studies

Given the conflicting evidence produced by the Valentino et al. and Morrow studies, it is a completely open question whether the laws of war can help to protect innocent lives during conflicts. However, notably this is not because of major theoretical differences among scholars on what variables to use or which conflicts to study in attempting to resolve this inquiry. Instead, it is because the limitations of observational research designs make it difficult—if not impossible—to directly test the influence of the laws of war on state conduct during conflicts. Four specific limitations that directly contribute to this problem are: lack of variation, limited sample size, endogeneity, and aggregation issues. Each will be discussed in turn.

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[14] Valentino et al. 2006.

[15] Morrow 2007.

First, any observational study on the impact of the laws of war is plagued by a lack of variation. Both of the studies previously mentioned—Valentino et al. (2006) and Morrow (2007)—examined whether ratification of particular treaties changed behavior during interstate wars. One major shortcoming of this approach, however, is that the outcome that the scholars primarily examined—intentional killing of civilians—is not only prohibited by treaties, but also by Customary International Law ("CIL"). It is this impossible to use large-n observational studies to examine whether efforts to codify and promote awareness of CIL has had an impact on state behavior during wars because CIL applies to every state, and as a result, there is not any variance in the applicability of CIL between observations.[16] Moreover, even setting aside the concerns that CIL present, there is no longer meaningful variation among states with respect to the most important treaties that govern interstate wars. That is to say, there are now 194 states party to the 1949 Geneva Conventions, and 170 states party to the 1977 Additional Protocol I of the Geneva Conventions. As a result, moving forward there is likely no longer enough variation in ratification of laws of war treaties to study the effect of these treaties on state behavior beyond the time period that has already been examined.

Second, any observational study trying to examine the impact of the laws of war will be hampered by small sample sizes. Both Valentino et al. (2006) and Morrow (2007) extended the time period for which they examined conflicts to the start of the twentieth century. As previously noted, however, efforts to codify and increase the precision of the laws of war took off in the second half of the twentieth century. This is significant because interstate wars have comprised an increasingly small share of armed conflicts since World War II.[17] Moreover, the most precise articulation of the laws of war for interstate conflicts was the 1977 Additional Protocol I to the Geneva Conventions (AP I). This sharply reduces the n of any observational study because between 1978 and 2007, only 15 interstate wars occurred.[18] As a result, even if scholars try to ignore the variance problem posed by CIL and opt to focus on ratification of AP I to study the influence of the laws of war on conduct during conflicts, there are very few conflicts available to study (of course, in terms of the real world practical consequences of conflict, 15 wars is too many, not too few, for any time period).[19]

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[16] It is worth noting that it is possible for a state to be a persistent objector to CIL, which would make it so that a norm of CIL was not binding on that particular state. That said, this author is unaware of any persistent objectors to the Geneva Conventions. Additionally, there certainly are not enough states that are persistent objectors to major IHL norms to create a balanced sample to study the influence of CIL on state behavior during conflict.

Third, observational studies focusing on the influence of the ratification of treaties on the laws of war are plagued by problems with endogeneity.[20] Even using sophisticated statistical techniques, it is incredibly difficult to tell whether states change their behavior as a result of ratifying IHL treaties, or whether states ratify IHL treaties because they are likely to already comply with the norms these treaties codify. This difficulty arises because the decision to ratify and the decision to comply have an endogenous relationship, and without the ability to randomly assign which countries are subject to treaties, it is difficult to determine which factor is having an influence. As a result, any observational study will suffer from the reality that it is difficult to convincingly model the decisions to ratify and comply in a way that can isolate the effects of ratification on compliance.

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[17] Moir 2009.

[18] Sarkees and Wayman 2010. This number is based on the number of interstate wars included in the Correlates of War (COW) interstate dataset v4.0 that commenced after 1977.

[19] The research presented in Chapter 2 was able to partially avoid these problems by focusing on civil wars. Although there are certainly still limitations to Chapter 2's research design, I was able to partially avoid the problems of variance and sample size just discussed by focusing on intrastate and not interstate wars. This is because there has been greater variance in the ratification of AP II compared to the ratification of the Geneva Conventions or AP I, and there have been more civil wars than interstate wars since AP I & II were negotiated in 1977.

[20] For an excellent discussion of how endogeneity and aggregation create obstacles for observational studies of the democratic peace, see Tomz and Weeks 2013.

Fourth, observational studies of compliance with the laws of war face aggregation problems. Theories on why states may comply with the laws of war focus on how treaty ratification would influence the individual policy decisions of leaders. For example, Morrow's (2007) theory suggests that a democratic leader would be more likely to refrain from targeting civilians during conflicts because of expectations of reciprocity. But instead of being able to test whether this particular mechanism can influence the outcomes of individual choices, scholars have only been able to test whether the total number of civilians killed in conflicts has been lowered in cases of mutual ratification. The macro-level data thus may give insight into overall outcomes, but cannot show whether or how treaty ratification shapes the preferences and beliefs of decision makers.[21]

3.2.3 The Advantages of An Experimental Approach

As I have argued, there are a number of reasons why observational studies have not been able to produce conclusive evidence on whether the laws of war help to protect civilians during conflicts. Of course, the uncertainty of the answers we are likely to obtain does not mean that this is not an important question to ask. Instead, it simply suggests that scholars interested in analyzing if IHL has been able to help protect civilians have to find new methods to try and shed light on this important topic. In this section, I will argue that there are several advantages to the use of survey experiments that can help to overcome the problems that inherently plague observational studies on compliance with the laws of war.

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[21] Tomz and Weeks 2013.

The first advantage of survey experiments is that this approach allows the researcher to design a scenario that can present a direct test of compliance with the laws of war. One problem with studying compliance with the laws of war is that it is difficult to find situations where a leader is directly confronted with the discreet decision to take an action that clearly violates international law. Instead, in the real world, decisions are made by a diffuse set of actors in situations with ambiguous facts. For example, the previous studies discussed—Valentino et al. (2006) and Morrow (2007)—both used civilian deaths as their dependent variable. But the law of war does not restrict the incidental killing of civilians, only the *intentional* killing of civilians or the undertaking of actions where the risk to civilians is excessive relative to the military advantages. As a result, any direct study of compliance would have to find a way to examine cases where leaders were faced with one of these two expressly impermissible choices, and could not simply look at total civilian deaths. While instances of these cases occurring in history may be difficult to find, it is relatively easy to design an experiment where leaders are faced with a clear choice to violate the laws of war in a discrete way.

The second advantage of survey experiments is that they make it possible to randomize information on the status of international law. As the previous section outlined, one difficulty in researching compliance with the laws of war specifically, and human rights treaties more generally, is that many important legal instruments have been widely ratified. The result is that there is not sufficient variance in observational studies to test theories of compliance. By randomizing whether subjects are provided information on international law, however, it is possible to test whether that information has the *potential* to change opinions. Randomization thus helps to solve the problems of insufficient variance that plague observational studies, and also can help to address endogeneity concerns as well.

The third advantage of survey experiments is that they are an excellent way to test the most credible theory for why certain states may comply with the laws of war. As previously discussed, there is not any evidence that states broadly comply with the laws of war. Instead, Morrow (2007) has presented evidence that *democracies* that have ratified IHL treaties are less likely to target civilians during war. Since decision makers in democracies are constrained by public opinion—and there is evidence that domestic political mechanisms drive compliance with treaty obligations broadly[22]—a critical way to test Morrow's claim would be to see if information on ratification even has the potential to change public opinion. If ratification of international legal agreements does not have the ability to change public opinion, it is unlikely that such treaties would provide a meaningful constraint on democratic leaders. As a result, testing the ability of information on the laws of war to change public opinion is perhaps the most direct way to test whether it is likely that democracies would be more likely to comply than non-democracies as Morrow (2007) suggests.

Fourth, surveys of public opinion are effective ways of studying elite opinion.[23] This is because elites not only have a strong incentive to follow public opinion, but also because public opinion polls are a surprisingly accurate way to infer elite opinion. Researchers that have studied public opinion and elite opinion on the same foreign policy questions have produced a range of evidence that suggests a strong correlation between the two groups.[24] The important implication is that studying mass opinion can be used as an effective way to study elite opinion on foreign policy questions. In the case of compliance with the laws of war, the implication is that it is possible to be skeptical that democratic leaders will respond to changes in public opinion that are a consequence of international law, but recognize that survey experiments still provide important evidence on how that same evidence might directly influence the preferences and beliefs of decision makers themselves.

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[22] See Simmons 2009; Dai 2007; Dai 2005.

[23] See Tomz and Weeks 2103.

3.3 Research Design

To test whether information on international law has the potential to change public support for conduct during conflicts, I embedded a random experiment in a survey. Before presenting the results, I will describe that experiment in the part that follows. First, I will briefly describe the motivations of this experiment, and the hypotheses that the experiment was designed to test. Second, I will discuss the process used to recruit subjects for the survey. Third, I will outline the survey. Fourth, I will present the tests that I conducted to ensure that the randomized treatments embedded in the survey were balanced.

3.3.1 Motivations & Hypotheses

The experiment that I conducted was specifically designed to test five hypotheses. First, the primary question of interest of this survey is whether information on the status of international law changes public opinion on the acceptability of violations of the laws of war during conduct. The small body of existing survey research conducted about international law more generally has suggested that informing individuals that a policy would violate international law does in fact change American public opinion,[25] so it would be reasonable to hypothesize that this result would occur in the law of war context as well.

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[24] See Holyk 2011; Herron et al. 2002.

Second, if information on international law does change public opinion, an important second question is whether this change in opinion has a "substitutive" or "additive" effect over other similar arguments that do not rely on the previous ratification of international agreements. As Tomz (2008) pointed out in the first experimental treatment of international law, even if international law changes opinion, if other arguments—such as appeals to morality—have an equal effect, then informing people about violations of international law simply is "substituting" it for another argument.[26] If information about international law and additional arguments have a combined effect, however, this "additive" effect might still change public opinion because having signed a treaty would give individuals an additional argument. Based on Tomz's evidence in the human rights context,[27] my hypothesis is that international law has an additive effect with other arguments. That is to say, I hypothesize that persons who already think an action is morally wrong will more strongly disapprove of that action when they hear it is also against international law."

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[25] See Chaudoin 2013; Wallace 2013; Tomz 2008.

[26] Tomz 2008, 19.

[27] Id. at 21.

Third, previous research has suggested that democracies are more likely to comply with the laws of war when their opponent has previously committed to do so.[28] Moreover, previous survey research on the effect of international law on public opinion on the use of torture has shown that learning that the opposition uses torture changes respondents' views.[29] As a result, I hypothesize that information on international law will have a greater effect when individuals are told that the opposition has made a previous commitment to obey the laws of war.

Fourth, previous survey research has shown that the influence of information on international law varies based on political ideology. Specifically, Wallace's research on torture shows that liberals are more likely to change their opinions, compared to conservatives, on the acceptability of torturing detainees for information during the war on terror after learning that the use of torture violates international law.[30] I thus hypothesize that information on the laws of war will have a greater effect on the opinions of respondents that have expressed liberal political views.

Fifth, it is still an open question why the effect of information on international law on public opinion varies based on political ideology. Previous experiments have not asked questions that tested potential mechanisms to explain why information on international law changes opinions, and thus researchers have not been able to explain why ideology might alter the effect of information. Although I do not have any strong priors on what the mechanism is that results in this variance based on political views, I did specifically design this survey experiment to test which of six potential mechanisms might explain variance based on ideology.[31]

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[28] Morrow 2007.

[29] Wallace 2013.

[30] Id.

3.3.2 Survey Recruitment

I developed and administered an identical survey in November 2012 and March 2013 to a combined sample of 2,077 U.S. adults. The respondents were administered the survey online, and were recruited using Amazon's Mechanical Turk (mTurk) service. Amazon's mTurk offers a pool of users a small fee to complete short tasks. I offered users from this pool a small cash incentive to complete a survey. Using mTurk for survey recruitment has the advantage of being a convenient and fast way to recruit subjects for experimental research.[32] Moreover, it is also a reliable way of conducting experimental research. Research on the reliability of using mTurk for experimental research has consistently demonstrated that mTurk produces the same treatment effects as experiments conducted on subjects recruited using other methods.[33] Most notably, Berinsky et al. (2012) used mTurk to replicate experiments that had been conducted using other methods to recruit subjects, and their results show that the results produced by mTurk are comparable to those produced by administering the experiment using other methods.[34] Moreover, experimental research conducted using mTurk has gained acceptance in political science. Studies conducted using mTurk have appeared in the field's most respected peer-reviewed journals.[35]

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[31] For an example of designing a survey to test causal mechanisms, see Tomz and Weeks 2013.

[32] See Mason and Suri 2012; Paolacci, Chandler and Ipeirotis 2010.

[33] See Germine et al. 2012.

[34] Berinsky, Huber, and Lenz 2012.

3.3.3 Experimental Design

To test whether information on the laws of war changes opinion on conduct during war, I embedded a randomized experiment in a survey. That survey had three parts. First, the respondents were asked a number of preliminary questions about their demographic background and their prior political views. Second, the respondents were presented with a hypothetical scenario under which a future president is forced to decide whether to continue a military campaign that would violate the laws of war. While reading this vignette, the respondents were randomly assigned to different treatment conditions that changed the information on international law that they were presented with. After reading the vignette, the respondents were then asked whether they approved of the president's policy decision. Third, the respondents were asked a series of questions designed to test the potential causal mechanisms by which information on international law could cause them to alter their views on the issue.

It was during the second part of the survey that the randomized experiment was conducted.[36] During that section of the survey, the respondents were presented with a vignette where the U.S. president was confronted with a choice on whether or not to take a military action that would be in clear violation of the laws of war. In the vignette, all respondents were first told that "[i]n a country that is a strategic ally of the United States, a rebel group has controlled an outlying region of the country for a long time. As a result of recent instability in the country, the rebels have left the areas they control and launched an attack on the country's capital in an effort to overthrow the government." The respondents were next informed that, "[t]he U.S. president responded by launching air strikes in support of our ally. After suffering initial casualties from the air strikes, the rebel forces took shelter in areas heavily populated with civilians. This made the U.S. military unable to continue air strikes while distinguishing rebel targets from civilian targets. Any continued bombing would result in excessive civilian casualties. This forced the U.S. president to consider whether to continue the bombing campaign."

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[35] See Arceneaux 2012; Huber, Hill, and Lenz 2012.

[36] The exact wording of the experimental portion of the survey is in Appendix 2.1.

At this point, respondents were presented with different sets of pro and con arguments about the merits of continuing the bombing campaign. All respondents were presented with the pro argument that, "[i]f the U.S. were to halt the bombing campaign, it is likely that the rebel forces would overthrow the government, and that the country would no longer be an ally of America." At this point, respondents were subjected to one of four treatment conditions. Respondents in the first treatment group, the control group, did not receive a con argument. The other respondents in the other three treatment groups were randomly given one of three con arguments. The specific text of those treatments are as follows:

- **International Law Treatment:** "On the other hand, continuing the bombing of civilians would violate international law. It is a violation of international law and treaties that the United States has signed to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained."
- **Morality Treatment**: "On the other hand, continuing the bombing of civilians would be immoral. It is immoral to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained."

- **Combined Treatment:** "On the other hand, continuing the bombing of civilians would violate international law. It is a violation of international law and treaties that the United States has signed to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained. Additionally, continuing to bomb civilians is not only a violation of international law, it is immoral. It is also immoral to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained."[37]

The international law treatment makes it possible to directly test the first hypothesis of this experiment, namely, does information on international law lower public opinion on the support for actions that violate international law? The treatment makes it clear that continued bombing would violate both international law generally, and the specific treaties that the United States has committed to.

The second two treatments make it possible to test whether international law has a substitutive or additive effect. The morality treatment was designed be comparable to the international law treatment. It specifically has a parallel structure, similar tone, and the same fundamental claim. This treatment was specifically worded to not claim that the United States was obligated in any way because of previous actions, and does not make a claim about the likelihood of retaliation. In other words, it is designed to test the argument that would have been available in the absence of international law on the topic. The combined treatment tests whether there is an additive effect by combining the international and morality treatment.

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[37] To avoid the possibility of ordering effects, the order in which the two arguments contained in this treatment was randomized.

To test the fourth hypothesis of the study—that the likelihood of reciprocity influences treatment effects—the experiment contained a second treatment condition. After respondents received one of the first four treatments, a second treatment was administered. This treatment concerned whether respondents were also given information about the rebel group's commitment to international law. In the first group, respondents were not given any information about the rebels' commitment to international law. In the second group, respondents were told that "[t]he rebel forces have publicly committed to comply with international law and not intentionally kill civilians, and there is not any evidence that they have broken that commitment." The experiment thus employed a 4x2 design, creating eight groups of respondents.

After randomly receiving one of these sets of arguments, respondents were told that "[u]ltimately, the president decided to continue the bombing campaign against the rebel forces because failing to do so would result in the loss of a strategic ally." The respondents were then asked of whether they Approve, Disapprove, or Neither Disapprove or Approve of the president's action. Immediately after, respondents who approved were asked whether they "strongly approved" or "somewhat approved"; respondents who disapproved were asked whether they "strongly disapproved" or "somewhat disapproved"; and respondents who indicated neither preference were asked whether they "lean towards approving," "lean towards disapproving," or "neither lean towards approving nor disapproving." The result was that respondents offered their opinion along a seven-point scale that is consistent with previous survey experiments studying international law.[38] Using this scale, I then created a binary variable where respondents that either strongly approved, somewhat approved, or leaned towards approving were coded as 1 and all other responses were coded as 0. This method is consistent with the approach used by previous researchers, and allows for consistent comparisons of the magnitude of treatment effect of providing information on international law with prior studies.[39]

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[38] See Tomz 2008; Wallace 2013. It is worth noting that Chaudoin 2013 used a six-point scale. To do so, he simply eliminated the option of allowing individuals to say that they "neither lean towards approving or disapproving."

3.3.4 Survey Balance

Before analyzing the results of the experiment, I first checked to ensure that the probability that respondents received a particular treatment was not skewed among the pre-treatment covariates measured. To do so, following Chaudoin 2013 I estimated a logit model with pre-treatment demographic variables to assess whether the probability of treatment was evenly distributed.[40] To do so, I regressed the respondent's gender, age, education level, party affiliation, citizenship and race on the binary variable to represent receiving each of 8 treatments. The results of this analysis are presented in Appendix 2.2. The results suggested that there is limited evidence that any variables were skewed along treatment groups. For the eight treatment groups, only four of the 72 total covariates achieved statistical significance. This is exactly what would be expected based on random chance.[41] As a result, it appears that the probability of the treatment was roughly evenly distributed.

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[39] See Chaudoin 2013; Wallace 2013; Tomz 2012.

[40] Chaudoin 2013, 18-20.

[41] Using a 0.05 p-value as the measure of statistical significance, one in every twenty variables should be statistically significant based on random chance. Which means over 72 variables analyzed, 3.6 should achieve significance random (72 * 0.05 = 3.6).

3.4 Results

This part presents the results from the survey experiment. It proceeds in five parts that correspond to the hypotheses discussed in the previous section.

3.4.1 Hypothesis 1: The Effect of International Law

The primary question of this study is whether information on international law changes public opinion on the acceptability of the government taking actions during conflicts that violate the laws of war. Figure 3.1 presents the percentage of respondents who approved of the presidents action that received the null and international law treatments. Among the respondents that received the null treatment—that is, no mentions of international law, morality, or reciprocity—only 34% approved of the president's action. Of the respondents who were told that continued bombing violated international law, just 24% approve of the president's decision to continue bombing. This difference illustrates the effect that information on international law has on public opinion. There is a 9% difference in approval rates between the null treatment and international law treatment groups. This difference is both substantively large, and highly statistically significant (the p-value for the difference is 0.01). Moreover, the magnitude of this treatment effect is consistent with the limited previous research on the impact of international law on public opinion.[42]

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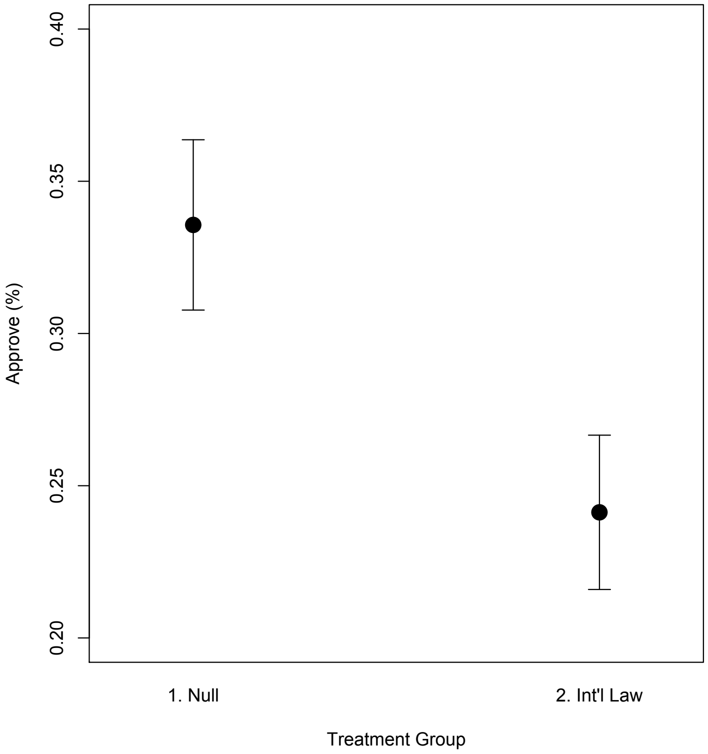
[42] Wallace (2013) found that information on the status of international law produced a 6% drop in approval for the use of torture. Similarly, Chaudoin (2013) found that information on a prior international agreement changed support for using import restrictions by 11%. As a result, the finding in this paper is comparable to other estimates about the potential magnitude that information on international law may have.

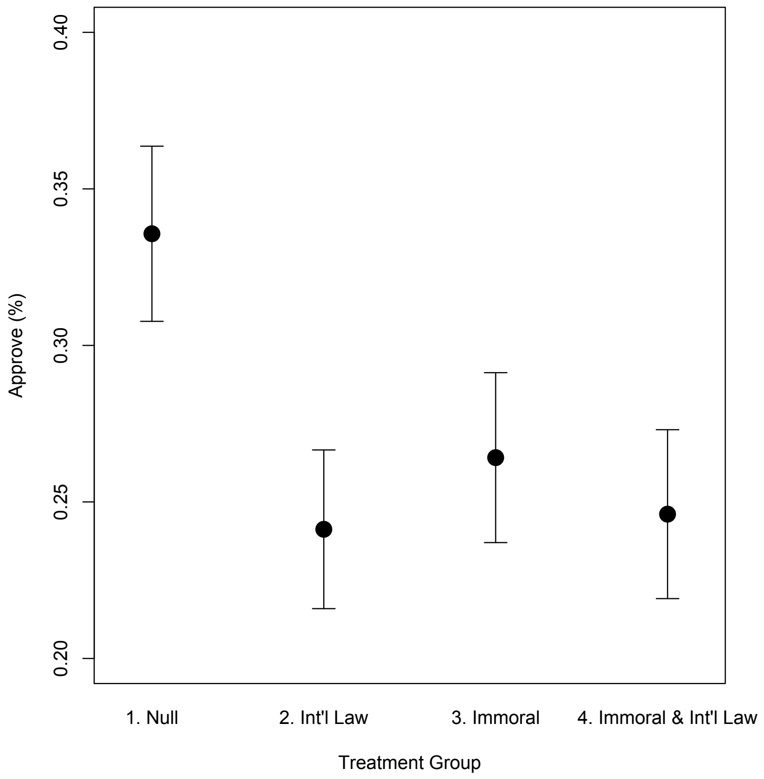
Figure 3.1: Treatment Effect of International Law



3.4.2 Hypothesis 2: The Substitutive or Additive Effect of International Law

Of course, even though information on the status of international law caused a 9% change in public opinion, that does not necessarily mean that the ratification of treaties would change the course of policy debates. After all, information on the status of international law would only be able to influence policy discussions if the fact that the treaty had been ratified presented a new argument that was more persuasive than the arguments available in the treaty's absence. As a result, I decided to test whether information on international law had a substitutive effect (and simply reproduced the treatment effect of other arguments), or had an additive effect when combined with other arguments (and thus caused additional changes in public opinion). Figure 3.2 presents results that address this question.

Figure 3.2: The Effect of International Law & Morality Treatments



The first two treatment groups shown in Figure 3.2 are identical to the treatment groups presented in Figure 3.1: the null treatment group and the international law treatment group. In addition, Figure 3.2 includes the results for the respondents who were told that continued bombing was immoral (the "morality treatment") and for the respondents who were told that continued bombing was *both* immoral and a violation of international law (the "combined treatment"). As previously noted, respondents who were given the international law treatment had a 24% approval rate of the president's decision to continue bombing when it would cause excessive loss of civilian life. The respondents who were only told that continued bombing was immoral, however, approved of the president's decision at a 26% rate. This rate is only 2% different, so although the international law argument appeared to be slightly stronger, this result is not statistically significant (the p-value is 0.54). Based on this information, however, it is possible to say that information on the laws of war does have a substitute effect over other possible arguments.

The next question is whether there is an additive effect. The combined treatment tests this proposition. The respondents in the combined treatment group approved of the president's decision at a rate of 25%. This is slightly higher than just being told that continued bombing violates international law, and slightly lower than being told that continued bombing is immoral. Of course, these differences are not statistically significant. The result then is that it is not possible to say that ratification of treaties on the laws of war has an additive effect. Instead, this information simply substitutes for being told that the behavior in question would be immoral.

3.4.3 Hypothesis 3: The Effect of Reciprocity

The next issue to analyze is whether information on international law has a greater effect when respondents are also told that there is the likelihood of reciprocity. As previously noted, Morrow (2007) studied the influence of the laws of war using observational data, and the results suggest that democracies are most likely to follow the laws of war when both parties to a conflict have previously committed to obeying the laws of war.[43] In fact, reciprocity is so important that at least one legal scholar has suggested that it is the only factor that influences compliance with the laws of war.[44] To test this possibility, I included a second treatment condition where each group of respondents was also told that the opposition had previously pledged to follow international law and does not appear to have broken that commitment. The results of that analysis are presented in Figure 3.3.

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[43] Morrow 2007, 569.

[44] Posner 2013.

Figure 3.3: The Treatment Effect of Reciprocity

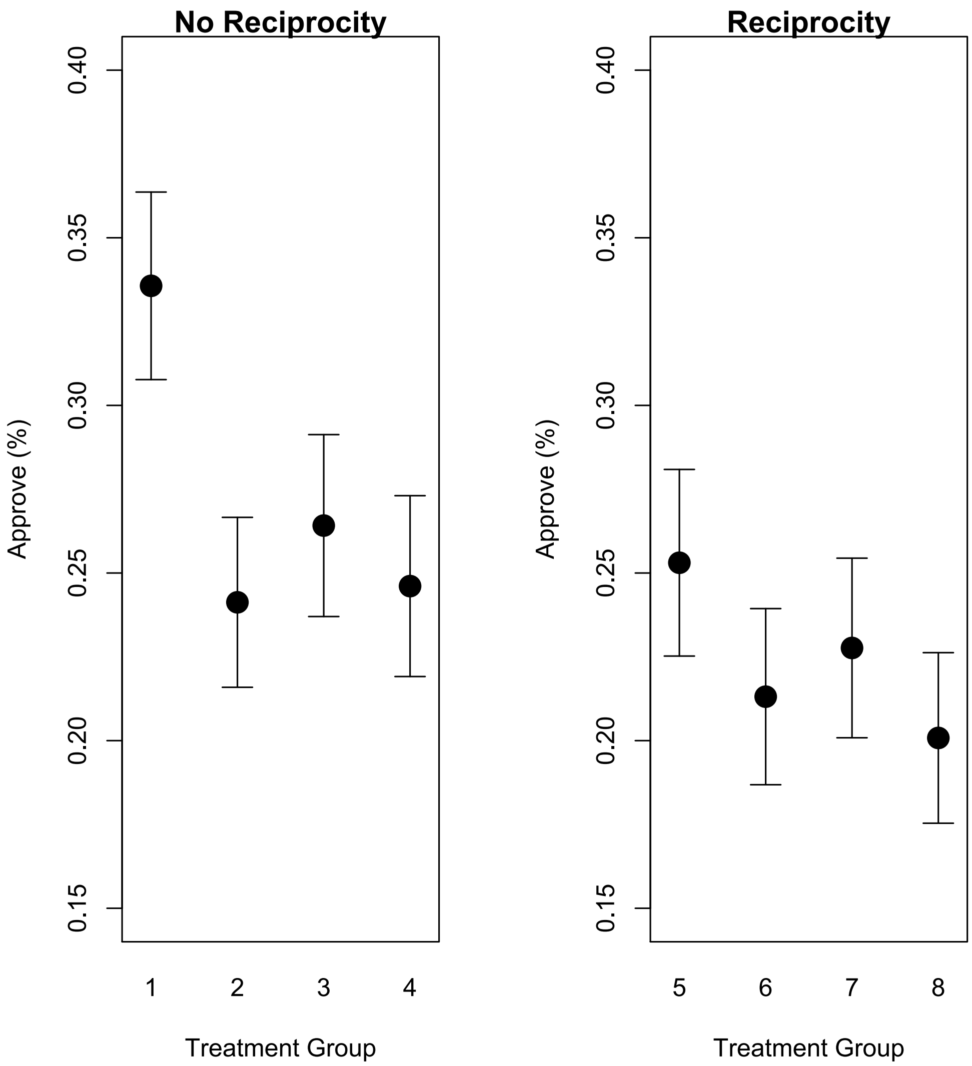

Figure 3.3 presents the results for all eight treatment groups in the study. The left panel is identical to Figure 3.2, and presents the results for the four treatment groups that did not receive a mention of the likelihood of reciprocity. The panel on the right is the results for the

respondents who were also given the reciprocity treatment.[45] The respondents who were only told that the other side in the conflict had pledged to obey international law for whom prior U.S. treaty commitments or morality was not mentioned (group 5) had a 25% approval rating for continued bombing. This is roughly comparable to the respondents who were told about prior U.S. commitments (group 2), told that continued bombing is immoral (group 3), or the combination of the two, without mention of reciprocity (group 4).

Interestingly, however, when the argument that continued bombing was immoral, violated treaties, and there was the chance of reciprocity was presented to respondents (group 8), the approval rating dropped to 20%. This is not only statistically significant compared to the null treatment (group 1), but also statistically significant compared to the treatment group that was simply told continued bombing was immoral (group 3). In fact, there is a 6% difference between group 3 and 8 (with a p-value of 0.09). This suggests that, although simply being told that an action violates international law may not be more powerful than saying it is immoral, telling individuals that both sides have made commitments to international law does have an additive effect. Because the public would be less supportive of violating the laws of war when they believe that the other side of the conflict will not do so, this supports Morrow's finding that democracies should be expected to be less likely to commit violations of the laws of war when both sides have previously committed to not doing so.

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[45] For the first experimental manipulation, group 5 received the null treatment, group 6 received the international law treatment, group 7 received the morality treatment, and group 8 received the combination treatment.

3.4.4 Hypothesis 4: The Effect of Ideology

As previously noted, prior research on the influence of international law on public opinion has suggested that the treatment effect is not uniform. Instead, individuals that have expressed liberal political views are more likely to change their opinions after being told about international legal obligations than are conservatives. To test whether this is true in the laws of war context, I broke down the sample by respondents that leaned towards the Democratic Party and those that leaned towards the Republican Party. These results are presented in Figure 3.4.

Figure 3.4 recreates Figure 3.1, but with the results broken out by Democrats and Republicans. As the results in the left panel show, Democrats are moved by the influence of international law (treatment 1 compared to treatment 2), but the result is quite small and not statistically significant. For Republicans, information on international law changes public opinion by 11% points, although this result fails to achieve statistical significance (the p-value is 0.18). Additionally, the results for the respondents that did not identify with either political party were similar. The reason that this different treatment effect is interesting, however, is that previous research has suggested that international law has a larger treatment effect on those with liberal political views.[46] Previous research has not identified the causal mechanisms that would cause one group to have different reactions to information about the status of international law, so it is difficult to speculate on why the treatment effect may be greater for Republicans in the survey experiment I conducted here.

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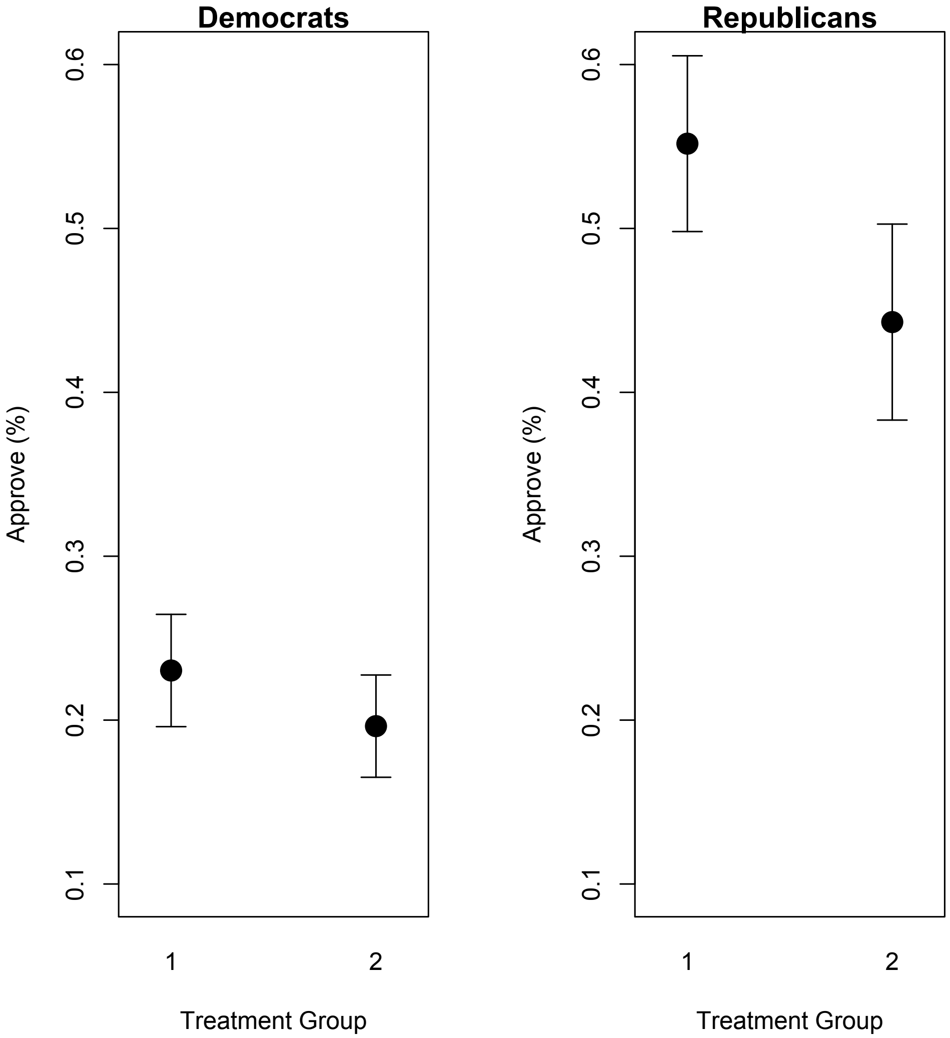
[46] Wallace 2013.

Figure 3.4: Treatment Effects by Political Affiliation



3.4.5 Hypothesis 5: Exploring Causal Mechanisms

One advantage of using an experimental research design is that it also makes it possible to explore the mechanisms that might account for the causal impact of the treatment. In this case, in addition to asking subjects whether they approved of continuing a bombing campaign that would result in excessive loss of life, I also asked all of the subjects that participated in the survey a series of questions that tried to explore why the various treatments may have influenced their opinions.

Specifically, each subject was asked five questions after being presented with the experimental vignette; these questions were aimed at exploring *why* information on the laws of war might lower support for continued military action.[47] Those six questions asked if: (1) continuing airstrikes that would kill civilians would be morally wrong (“Morality"); (2) violating an international commitment would be wrong (“International Commitment"); (3) bombing civilians is likely to result in other countries taking actions against the U.S. (“International Response"); (4) continued bombing would increase the number of civilians killed by rebels (“Continued Death"); (5) stopping bombing would increase the number of civilians killed by rebels (“Quit Death").[48]

The advantage of asking these questions is that, by using mediation analysis techniques developed by Imai et al. (2011), it is possible to evaluate the process by which a treatment variable influences an outcome.[49] By using a possible outcomes framework, the technique developed by Imai et al. uses a two-stage process to first estimate the impact of the treatment on the potential mediator, and then uses a second regression to estimate the influence of the treatment and the mediator on the outcome of interest.[50] In the case of my experiment, this means that, by asking the previous five questions designed to test possible reasons that information on the status of international law might change opinions, it is possible to evaluate why the various treatments change approval for continued bombing of civilians.

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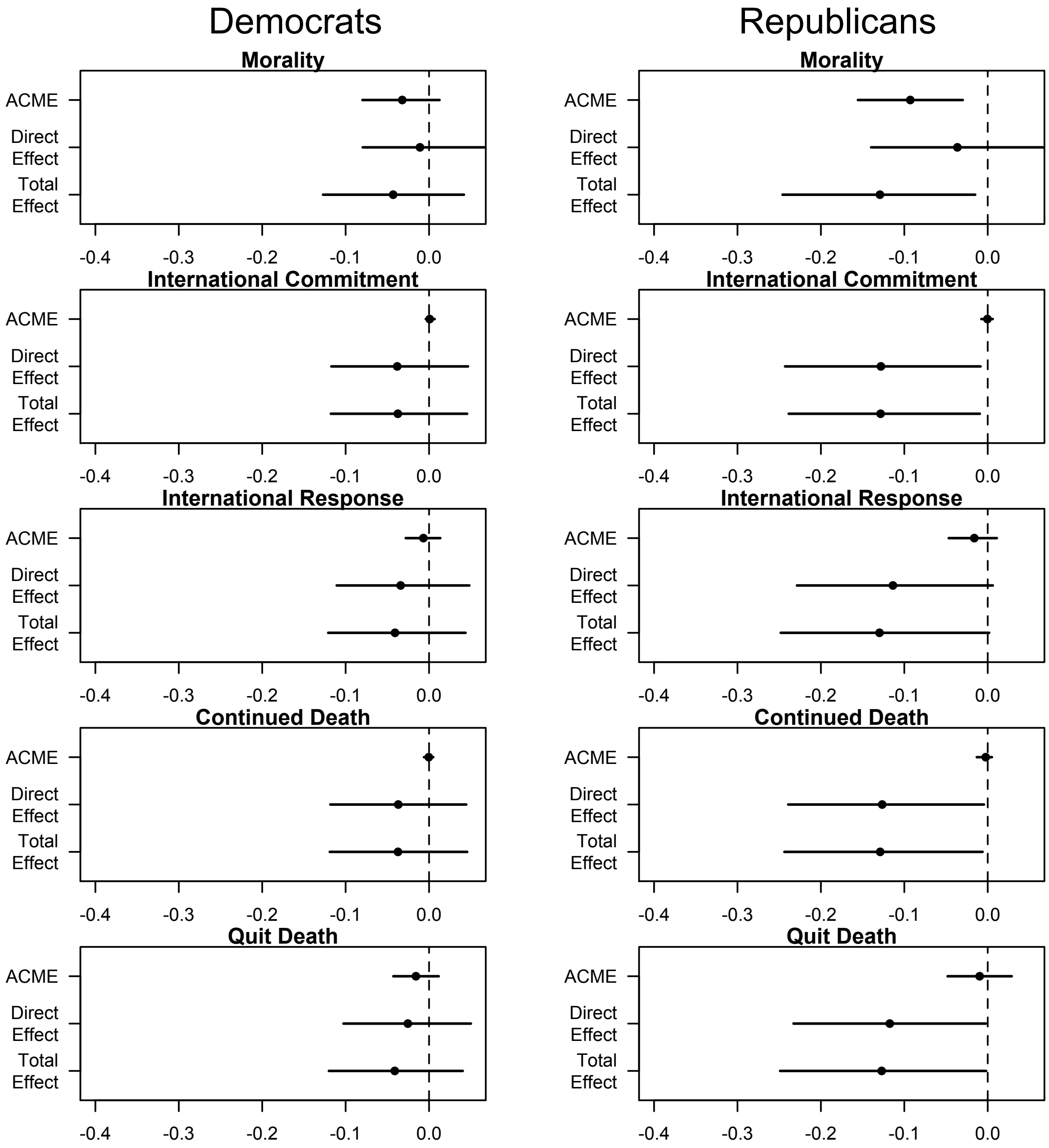
[47] The order of these six questions was randomized for each subject to guard against the possibility of any ordering effects.

[48] Exact wording of each of these questions is found in Appendix 2.1.

[49] Imai et al. 2011.

[50] For examples of survey experiments using this method to analyze causal mechanisms, see Tingley and Tomz 2014; Tingley and Tomz 2012; and Tomz and Weeks 2013.

Figure 3.5: Mediation Effects of Causal Mechanisms by Party Affiliation



Given the counterintuitive result presented in the last section—that information on international law has a larger treatment effect for Republicans than Democrats—I subset the data to only compare the results presented in Figure 3.4. I then used the “mediate" software designed to implement the method developed by Imai et al. (2010) to estimate the “Average Causal Mediation Effect" (ACME) for each of the five potential mechanisms I collected survey data on.[51] The results of this analysis are presented in Figure 3.5. For each of the five questions, the Figure shows the estimated influence of treatment that is conducted through the proposed mechanism (“ACME"), the direct impact of the treatment itself (“Direct Effect"), and the overall sum of these two effects (“Total Effect").

As Figure 3.5 shows, there does not appear to be a clear mediation effect of any of the possible causal mechanisms. For each of the five models for Democrats (the left hand side of Figure 3.5), not a single ACME achieves statistical significance.[52] For Republicans, however, this is not the case. Instead, the first mechanism—Morality—has a statistically significant mediation effect. This ACME is 10%, with a total direct effect of 13%. For this total direct effect, 71% is via mediation through the morality mechanism. To put this in other words, the reason that learning that continuing bombing campaigns that will result in excessive civilian lives violates international law changes the opinion of Republicans is because it convinces a large number of respondents that the activity is immoral. So although Democrats may already hold this view, for Republicans the prior commitment to international law changes their opinion.

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[51] This analysis was conducted using the “mediate” package for R. See Imai et al. 2010.

[52] The Figure presents the effects graphically, with the lines representing the .90 percent confidence interval. If a line crosses the vertical line, it means that it is not significant at the 0.1 level.

3.5 Conclusion

As this paper has outlined, previous observational studies trying to determine if ratification of treaties on the laws of war changes behavior have produced inconclusive results. Moreover, there are a number of reasons that it is unlikely that any research design based on observational data will be able to resolve this debate. As a result, it is important to look for new methods to analyze this important question. I have argued that experimental research is one promising approach.

By administering a survey experiment that both randomized information on the status of international law and included questions designed to test causal mechanisms, this project has been able to make four important contributions to our understanding of compliance with the laws of war. First, the results of this experiment suggest that even without information about international law, support for actions that would violate its core principles is quite low. This provides evidence that it would be reasonable to assume democracies should be unlikely to violate the laws of war, regardless of treaty ratification. Second, information on the ratification of relevant treaties lowers support for violations of the laws of war by roughly 9 percentage points. Not only is this a large enough margin to have the potential to sway policy outcomes, but also it is a substantively larger treatment effect than some other published experimental research has found for other types of international law. Third, information that an opponent has committed to comply with the laws of war makes support for violation of such laws even lower. This is consistent with previous statistical evidence and theoretical arguments that posit that concern for reciprocity is a major driver of compliance. Fourth, information on international law has a larger treatment effect on Republicans than Democrats. This finding has been the opposite in studies on other topics governed by international law, and suggests that the reactions of conservatives to international law is more nuanced than might have previously been postulated. Fifth, the analysis of causal mechanisms suggests that the international law treatment changes the views of Republicans on the morality of the underlying action. In other words, prior commitments to international law can alter the moral calculations of citizens in a way that has the possibility of changing perception of conduct during conflicts.

Finally, it is my hope that this experiment has not only shed light on the debate on whether countries comply with IHL treaties that they have ratified, but also helped to demonstrate the viability of using experimental research to study international law. This is the first study to use experimental methods to test theories of compliance with the international laws of war while also using an experimental design that allowed for mediation analysis to be performed. As a result, hopefully this project has helped to demonstrate that experimental research can help not only to test whether states are likely to comply with their international legal obligations, but also to explore *why* they would comply. It is only by finding answers to the latter question that we will be able to understand whether international law has the potential to help solve critical problems, such as the need to reduce needless civilian causalities during war.

Chapter 4

The Influence of International Law on Domestic Policy: An Experimental Study

4.1 Introduction

Do states change their policies as a consequence of the international human rights agreements that they sign? Over the last decade, this has been one of the most hotly debated questions in the study of international law.[1] Previously, scholars had often focused on the rates of compliance with international law, and proudly proclaimed that states generally comply with international law most of the time.[2] Skeptics, however, began to point out that high rates of compliance are not evidence of the importance of international law, and instead, it may simply be the case that states only agree to international legal commitments that require policies that they would have had in the absence of the agreement.[3] These skeptics have been especially critical of the idea that international law could change human rights practices.[4] After all, human rights treaties do not contain enforcement mechanisms and states have not taken steps to hold countries accountable for failing to live up to prior commitments.

In response to that powerful argument, a number of scholars have begun to propose theories of why, even in the absence of external enforcement, states might still change their behavior after ratifying human rights treaties. Although a number of theories have been put forward,[5] the theory that has gained the most traction is the argument that the presence of an international obligation changes public support for domestic efforts to change policy to bring a country's practices into compliance with its international commitments.[6] Although this domestic politics theory of compliance does not predict that ratifying human rights practices would change the human rights practices of autocracies, it does hypothesize that it would for states that are at least partially democratic. In other words, the theory is that in democracies, domestic actors are able to use the state's prior ratification of international treaties to bring about changes in human rights practices that would have otherwise not occurred because the presence of an international legal obligation changes political support for reform.

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[1] See Simmons 2010, 288-292.

[2] See Chayes and Chayes 1993.

[3] See Downs, Rocke, and Barsoom 1996.

[4] See Goldsmith and Posner 2005.

[5] See, e.g., Koh 1999.

In support of this theory, a small, but growing, body of research has shown that compliance with international human rights treaties may be driven by domestic politics.[7] In the most notable effort, Beth Simmons has used instrumental variable regression to show that that ratifying the four of the core international human rights treaties has caused states that are transitioning or stable democracies to change their human rights practices.[8] Despite the incredible progress that Simmons' study made demonstrating that in certain circumstances international law has a causal effect on policy outcomes, it still fall short of directly testing the domestic politics mechanism of compliance with international law. Simmons study has shown that ratifying human rights agreements has a statistically significant impact on democracies' later human rights practices, but it did not directly tested whether this is because the presence of international law has helped change the political support for reform.

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[6] Simmons 2009.

[7] Dai 2007; Dai 2005.

[8] Id.

This project attempts to directly test whether ratification of human rights agreements increases public support for altering human rights practices. To do so, I have conducted the first randomized experiment embedded within a survey testing the influence of international law on a purely domestic policy issue: reforming the use of solitary confinement in prisons. This experiment directly tested whether respondents held different opinions as a result of being told that critics of the use of solitary confinement argue that the United States had previously ratified human rights treaties regulating the practice. The results of this experiment not only demonstrate that information on prior treaty ratification does have a small but statistically significant effect on public opinion, but that generic appeals to human rights do not. In other words, ratification of human rights treaties causes changes in public opinion that mere appeals to human rights do not.

By producing that result, this study makes at least three important contributions to the literature on international human rights. First, this paper is the first to use experimental methods to show that prior international legal commitments change opinions on *domestic* policy. All prior experimental efforts to study the effect of international law on public opinion have focused on whether it changes opinions on foreign policies,[9] which ignores the increasingly dense web of agreements that seek to regulate countries domestic policies. It is important to study whether those agreements have the potential to change state practices. Second, this paper is the first study designed to explicitly test mechanisms that have been theorized to drive compliance with human rights treaties. Even if scholars accept the findings using observational data that suggest democratic states change their behavior as a result of signing human rights treaties, understanding why that is the case is important. This paper thus sets out to explicitly test one causal mechanism that may explain why ratifying international agreements results in changes in policy. Third, this project continues to help grow the small body of scholarship that has used experimental methods to study the influence of international law more broadly. Experimental methods present a promising way to overcome many of the obstacles that have plagued experimental studies,[10] and helping to demonstrate their potential to produce new insights is an important goal of this project.

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[9] Chaudoin 2013; Wallace 2013; Putnam and Shapiro 2009; Tomz 2008.

This paper proceeds in four parts. In Part 4.2, I law out the development of the domestic politics theory of compliance with human rights agreements, and discuss why experimental methods present a promising way forward to test the theory. In Part 4.2, I explain the experiment that I have designed and conducted on whether information on the status of international law changes the views of individuals on a proposed reform to American policy. In Part 4.4, I present the results of that experiment. Finally, Part 4.5 concludes.

4.2. Theories of Compliance with International Law

One of the most important, and active, debates in international legal scholarship is whether states change their domestic policies as a consequence of making international commitments to human rights.[11] In this section I outline that debate and lay out the argument for why an experimental approach may help to make progress on that question. First, I review the literature on international law that has expressed skepticism that states change their domestic policies as a consequence of ratifying international treaties without enforcement mechanisms.

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[10] See generally Chilton and Tingley 2013.

[11] See generally Simmons 2010, 288-292.

Second, I discuss how theories have developed arguing that commitment to international agreements on human rights changes state behavior by altering the domestic politics within the country. Third, I explain why observational studies have been unable to provide conclusive evidence whether, and why, international legal commitments cause states to change their domestic policies. Fourth, I outline that merits of using an experimental approach to test this domestic politics theories of compliance.

4.2.1 Skepticism Over International Law's Influence

Scholars of international law and international relations have long been skeptical of the idea that states would change their behavior as a consequence of international law.[12] Scholars that hold these views—commonly associated with realism—have been willing to concede that states largely comply with their international legal obligations.[13] They argue, however, that this fact should not be taken as evidence that international law changes state behavior.[14] Instead, they argue that international legal agreements that are made reflect existing state power relationships and state interests at the time that the agreements are formed.[15] As a consequence, treaties themselves do not actually change state behavior—they simply are a statement of existing realities about the state of the world. As a result, states should be expected to comply with international agreements when it is in their interest to do so, and disregard international commitments when they are no longer consistent with the state's interests.[16]

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[12] For a good discussion of the "conventional wisdom" of the influence of international law, see Simmons 2009, 114-116.

[13] See Morgenthau 1985, 295. For an articulation of the view that states generally comply with international law, see Chayes and Chayes 1993.

[14] See generally Downs et al. 1996.

[15] See Mearsheimer 1995.

Scholars that are skeptical about the power of international law to change state policies generally are especially critical of the idea that states would alter their behavior as a consequence of signing international human rights agreements.[17] Although there are several reasons that motivate this view, perhaps the most important is that states do not pay a large price for violation.[18] Modern human rights treaties have not included external enforcement mechanisms, and states have largely not retaliated against foreign states simply for failing to live up to the commitments that they have previously made. As a result, given the lack of threat of external enforcement mechanisms, the common refrain is that these treaties do not serve a meaningful constraint on state behavior. After all, as Beth Simmons concluded after reviewing this argument, "[i]f we are looking for empathetic enforcement [of human rights treaties] from other countries, we will be looking in vain for a long time."[19]

4.2.2 Domestic Theories of Compliance

Against this backdrop, a number of scholars have begun to develop theories explaining how signing international human rights agreements might alter state behavior despite the absence of external enforcement mechanisms.[20] These scholars have agreed with realists that international human rights agreements do not provide a meaningful external constraint on state behavior. They do argue, however, that international human rights treaties "empower individuals, groups, or parts of the state with different rights preferences that were not empowered to the same extent in the absence of the treaties."[21] In other words, human rights treaties change state policies because they change the balance of power at the domestic level.

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[16] For a discussion of realists that hold this view, see Dai 2007, 16-19.

[17] For an excellent in depth articulation of this view, see Goldsmith and Posner 2005, 107-134.

[18] Id. at 120.

[19] Simmons 2009, 116.

In the most extensive articulation of this theory, Simmons has argues that signing human rights treaties empower three distinct sets of actors: the executive, the judiciary, and citizens.[22] Simmons thus argues that each of these actors presents a unique mechanism through which ratification of human rights agreements can result in changes to domestic policies. The ratification of human rights treaties can thus result in changes to domestic policies, without any threat of outside enforcement, through one of these three mechanisms.

First, Simmons argues that human rights treaties empower the executive by creating an exogenous shock to the national agenda.[23] The argument is that executives may not wish to expend their political capital by pushing a human rights topic into the country's national debate. When an international agreement comes into existence, however, the executive is able to easily place the issue onto the legislative docket. The result is that the country may enact domestic policy changes to comply with a new international agreement when they would have not otherwise addressed the issue in the absence of the agreement.

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[20] See Dai 2005; Dai 2007; Simmons 2009.

[21] Simmons 2009, 125.

[22] Id. at 126.

[23] Id. at 127-129.

Second, Simmons also argues that the ratification of human rights treaties has implications for a country's judiciary.[24] The reason is that for most countries, a ratified treaty is a valid domestic law that creates legal rights that are enforceable through the courts. The treaty thus creates an important tool for litigants that would like to expand the protection of human rights within their country. They are able to cite to the presence of the treaty, which then forces judges to think about how the agreement should be enforced domestically. Having ratified the agreement thus changes the legal arguments available to plaintiffs, and in turn, the rights that judges are obligated to respect.

Third, the final, and principal, mechanism that Simmons discusses is how ratification of human rights treaties can mobilizes and empowers citizens. When states sign international human rights agreements, they have made a public commitment to certain standards of rights protections. When individual citizens perceive that the rights that are provided do not correspond to that prior commitment, Simmons suggests that this has two important effects.[25] First, it creates a perceived "rights gap" which makes citizens more likely to demand that their rights be respected. Second, it changes the social environment by making others more sympathetic and tolerant of such demands; which improves the likelihood of success. The consequence is that the shift in pubic attitudes caused by the ratification of the treaty makes it more likely that the country will alter its domestic policies in order to come into compliance with the treaty.

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[24] Id. at 129-135.

[25] Id. at 136.

4.2.3 Problems with Observational Evidence

Although Simmons,[26] Dai,[27] and others[28] have made considerable progress testing their theory that domestic political changes can cause states to change their policies as a result of signing human rights treaties,[29] there have still been lingering questions over the robustness of the empirical results that have been produced using observational data.[30] This is not simply because of shortcomings in those study's designs. Instead, there are at least two problems inherent to efforts to study the influence of human rights treaties on state behavior using observational data.

The first is that states that sign human rights agreements are decidedly non-random.[31] As realists have pointed out for years, states that sign human rights agreements often do so because they either already are compliant or have had a change in preferences that have made the government interested in altering their policies to do so.[32] As a result, since the "treatment" of having signed a human rights treaty is not randomly assigned, it is difficult to causally analyze the consequences of those agreements using observational data. To compensate for this problem, scholars have used a variety of complex statistical methods, including instrumental variable regression,[33] selection models,[34] and matching.[35] These methods, however, are not without shortcomings. For example, it can be incredibly difficult to find reliable instruments or to match observations without omitting important variables. The result is that skeptics of the power of international agreements to change state human rights practices often have grounds to doubt even the most careful attempts to causally analyze the influence of human rights treaties on changes in state behavior.[36]

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[26] Simmons 2009.

[27] Dai 2007.

[28] See, e.g., Hill 2010.

[29] Dai 2007; Simmons 2009.

[30] See Posner 2012.

[31] See generally von Stein 2005.

[32] See Downs, Rocke, and Barsoom 1996.

The second limitation is that data availability has made it impossible to directly test the mechanisms that have been hypothesized as having the potential to cause states to change their behavior as a consequence of prior commitments. As previously noted, Simmons and others have suggested specific mechanisms for how ratifying international agreements could alter the domestic political landscape in a way that results in changes in policy. Although these mechanisms have been explored using qualitative analysis,[37] it is incredibly difficult to build a large-N dataset that specifically tests any one of these mechanisms. Instead, scholars have simply been able to show that ratification has resulted in changes in the behavior of democracies but not non-democracies, even when controlling for a variety of other factors.[38]

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[33] See, e.g., Simmons 2009.

[34] See von Stein 2005.

[35] See, e.g., Simmons and Hopkins 2005; Hill 2010.

[36] See Posner 2012.

[37] See Simmons 2009; Dai 2007.

As a result of these two obstacles, empirical approaches using observational data have not been able to either eliminate the possibility that selection effects are driving their results, or to prove that any positive relationship is attributable to one of the specific causal mechanisms that have been previously theorized.

4.2.4 Designing an Experimental Test

On promising way to gain leverage on the question of whether states change their behavior as a result of signing international human rights treaties is to conduct an experiment. Despite the fact that experimental methods have become increasingly widely used by political scientists and legal scholars over the last decade, they have been scarcely used to study international law.[39] This is surprising given the fact that scholarship on international law has been increasingly concerned with finding ways to test for causal relationships, but has only slowly begun to turn towards experimental methods that randomize treatment as a way to do so.

Although a limited number of experiments have recently been conducted that have attempted to test the influence of international law,[40] there has not yet been a single effort to test whether ratification of human rights treaties could result in changes to state behavior. Instead, the experiments that have previously conducted have analyzed the relationship between information on the status of international law and public opinion on international issues.[41] For example, these experiments have tested whether information on international law makes individuals more supportive of torturing foreign detainees in the war on terror[42] or more supportive of imposing trade sanctions on foreign countries.[43]

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[38] Simmons 2009.

[39] See generally Chilton and Tingley 2013.

[40] See Tomz 2008; Putnam and Shapiro 2012; Wallace 2013; Chaudoin 2013.

As a result, conducting an experiment is a promising method of testing domestic theories of compliance with human rights agreements that has not yet been used. This is an oversight because experimental methods present an excellent way to directly test the third causal mechanism proposed by Simmons[44]—that is, does learning that a country has signed an international agreement make citizens more supportive of proposals to reform their governments human rights policies on that issue area?

Of course, designing an experiment capable of answering this question without deceit requires finding an issue area where the country that the citizens surveyed live in has signed a human rights treaty, but is currently at least arguably not living up to its obligations. Fortunately, this is a difficult task for an American researcher proposing to conduct an experiment on a pool of American respondents. Although the United States may have anonymously patterns of ratifying international agreements,[45] the United States is largely compliant with the human rights agreements that it has signed.

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[41] But see Findley et al. 2013 (using a field experiment to test how information on international law influences the willingness of private business to offer anonymous incorporate).

[42] Wallace 2013.

[43] Chaudoin 2013.

[44] Simmons 2009, 139

[45] Simmons 2009, 39-47.

One area, however, that at least some commentators have argued that the United States has policies that are inconsistent with the international agreements that it has signed is the use of solitary confinement. Solitary confinement is frequently used in American prisons,[46] but scholars have argued that this is inconsistent with a number of international agreements.[47] Although this is a debatable and controversial claim—on which I have no position—it at least provides a clear policy area where it would be reasonable to say that critics argue that the U.S. is currently in violation of the human rights agreements that it has ratified.[48] As a result, in the first experimental test of whether the prior ratification of human rights treaties makes individuals more supportive of changes to domestic, I have conducted an experiment on how information on the status of international law changes support for the use of solitary confinement in American prisons.

4.3 Experimental Design

This section describes the experiment that I have conducted to test whether ratification of international human rights agreements changes the views of American's on domestic policy questions. In this part, I first outline the motivations for the experiment. Second, I explain the survey recruitment process. Third, I describe the experiment itself. Fourth, I discuss the diagnostics conducted to test the reliability of the experiment before analyzing the results.

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[46] For background on this topic, see the American Civil Liberties Union's report, "Unfinished Business: Turning the Obama Administration's Human Rights Promises into Policy," at <http://www.aclu.org/files/assets/unfinished_business_aclu_final.pdf> (last visited April 13, 2013).

[47] See Hresko 2006; Vasilaides 2005.

[48] The claim that has been made is that the extended use of solitary confinement is cruel and unusual punishment that violates the Universal Declaration of Human Rights, the International Covenant on Civil and Political Rights, and the Convention Against Torture. See Hresko 2006, 17.

4.3.1 Motivations & Hypotheses

The goal of this experiment is to gain insight into whether a prior commitment to a international human rights agreement changes the views that individuals hold on domestic policy. This experiment specifically seeks to address four questions. First, does learning the fact that the United States has previously signed an international treaty on a topic change respondents' views on issues of domestic policy? As previously noted, one theorized mechanism for why democracies might changes their policies after signing human rights treaties is that it changes domestic public opinion. To date, however, experimental research has not been conducted to establish this link. Second, if there is a change in respondents' views on domestic policy, what is the magnitude of that change? For the changes in respondents' views caused by international commitments to have an outcome consequential impact on policy, the magnitude of that change must be sufficiently large to change policy. Third, how does the magnitude of the effect of information of international law compare to similar arguments on the same topic? A claim necessary to the argument that international commitments can result in policy changes by altering public opinion is the corollary that these changes are larger than other similar arguments that do not invoke international commitments. For example, if arguing that executing minors violates their human rights has the same effect on public opinion as arguing that executing minors violates human rights treaties that have previously been signed, then the added benefit of the agreement is less clear.[49] Fourth, if information on international law changes public opinions, why is that the case? Does it change minds because people would prefer not to violate previous agreements, because they now are more likely to view the act as immoral, or because information on the status of international law creates a gap between the domestic policy and international standards? To answer this final question, This experiment directly tests the previously theorized mechanisms for how information on international law may change opinions.

4.3.2 Subject Recruitment

This experiment was administered to 1,859 respondents in April 2013. The respondents were all recruited online using Amazon's Mechanical Turk (mTurk) service. Through mTurk, individuals are able to offer a pool of users a small fee to complete a short task—in this case, completing a survey.[50] The appeal of mTurk is that it is a very cost effective, convenient, and fast way to recruit subjects for experimental research.[51] Although it might be reasonable to think that there is a trade-off associated with using mTurk compared with more traditional methods of subject recruitment, a growing body of research has consistently demonstrated that mTurk produces the same results as experiments conducted through other means.[52] For example,

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[49] Of course, it would still be possible that international agreements could have what Tomz (2008) refers to as "additive effects." That is, that signing an international agreement provides an "extra" argument that can move public opinion further than simply using the arguments available without the agreement would. In addition to Tomz's research, Chapter 3 of this dissertation tested this phenomenon as well. There is currently limited evidence that additive effects for international law are robust.

[50] Respondents were paid $0.50 to $0.75 for completing this survey.

[51] See Mason and Suri 2012; Paolacci, Chandler and Ipeirotis 2010.

[52] See Germine et al. 2012.

Berinsky et al. (2012) have replicated experiments that have been conducted using other methods on samples of subjects recruited through mTurk, and their results show that the results produced by mTurk are statistically the same as the results using other pools of respondents.[53] Moreover, experimental research conducted using mTurk to recruit subjects has now appeared in the most respected peer-reviewed political science journals.[54] Of course, it is important to note that subject pools recruited through mTurk tend to be younger and moral liberal than the population as a whole[55]—which was the case for my sample as well.

4.3.3 The Experiment

Does information on the status of international law change the opinions that individuals hold on purely domestic policy? In the first experimental attempt to answer that question, I embedded a randomized experiment in a survey conducted in April 2013. The survey had three parts. First, the respondents were asked a series of demographic questions about their background and political beliefs. Second, the respondents were told of a proposed policy reform, and randomly assigned into one of three treatment groups that altered the slate of arguments that they received in support of the change in policy. The respondents were then asked if they approve or disapprove of the proposed policy reform. Third, the respondents were asked a series of questions that directly tested possible causal mechanisms for how information on the status of international law might change their opinions.

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[53] Berinsky, Huber, and Lenz 2012.

[54] See Arceneaux 2012; Huber, Hill, and Lenz 2012.

[55] Tingley and Tomz 2014; Tingley and Tomz 2012.

The second part of the survey contained the randomized experiment. For the experiment, the respondents were told that they were going to read about a policy currently used in American prisons that lawmakers have been considering reforming.[56] The survey then described the current use of solitary confinement in American prisons. Respondents were told that prisoners are often subject to solitary confinement for extended periods of time, and that these periods can last years. Respondents were further told that while in solitary confinement that prisoners can be held in their cell for up to twenty-three hours a day, and that during these periods that the prisoners are deprived of human contact.

Every respondent then received the same argument in support of the continued use of solitary confinement. The respondents were specifically told that: "Supporters of the use of solitary confinement argue that its use is necessary to maintain prison discipline and ensure the safety of prisoners and guards alike." After being presented with that argument, the respondents were randomly assigned to one of three treatment groups. These treatment groups either were not presented with additional information (the control group), or were presented with one of two arguments against solitary confinement (the treatment groups). The specific text of the two treatments was:

- **Placebo Treatment**: "Critics of the use of solitary confinement argue that it should be eliminated except in the most extreme cases because it violates the human rights of the prisoners held in solitary confinement."

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[56] Exact wording of the experiment is in Appendix 3.1.

- **International Law Treatment:** "Critics of the use of solitary confinement argue that it should be eliminated except in the most extreme cases because it violates international human rights treaties that the United States has signed."

The first treatment, which refers to human rights, is a "placebo" treatment. The treatment informs subjects that critics of the use of solitary confinement violates human rights, but the source of the human rights referred to in this treatment was left intentionally vague. Respondents are left free to infer that the human rights invoked are rights in a general moral sense, or human rights that are specifically codified in domestic or international laws. Additionally, the treatment does not make any arguments about consequences that result from violating the human rights of prisoners. The presence of this treatment helps to test the effect that a general against solitary confinement has on changes in public opinion, and to invoke the concept of human rights but without a codified international agreement behind it.

The second treatment specifically refers to the presence of an international human rights treaty. The intent of this treatment is to test whether international law changes opinions on a specific policy issue as directly, and simply, as possible. Unlike some other experiments that have tested the influence of international law on opinions over foreign affairs treaties, this experiment did not include any claims about what the consequences of violating international law would be.[57] There are two rationales for this decision. First, specifically stating a consequence of violating international law might simply pick up changes in opinion due to the risk of consequences that are nothing to do the prior commitment to international law. Second, in the human rights context, international law does not have an enforcement mechanism. This is why critics have been especially skeptical that signing human rights agreements might change policy. As a result, the treatment simply states the fact that previously signed agreements would be violated. This provides the cleanest test of whether signing international treaties might change opinions on domestic policy issues.

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[57] See Chaudoin 2013. Chaudoin tells respondents that a policy "violates trade agreements between the U.S. and Europe, *and Europe would sue the U.S. at the World Trade Organization*" (emphasis added).

These two treatments were intentionally designed to be similar in as many respects as possible. They employ parallel sentence structures, comparable tones, and the same number of words. The hope was that by designing the treatments in this way, that it would be possible to isolate what effect, if any, international law had on changes in public opinion.

After receiving either the control, placebo, or international law treatment, the respondents were told that: "American lawmakers have been considering reforms that would eliminate the use of solitary confinement expect in extreme circumstances where keeping the prisoner in the general population would pose immediate safety risks." The respondents were then asked whether they Approve, Disapprove, or Neither Disapprove or Approve of the proposed reform. Immediately after, respondents that approved were asked whether they "strongly approved" or "somewhat approved"; respondents that disapproved where asked whether they "strongly disapproved" or "somewhat disapproved"; and respondents that indicated neither preference were asked whether they "lean towards approving" or "lean towards disapproving." The result was that respondents offered their opinion along a six-point scale.[58]

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[58] This approach follows Chaudoin 2013. Previous experimental research on the effect of international law on public opinion had used a seven-point scale; the difference being that respondents could say they neither leaned towards agreeing or disagreeing. See Wallace 2013;

4.3.4 Survey Balance & Receipt of Treatment

Before analyzing the results, I will first present two tests of the reliability of the results. The first test is an analysis to ensure that the treatment assignment was not skewed among respondents based on their demographic characteristics. To test this, I regressed a dummy variable for each of the three treatments on six demographic factors recorded for each respondent.[59] Specifically, I regressed the treatment received on the respondents' age, gender, education level, political party, citizenship, and race. The results of this analysis are presented in Appendix 3.2. In that table, not a single demographic variable was a statistically significant predictor of being assigned to a particular group. Based on these regressions, we can thus not reject the null hypothesis that treatment assignment is balanced across all three treatment groups.

As an additional test of the reliability of the experiment, I include a question at the end of the survey to test whether respondents had actually received the desired treatment.[60] First, respondents were asked to correctly identify which of two arguments that supporters give for why solitary confinement should continue to be used.[61] Respondents were able to correct identify the argument in support that they received at a 98% rate. Second, all respondents were asked whether they were told that critics of the use of solitary confinement "violates the rights of prisoners,"

Tomz 2008. The downside of doing so, however, is that many responses have to be discarded for analysis because the respondent did not choose between whether they approve or disapprove.

[59] This approach follows Chaudoin 2013.

[60] This test also follows the approach used by Chaudoin 2013.

[61] Respondents were asked whether they were told the argument they actually received or whether solitary confinement should continue to be used because the prisoners that receive the punishment deserve it.

"violates international treaties the United States has signed," or "neither." The wording of the options presented were intentionally slightly different than the wording used when the treatments were presented to serve as a test of whether respondents actually processed the argument they were given. Despite that fact, over 50% of respondents could correctly identify the treatment they received. For both of these questions, I can easily reject the null hypotheses that respondents randomly guessed at which arguments they had been told at the 0.001 level.[62]

4.4 Experimental Results

This section presents the results of my experiment. First, I discuss the results of the primary experimental manipulation embedded in the survey. Second, I analyze the influence that political ideology had on the results. Third, I present the results of the mechanism questions asked during the third part of the survey.

4.4.1 Primary Results

The primary results from this random experiment are presented in Figure 4.1. The first treatment group shown in the graph is the null group; that is, the respondents that did not receive an argument in favor of solitary confinement reform. On a scale from 1 to 6—with 1 strongly disagreeing with solitary confinement reform and 6 strongly agreeing with it—the null groups average response was 4.13 (90% CI: 4.01, 4.25). The second treatment group shown in the graph received the placebo treatment. Specifically, they were told that solitary confinement violates the human rights of prisoners. These respondents had a near identical response to the null group—their average response was also 4.13 (90% CI: 4.02, 4.25). The final treatment group shown in the graph is the one of primary interest for this experiment—the respondents that were told solitary confinement violated human rights treaties the United States has signed. This group had an average response of 4.34 (90% CI: 4.23, 4.46).

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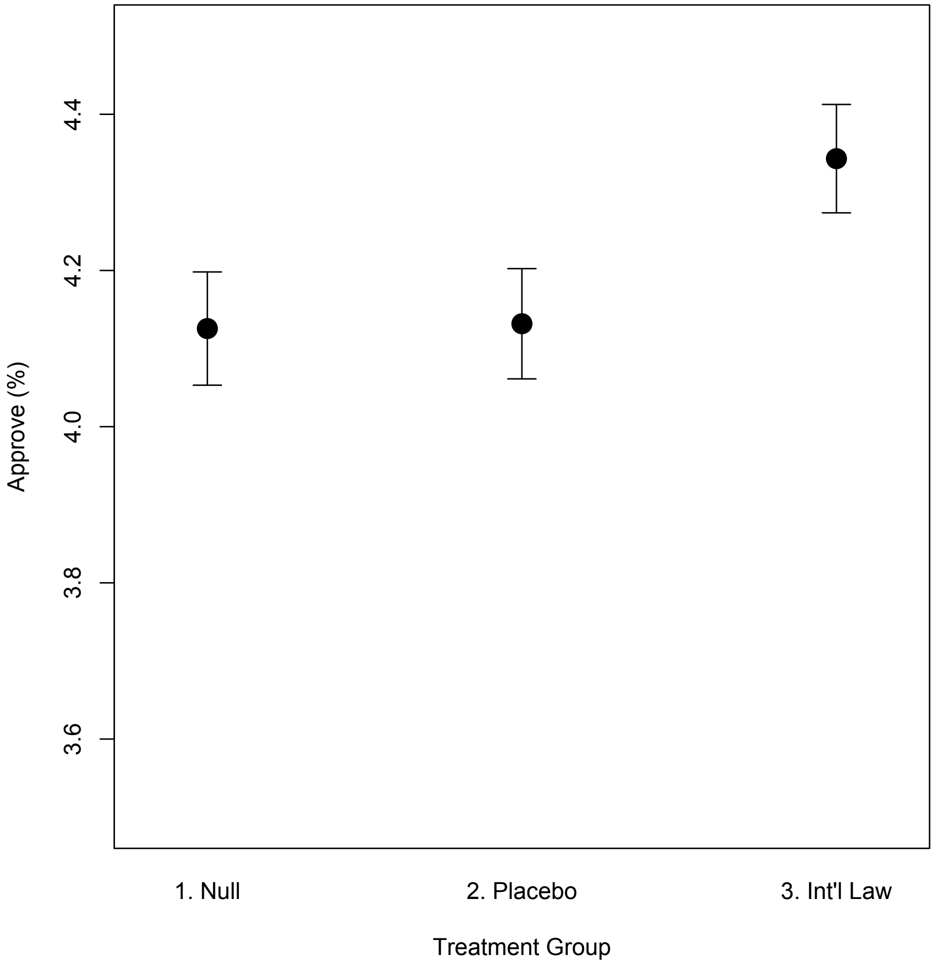
[62] This is based on a simple t-test of whether the responses where statistically different for both questions. The first question this mean that the proportion of correct responses was statistically different than 0.5 (since there were two options available), and for the second question that the proportion of correct responses was statistically different than 0.33 (since there were three options available).

Figure 4.1: Treatment Effects for Overall Sample



There are several interesting things about these results. First, the overall level of support for solitary confinement reform is generally high. For example, without receiving an argument in favor of doing so, 66% of respondents in the null group expressed approval of reform.[63] Second, the placebo treatment did not move the needle at all. Being told that solitary confinement "violates the human rights of the prisoners" had no effect on support for reform. Third, being told that critics argue that the widespread use of solitary confinement "violates international human rights treaties that the United States has signed" had a statistically significant effect on public opinion. This information increased support for reforming the practice over both the null group (p-value = 0.03) and the placebo group (p-value = 0.03).[64] This is particularly strong evidence for the proposition that ratification of international human rights treaties has the potential to result in changes in public policy, because information on ratification had a statistically significant change in opinion over a near identically worded treatment that did not mention international agreements.

4.4.2 Results by Partisan Identification

Of course, it is possible that this treatment effect is not consistent across ideology. As was previously noted, subjects recruited through mTurk skew younger and more liberal than the overall population, and there is evidence that reactions to international law vary based on political ideology.[65] To investigate this further, I subset the sample into respondents that self-identified as Democrats (or Democratic Leaning) and Republicans (or Republican leaning).[66] The results of this analysis are presented in Figure 4.2.

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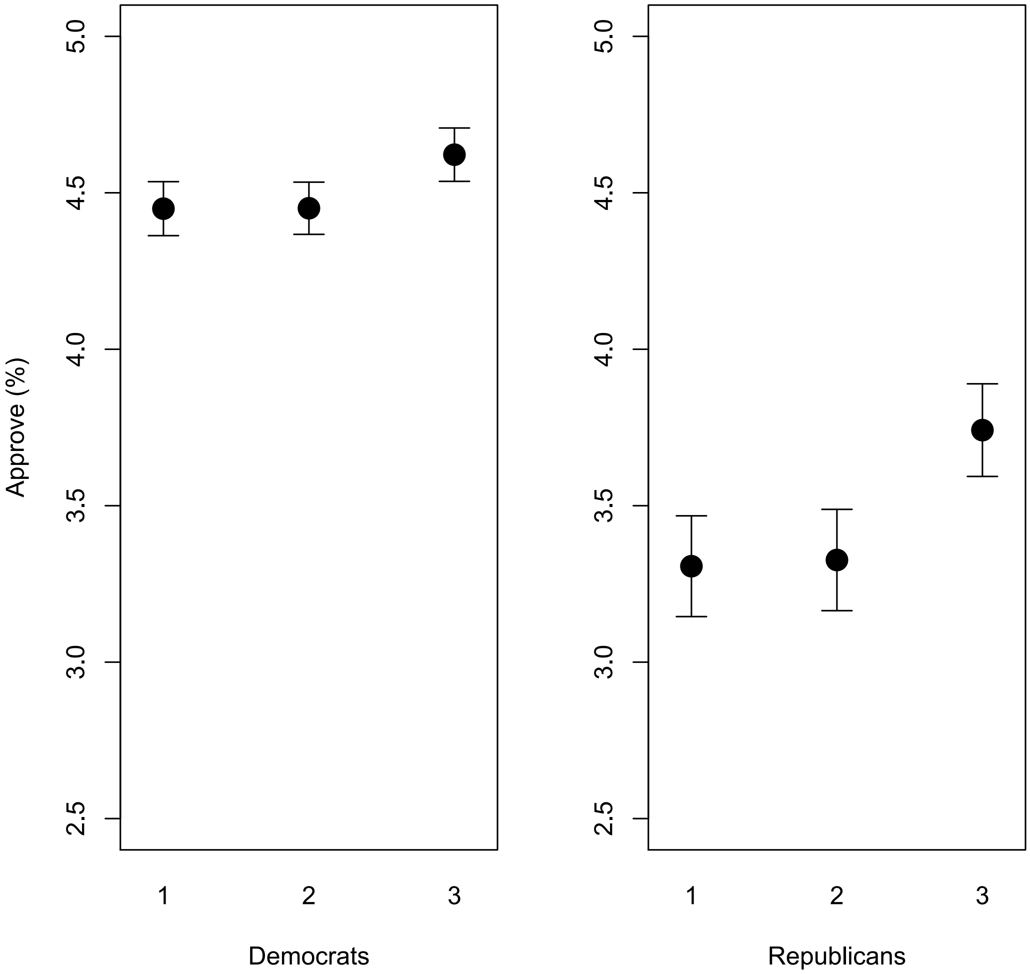
[63] This is percentage of respondents that leaned towards agreeing with reform, somewhat agreed, or strongly agreed.

[64] This translates into roughly a 4% increase in support for reform. This is roughly consistent with the 6% change that Wallace (2013) found in support for the use of torture after respondents were given an international law treatment.

Figure 4.2: Treatment Effects by Partisan Identification



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[65] Wallace 2013.

[66] Independent's that did not indicate a leaning were excluded.

As Figure 4.2 shows, the pattern for both Democrats and Republicans is roughly the same. Democrats are slightly more supportive of solitary confinement reform than the overall population. Democrats in the null treatment group supported solitary confinement reform at a 4.45 rate. Information on international law increased support by 0.17 to 4.62; although this increase falls short of conventional levels of significance (p-value = 0.16). Republicans, on the other hand, were less supportive of solitary confinement reform overall—the null treatment group averaged 3.31. Information on international law, however, increased approval to 3.74. This was an increase of 0.44, which was both substantively and statistically significant (p-value = 0.05). This suggests that information on the status of international law on domestic human rights practices, at least over solitary confinement, actually has a bigger effect on Republicans than Democrats. This result is perhaps surprising given the fact that previous research has suggested that Republicans' opinions are less affected by information on international law.[67]

To investigate this phenomenon further, following Wallace (2013) I estimated an ordered logit model that controls for demographic covariates.[68] The dependent variable for this analysis is the full six-point approval measure. The results of this analysis are presented in Appendix 3.3. Even controlling for a variety of demographic characteristics of the respondents, the results in Appendix 3.3 are consistent with the results that have already been presented. The international law treatment has a statistically significant effect at the 0.05 level, and increased support for solitary confinement reform by roughly 0.27. Similarly, Republican respondents were roughly a full point less supportive of solitary confinement reform. Additionally, the results of this analysis also suggest that age and race influence support for solitary confinement reform. The important takeaway for the purpose of this project, however, is that the international law treatment's effect is robust to controlling for demographic factors that may influence treatment effects.

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[67] Wallace 2013.

[68] Wallace 2013 uses this method to further examine the effect of information on international law on support for torture in the war on terror.

4.4.3 Mechanism Results

As the results presented so far have shown, information on the fact that the United States has signed a human rights treaty has a statistically significant impact on support for reforming solitary confinement practices. The next obvious question is: why this would be the case? One advantage of experimental methods is that they not only make it possible to directly test whether hypothesized treatment effects exist, but that they also make it possible to directly test causal mechanisms.

To take advantage of that fact, after respondents completed the main experiment, I asked their opinion on three questions designed to test which causal mechanism might be driving any treatment effected caused by information on international law.[69] The order these questions were presented was randomized to avoid the possibility of any ordering effects. Those questions were specifically designed to test the following possible causal mechanisms:

- **Commitment**: One hypothesis that has been previously put forward to explain why international law might change opinions is that people find it important to honor commitments, regardless of their contents. To test this possibility, I asked respondents how important they believed it was for the United States to honor its international treaties that it has previously signed.

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[69] The exact wording of these questions is in Appendix 3.1.

- **Morality**: Another possibility is that information on the status of international law increased the likelihood that individuals will view a particular action as immoral. This idea is simply that individuals' views on the acceptability on a given practice will change once they think that others have labeled it as unacceptable. To test this possibility, I asked respondents whether they viewed subjecting prisoners to solitary confinement as immoral.
- **International Standards**: One hypothesis, specifically put forward by Simmons (2009), is that information on prior ratification of human rights treaties creates an expectation gap.[70] That is, finding out that the government has made a pledge to honor a right internationally, but it is not actually providing that right domestically makes respondent crave that right more. In an attempt to test this possibility, I simply asked whether respondents believe that the U.S. treatment of prisoners should confirm to international standards.

Using mediation analysis methods developed by Imai et al., it is possible to directly test the influence that a hypothesized causal mechanism has on a treatment effect.[71] The basic intuition of these methods is that they use a two-stage process: first, estimating the impact of the treatment on a potential mediator; and second, estimating the effect of the treatment and mediator on the dependent variable of interest (in this case, support for solitary confinement reform). In other words, using this process, it is possible to test the extent to which information on international law changed public opinion as a consequence of changing respondents' views on one of these three mechanism questions.

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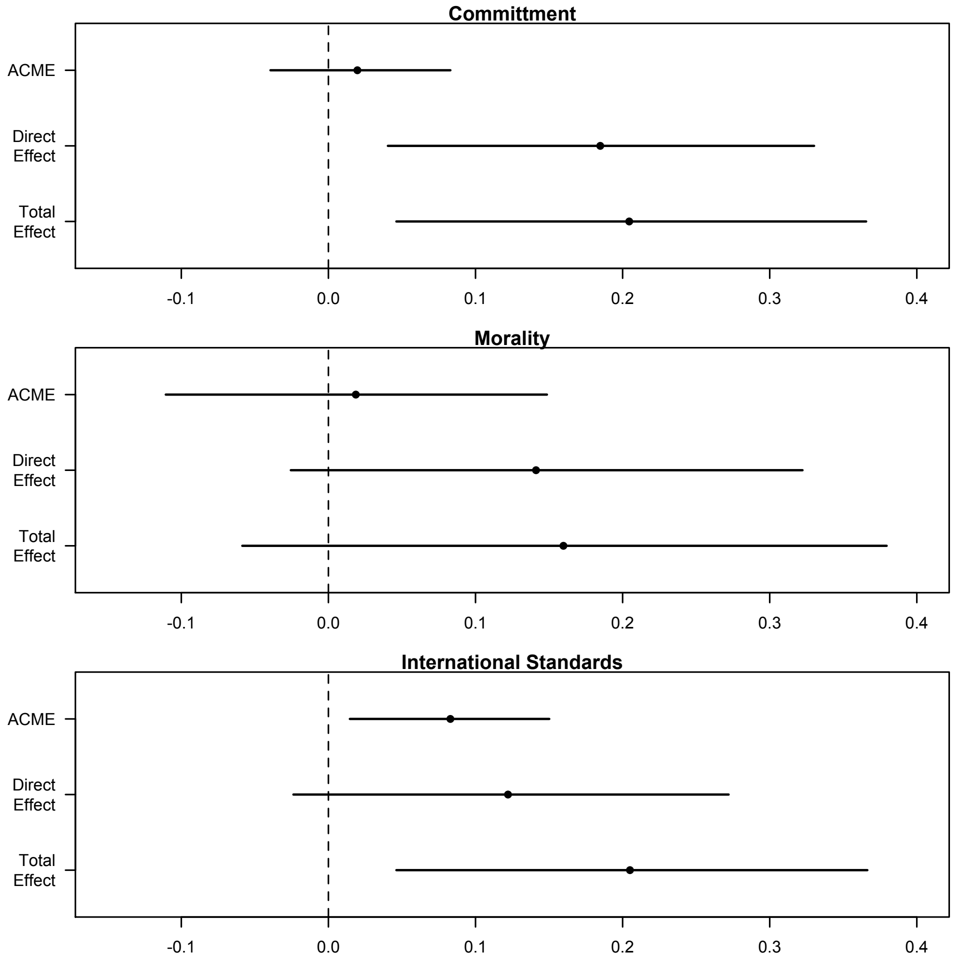
[70] Simmons 2009,136.

[71] Imai et al. 2011; Imai et al. 2010.

Figure 4.3: Mediation Effects of Causal Mechanisms



To use this method, I first subset the data to include only respondents that received the null treatment or the international law treatment. I then used the "mediate" software designed by

Imai et al. to test the three causal mechanisms previously discussed.[72] The results of this analysis are presented in Figure 4.3. For each of the three possible causal mechanisms, the figure shows the estimated influence of the treatment that is conducted through the hypothesized mechanism ("ACME"), the direct impact of the treatment itself ("Direct Effect"), and the overall effect of these two effects ("Total Effect").

As Figure 4.3 shows, the first two hypothesized mediators did not have a statistically significant impact on the influence of international law to changes in public opinion. The third mechanism, however, did have an effect. The international law treatment mediated through the international standards mechanism had a 0.1 effect on the overall outcome. This result is statistically significant at the 0.05 level. The overall treatment effect for the international standards mechanism is 0.21, which is also significant at the 0.05 level. This result suggests is that at least part of the reason that information on international law changes public opinions on domestic policies is by changing attitudes about what the international standard of human rights practices is in a given issue area. In other words, being told that the use of solitary confinement violated international law made respondents believe that the U.S. practice was not consistent with international human rights standards, which made those respondents more likely to support reform to the current human rights practices.

4.5 Conclusion

One of the most important questions in international law is whether ratification of international human rights treaties can actually cause countries to change their human rights practices. Recently, research has started to show that ratification of these agreements likely does have a causal impact on the human rights practices of states that are at least partially democratic. As this paper has argued, however, these studies have been unable to directly test exactly *why* ratification might alter human rights practices in democratic states.

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[72] Imai et al. 2010. I specifically used the "mediate" package for R.

This paper is an effort to start answering that question. The results of this survey show that information on prior treaty ratification has a statistically significant impact on public opinion. Moreover, this study also showed that it is not the case that any arguments would have this same impact on opinion—a nearly identically worded treatment that appealed to human rights without mentioning treaty ratification did not sway opinions at all. This suggests that ratification of human rights treaties can change public opinion for reforms, which in turn improves the political climate for activists and politicians that hope to push policy changes consistent with those prior commitments. In other words, ratifying human rights treaties may help pave the way for later policy reforms.

Of course, these results should be viewed in context. First, this survey only tested how information on the ratification of human rights treaties changed public opinion in one substantive issue area—solitary confinement reform. It is thus obviously possible that information on prior treaty commitments might have smaller or larger treatment effects in other issues areas. Second, it is important to remember that America might not be a typical case. Other countries might have divergent changes in public opinion as a consequence of signing prior human rights agreements. For example, Simmons has suggested that the ratification of human rights treaties has the largest impact in states that are transitioning democracies (and not stable democracies like the United

States).[73] This might suggest that the treatment effects found in this experiment might actually be larger if it were conducted on a sample of respondents from a country that is transitioning to democracy. Third, it is important to note that modest changes in public opinion do not automatically result in changes in public policy. This study has not demonstrated the entire causal chain between signing human rights treaties and improved human rights practices; it instead has simply tested one link in that chain.

With those caveats in mind, this study still makes a small but important contribution. If information on human rights treaties can change public opinion where generic appeals to human rights cannot, the trend of increased legalization of human rights over the last 60 years may have done more to improve human rights practices than many scholars have given them credit for. This is not because ratifying human rights treaties is a panacea that automatically results in improved human rights practices. It is instead because treaty ratification helps to improve the baseline political support for activists and politicians striving to change public policies. In other words, ratification of human rights treaties matters—but only if that ratification is used as a tool in a broader mobilization effort for change.

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[73] Simmons 2009, 360.

Chapter 5

Supplying Compliance: Domestic Sources of Trade Law and Policy

5.1 Introduction[1]

Consider three examples of American actions to cure WTO violations:

- In 1996, the WTO Dispute Settlement Body finds the American ban on imported shrimp caught without turtle-safe nets to be a violation of the General Agreement on Tariffs and Trade. From 1996 through 1998, the State Department cures the violation by engaging in extensive negotiations with the affected states and by revising agency regulation regarding the certification of acceptable shrimping methods.
- In 2000, the WTO Dispute Settlement Body finds the American tax treatment of domestic exports to be a prohibited subsidy. In 2004, Congress complies with this decision by repealing the relevant sections of the tax code.
- In 2003, the WTO Dispute Settlement Body finds the American use of safeguard actions against imported steel to be a violation of the Safeguard Agreement. President George W. Bush complies by withdrawing the safeguard through an executive order.

In theorizing about international law, much of the focus has been on the "demand-side" of state compliance: how can the foreign governments, international institutions, NGOs, or domestic interest groups pressure a government to respect their treaty commitments? The national government is modeled as a transmission-belt, aggregating national and international demands and producing a policy outcome.

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[1] This paper was co-authored with Rachel Brewster, Professor of Law, Duke Law School.

As the examples above illustrate, however, compliance often requires action from different parts of the domestic government. Although the United States—as a nation—is responsible for all violations of international law, *who* within the state needs to take action to cure a violation depends on the specific measure. Different political actors within the U.S. government can be the "suppliers" of compliance depending on what trade measures need to be altered. As a result, the focus on the demand-side view of compliance by scholars has neglected the influence of domestic institutions in supply policy outcomes and obscured the effects of domestic governmental politics on patterns of compliance with international law.

Our study is the first effort to illustrate how examining the supply-side is necessary to understanding states' compliance behavior. Specifically, we analyze American compliance with legal challenges at the World Trade Organization (WTO). The WTO treaty agreements include a dispute settlement system that provides third party adjudication of trade law violations, and the WTO has the authority to approve retaliation if a respondent state fails to comply with an adverse judgment. The U.S. government's supply of compliance for WTO disputes varies for different trade issues. For some issues, such as the application of safeguards, the executive has nearly complete discretion in resolving the dispute. Other issues require executive agencies to amend federal regulations. Finally, other topics, such as agriculture subsidies or intellectual property law, demand legislative action.

We hypothesize that which actor is required to respond to a WTO violation matters because government institutions are differently situated in terms of their decision-making procedures, their engagement in foreign affairs, and their constituencies. These factors make the executive branch more likely to comply and act quickly to do so than Congress. First, it is simply easier for the executive branch to act. Complying with an adverse WTO ruling requires an affirmative change of national policy. All else being equal, the executive branch can act faster than Congress because it has fewer procedural hurdles. Second, the executive branch has a greater interest than Congress in maintaining good international relations on a day-to-day basis. The U.S. government's refusal to comply with an international court's decision may harm the executive branch's effectiveness in foreign affairs—lowering the executive's perceived job performance—but voters may view the same behavior by members of Congress positively.

To test this theory, we examine how the U.S. government alters national policy in response to the cases initiated within the WTO dispute settlement procedure. To do so, we have compiled the first data set of the policy actions the U.S. has taken in response to other states' requests for DSU consultations. After controlling for important characteristics of the state filing the request and the importance of the affected domestic industry, we are able to demonstrate that who within the government supplies compliance is the *best predictor* of whether and when the U.S. government complies with WTO rulings. The need to include Congress in the compliance process both decreases the likelihood of compliance and delays compliance more than any other factor.

This finding has important implications for international economic law and international law more generally because it suggests that there may not be a unitary model to explain states' responses to international law. Instead, this work demonstrates that U.S. compliance with trade obligations varies depending on which domestic political actors are engaged in the policy process. As such, theoretical approaches that treat states as unitary actors, even when responding to international court judgments against the state, obscure important variances in policy responses that are critical to understanding compliance outcomes.

The discussion proceeds in five parts. Part 5.2 discusses dispute resolution procedures at the WTO and previous scholarship that has studied compliance with WTO decisions. Part 5.3 examines U.S. compliance procedures before developing the hypothesis that the executive branch should be expected to comply with adverse WTO decisions more often and more quickly than Congress. Part 5.4 describes the data that we have collected to test this hypothesis, and Part 5.5 presents our empirical results. Part 5.6 discusses the implications of these results to compliance studies and WTO dispute resolutions specifically.

5.2 Background on Compliance with the WTO Dispute Settlement Process

Compliance is a major concern in any study of international law. In a system without central enforcement where states rely on self-help mechanisms, the willingness of states to comply with international rules that go against their perceived self-interest (immediate or otherwise) is always in question. Consequently, the field of international law is highly focused on the question of why states comply and how to increase compliance. This is not only true of international law generally, but international trade law specifically. This section explains the WTO dispute resolution system, and then discusses existing scholarship that has sought to explain what drives compliance with WTO decisions.

5.2.1 Litigation at the WTO

The WTO has one of the most well known systems of state-to-state adjudication.[2] In the Dispute Settlement Understanding (the agreement creating the WTO dispute resolution process), the member states of the WTO created a quasi-judicial system that granted the Dispute Settlement Body (DSB) compulsory jurisdiction over member states' disputes.[3] If one member alleges that another member is violating its WTO trade obligations, the injured member can bring its complaint to the DSB.[4] If a member files a formal complaint, the parties are required to engage in consultations to attempt to settle the dispute for at least sixty days. If consultations fail, the DSB begins the two stage adjudicative process. The parties can mutually agree to suspend or end the case at any point in the process.

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[2] For an overview of the WTO dispute resolution process, see Posner and Sykes 2013, 281-287.

The first stage includes a "trial" phase where a panel of ad hoc arbitrators, chosen by the parties, hears evidence from both parties and issues a ruling. One or both of the parties can appeal the arbitrators' decision on issues of law to the Appellate Body. If the panel decision is not appealed, then the DSB votes to adopt reports based on a "reverse consensus" rule. That is, unless there is a consensus in the DSB to reject the panel's report, the report is adopted. As of this writing, the DSB has never failed to adopt a report using the reverse consensus rule and DSB adoption of reports is generally considered to be a near "automatic" process.[5]

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[3] The DSB consists of all of the member states of the WTO. The DSB grants requests for a panel hearing and adopts the decision of the arbitrators or the Appellate Body by reverse consensus. Reverse consensus means that the panel is authorized and the report is adopted unless no state—including the state that is requesting the panel or has won the legal case—supports the motion.

[4] Technically, there can be either a violation of the WTO agreements or a "nullification or impairment" of the complaining member's benefits under the agreement. We use the term violation to refer to both for ease of exposition. The DSU gives the DSB compulsory jurisdiction for all disputes concerning the WTO Agreements included in Annex 1. There are some WTO Agreements over which the DSB does not have jurisdiction.

[5] Goldstein and Steinberg 2008, 266.

If the panel decision is appealed, then the case is referred to the Appellate Body, a standing body of seven members, who hear appeals in groups of three.[6] The Appellate Body hears appeals only on questions of law and will amend the reasoning of the panel if it finds that the panel erred in its interpretation of the WTO Agreements. The Appellate Body's report is also adopted by the DSB on a reverse consensus basis. If the adopted report finds that the respondent state is in breach of its WTO obligations, than the DSB will recommend that the respondent state brings its measures into compliance with the WTO Agreements and within a reasonable period of time—typically 12 to 15 months.

The adoption of the panel or Appellate Body report is often not the end of litigation process for specific trade disputes. Members are increasingly engaging in "compliance proceedings" after the initial adjudication. These proceedings address the question of whether the respondent state's changes to the challenged policy are sufficient to cure the violation. If the complaining government believes that the violation has not been cured, then it can request a compliance panel (an Article 21.5 panel) to evaluate the sufficiency of the respondent state's actions. Like the merits panel report, either party can appeal the panel report to the Appellate Body. The DSB adopts these reports on a reverse consensus basis.

The second stage of the litigation process is the remedy stage. If a respondent state fails to cure violations of WTO rules within the reasonable period of time set out by the DSB, then the complaining state can request that the DSB authorize it to suspend trade concessions to the respondent state.[7] The respondent state can request a panel to arbitrate the maximum extent and the possible forms of the suspension.[8] The parties cannot appeal this ruling. The DSB implements the panel's ruling by authorizing the complaining government to suspend concession up to the level determined by the panel.

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[6] A three-judge panel of Appellate Body members hears the appeal. The WTO agreements have no provision for the Appellate Body to sit en banc. Appellate Body members serve for a four-year term that may be renewed by the membership once.

The retaliation does not make the complaining party whole. The ability to suspend benefits is often economically costly to the state (i.e. raising tariff levels can be expected to harm the economies of the complaining and the respondent state although some political benefits may accrue). In addition, the remedy offered by the WTO is only prospective. The retaliation authorized by the DSB (and determined by the panel) is based on the complaining state's *current level* of injury from the respondent state's policy, meaning the complaining state cannot retaliate for any loss of benefits from the violating policy that occurred before the remedy panel's hearing. This is true even if the respondent state maintained the policy well after the DSB's reasonable period of time to comply with the rule expired. This system creates an incentive for respondent states to drag their feet and extend the litigation process for as long as possible.

The nature of litigation at the WTO creates two different measures regarding compliance with the WTO process. The first measure is whether the member state formally comes into compliance at all. This is the more straightforward inquiry of whether the state ultimately decides to alter the policy to conform to WTO legal obligations. The second measure is how long it takes the state to comply. Even if the state ultimately decides to amend its measure, how long the respondent state maintains the illegal policy after the adverse DSB decision (the ruling on the merits) is important in determining the quality of the state's compliance. Because the WTO system only permits a prospective remedy at the end of the litigation process, the timing of compliance is particularly important in trade law.

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[7] The authorization is specific to the complaining state (or states, if there is more than one complaining party). Third parties to the litigation (or other states that are injured by the respondent's violation) are not permitted to suspend trade benefits.

[8] Article 22.3 discusses when different forms of retaliation are permitted. Sectoral retaliation refers to the sectors in the GATS and TRIPS agreements (all goods are considered to be in the same sector under the GATT agreement). Across agreement retaliation refers to complaining state actions that withdraw benefits in agreement (GATT, GATS, or TRIPS) other than the one the violation was in.

5.2.2 Research on Compliance with WTO Decisions

In the last decade, there has been a growing body of literature trying to explain when and why states comply with adverse decisions from the WTO. Perhaps the most developed are of this scholarship examines whether the existence and the design of third-party dispute resolution institutions improve compliance rates or otherwise enhance the effectiveness of the treaty regime.[9] For instance, Daniel Kono finds that the existence of a dispute resolution panel improves trade liberalization between states but that the form of the panel—more or less legally binding—does not matter.[10] Marc Busch and Eric Reinhardt find that the institutional shift from GATT dispute resolution to WTO dispute resolution does not improve compliance rates for GATT issues and does not improve compliance in EU-US disputes.[11] Eric Posner and John Yoo argue that the design changes between the GATT to WTO have decreased compliance rates because more independent adjudicators make decisions that are less acceptable to the disputing parties.[12] By contrast, Barnhard Zangl argues that the GATT to the WTO shift has increased compliance with rulings on EU-US disputes and intra-OECD disputes.[13] Scholars also argue that more legal dispute resolution institutions should increase compliance by raising reputational costs on breaching states and lowering the reputational costs of sanctioning on complaining states.[14]

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[9] There is a rich literature on the question of whether to initiate a WTO dispute. See Davis 2012; Sattler and Bernauer 2010; Davis and Bermeo 2009; Guzman and Simmons 2002. We do not address issues of initiation here.

[10] Kono 2007.

[11] Busch and Reinhardt 2003 and 2003a. Busch and Reinhardt find that the WTO dispute resolution system has an overall higher rate of compliance than the GATT dispute resolution system, but they argue that this is due to the inclusion of the TRIPS and GATS agreements (and the high levels of state compliance with DSB decisions on these agreements) in the WTO.

It is notable that all of the current approaches examine compliance questions at the state level. Intra-state variation in compliance levels is not measured even if there are theoretical reasons to expect that various governmental bodies would respond differently to international adjudicative decisions.[15] In WTO-specific and more general compliance studies, the greatest focus by far is on the demand side of compliance—that is, the pressures on the breaching state from foreign governments, NGOs, or domestic interest groups to conform its actions to be consistent to international law and how institutional design can maximize this pressure. Only to a much lesser extent does the field of international law also examine at the supply side of compliance—the internal state policy process of curing breaches of international law. The only work that currently incorporates domestic institutions are studies of the influence of domestic courts in enforcing international or supranational judicial decisions.[16] These studies actively discuss domestic institutions, but decision-making is moved out the hands of elected officials. In addition, the WTO agreements and DSU rulings are not judicially enforceable under US or EU law, and thus, domestic courts are not an active part of the compliance process with global trade issues in the two largest trading states.[17]

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[12] Posner and Yoo 2005.

[13] Zangl 2008; Zangl et al. 2011.

[14] Abbott and Snidal 2000; Helfer and Slaughter 2005; Guzman 2008; Thompson 2009. Most scholarship on how best to reform the WTO comes from legal scholars who focus on the remedies available under WTO rules. These recommendations are also pitched at the state level and include calls to increase the level of retaliation available for breach (Davey 2009), to allow for the collective application of retaliation (Paulywn 2000), and monetary damage awards (Bronkers and van den Broek 2005).

[15] Goldstein 1996; Brewster 2006.

5.3 Developing a Supply Side Theory of Compliance

As the last section illustrated, discussions of compliance with international trade law (or international law in general) focus on the demand side of compliance, and as a result, treat the state as a unitary actor.[18] An alternative view of the state—from the supply side—would focus on the role that different government institutions have on compliance. It is our contention that this emphasis on the supply side rounds out the compliance picture. Government institutions mediate competing policy demands from domestic and foreign groups, but instead of viewing the government as a transmission belt for interest group pressure, we view political actors as having unique concerns and different abilities to act. As we argue, shifting compliance decision between political bodies within the same state can lead to different compliance outcomes. We explore this idea by examining U.S. Compliance with adverse WTO decisions. In this part, we first explain the U.S. compliance process, before turning to developing an argument for why the executive branch should be expected to comply more often and more quickly than Congress. After doing so, we outline the advantages and limitations of examining U.S. responses to WTO decisions to gain insight to the influence of domestic institutions on compliance outcomes.

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[16] Burley and Mattli 1993; Helfer and Slaughter 1995; Alter 1998.

[17] Bronkers 2005.

[18] Goldsmith and Posner 2005; Busch and Reinhardt 2003; Guzman and Simmons 2002.

5.2.1 The U.S. Compliance Process

Given the distribution of powers in the American system, who within the state has the authority to comply with the WTO ruling varies depending on the challenged policy. There are several actors who, for different types of policies, will have the power to comply with WTO law. Two actors are discussed below, although more can be relevant.[19]

The Executive Branch. The president alone—that is, not acting through an administrative agency—has the power to comply with some rulings against the U.S. Where the president has the independent authority to act, either because it falls within the executive branch's constitutional powers or because of delegated power from Congress, the president can act unilaterally to supply compliance. For instance, the president acting alone can cure disputes regarding safeguard action. A safeguard is a domestic trade remedy that allows a state to raise tariffs on imports when there is an unexpected surge in imports that injures or threatens to injure a domestic industry.[20] In previous trade legislation, Congress has delegated to the president the exclusive power to apply safeguard measures and to withdraw them.[21] For instance, President Bush provided the steel industry with safeguard protection in 2002, and President Obama similarly raised tariffs on imported tires in 2009. In both instances, the International Trade Commission—a bi-partisan six member independent agency—had recommended that safeguards be imposed, but the president has the final decision of whether to impose the safeguard action and the level of protection to grant.[22] The decision to withdraw the safeguard is also vested exclusively in the president's discretion. When President Bush withdrew the steel safeguard measure in 2003, he could do so without the consent of Congress and without an agency determination that protection was no longer warranted.

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[19] Congress and the executive branch are two leading actors in compliance with international trade law, but other governmental entities can have supporting roles. The U.S. federal court's interpretation of statutory language or rulings on the limits of agency rule-making can either put the U.S. in violation of the WTO agreements or can be a source of compliance. Sub-national state governments' tax or subsidy programs can also cause and resolve trade complaints.

[20] Domestic trade remedies are trade actions imposed by the national authorities of the state. Three major actions are safeguard actions, anti-dumping duties, and countervailing duties. States are not required by the WTO to adopt domestic trade remedy rules, but WTO rules govern the application of these remedies if a state chooses to adopt them.

Other trade issues are handled primarily by administrative agencies. These include anti-dumping and countervailing duty measures, as well as host of regulations that have international trade effects. For instance, the Commerce Department issues rules regarding the methodology for dumping and countervailing duty determinations that have led to adverse DSB decisions against the U.S.[23] The authorizing statute does not require a specific methodology and the Commerce Department has significant discretion in developing these rules. Here, the most immediate source of compliance is the Commerce Department, which could alter its methodologies through its internal rule-making procedures. Congressional action is another source of compliance but legislation is not strictly necessary for the violation to be cured.

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[21] Section 201 of the 1974 Trade Act.

[22] A majority or tie vote of the International Trade Commission is required before the president can impose a safeguard action. Once the safeguard is authorized, the president has the sole authority to decide whether to impose the safeguard, how high the tariff should be, and what markets to exclude from the safeguard. The president can unilaterally alter or eliminate the safeguard action at any time. Section 204 of the 1974 Trade Act.

[23] We include a dummy variable for trade remedy cases to make sure that domestic trade remedy issues are not driving our results. This issue is discussed more in Part 5.4.

Other administrative agencies can also be the source of violation and compliance. As discussed in the introduction, the State Department altered its internal rules for implementing a ban on imports of shrimp caught without turtle-exclusion devises. The relevant policy—the ban on the import of certain shrimp—was mandated by statute, but the State Department's regulations implementing the statute were the source of the trade violation. Other federal agencies including the Environmental Protection Agency and the Agriculture Department have also created and cured international trade violation through their rulemaking processes.

Finally, the line between agency action and sole executive action can sometimes be blurred. The executive branch is structured as a hierarchy. Although agencies are delegated power by the legislature, monitored by congressional committees, and have to comply with rule-making procedures, the president has the power to appoint and dismiss top agency policymakers. Agency heads are members of the president's cabinet and presumably follow the president's policy lead. This relationship makes parsing agency action and presidential action difficult. In addition, the presidents are entering into sole executive agreements with foreign governments to alter administrative agency behavior. For instance, the president has entered into international compacts regarding the long-standing Softwood Lumber dispute (addressing countervailing duty issues) and controversies involving the methodology for calculating anti-dumping duties. As sole executive agreements, these compacts do not need any legislative approval to enter into force. Sole executive agreements blur the line between executive action and agency action because they involve issues within the agency's policy scope but are addressed in executive agreements. As a result, we group the administrative agencies and sole executive policies into one category of executive branch action.

Congress. When the challenged policy is set by the text of a statute, then compliance requires engaging the domestic statutory process. Here, the answer to the question of "who complies?" is multiple actors: bicameral majorities when there is presidential support of the legislation or veto-proof bicameral majorities when there is not presidential support. What issues require congressional attention is defined by the nature of the trade law allegation and the domestic governance system. If Congress has previously delegated policy power to an administrative agency and the agency has issued a ruling that creates the violation, then congressional action is not necessary to cure the violation. By contrast, if the violation requires a change to the statute, then Congress must act directly. Trade topics that generally need direct congressional action include intellectual property rules, tax law, and agriculture subsidies.

The topics that require congressional action are not necessarily more politically sensitive or considered higher stakes issues. Congress delegates to the executive branch the ability to make determinations concerning sensitive topics, such as relations with foreign nations, 'unfair trade' practices that threaten import-competing firms (and local employment), as well as environmental and labor standards. Instead, there generally is a need for congressional action when Congress has not delegated implementation of a policy to an agency—as is often the case in intellectual property—or when the existence, not the implementation, of a government policy is challenged. Thus, some very small stakes issues require congressional action—such as, a dispute over a single trademark—while some high stakes issues—such as, disputes over environmental policy—are delegated to the executive. We discuss the nature of disputes that require congressional action in more depth in the empirical sections.

The discussion of the domestic government's supply of compliance raises an issue that can usefully be addressed here. Compliance with international trade rules can almost always be achieved through a statutory enactment. Except for rare constitutional cases, such as individual rights issues or federalism issues (which rarely arise in international trade), Congress has plenary power over foreign commerce. Thus, any government act that violates international trade law can be corrected through statutory means. For instance, Congress could amend American anti-dumping rules to prohibit the use of certain methodologies (or anti-dumping duties entirely) even if the Commerce Department refused to amend its internal rules. While this is true, collapsing the existing domestic political system of compliance into the statutory process ignores sources of compliance that may be far easier to achieve than changes to federal legislation. The federal legislative process represents a very high bar in terms of the difficulty in achieving policy change. Significant political capital is necessary to get an issue on the legislative agenda and there are multiple veto points in the legislative process. In addition, to view all compliance as a matter of legislation ignores long-standing governmental practice regarding compliance with international rules.

5.3.2 Domestic Institutions and Rates of Compliance

As we have just explained, which branch of the U.S. government has the authority to comply with adverse WTO decisions varies based on the issue. This does not mean, however, that both the executive branch and Congress should be viewed as equally likely to comply (as unitary actor models assume). Instead, we hypothesize both that the executive branch is more likely to comply, and also that it will do so more quickly.

We hold this view for several reasons. First, the executive branch is uniquely concerned with foreign policy. Compared to Congress, the executive branch is responsible for maintaining good foreign relations and its performance is based more on foreign policy success. While there are foreign relations committees, members of Congress are less engaged in international affairs. Second, domestic institutions have varying capacities to act and act quickly. The executive branch has fewer veto players than congressional action does. The president can unilaterally act in some areas of foreign affairs and can form sole executive agreements that regulate some elements of administrative law. Executive agencies have more complicated procedures for altering regulations and these regulations are subject to legal challenge, but agencies act as a part of the hierarchical structure of the executive branch. By contrast, decision-making in Congress is more difficult. Bicameralism and super-majority voting rules in the Senate establish barriers to altering status quo policies.

Applied to WTO litigation, this model predicts that compliance will be lower if congressional action is necessary to supply compliance. Congress has fewer interests in having high levels of compliance with international law to maintain good foreign relations than the executive branch. In addition, Congress has a more difficult time acting to change established policies than the executive branch. All else being equal, we expect that the executive branch will comply with adverse WTO decisions more often and in a shorter period of time than Congress will.

5.3.3 Advantages & Limitations of Our Approach

Before proceeding to the discussion of our data collection and results, it is worth noting that our decision to study the supply side of compliance by focus on U.S. compliance with adverse DSB decisions has both advantages and limitations. An advantage of studying compliance in the WTO setting includes the existence of a dispute resolution system with compulsory jurisdiction. This resolves the problem of auto-interpretation in international law: governments will frequently dispute whether a violation of international law exists and, without a third party adjudicator, it is hard to collect an objective sample of "violations." Compulsory jurisdiction also solves a selection bias issue. If dispute resolution is voluntary and the parties only agree to adjudicate "politically easy" cases, then the sample of decisions may be biased (because the "politically hard" cases are never heard). The results of these studies may be overly optimistic in terms of states' willingness to comply with adjudicatory rulings. The WTO's compulsory jurisdiction decreases this selection bias because the respondent state need not agree for the adjudicatory system to proceed. The WTO dispute resolution additionally has one of the high caseloads for an international dispute resolution system, so there are a sufficient number of cases to provide a meaningful quantitative analysis.

Of course, our sample of WTO cases may still have problems with selection bias. Some trade disputes may be resolved through diplomatic means before a request for consultation is ever filed at the WTO. If so, then these cases do not become part of our data set. As a result, our data set may be biased in the sense that the dispute has to be difficult enough to resolve that it cannot be handled diplomatically. In addition, some disputes will not involve a sufficient quantity of trade to be worthy of the expense and energy of international litigation. Some states may also not have the financial or legal capacity to meaningfully engage the WTO system, and thus, will engage the dispute resolution system less frequently than states with greater financial and legal capacities. This tendency to avoid WTO litigation may also be heightened when the U.S. is the respondent state because developing countries may fear a diplomatic backlash to trade litigation. On the whole, the data set is not free of selection bias concerns, but it provides a good sample of cases involving a variety of trade issues brought by a wide range of complaining states.

Our study also focuses exclusively on the U.S. as the respondent in WTO litigation. We do so because we are interested in how the domestic institutional source of trade policy influences a state's compliance behavior, and therefore, we must to open the "black box" of the state's decision-making. This is a state specific inquiry. We have chosen to look to the U.S. because it is a frequent party to trade litigation and we are familiar with its trade policy processes. The inquiry is highly relevant to understanding patterns of trade law adjudication—the U.S. is one of the most common defendants at the WTO—but this comes with some limitations. Because our study focuses on one governmental structure, it may not be generalizable to all states. In addition, the economic power of the U.S. may provide the government with a greater capacity to resist international calls for compliance; thus its rates of non-compliance may be higher than in other states.[24] However, understanding how international law and international adjudicative decisions influence policy in economically powerful states like the U.S. provides important insight into constraining effects of international law.

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[24] The American dualist legal structure may also give political actors in the U.S. government more domestic legal leeway to resist implementation of international legal rulings. We do not want to emphasize this point too strongly, however, because other more monist states also give political actors greater leeway with regards to international trade obligations. See Bronckers 2005.

5.4 Data

To test these expectations, we have built an original dataset of disputes filed against the U.S. in the WTO. Because our theory makes specific predictions on how the actor required to supply compliance influences whether and when compliance will occur, we have collected a large amount of information on each dispute that has not previously been collected or analyzed by scholars. In this section, we briefly outline the process we have used to construct the l dataset built for our project and explain the coding decisions that we made along the way. First, we outline the universe of cases that is included within our dataset. Second, we discuss the dependent variables used to test our theory. Third, we describe the independent variables that we have collected to test our theory of compliance.

5.4.1 Universe of Cases

The first decision that we made while constructing our dataset is determining which cases to include. As of December 31, 2011, there had been 113 requests for consultations filed with the WTO in which the U.S. was the respondent. It would be inappropriate to assume based on this fact, however, that the correct number of observations to look at to test our hypothesis would be 113 cases. This is because the total includes cases that were consolidated and cases where the U.S. prevailed and did not have to take subsequent compliance actions. Additionally, in many cases it would be inappropriate to include cases that were settled before litigation was completed. As a result, determining the universe of cases for our study required in-depth classification of all of the requests for consultation with the U.S. filed at the WTO.

Given these concerns, we used a three-step process to cull the cases to give us our final universe of cases. First, any disputes that were either consolidated with earlier cases or repeats of early cases on the exact topic were turned into a single dispute. For example, DS2 brought by Venezuela and DS4 brought by Brazil were consolidated into a single observation because they were consolidated during the dispute resolution process. Likewise, DS85 and DS151 were treated the same way because they were both cases brought by the European Union on the same issue. This resulted in 23 cases being removed from the initial 113 disputes.[25] Second, since the U.S. is not expected to take steps to comply in disputes that it either won or litigation is still ongoing, these disputes were also removed from the dataset. To identify these cases, we relied on a document produced by the U.S. Trade Representative Office that identified cases where the "US won on the core issue(s)."[26] This resulted in 35 additional disputes being excluded from the dataset. Third, we excluded cases that were settled without the litigation process being completed. We compiled this list both by relying on the USTR document previously mentioned[27] and the reported status of cases reported on the WTO website. This resulted in 18 additional cases being removed from our dataset.[28] After these steps, we were left with 37 cases where the U.S. did not prevail on the core issue at stake in the dispute that formed the primary universe of cases for our empirical tests. Table 5.1 presents a breakdown of the 113 disputes filed with the WTO prior to December 31, 2011.

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[25] For a discussion of how consolidated cases and cases brought by multiple complaints were treated for coding, see notes 38 - 43. For a discussion of an alternative approach that we took to address these cases, see notes 65 - 67.

[26] *Snapshot of WTO Cases Involving the U.S.*, U.S. Trade Representative, December 21, 2011.

[27] Id.

[28] We still collected compliance information on all of the cases that were settled, which we used to provide a robustness check to our primary results. See text accompanying notes 63 - 64.

Table 5.1: Breakdown of the Universe of Cases

Category of Cases	Total Cases
Consolidated or Repeat Cases	23
US Prevailed on Core Issue / Monitoring in Progress	35
U.S. Settled / No Longer in Progress ("Settled Cases")	18
U.S. Did Not Prevail on Core Issue	
Dispute Resolved ("Compliant Cases")	24
Dispute Not Resolved ("Non-Compliant Cases")	13
Total Cases	**113**

5.4.2 Dependent Variable

After establishing our universe of cases, the second task was to determine the relevant dependent variables to test our theory. In our cases, there are two outcomes that we were primarily interested in: (1) whether compliance occurred; and (2) how long it took compliance to occur.

The first task was to determine how to code whether compliance has occurred in a given dispute. Determining how to do so was made easier by a Congressional Research Service report that documents the status of the WTO disputes that the U.S. has been part of at the end of the year. The latest report was published on January 25, 2012.[29] That report listed 13 cases where the U.S. is currently not in full compliance with the WTO's decision.[30] These cases were thus coded as "1," whereas the other 24 cases that the U.S. lost but did not settle were coded as "0". This then became the dependent variable for the results presented in Part 5.5.1.[31]

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[29] Grimmett 2012.

[30] These thirteen cases are: DS160, DS176, DS184, DS217 & DS234, DS267, DS285, DS294, DS322, DS344, DS350, DS379, DS 382, and DS404.

The second task was to determine how to code the length of time it took for compliance to occur. To do so, we collected data on the date that each conference request was filed.[32] We then also collected data on the date that the U.S. complied by curing the violation found in the WTO litigation. Measuring the end date was complicated and defining an exact date proved to be difficult. In constructing this variable, we first checked reports filed with the DSB and looked for when the complainant states reported that compliance had occurred. After doing so, we then checked the Federal Register to determine the exact date that the compliance action occurred. When possible, we then used this as the end date for the total compliance time. When we could not determine a date via the Federal Register, we used the date that the complainant state reported to the DSB that the U.S. was now complaint (the sources used for each case are listed in Appendix 4.1). Finally, for cases categorized by the Congressional Research Service as not being fully complaint, we recorded the date that partial compliance occurred; for the three cases where no compliance actions had been taken, we treated these cases as censored observations.[33] After collecting an end compliance date for each case, we then calculated the number of days that elapsed between when the consultation request was filed and when compliance occurred.

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[31] The CRS report was reported before the U.S. reached an agreement on zeroing cases in February 2012. We attempt to address this issue in our section on robustness. See text accompanying notes 62 - 63.

[32] In cases were the complaint was consolidated, the earlier conference request date was used.

[33] See Grimmett 2012. The three cases where the U.S. is completely non-complaint are: DS176, DS285, and DS379.

This served as the dependent variable for the results reported in Part 5.5.2. Table 5.2 presents summary information for the dependent variables.

Table 5.2: Summary of Dependent Variable Collection

Category	**Total Cases**	**Total Compliance Time (days)**
Compliant Cases	24	1,022
Non-Compliant Cases	13	2,254
Overall	**37**	**1,455**

5.4.3 Independent Variables

The third step that we took to construct our dataset for this project was to collect a range of independent variables that allowed us to operationalize and test our theory of compliance along with competing explanations for if, and when, the U.S. complies with adverse WTO rulings. We did this by collecting independent variables that capture four features of each dispute.

First, we collected two variables that attempt to capture relevant domestic political features of each dispute. The first variable, *Congress Required*, is whether congressional action would be required to bring the offending measure into compliance. This is the most critical variable to our paper, and was designed to help us test our theory that the actor expected to supply compliance is a major factor in determining how and when the U.S. takes steps to comply with WTO decisions. This is a dummy variable coded as 1 if Congress would have to take a vote to remove or change legislation to remedy a violation alleged in the initial complaint. The variable was coded as 0 if the U.S. could become complaint by either allowing a measure to expire, or by the president or an executive agency taking unilateral action.[34] The second variable in this category is whether there was *Divided Government* at the time a complaint was filed. This is a dummy variable that was coded as 1 if the president's party did not control both houses of Congress. The justification for including this variable in our analysis is that there is evidence that divided government influences the American patterns of adjudication in the WTO.[35]

Second, we collected two variables that are designed to capture the relationship between the U.S. and the complainant(s). The variable *USA Exports* attempts to capture the trading relationship between the two countries.[36] The variable is a natural log of the total value of the exports from the U.S. to the complainant's country in the year the conference request was filed.[37] In cases of multiple complainants, the total value of the exports for the complainant countries was added together.[38] Additionally, a dummy variable was coded for whether the U.S. has a *Formal Alliance* with any of the complainant states. This variable is included because there is evidence that alliances influence the likelihood of trade disputes in the WTO.[39] A dispute was only coded as "1" if one of the complainant countries had a "Type 1" alliance according to the Correlates of War dataset, which signifies that the U.S. has a formal military alliance with one of the countries that initiated the dispute.[40]

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[34] This variable was coded blind based on the content of the initial complaint. The result was that seven cases were coded as requiring Congressional action: DS108, DS136 & DS162, DS160, DS176, DS217 & DS234, DS267, DS285, and DS152. For a discussion of an alternative approach used to code this variable, see notes 57 - 61.

[35] See Davis 2012, 63. Davis's evidence suggests that divided government influences the decision to bring WTO disputes as a complainant, but her basic argument that constraints on the executive make negotiations more difficult would suggest it would be more difficult to quickly settle disputes as the respondent.

[36] We did not include an imports variable in our analysis because the correlation with exports was 0.95. Substituting imports for exports does not change our results.

[37] This source for this data is the U.S. Department of Commerce, Bureau of the Census, Foreign Trade Division, Trade Flow Data for 2011.

[38] For a discussion of an alternative approach used for cases with multiple complainants, see test accompanying notes 65 - 67.

Third, we collected three variables that was designed to capture the relevant characteristics of the country, or countries, that initiated the dispute. The natural log of the country's *GDP Per Capita* was recorded for the year that the request for consultation was filed.[41] Additionally, the natural log of the *Population* was recorded for the year that the request for consultation was filed.[42] Finally, as a measure of the complainant countries regime, we use the country's *Polity Score*. This is a measure of whether a country is autocratic or democratic on a scale of -10 to 10. This variable is based on the "polity2" variable from the Polity IV project.[43]

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[39] See Davis 2012, 92-100. It is worth noting that there are many other recent studies on WTO disputes that do not include a variable for alliances between dyads. Id. at 93. We believe, however, that after Davis' research it is appropriate to include this measure.

[40] Gibler and Sarkees 2004. It is worth noting that we have elected to use the "COW" Alliance data set as opposed to the "ATOP" dataset. Although the ATOP dataset was used by Davis, the ATOP data is only available through 2004. Davis 2012, 94. In contrast, the COW data is extended to 2008, and thus covers a greater portion of our sample. See Gibler 2009.

[41] This data is from the World Bank Development Indicators. Since Taiwan is not included in the World Bank data, Taiwan's GDP Per Capita was taken from the CIA World Fact Book.

[42] This data is also from the World Bank Development Indicators.

[43] Marshall and Jaggers 2011. The European Union was given a value of "10" in all years. Antigua and Barbuda are not included in the Polity IV dataset, but were coded as 5 based on a value of 4 in the Freedom House Political Freedom Index (which translated to a polity score of 5 for other countries with the same Freedom House score). Disputes with multiple complainants had their polity score averaged.

Fourth, we collected two variables that capture the characteristics of the individual dispute.[44] For the first, we coded whether each case was a *Trade Remedy Case*. Disputes were coded as Trade Remedy cases if they were classified by the WTO for being about anti-dumping, safeguards, or countervailing measures. For the second, we coded the *Contributions* made in the U.S. by interest groups and lobbyists representing the sector at issue in each dispute. Each dispute was coded as relating to one of thirteen sectors based on a categorization scheme developed by the Center for Responsive Politics.[45] The natural log is the total political contributions made by each sector to candidates and committees in the election cycle prior to when the request for consultation was filed.[46] Although scholars have used both political contributions[47] and sector employment[48] as measures of the political influence of industries when studying compliance with WTO decisions, we believe that using the political contributions variable is the most direct way to capture which industries will have political clout that might influence the U.S. government's compliance decisions. Table 5.3 provides a summary of the independent variables.

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[44] Unfortunately, it is not possible to directly control for the "value" of the dispute. The value of dispute is not determined during the dispute resolution process and is only set if retaliation is authorized (which happens rarely). Additionally, the parties tend to have vastly different views of the amount of trade losses—i.e. in the US-Antigua gambling dispute, the US argued that the annual value of the lost trade was $3.3 million, Antigua put it at $3.443 billion, and the panel finally awarded $21 million. See *United States-Measures Affecting the Cross-Border Supply of Gambling and Betting Services*, Recourse to Arbitration by the United States under Art. 22.6 of the DSU, Report of the Arbitrator, WT/DS285/ARB.

[45] Information on this data is available from the Center for Responsive Politics, *available at* <http://www.opensecrets.org/industries/methodology.php>.

[46] There are two types of cases for which it is difficult to classify which sector of the economy is affected. First, for zeroing cases we used "steel" as the effected industry because the underlying products were primarily forms of steel (i.e. steel bearings). Second, to provide the most difficult test for our theory, for cases that did not directly implicate a specific industry (i.e. DS108: US–Foreign Sales Corporations), we classified these disputes as being part of the sector with the highest donations in the previous election cycle.

[47] See Davis 2012, 126-127.

[48] See Hofmann and Kim 2011.

Table 5.3: Descriptive Statistics

Variable	Mean	SD	Min	Max
US Domestic				
Congress Required	0.19	0.40	0	1
Divided Government	0.73	0.45	0	1
Relationship				
USA Exports	10.40	1.95	4.85	13.30
Formal Alliance	0.70	0.46	0	1
Complainant				
GDP Per Capita	9.26	1.31	6.01	12.02
Population	18.95	1.99	11.31	21.57
Polity Score	6.82	5.15	-7	10
Dispute				
Trade Remedy Case	0.68	0.48	0	1
Contributions	18.78	0.64	17.62	19.86

5.5 Results

After building this dataset, we performed a range of statistical tests to determine whether the domestic sources of policy actions needed to bring the U.S. into compliance with WTO decisions directly influence whether and when the U.S. complied. In this section, we present the results of those tests. First, we present models that estimate the influence of the *Congress Required* variable and other variables have on whether the U.S. actually complied with the WTO's ruling. Second, we present models that estimate the influence of the *Congress Required* variable on the amount of total time that elapsed from when a conference request was filed until the U.S. came into compliance. Third, we discuss a series of robustness checks that we performed to try and ensure that our results were not merely a result of coding decisions or model dependency. All of our results provided strong support to our theory that the actor required to comply is a significant determinate of if, and when, the U.S. complies with WTO decisions.

5.5.1 Compliance

The first test of our theory that we performed is estimating the impact of whether Congress was required to act on whether the U.S. fully complied with the WTO's rulings. For this test, the number of observations was the 37 disputes the U.S. did not settle or prevail on the core issue in the dispute. Of these 37 cases, there were 13 disputes where the U.S. as not being compliant as of January 25, 2012. We then estimated a series of logit models that estimated the impact that a range of variables had on whether a case would be one of the 13 non-compliant cases.[49] Figure 5.1 presents the results of these tests.[50]

Figure 5.1 presents the simulated first differences as each variable moves from its minimum to maximum value of logit models estimating compliance with WTO decisions as the dependent variable.[51] In each graph, each line represents the point estimate and confidence interval for an individual variable included within the model. Point estimates to the right of zero means that the variable is associated with a high probability of non-compliance. Statistically significant variables are presented as solid lines, and all others are dotted lines.

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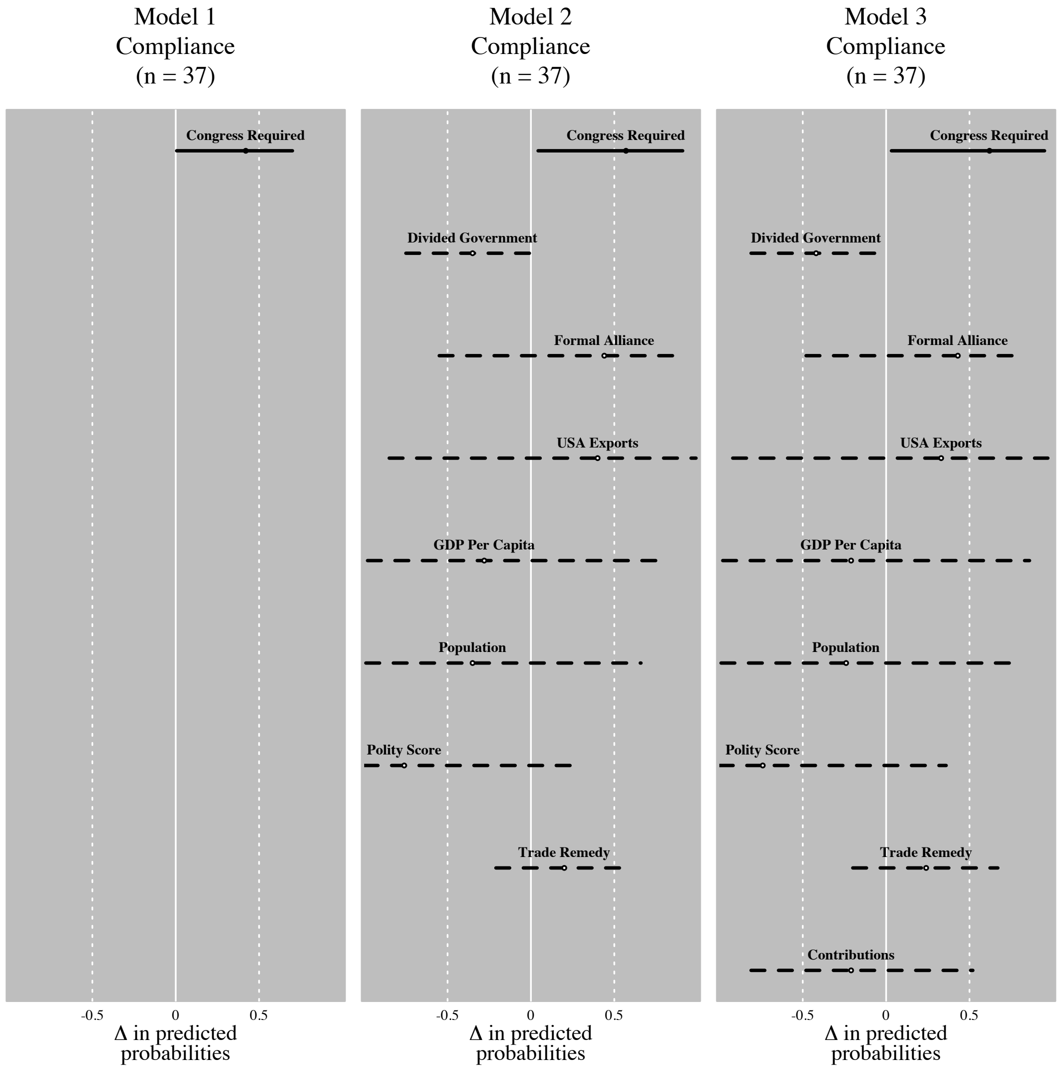
[49] Using probit instead of logit models only increases the statistical significance of our results.

[50] All tests were conducted using "Zelig." See Imai, King, and Lau 2007.

[51] For a discussion of the merits of using simulating first differences, see King, Tomz and Wittenberg 2000.

Figure 5.1: Logit Models Estimating Likelihood of Non-Compliance



As Figure 5.1 clearly shows, in each of the three models estimated, disputes where Congress was required to act are associated with roughly a 50% higher probability that the U.S. will be non-compliant with a WTO decision. This ranges from a 42% higher probability in Model 1 to a 62% in Model 3. This result is consistent in the parsimonious model presented in

Model 1, when controlling for a range of independent variables that account for alternative explanations in Model 2, and even when including data on the political contributions associated with the relevant sector of the economy at issue in the dispute in Model 3. In fact, not only is the *Congress Required* variable significant in each model presented, it is the *only* variable that achieves statistical significance. These results thus lend strong support to our theory that the source of domestic policy measures is a significant issue in the state's decision of whether to comply with WTO decisions.

Figure 5.1 also provides evidence on what other factors influence, or do not influence, compliance decisions. Neither the results in Model 2 or Model 3 provide any evidence that characteristics about the complaining state or states influence the U.S. government's likelihood of complying with WTO decisions. Interestingly, the level of U.S. exports to the complaining state, which is a measure of the potential retaliatory capability of the complaining state, does not have any stastically significant effects. This is contrary to realists and institutionalists approaches expectations that concerns for retaliation or reciprocity are driving governments' compliance decisions. In addition, the existence of other formal alliances with complaining states also does not appear to influence compliance decisions. While this finding does not disprove the institutionalist's expectation that having more formal treaty alliances will lead to greater linkages between regimes and thus greater compliance in all regimes, the evidence is not supportive of the idea that a web of treaty relationships between country dyads will improve compliance. In addition, the results of Model 2 and Model 3 do not support the conjecture by liberal theory that democracies will be more likely to comply with international obligations when dealing with other democracies. A higher polity score, indicating more democratic institutions, did not have any statistically significant effect on the U.S. compliance levels indicating that the

U.S. is no more likely to comply with international law obligations to other democracies than to non-democracies.

More broadly, none of the independent variables accounting for external pressures on the state to comply were statistically significant. This suggests as a general matter that domestic pressure, rather than international pressure, is responsible for understanding *the variance* in U.S. compliance decisions. This does not necessarily mean that external pressure is not a significant cause of compliance. Rather, it is possible that the level of external pressure to comply may be constant for all cases, and so external pressure is not a good predictor of when the U.S. chooses to comply (or not) for any particular case. The level of external pressure may help establish a baseline level of compliance for all cases but does not predict movement around the baseline. However, the fact that dyad specific factors are not important is still notable. Basic realist propositions, such as the U.S. is more likely to comply when sued by a more economically powerful state, turn out to not be supported. Neither are propositions that "interdependence" between country dyads (through shared democratic governance structure or formal alliances) should lead to greater compliance with treaty obligations borne out.

Finally, the other case specific variables were also not relevant to the compliance decision. The level of political contributions in Model 3 did not have a statistically signficant influence on compliance. This is notable because it indicates that a simple interest group lobbying model is not a very good predictor of compliance on trade issues. More importantly for this study, controlling for political contributions means that differences between the actions of Congress as compared to the actions of the executive branch are not driven by interest group action. The differences between U.S. compliance decision when congressional action is needed or not persist even when we account for political contributions. This result means that it more likely that the nature of the two institutions, not interest group politics, is making the *Congress Required* variable important. The study also accounts for cases that challenge American domestic trade remedy actions. Trade remedy actions involve domestic level decisions to apply safeguards, anti-dumping measures, and countervailing duties, and these decisions are frequently challenged at the WTO. To make sure that these cases were not driving our results, we included a dummy variable to account for any trade remedy specific variation. Some trade scholars may be surprised that this variable is not statistically significant, indicating that the U.S. is no more likely to comply in a trade remedy cases than in any other issue area.

5.5.2 Total Compliance Time

The second test of our theory was estimating the impact of whether congressional action was required to bring a measure into compliance on the total amount of time that compliance took. The total amount of time that compliance takes is a measure of the *quality* of compliance. Because the WTO litigation process can be manipulated by dragging out the panel and appeals process through requests for compliance panels and other delaying tactics, discussion of compliance are not exclusively focused on whether a nation ultimately complied but also on how long compliance takes.[52] This test attempts to measure the quality of compliance by accounting for the compliance timeline.

For these tests, once again, the number of observations was the 37 disputes where the U.S. did not settle or prevail on the core issue in the case. For each of these cases, the dependent variable was calculated as the number of days from when the conference request was filed until when the U.S. took an action to come into compliance.[53] Using this new dependent variable, we then estimated a series of models containing the same independent variables used in Figure 5.1. Using this data, we estimated a series of Cox Proportionate Hazard Models.[54] The reason that we selected the Cox model is that it has the advantage of not requiring assumptions about the distribution of time until an event occurs.[55] Figure 5.2 presents the results of these tests.

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[52] Davey 2009, Brewster 2012.

Using a similar method to the one we used to present results in the last section,[56] Figure 5.2 presents graphical representations of three Cox proportional hazard regression models. These three models include the same independent variables as Figure 5.1. The difference, however, is that Figure 5.1 presented the results of logit models that can be interpreted as the change in probability that an event will occur—in our case, non-compliance with a WTO decision. In contrast, the results presented in Figure 5.2 are hazard ratios. Hazard ratios with a value of less than 1.0 mean that an event will take longer to occur, whereas hazard ratios with a value of greater than 1.0 is likely to occur more quickly.

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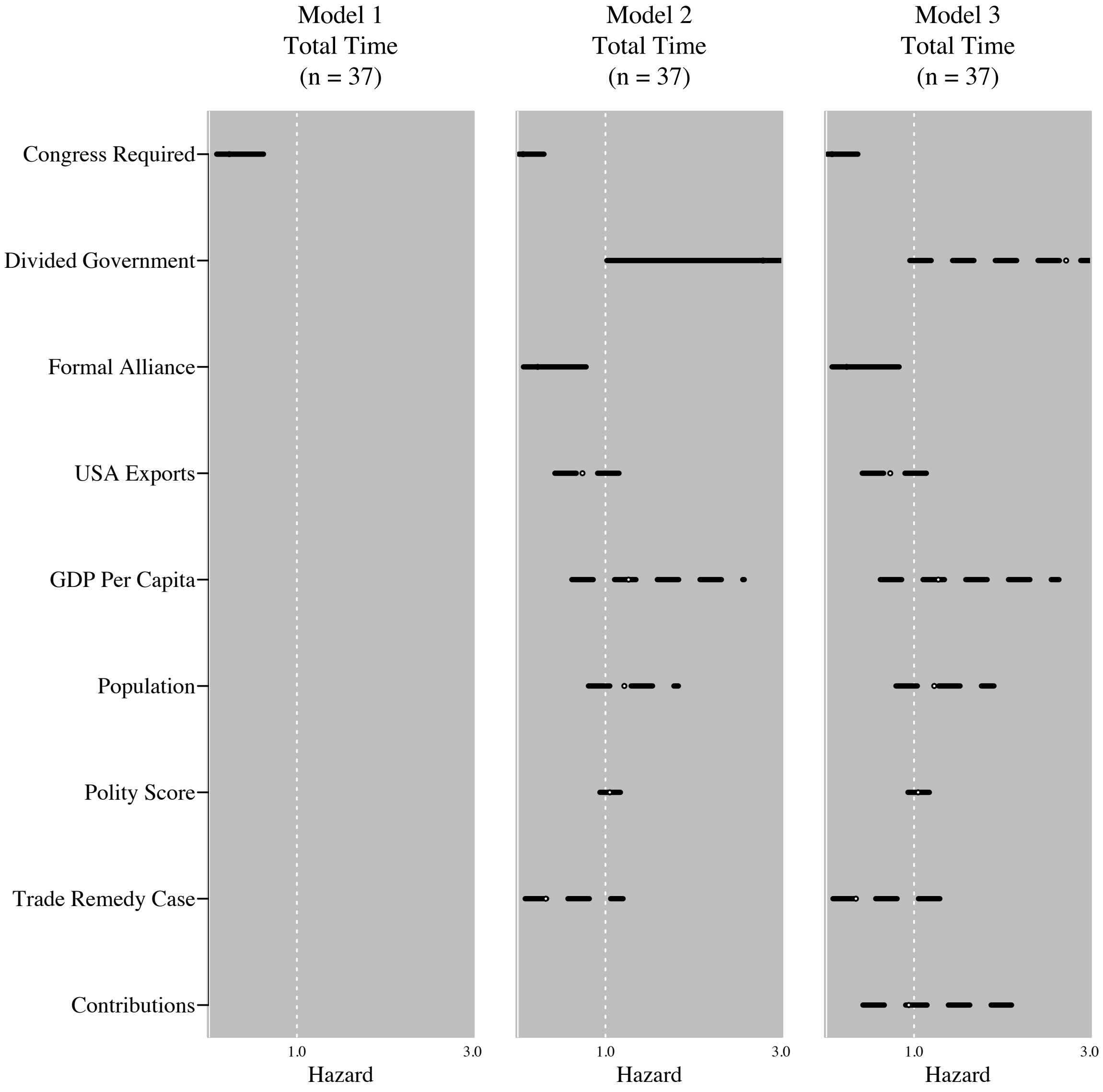
[53] For the 13 cases that the U.S. was non-compliant, we used the date when the U.S. came into at least partial compliance. The three cases where no steps have been taken are treated as censored observations as of August 31, 2012.

[54] Cox 1972.

[55] Box-Steffensmeier and Zorn 2001, 974.

[56] See text accompanying note 51.

Figure 5.2: Cox PH Models Estimating Total Compliance Time



In all three of the models included in Figure 5.2, the *Congress Required* variable is below 1.0 and is statistically significant. The hazard ratio in these models for the *Congress Required* variable ranges from 0.23 in Model 1 to 0.07 in Model 3. In all three cases, this result is statistically significant at the 0.01 level. In other words, these models suggest that we can say with 99% confidence that the disputes that require congressional action to be resolved take longer than other disputes. This result is robust whether in the parsimonious model or with all of our covariates included. These results support our theory by presenting strong evidence, even controlling for competing theories, that who is required to provide compliance has a significant impact on how long it takes the U.S. to comply with adverse WTO rulings.

Figure 5.2 also presents interesting results with regards to competing hypotheses concerning compliance. Again, the retaliatory capacity of the complaining state (or states) does not speed up compliance. The polity score of the complaining state is not significant indicating that the U.S. is not more likely to comply with adverse WTO judgments faster if the complaining state is another democracy. More difficult to explain is the formal alliance variable. This independent variable is statistically significant but it works in the opposite manner than institutional theory would predict. The U.S. is likely to take longer to comply with WTO decisions when it has a formal alliance with the complaining state than when the complaining state is not a security ally. The direction of the variable indicates that having greater interdependence in formal treaty regimes does not lead to a higher quality of compliance in terms of timing.

5.5.3 Robustness Checks

In order to ensure that our results are not the result of either coding decisions or model dependency, we performed a number of robustness checks. Each of these checks present further evidence to support our theory that a driving factor in the U.S.' compliance with WTO decisions is which branch of government is required to take action.

First, one concern is that our coding decisions of the *Congress Required* variable may not have included all of the cases where congressional action was required for the U.S. to remedy the violation alleged in the initial compliant. Given that our initial coding of the variable was intentionally conservative to create a difficult test for our theory, we coded four alternative versions of the *Congress Required* variable to ensure that our results were not based solely on our cautious coding decisions. For these alternative variables, we added additional cases to our initial list of those requiring congressional action. Specifically, we changed the coding of: (1) Helms-Burton;[57] (2) softwood lumber;[58] (3) zeroing;[59] and (4) all three of the previous cases simultaneously.[60] After doing so, we re-estimated the models presented in Figures 5.1 and 5.2 using each of the four alternate versions of the *Congress Required* variable. For all of the models and alternative variables, the results were substantially the same as those presented in the paper.[61]

Second, another concern is that our results for estimating whether the U.S. would comply with an adverse WTO decision presented in Figure 5.1 were driven by the timing of the Congressional Research Service report that we used to code the dependent variable.[62] The concern is that shortly after the CRS report was released, the U.S. finally reached a deal to resolve a number of zeroing cases. As a result, we created an alternate dependent variable that changed the coding for the three zeroing cases listed in the CRS report as non-compliant to compliant.[63] After doing so, we re-estimated the models presented in Figure 5.1. The *Congress Required* variable remained statistically significant at the 0.05 level or higher in all three models.

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[57] DS38.

[58] DS236.

[59] DS294, DS322, DS350, DS402.

[60] DS38, DS236, DS294, DS322, DS350, DS402.

[61] All of the models estimated were statistically significant at least the 0.05 level, with the exception of some of the models in Figures 5.1 and 5.2 when the *Congress Required* variable was altered to include only the softwood lumber case (DS236). Of these, all of the models were significant at the 0.1 level with the exception of Model 3 in Figure 5.1, which had a p-value of 0.13.

[62] See text accompanying notes 29 - 31.

Third, it is possible that the models that we used to estimate the total compliance time were biased because the models presented in Figure 5.2 did not include cases that had been settled. To address this possibility, we collected data on the amount of time that elapsed during the litigation and compliance process for cases that ultimately settled. The impact for Figure 5.2 was that there were eleven additional cases that we have information on when the settlement took place (for the other seven settled cases, it does not appear that the result of the negotiations was ever reported to the DSB, and thus we do not know even the rough date of when the U.S. took a compliance action).[64] After including these cases, we re-estimated the models presented in Figure 5.2 with 48 instead of 37 observations. In all three models, the *Congress Required* variable remained statistically significant at the 0.05 level or higher.

Fourth, it would be reasonable to be concerned that our results were at least partially driven by our decision on how to code control variables for cases brought by multiple complainants.[65] For cases with multiple complainants, we elected to code the *Exports*, *GDP Per Capita*, and *Population* as the sum of the totals for all of the complainants, and the *Polity Score* as the average for all of the complainants. In their paper on compliance with WTO decisions, however, Hofmann and Kim took an alternate approach and elected to code control variables based on the values for the complainant with the largest GDP.[66] Although we are generally concerned that this approach fails to account for the possibility that the stakes may be meaningfully higher when there are multiple complainants than if the complainant with the largest GDP had brought the complaint alone, we recoded our variables for *USA Exports, GDP Per Capita*, *Population*, and *Polity Score* using Hofmann and Kim's approach. After recoding these variables for cases with multiple complainants, we re-estimated the models presented in Figures 5.1 and 5.2. After doing so, our results remained substantively the same.[67]

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[63] DS294, DS322, DS350.

[64] The eleven additional cases are: DS6, DS32, DS38, DS39, DS40, DS85, DS88, DS89, DS250, DS281, and DS282.

[65] This includes both consolidates cases and cases where two countries were complainants on the same request for consultation.

Fifth, a final concern that we attempted to address is the fact that the measure of compliance time presented in Figure 5.2 could be biased because measuring from when a conference request was filed until compliance means our variable includes both the "litigation time" and the "compliance time." It could be the case that in disputes where Congress is required to act it takes longer to litigate, but that after the litigation has completed, the U.S. complies just as promptly as other cases. To ensure that this possibility was not driving our results, we attempted to directly measure "compliance time." To do so, for each case we collected the date that the final panel—or appellate body—report for each case was adopted. We then calculated the number of days that elapsed from this point until the date when compliance occurred. After doing so, we re-estimated the models presented in Figure 5.2 with this new dependent variable. The significance and substantive effect of the *Congress Required* variable was comparable to the results presented in Figure 5.2. We additionally performed all of the other robustness checks discussed in this section with the new "Compliance Time" dependent variable, and these robustness checks did not substantively change the results.

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[66] Hofmann and Kim 2012, 118.

[67] The models in Figure 5.1 had a p-value of 0.06, whereas the models in Figures 5.2 were significant at the 0.01 level.

5.6 Conclusion

The question of why and when states comply with international law is one of the foundational inquiries in international legal studies. This work attempts to examine compliance actions empirically by studying the compliance behavior of the U.S. in responses to adverse WTO dispute resolution decisions. While focusing on the U.S. government alone has some limitations in terms of how well the findings here can generalize to other states, this study has potentially important implications for understandings of international law.

First, opening the "black box" of the state is critical to explaining patterns of compliance. At least in trade law, different domestic actors can be the source of policy compliance on different issues. This study demonstrates that when the executive branch has the power to comply with adverse WTO decisions, then the likelihood of compliance is significantly high and the compliance comes significantly faster than if congressional action is needed. What actor within the state has the capacity to cure the violation is not significant in determining the "state's" compliance, it is also the best predictor of compliance.

Of course, it is possible that issues for which congressional action is necessary are more "high politics" issues than those than can be handled by the executive branch. If so, then the *Congress Required* variable might be picking particularly sensitive trade issues that are harder to resolve, in addition to difference inherent to the different political actors. This is unlikely for two reasons. First, the study includes a variable for the political contributions of the affected industries, so this variable should pick up and account for issues that are politically sensitive because of interest group politics. Second, the trade disputes requiring congressional action are set by the issue area, not by the level of controversy. Congress and the executive branch both deal with a mix of high and low politics issues. Some high controversy issues, such as agriculture subsidies and tax rates, require congressional action, but so do some lower politics issues, such as the payment of countervailing duty awards and some minor points of intellectual property law. In addition, many of the issues addressed by the executive branch are "high politics" disputes, such as claims of national security and environmental policy.

Our empirical analysis finds that the question of who supplies compliance overwhelms the influence of all international factors in predicting compliance, including the economic size of the complaining state, and other domestic factors, including political contributions. This suggests that it is not useful to talk about a "state's" level of compliance when analyzing patterns of compliance. Rather, compliance behavior must be disaggregated based on the source of compliance to be coherently understood. If members of Congress are fundamentally less receptive to appeals that abide by international obligations than members of the executive branch (either because of their constituencies, their lack of participation in the day-to-day practice of foreign affairs, or super-majority voting rules) then our focus should shift to more domestic level variables to understand the effects of international law.

This study also suggests that international relations theories that have expectations for "state" action may be overly broad. Different actors within a state may operate based on different logics and the efforts to treat the state as unitary obscures important causal factors. Our study is generally supportive of the idea that executive branch actors may experience more of a "compliance pull" with international law than members of Congress. This could be based on the executive branch's day-to-day operation of foreign affairs, concerns about reciprocity with foreign counterparts, or perception of the legitimacy of the dispute resolution process. Members of Congress may have lower concerns about reciprocity (particularly on daily basis), have less exposure to the dispute settlement processes, and thus, have lower levels of confidence in the legitimacy of the process. If this is true, then it has implications for broader ideas of compliance, in addition to more narrow proposals for designing dispute settlement regimes.

In broad brushstrokes, this study indicates that institutionalist logic may have more force when dealing with executive branch officials. Concerns about reputation or a desire to be perceived as a "law-abiding" state may have greater influence on government officials who deal with the international system directly.[68] In addition, the managerialist approaches that emphasis on the importance of "jaw-boning" or "shaming" may also find a more fertile ground when dealing with executive branch officials.[69] By contrast, members of Congress may not be influenced by concerns about the perceptions of foreign policymakers or international officials because they interact with these audiences less often. Jaw-boning may be less effective with members of Congress because they are not at the bargaining table and do not otherwise make themselves available. Furthermore, the same activities that might be embarrassing to an executive branch official, such as openly refusing to abide by an international court decision, may be the source of pride or greater domestic support to a member of Congress.

When dealing with members of Congress, other logics may better describe compliance behavior. Members of Congress appear less responsive to the current levels of remedies available at the WTO than the executive branch. Legislators may nonetheless be responsive to the material consequences of non-compliance that would affect their constituencies. Thus, the levels of cooperation that can be sustained when legislative action is necessary may depend on the level of retaliation that can be authorized for non-compliance.[70] In dispute settlement design terms, this means that permitting higher retaliatory remedies, including retrospective damages or progressively higher damages, may be helpful to create the necessary domestic conditions for compliance to occur.

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[68] Guzman 2008; Abbott and Snidal 2000; Keohane 1984.

[69] Chayes and Chayes 1993.

Finally, greater delegation to executive branch officials will make compliance more likely and quicker. Thus, much of the work of compliance may lie in domestic structures—the statutory system and the level of policy discretion allocated to the executive—as much as in the design of international treaty regimes. By contrast, when compliance involves congressional action, the U.S. pattern of lower and slower compliance may be a bargaining asset. As Robert Putnam's two-level game suggests, the existence of a well-known domestic constraint may allow the U.S. to settle disputes on more favorable terms (i.e., what the executive branch can accomplish unilaterally).[71] Complaining states may even be deterred from bring claims if they expect that compliance will not be forthcoming or will be particularly slow.

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[70] Downs, Rocke, and Barsoom 1996; Goldsmith and Posner 2005.

[71] Putnam 1996.

Chapter 6

The Politics of the United States' Bilateral Investment Treaty Program

6.1 Introduction

In the last sixty years, over 2,600 Bilateral Investment Treaties (BITs) have been negotiated between pairs of countries.[1] Taken together, these treaties create a regime of international law that provides protections for individuals and corporations seeking to invest their capital in other states.[2] Although the United States ranks first in the world in both foreign direct investment inflows and outflows,[3] America was a late entrant into the BITs regime. The United States did not express a willingness to start a BITs program until 1981[4]—twenty-two years after Germany negotiated the world's first BIT[5]—and the United States did not have its first BIT in effect until 1988.[6] To date, the United States has entered into nearly fifty BITs.[7]

As the number of BITs that the United States is party to have grown, so has the amount of scholarship seeking to understand the economic and legal consequences of these agreements.[8] This line of research has primarily focused on trying to understand why developing states would choose to enter into BITs,[9] and what the impact of those BITs has been on rates of foreign direct investment.[10] But what has not been empirically examined is why the United States has expended time and energy negotiating BITs over the last thirty years.

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[1] See Salacuse 2010, 428.

[2] See id. (using regime theory to analyze the current system of bilateral treaties governing international investment law). See also Alvarez 2010.

[3] Based on data available through the UNCTAD database for World Foreign Direct Investment flows, available at <www.unctad.org> (last visited April 2, 2013). See also Sachs and Sauvant 2009, xxx-xxxiii (discussing the united states ranking in world investment flows).

[4] See Salacuse 2010, 433.

[5] Id.

[6] For a list of BITs the United States has signed, see Appendix 5.1.

[7] Id.

[8] See generally Shaffer and Ginsburg 2012, 35-38 (discussing scholarship on international investment law).

Despite the fact that it has not been quantitatively studied, scholars discussing the topic have uniformly argued that the United States' BITs program is motivated by a desire to influence the development of international investment law to protect American investments abroad.[11] Although it is certainly at least partially true that the United States was concerned with the development of international investment law and hoped that these treaties would help American investors, there are several reasons to believe that the United States was not primarily motivated to enter into BITs to produce a more favorable climate for American individuals and corporations to invest in foreign countries. First, U.S. BIT negotiators have warned treaty partners that they should not expect a wave of new investments as a consequence of these agreements,[12] which is strong evidence that U.S. officials themselves are aware of the fact that the economic impact of these agreements is likely quite limited. Second, there appears to have been limited pressure on the United States to ratify BITs after they were signed,[13] suggesting that U.S. investors are not eager to avail themselves of any new opportunities or protections that BITs may provide. Third, evidence suggests that BITs do not have a positive impact on investment flows between the United States and partner countries.[14] Fourth, there is reason to believe that BITs do not influence companies' investment decisions,[15] which calls into question whether BITs are negotiated solely to provide increased protections for capital exporters.

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[9] See, e.g., Elkins, Guzman, and Simmons 2006; Tobin and Busch 2010; Guzman 1998.

[10] See generally Sauvant and Sachs 2009.

[11] See Lang 1998, 457; Vandevelde 1998, 201-2 ("The purpose of [BITs negotiated by the United States] is to protect investments of each party's nationals and companies in the territory of the other.").

[12] See Alvarez 2010, 621 n.69; Vandevelde 1998, 212.

[13] See infra Table 6.1.

Given the limited interest in, and effect of, the investment benefits of BITs, it is worth reconsidering why the United States has actively pursued a BITs program for over thirty years. In this paper I argue that the United States has not used BITs as a tool to protect investments, but instead as a foreign policy tool to improve relationships with developing countries. BITs are an appealing tool to use for this purpose because they are low cost, only require the United States to make "redundant" obligations, easy to sell politically, and require minimal effort to negotiate given their standardized nature. The United States then extends the ability to negotiate into these agreements to countries that it would like to improve its relationship with. The potential BIT partners are frequently eager to join these treaties, not because they are mistakenly under the belief that the agreement will lead to new investments, but instead because the signal sent by the treaty—that the country now has a special relationship with the United States that might produce economic dividends—provides the country's leader with a domestic political benefit. As a consequence, even without producing an economic benefit, the BIT then leads to the treaty partner becoming closer to the United States politically because the BIT provides some domestic political cover for the government to do so. The implication of this theory is then that investment considerations will not explain which countries the United States has chosen to enter into BITs with, and that looking solely at the investment consequences of the treaties will not help to demonstrate whether they have achieved their true purpose—improving relationships and alliances.

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[14] See Peinhardt and Allee 2012 (showing that the BITs the United States negotiates do not result in an increase in capital flows with the treaty partners); Yackee 2008 (finding that even BITs with strong investor protections are not associated with increased investment). See also Shaffer and Ginsburg 2012, 36-38 (discussing scholarship on the financial impact of BITs); Salacuse 2010, 468 ("[M]uch research has questioned whether investment treaties have in fact increased investment flows to poor countries.").

[15] See Yackee 2010.

In this paper, I empirically test the theory that political considerations have predominantly motivated the United States' BITs program in two ways. First, I examine the factors that influenced what countries the United States signed BITs with. After performing a series of statistical tests, I show that a country's level of investment risk, capital flow with the United States, and capital stock does not have any discernable impact on the likelihood that the United States will sign a BIT with that country. Moreover, I present evidence that suggests that the United States is more likely to sign BITs with countries that are strategically and politically important to U.S. interests. It is my hope that these results will help to demonstrate that the United States' BIT program was (and is) driven by political, and not investment, concerns.

Second, I examine the political consequences of BITs, and show that signing a BIT with the United States makes a country more likely to support actions that help the United States achieve its international strategic objectives. To do so, I use a recently developed statistical method that has shown potential as a way to study the effects of treaty ratification—"Life History Matching."[16] Using this approach, observations (countries) that have received a particular treatment (ratified a treaty) are matched with similar observations (countries) that have not received the treatment (ratified a treaty), but that have similar values for a range of covariates over a period of several years.[17] The difference between this approach and other matching methods is that, by matching on several years' worth of data as opposed to a single year, it is possible to match pairs of observations that have similar trends over time. After using life history matching to pair observations for cases where BITs existed to those without BITs, I then tested the influence of BIT ratification on whether the treaty partner was likely to support the United States in a number of its international strategic objectives, specifically examining whether the treaty country: (1) voted similarly to the United States in the United Nations General Assembly; (2) allowed the United States to deploy troops within their country; and (3) supported the invasion of Iraq in 2003. My results suggest that having a BIT in effect with the United States causes the treaty partner to be more supportive of the United States in the UN Generally Assembly, and may have allowed the U.S to deploy troops on the partner's soil, but did not make a country more likely to join the Iraq War Coalition. In other words, this study not only shows that the United States chose which states to pursue BITs with based on political calculations, but also that the decisions to sign those BITs have produced modest—although not amazing—political dividends.

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[16] See Nielsen and Simmons 2011 (using life history matching to test whether developing countries receive rewards for ratifying human rights treaties).

This project makes at least several important contributions to the literature on international investment law. First, this project should cause commentators and scholars to completely change the narrative offered to explain why the United States has pursued BITs with developing states. The literature on U.S. BITs is currently littered with scholars asserting that these treaties were negotiated to produce more favorable conditions for American investors. After this project, this explanation should be jettisoned and replaced with the new explanation that BITs have been negotiated as a low cost foreign policy tool. Second, this paper is the first quantitative effort to directly consider why the United States might have chosen to pursue BITs with specific countries. Previous efforts have focused on explaining the proliferation of BITs from developing countries' perspectives, but have not flipped the question and asked why the United States—or developed countries more broadly—would take the time to negotiate and ratify investment treaties that have limited direct influences on capital flows. Third, this project develops a theory that tries to explain both why the United States and developing countries would be motivated to sign investment treaties even if they do not produce investment benefits, and why those treaties could still result in benefits—albeit political ones—for the United States. Finally, this project provides evidence suggesting that the United States' BITs program has produced benefits that have not yet been understood or explored. This evidence should cause scholars that are skeptical of the economic benefits of BITs to reassess the program based on the political advantages that they may provide.

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[17] See Nielsen 2011.

This paper proceeds as follows. Part 6.2 discusses the growth of the United States' BITs program, and then presents the conventional explanation that the program was designed to promote the development of international law and foster a welcoming climate for American investors in order to situate the reader in the literature on this subject to date. Part 6.3explains the flaws in the conventional explanations of the United States' BITs program, and presents a theory that the United States chose its BITs partners based on political calculations with the hopes of accomplishing political goals. Part 6.4 explains the empirical strategy that is used in this paper to test the political theory of BIT formation and discusses the dataset that was collected for this project. Part 6.5 presents the results of a series of empirical tests demonstrating that BITs the United States has signed cannot be explained by investment considerations, and showing that political concerns provide a more plausible explanation. Part 6.6 conducts a series of empirical tests to show that BITs have provided a number of political benefits to the United States. Part 6.7 discusses the results and concludes.

6.2 The Growth of the United States' BITs Program

Over the last several decades, there has been a dramatic increase in the number of Bilateral Investment Treaties in effect. As BITs have proliferated, so has scholarship trying to understand the growth of these treaties and their consequences.[18] That line of scholarship, however, has not yet provided a convincing account of why the United States has pursed BITs with developing states. This part of the paper begins that project by first providing an overview of the growth of BITs worldwide. Second, I describe the United States' history with Bilateral Investment Treaties. Third, I lay out the standard explanation of the United States' BITs program: namely, the idea that the program was motivated by a desire to protect American investments abroad. This part of the paper thus outlines the conventional understanding of the BITs program discussion, familiarity with which is a necessary precondition to comprehending my argument in the subsequent section that conventional wisdom is incorrect and instead the United States has not pursued BITs to protect investors, but instead to promote America's geopolitical interests.

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[18] See generally Schaffer and Ginsburg 2012, 35-38.

6.2.1 The Emergence of BITs

Historically, protections for foreign investors was a topic governed by customary international law ("CIL").[19] Under this the established principles of CIL, governments where required to provide prompt compensation for any property owned by foreign nationals that was expropriated. By the 1950's, however, the consensus for providing compensation for expropriation under customary international law was breaking down.[20] Developing countries that were "hosts" for international investments were increasingly nationalizing assets and protesting against the standards of compensation that were considered part of customary international law at the time.[21] It was during this period that, in 1959, Germany created the first Bilateral Investment Treaties with Pakistan and the Dominican Republic.[22] Other European countries followed Germany's lead, and quickly established their own BITs programs to protect the overseas investments of their citizens and corporations in the developing world.[23]

Although there were differences between the treaties negotiated by different countries, these agreements generally sought to force developing countries to guarantee specific protections to foreign investors and to provide a guaranteed right for investors to bring arbitration for alleged violations of those rights.[24] In fact, most BITs shared a number of common features.[25] First, the BITs focused principally on investment issues, and were primarily negotiated between one developed and one developing state.[26] Second, the agreements contained "a guarantee of national and most favored nation treatment for covered investment[s], a promise of fair and equitable treatment for covered investment[s], a commitment to pay prompt, adequate and effective compensation for expropriation of covered investment[s], and restrictions on exchange controls."[27] Third, and perhaps most uniquely, the agreements bound countries to agree to arbitration directly with investors claiming violations of the agreements.[28]

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[19] Id. at 812-13. For a good general overview of the development of international investment law, see Dolzer and Schreuer 2008, 17-24.

[20] See Elkins, Guzman, and Simmons 2006, 812-14.

[21] See id.

[22] See Salacuse 2010, 433.

[23] See id. It is perhaps worth noting that European Countries were particularly interested in negotiating BITs with their former colonies. Id.

Today, there are over 2,600 BITs in effect.[29] There are additionally several hundred more economic and trade agreements that include investment chapters, for a total of over 3,000 bilateral agreements that seek to regulate international investment.[30] Although these agreements started as exclusively treaties between one developed and one developing state—known as "North-South" treaties—there are now over 600 treaties between developing states as well—

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[24] See Elkins, Guzman, and Simmons 2006, 813-14.

[25] See Vandevelde 2006, 170 ("These new bilateral investment treaties were remarkably uniform in content and contain several distinct features.").

[26] Id.

[27] Id. at 172. See also Salacuse 1990, 664-73 (discussing the various provisions of BITs, which are "similar and relatively limited in number").

[28] Id. at 174.

[29] See Salacuse 2010, 428.

[30] Id. at 428-9.

known as "South-South" treaties.[31] Together, these treaties have formed a dense web of international investment law that some have even gone as far as to argue has reshaped customary international law on investments.[32]

6.2.2 The United States' Experience With BITs

In 1977, twenty years after European countries first began to use bilateral treaties to protect foreign investments, the United States launched its BITs program.[33] At the time, the United States viewed the use of BITs as a way to increase investor protections abroad, both by directly enshrining legal rights in treaties and by generally encouraging the spread of pro-investment principles in international law.[34] The United States additionally viewed the concessions contained with in BITs that would make it easier for foreign nationals wishing to invest their capital in the United States as "redundant" to existing protections already provided in American domestic law.[35] After years of work, the Untied States finally produced a draft model BIT to use in negotiations with other countries in December 1981.[36]

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[31] Id. at 433-34. See also Dolzer and Schreuer 2008, 21 (noting that between 2003 and 2006, treaties between two developing states "outnumbered those between developed and developing states").

[32] Id. at 429.

[33] For discussions of the establishment of the U.S. BITs Program, see Vandevelde 1993, 624-627; Gann 1985, 373 (discussing the establishment of the U.S. BITs program); Vandevelde 1992.

[34] See Gann 1985, 373-76. See also Elkins, Guzman, and Simmons 2006, 815-816.

[35] See Gann 1985, 373-76.

[36] See Vandevelde 1993, 627.

All though the program was started in the Carter Administration, the United States did not start negotiating its first BITs—with Egypt and Panama—until 1982.[37] After this, the United States negotiated eight additional BITs in the next four years.[38] Those treaties were with: Morocco, Zaire, Cameroon, Bangladesh, Senegal, Haiti, and Grenada.[39] After a hiatus, the program was given new life by the collapse of the Soviet Union.[40] As eastern block states moved towards opening their markets, the United States began negotiating a BIT with the Soviet Union, and several formerly communist states, such as Poland.[41] When the Soviet Union disintegrated, the United States quickly negotiated BITs with a number of former Soviet Republics.[42] At the same time, the United States also continued to negotiate BITs with countries in Africa,[43] Asia,[44] and South America.[45] A map of the countries the United States has signed a BIT with is shown in Figure 6.1.

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[37] See id; Sachs 1984 (discussing the first U.S. Bilateral Investment Treaties and comparing them to previous international investment law).

[38] See Vandevelde 1993, 627-28.

[39] For a complete list of the BITs the United States has signed, see Appendix 5.1.

[40] See Vandevelde 1993, 630.

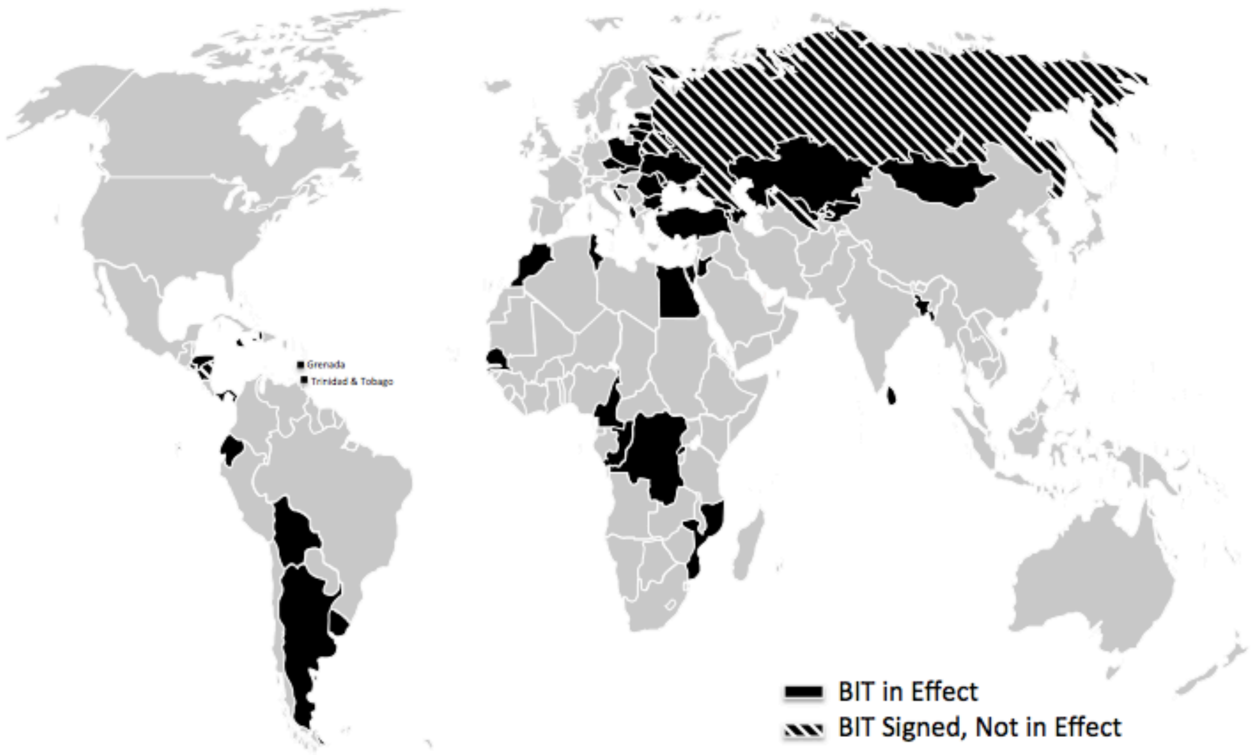
[41] Id.

[42] Id. at 630-31.

[43] See, e.g.,Treaty Between the United States of America and the Government of the People's Republic of the Congo Concerning the Reciprocal Encouragement and Protection of Investment, Feb. 12, 1990, S. TREATY DOC. NO. 1, 102d Cong., 1st Sess. (1991).

[44] See, e.g., Treaty Between the United States of America and the Democratic Socialist Republic of Sri Lanka Concerning the Reciprocal Encouragement and Protection of Investment, Sep. 20, 1991, S. TREATY DOC. NO. 25, 102d Cong., 2d Sess. (1992).

Figure 6.1: Countries That Have Signed a BIT with the United States



As of June 2012, the United States had signed forty-eight BITs.[46] Figure 6.2 shows how this growth has proceeded. Of the forty-eight treaties, forty-one had been ratified by both parties and gone into effect.[47] These BITs have been negotiated with a set of countries that is incredibly

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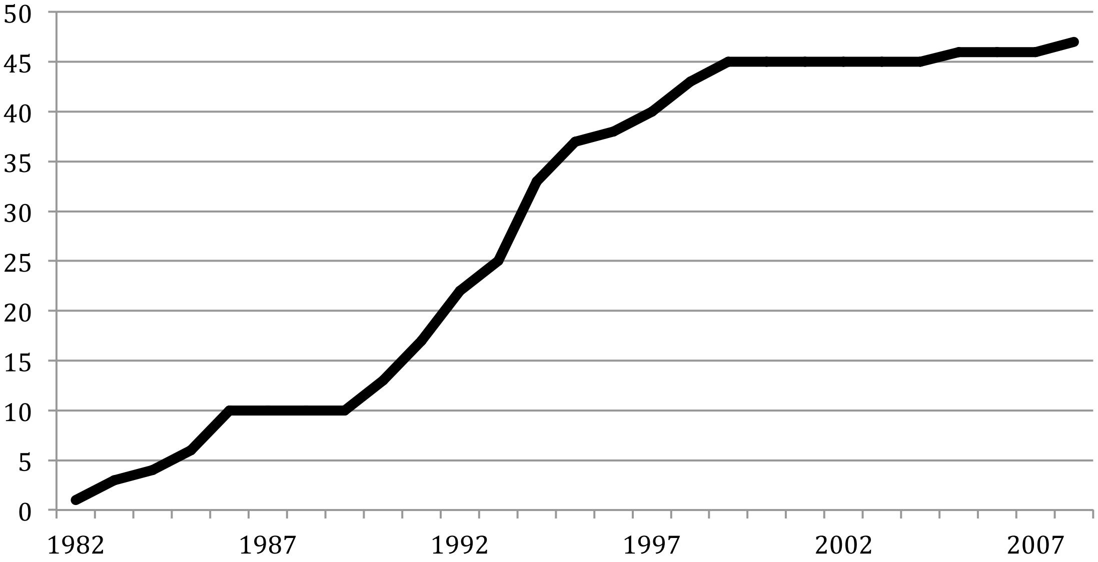
[45] See, e.g., Treaty Between the United States of America and the Argentine Republic Concerning the Reciprocal Encouragement and Protection of Investment, Nov. 14, 1991, S. TREATY DOC. NO. 2, 103d Cong., 1st Sess. (1993).

[46] See Legal Update, *U.S. Bilateral Investment Treaties: Recent Development*, by Mayer-Brown (June 11, 2012), *available at* <http://www.mayerbrown.com/files/Publication/ae444628-6a40-4a3a-b6bafe81cbde0a3e/Presentation/PublicationAttachment/bb82d4d6-b014-4a3c-935f-00dee2cc9748/Update_US_Bilateral_Investment_Treaties_V1_0112.pdf> (last visited December 9, 2012).

[47] Id.

geographically diverse, including treaty partners from Africa, Asia, Central America, Europe, the Middle East, and South America.[48] Additionally, the "Model BIT" that the United States uses as a basis for negotiation has evolved over the years to become increasingly complex and precise.[49] As a result, although the United States was slow to adopt the practice of using bilateral treaties to protect investment, America is now an active participant in the BITs regime.

Figure 6.2: Total Number of BITS Signed by the United States



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[48] Cf. "United States Bilateral Investment Treaties," *available at* <http://www.state.gov/e/eb/ifd/bit/117402.htm> (last visited December 7, 2012).

[49] See generally Alvarez 2012.

6.2.3 Conventional Explanations for the United States' BITs Program

Although there have been a number of attempts to empirically examine why developing countries choose to enter into bilateral investment treaties,[50] there have not been any attempts to study why the United States specifically—or even developing countries generally—decide to negotiate and sign BITs. Despite the lack of rigorous qualitative or quantitative research on the topic, most commentators have claimed that the United States entered into BITs to reinforce international legal principles on the treatment of foreign capital and to secure the rights of American investors.[51] In other words, the United States' motivations were straightforward: investment treaties were negotiated to promote and protect investment.[52] This explanation has been put forth to explain both the motivations of the United States[53] and those of developed countries more generally.[54]

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[50] See, e.g., Elkins, Guzman, and Simmons 2006. See also Guzman 1998 (trying to explain the "paradoxical behavior" of Least Developed Countries (LDCs) that enter into BITs with developed states).

[51] In perhaps the clearest statement of this motivation for the U.S. BITs Program, Secretary of State George Shultz claimed that BITs were designed "to protect investment not only by treaty but also by reinforcing traditional international legal principles and practice regarding foreign direct private investment." Elkins, Guzman, and Simmons 2006, 815-16 (citing George P. Shultz, Transmission letter to the president recommending transmission of the U.S.-Turkey Bilateral Investment Treaty, 1985").

[52] The one exception that this author is aware of is Vandevelde 1993. Although in that article Vandevelde argues that the motivations of the program have shifted over time, and that some BITs are political, Vandevelde does not consistently make this argument in later writing. See Vandevede 2005, 171.

[53] See, e.g., Lang 1998, 457 (arguing that the United States' goals in negotiating BITs are: (1) protecting U.S. investment abroad; (2) encouraging adoption of market-orientated domestic policies; (3) promoting development of international law that meets these objectives); Gann 1985, 374 ("The BIT program resulted from the U.S. government's determination that a more favorable framework for U.S. investment in developing countries should be great. This new

Given the fact that commentators have uniformly described BITs as treaties that developed states use for the purpose of promoting and protecting investment, it is thus unsurprising that scholarship evaluating the success of BITs has focused almost exclusively on whether the agreements have been successful methods of attracting capital.[55] Although there have been a few efforts to study the effects of BITs along other dimensions—for example their effect on litigation[56] or the likelihood that their ratification leads to Preferential Trade Agreements (PTAs)[57]—these studies have still conceived of BITs as treaties created for economic reasons. The result is that scholars have viewed BITs as a tool the United States uses to promote investment that should be evaluated primarily based on how well it does at accomplishing that goal.[58]

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framework has a twofold purpose: to encourage as well as to protect such investment."); Vandevelde 1988, 1-2 ("The purpose of [the BITs the U.S. has negotiated] is to protect the investments of each party's nationals and companies in the territory of the other.").

[54] See, e.g., Vandevelde 2005, 171 ("[T]he motivation for the developed country to conclude the agreements was to obtain protection for its foreign investment."); Tobin and Busch 2010, 2 ("Wealthy states want BITs as an institutional check against uncompensated expropriation."); Salacuse 1990, 661 ("[Developed states] primary objective has been to create clear international legal rules and effective enforcement mechanisms to protect investment by their nationals in the territories of foreign states."); Hamilton and Rochwerger 2005, 1 (claiming the "aim [of BITs is] to encourage foreign investment").

[55] See, e.g., Salacuse and Sullivan 2005.

[56] See Simmons 2013.

[57] *See* Tobin and Bush 2010.

[58] See, e.g., Gann 1985, 440-41 (arguing that the U.S. BITs program is "an important tool in [the U.S.'s] overall effort to establish and economic and legal environment that fosters the efficient allocation of international capital").

6.3 A Political Theory of BIT Formation

Although it is likely at least partially true that the United States hoped that signing BITs would help to promote and protect investment, there are serious flaws with the argument that the United States signed BITs with the primary intent of protecting the capital of American investors. In this part, I lay out these flaws and then make the case that these limits should cause us to call the conventional narrative on the motivations of the United States' BITs program into question. I argue that BITs instead should be understood as a tool used to improve relationships with strategically important states, and that, as a consequence, evaluating their efficacy requires examining the political benefits that the United States has received from its investment treaty partners. To make that case, I first present a range of evidence suggesting that the United States' BITs program cannot be explained purely as an effort to protect investors. Second, I develop a theory that the United States used BITs as a method to cultivate improved relationships with politically important countries. Third, I develop a list of testable hypotheses that logically arise from that political theory of BITs.

6.3.1 Limits to Economic Explanations for BIT Formation

As noted above, many prior explanations for why America started a BITs program have centered on the United States' desire to secure investor rights in developing states while also promoting market friendly international economic law.[59] This explanation for why a developed state would negotiate BITs is not unique to the United States, but instead has been attributed to wealthy states more broadly.[60] This explanation, however, fails to account for why the United States, or wealthy countries more generally, would ratify BITs with *particular* developing states. Moreover, there are several reasons why a purely economic based theory of BITs—namely, the theory that they are pursued solely as a means to secure investor rights—is insufficient to account for the growth of BIT development and ratification over the last several decades.

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[59] See Gann 1985, 439-41; Sachs 1984, 195; Vadevelde 1993; Elkins, Guzman, and Simmons 2006, 815-16 (citing a statement by Secretary of State George P. Schultz on the early design of United States' BITs).

First, there is evidence that the government officials who negotiated BITs did not expect a large increase in Foreign Direct Investment (FDI) as a consequence of the agreements.[61] In the words of one commentator, "as veteran U.S. BIT negotiators have repeatedly pointed out, U.S. negotiators have routinely alerted prospective BIT Partners not to expect that BITs would necessarily increase such flows from U.S. investors . . ."[62] Of course, it is still possible that the United States hoped that BITs would help to protect existing investments, or the investments that would be made after the creation of a BIT, even if they were not flowing at a higher rate. The importance of this admission, however, is that if U.S. negotiators were skeptical that specific investment treaties would lead to new FDI from U.S. based investors, it is plausible that the officials pursuing the BITs were motivated by reasons other than simply increasing protections for American citizens and corporations.

Second, it appears that there is little pressure on the United States to *ratify* a BITs that it has signed. As evidence of this fact, I have collected data on the amount of time that Congress spent considering BITs and PTAs. As Table 6.1 shows, it has taken an average of 1,259 days for BITs that the United States has signed to be ratified and go into effect.[63] This compares to an average of 493 days for the PTAs that the United States has signed to go into effect.[64] Perhaps even more interestingly, all of the BITs that the United States Senate has ratified have been approved by unanimous voice votes. This suggests that the long delays that have occurred before BITs are ratified are not a result of political opposition, but instead occur because the ratification of BITs is not a priority. This information also makes it plausible to infer that investors do not aggressively apply pressure to the Senate to approve BITs so that they can enjoy the increased protections that purportedly motivate the creation of the agreement in the first place under conventional views.

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[60] See Tobin and Busch 2010, 2 ("Wealthy states want BITs as an institutional check against uncompensated expropriation.").

[61] See, e.g., Vandevelde 1998, 524. See also Vandevelde 1988, 212 ("United States negotiators were candid, however, about the lack of evidence that BITs actually would attract new investment.").

[62] Alvarez 2010, 621 n.69; Vandevelde 1993.

Table 6.1: Consideration of BITs & PTAs by the U.S. Congress

	BITs	PTA
Days Until Introduced to Congress	442	427
Days Under Consideration by Congress	446	37
Days between Passage and Going Into Effect	429	328
Total	1,259	493

Third, BITs have not had an effect on investment flows between the United States and the countries that it has negotiated these treaties with.[65] Overall, the evidence that BITs directly increase the flow of FDI between the two countries that have negotiated them is mixed.[66] In the last several years, there have been a number of studies showing that BITs do not have any direct positive effect on FDI,[67] whereas competing studies using different methodology that focus on total increases in investment—and not just bilateral increases—have found that BITs do in fact have a positive effect on overall FDI.[68] One recent commentary on the state of the scholarship on the topic concluded that only bilateral "investment flows between BIT parties find that BITs have little impact, whereas studies focusing on overall investment flows into BIT parties find that they have positive effects."[69] When looking solely at the United States, however, the existing evidence is much clearer: the U.S. BITs program has not had a statistically significant influence on investment patterns between the United States and its treaty partners.[70]

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[63] For a list of the time that BITs were under consideration by Congress, see Appendix 5.3.

[64] For a list of the time that PTAs were under consideration by Congress, see Appendix 5.4.

[65] See Peinhardt and Allee 2012.

Fourth, there is evidence that BITs might not influence investment decisions.[71] In one recent study, Jason Webb Yackee compiled evidence from a number of unique sources to argue that BITs do not impact the investment decisions of U.S. companies. Specifically, this study provided evidence from a survey of general counsels to United States-based multinational corporations and found that these individuals did not believe that the presence of a BIT impacted their companies' investment decisions.[72] Moreover, Yackee also compiled survey evidence from providers of political risk insurance that found that those insurers do not factor the presence of a BIT into their underwriting decisions.[73] These alternative sources of evidence also suggest that the United States may be interested in BITs for more than purely economic reasons.

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[66] See generally Schaffer and Ginsburg 2012, 36-38 (summarizing the existing literature on the impact of BITs on investment). See also Yackee 2010, 405-14 (reviewing empirical studies assessing the impact of BITs on FDI).

[67] See, e.g., Yackee 2010. See also Gallagher and Birch 2006 (finding no increase in U.S. investment as a result of BITs).

[68] See, e.g., Neumayer and Spess 2005; Salacuse and Sullivan 2005.

[69] See Schaffer and Ginsburg 2012, 37.

[70] See Peinhardt and Allee 2012.

[71] See generally Yackee 2010.

6.3.2 A Political Theory of BIT Formation

Taken together, the arguments presented in the previous section of this paper all suggest that the United States' continued use of BITs may be attributable to more than a simple desire to increase protections for individuals and corporations that invest their capital abroad. Instead, this evidence suggests the United States may have other motivations for negotiating BITs generally, and for picking which specific countries to negotiate them with. My theory is that, counter to the conventional narrative of the United States' BITs program, the United States was not motivated to sign BITs because it was concerned with protecting investments abroad, but instead used BITs as a foreign policy tool to improve its relationships with developing countries. In this section, I systemically outline the motivations for both the United States and potential treaty partners in signing BITs.

1. Features of BITs make them a useful foreign policy tool

The United States wants to improve its relationship with existing and potential allies, and BITs present a promising way to do that for at least four reasons. First, BITs are low cost. Unlike other tools that can be used to improve alliances—such as foreign aid—BITs do not require the United States to outlay funds. Second, BITs only require the United States to undertake “redundant” promises; or at least they were initially seen by U.S. policymakers that way.[74] That is to say, investors with capital in the United States are already given access to U.S. courts, and the government believed that it was unlikely to expropriate capital in any event.[75] Thus, the promises extracted from the US were already things the government had pledged to do and created no new obligations. Third, BITs are easy to sell domestically. To both the United States Congress and the public, these treaties are easy to present as a way to ensure that American investors are protected and given the same legal rights abroad that America extends to foreigners. Fourth, there is a standard model in place so that negotiating additional BITs requires relatively little effort.[76]

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[72] See id. at 426-34.

[73] See id. at 422-26.

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[74] See Gann 1985, 374 (“From the United States’ standpoint, the rights and duties under the BITs are redundant because investments in the United States already receive substantial and nondiscriminatory protection.”).

[75] *See also Investment Protections in U.S. Trade and Investment Agreements: Hearings Before the Committee on Ways and means of U.S. House of Representatives*, 11th Cong. 65 (2009) [hereinafter *2009 Hearings*] (testimony of Linda Menghetti, Emergency Committee For American Trade) (noting that protections for foreigners are “somewhat redundant in that they have very strong protections already in the U.S. law and Constitution. And when they do challenge it, what they find is, again, the U.S. provisions form takings to due process and transparency issues all incorporated in that dispute resolution process . . . .”).

[76] *Bilateral Treaties Concerning the Encouragement and Reciprocal Protection of Investment: Hearing Before the Senate Committee on Foreign Relations*, 104th Cong. 4 (1995) (testimony of Daniel Tarullo, Assistant Secretary of State for Economic and Business Affairs) (“The BIT Program, I think we should take good note, is a relatively low cost . . . [because] . . . BITs are negotiated on the basis of a prototype document and only minor changes to that prototype language are generally accepted. As a result, the program requires only modest negotiating resources.”).

The United States' experience negotiating a BIT with Grenada make these benefits clear.[77] In 1983, the United States invaded Grenada. Three years later, the Grenadian Prime Minister was receiving political treatment at Walter Reed Army Hospital.[78] During that time, the United States Trade Representative visited the hospital and signed a BIT—without any deviations from the standard model—with the prime minister after just an hour of discussion.[79] After the signing, the United States was able to use the BIT as evidence that the invasion had been a success,[80] and Grenada was able to point to the BIT as a sign of the regime's political stability and close ties to the United States.[81]

2. The United States Is Able to Use This Tool to Improve Relationships

My theory thus posits that the United States has a robust BITs program because it is willing to use BITs as a foreign policy tool in situations where it hopes to improve its relationship with a developing state. If the United States were hoping to simply promote international law that is favorable to investment and provide protections to America individuals and companies, the United States should theoretically be willing to negotiate, sign, and ratify BITs with any state that was willing to agree to the U.S. model BIT. This at least would be a reasonable conclusion given the twin beliefs by American negotiators that BITs will not impact existing investment flows[82] and that they only require the United States to make redundant concessions.[83] If the United States were instead concerned with the political ramifications of signing a BIT, however, the U.S. might only be willing to sign BITs with governments it wished to either become allies with, or with which it sought to improve existing alliances; this is exactly what has occurred.

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[77] See generally Vandevelde 1993, 169.

[78] Id.

[79] Id.

[80] Id.

[81] Id.

The fact that the United States is only interested in signing BITs with countries it hopes to improve its alliances with, regardless of their investment potential, can perhaps best illustrated by the United States' failure to ratify the BITs it negotiated with Panama and Haiti.[84] Both countries were among the first to negotiate and sign BITs with the United States.[85] Before the Senate approved those two BITs, however, the governments in both countries were overthrown by regimes that the United States did not support.[86] As a consequence, the United States did not ratify the agreements with these countries because doing so was deemed inconsistent with the foreign policy objectives of the United States.[87] In other words, the United States cared less about the potential to protect American investments in Haiti and Panama—places experiencing serious unrest where such investor protections might therefore be thought to be uniquely important—than it did about the politics of having the treaty in place with a government that it did not support.

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[82] See Alvarez 2010, 621.

[83] See Gann 1985, 374.

[84] See generally Vandevelde 1993, 169-70.

[85] See Appendix 5.1.

[86] See generally Vandevelde 1993, 169-70.

[87] Id.

3. Potential treaty partners sign benefits because of domestic and international rewards

If foreign governments are warned that BITs will not lead to new investment, the next natural question is why foreign governments would be willing to sign them. After all, the BITs do place obligations on the treaty partner, but they do not result in clear benefits. I propose that the answer is that foreign leaders believe that the BITs will either receive domestic benefits or future rewards from the United States. The domestic benefit is that the will be able to hold up the treaty as a sign of increased support from a major world power, that they can argue has the potential (whether they believe it or not) to produce economic benefits. Additionally, I posit that, since the treaty partners are aware that the United States has signed BITs with countries that it wishes to be close to, countries have the reasonable expectation that the treaty may lead to non-economic rewards.

The delay in the U.S. BITs with Senegal and Morocco going into effect perhaps illustrate the fact that signing a BIT with the United States may be able to provide domestic benefits to the political leaders of treaty partners. Senegal signed a BIT with the United States in 1983, and Morocco did so in 1985.[88] Both of these BITs were ratified by the United States in 1988.[89] Instead of ratifying the BITs promptly, however, both of the partner countries to the ratified BITs delayed doing so. Senegal did not ratify its BIT with the United States until late 1990, and Morocco did not do so until the summer of 1991. The reason that these countries delayed ratification is that they hoped to be able to ratify the BITs at a sufficiently prominent ceremony in their own country to highlight the occasion, and delayed ratifying until they were able to do so.[90] In other words, the countries were not in a hurry to gain investment, they were instead determined to extract a domestic political benefit from having signed a treaty with the United States.

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[88] See Appendix 5.1.

[89] See Appendix 5.3.

The fact that U.S. Treaty partners also hope for rewards from the United States can be illustrated by the case of Uruguay. Professor Jeswald Salacuse argues that in 2005 a liberal government in Uruguay was motivated to renegotiate a BIT that had been signed, but not ratified, with the United States because the Uruguayan government believed that it would help to strengthen the two countries' relationship more broadly.[91] Based on this evidence, Salacuse concludes that, "even though a developing country may not be guaranteed to see increased investment flows from its developed-country treaty partner as a result of signing a BIT, it may have strong expectations that the BIT will result in other benefits."[92]

4. BITs Produce Political Benefits After Ratification for the U.S.

As I have argued, BITs are a foreign policy tool that the United States uses with potential allies, who are willing to sign the agreements because of the expectation that they will produce domestic political benefits and potential rewards from the United States, even if they do not lead to have an impact on the level of the countries foreign direct investment. The reason that this is the case is that the BITs provide domestic political cover for leaders in developing states to move closer to the United States on international issues. For example, if a leader believes that supporting the United States' foreign policy goals will be in the long-term interest of the countries government, it will be easier to do so if it can point to its special investment status with the United States.

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[90] *Bilateral Investment Treaties Treaty Doc. 99-14 and Treaty Doc. 101-18: Hearing Before the Senate Committee on Foreign Relations*, 101st Cong. 11 (1990) (testimony of Eugene J. McAllister, Assistant Secretary for Economic and Business Affairs, Department of State) [hereinafter *McAllister Testimony*].

[91] Id.

[92] Id.

This view finds support in the opinions expressed by the United States State Department in Senate Hearings in the passage of BITs. As previously noted, State Department negotiators consistently expressed the view that they do not think that signing a BIT with the United States will have an impact on American investment patterns in the partner country. During congressional hearings, however, the State Department has expressed its pleasure that it is signing BITs with states that are of greater political importance,[93] and subsequently, its view that supporting particular regimes with BITs will increase their reliability as allies. In other words, by lending support to foreign leaders by signing the BITs, those leaders will later be more predisposed and able to support the United States—regardless of what investments materialize.

6.3.3 Testable Hypothesis of the Political Theory

The theory of BITs that I have just outlined is able to produce several testable hypotheses. First, I have argued that the United States has not been motivated to sign BITs because of concerns about investment risk with particular allies, but instead because there is a perceived political benefit to signaling a stronger relationship with a particular country. If this is true, countries that the United States signed a BIT with should not be more likely to be risky hosts for investment, nor should they be more likely to be exceptionally popular destinations for United States investments. As a result, this theory suggests:

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[93] *McAllister Testimony*, 13 ("[T]he countries we are negotiating with are, in general, larger economies and *have a greater political significance* than the earlier round of negotiations we did for investment treaties. We are very pleased with that.") (emphasis added).

H_1 : Risk to U.S. investment will not predict which countries the United States has formed BITs with.

H_2 : U.S. investment flows will not predict which countries the United States has formed BITs with.

Additionally, I have not only argued that investment considerations fail to explain the pattern of which countries that the United States has signed BITs with, but instead, that the countries were selected based on their strategic and political importance to the United States. As a result, measures of a country's strategic importance to the United States should be a reliable predictor of which countries the United States has negotiated a BIT with. The third hypothesis is thus that:

H_3 : Political considerations will predict which countries the United States has formed BITs with.

Finally, this theory has argued that, even if the signing of a BIT with the United States does not have an impact on investment patterns, the BIT partner will still become a more reliable ally of the United States as a consequence of the BIT. This is because the BIT will make it easier for the foreign leader to support the United States, and more predisposed to do so. As a result, the fourth hypothesis is that:

H_4 : Countries that have formed BITs with the United States should be more likely to provide political support to America.

6.4 Data

This part of the paper describes that process used to create a dataset to test the theory outlined in the previous section. First, I describe the empirical approach taken by this paper and the general construction of the dataset that was required to facilitate this approach. Second, I explain the dependent variables that were used to test the hypotheses I have previously outlined. Third, I explain the independent variables that were collected to test those hypotheses while controlling for other important factors that would have influenced the decisions to sign BITs with treaty partners, and the consequences of those agreements. This data will then be used in Part 6.5 to test the determinants of signing BITs, and in Part 6.6 to test the consequences that signing BITs have on support for U.S. foreign policy objectives.

6.4.1 Universe of Cases

The dataset created for this project includes an observation for each dyad in which the United States is a member from 1981 to 2009.[94] The reason is that this period covers the time during which the United States had an active BITs program.[95] This resulted in a dataset of 5,172 observations. For each observation, the value for each independent variable that varies over time was collected for six years (the observation year and the prior five years). As will be explained later in Part 6.6.1, collecting several years of data for each observation made it possible to find the best possible match for every observation for which the United States had a BIT in a given year to a comparable country where there was not a BIT; this allows for more credible causal analysis of the consequences of the BITs the United States has signed.

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[94] As a basis for the dataset, I took every U.S. dyad from 1981 to 2009 from the Correlates of War Trade Flow Dataset. See infra note 121.

[95] The time covered for the dataset ends in 2009 because of limitations on the availability of independent variables that have not yet been released for more recent years.

6.4.2 Dependent Variables

As has previously been noted, this project seeks to understand why the United States has signed BITs with particular countries, and then to analyze the political consequences of those agreements. As a result, multiple dependent variables were used for this project.

The first dependent variable—which was used to study the factors that influence BIT formation in Part 5is the presence of a Bilateral Investment Treaty between the United States and the other member of each dyad in a given year. Data was collected on both when the treaty was signed, and when the treaty went into effect. The coding of BITs that the United States is party to that have been signed and entered into force was taken from a list maintained by the State Department.[96] One thing that is worth noting is that I did not include Preferential Trade Agreements that included investment chapters in the list of BITs (i.e. NAFTA). The reason is that these agreements are considerably more complex than a BIT and are likely motivated by different factors; as a result, it would be inappropriate to group them into a rigorous analysis of the U.S. BITs program.

The next set of dependent variables—used to evaluated whether the BITs program has produced political dividends for the United States in Part 6.6—were chosen because they measure the strength of relationships between two states in a way that makes it possible to detect a benefit for the developed state. One previous study that looked at the rewards of treaty ratification has considered a range of dependent variables, including trade flows, foreign aid, and favorable statements made by foreign officials.[97] Although these are undeniably appropriate measures to test certain theories, they present a clear shortcoming for the purposes of this study. That problem is that these material rewards of treaty ratification are likely to flow from developed to developing states. For example, foreign aid may increase to developing states after they have concluded a BIT with a developing country, but foreign aid does not flow in the opposite direction. In other words, Rwanda may expect that signing a BIT with the United States will lead to being viewed more favorably when it is time for America to decide how to distribute foreign aid dollars, but it would be patently unreasonable for the United States to expect aid from Rwanda.[98] That said, this does not mean that the United States does not expect a benefit from negotiating a BIT with Rwanda, it is simply means that the political benefit expected to result may be more difficult to measure. The challenge is thus to find dependent variables that capture the strength of a geopolitical relationship where the developed state would be likely to be on the receiving end of the benefit.

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[96] *See* "United States Bilateral Investment Treaties," *available at* <http://www.state.gov/e/eb/ifd/bit/117402.htm> (last visited December 7, 2012). See infra Appendix 5.1.

The first dependent variable used as a proxy for the strength of the United States' relationship with its BITs Partner is the affinity in United Nations General Assembly voting between the two countries.[99] Voting in the UN is a public action that, although it may often be purely symbolic, is at least possible to clearly measure as a proxy of closeness in preferences and policies between states. Given these properties, previous international relations scholarship has used UN voting as a way to directly measure changes in the relationships and alliances between states over time.[100] One specific variable that has been developed to measure similarities between countries in their UN voting is Erik Gartzke's *UN Affinity* variable.[101] This variable records the correlates of every country's UN votes in each year's General Assembly,[102] and then rank-orders those correlations on a scale of -1 to 1. The advantage of this process is that it is able to incorporate information on the entire system of correlations instead of simply looking at the how many votes countries have in common in a given year. This variable was recently updated and made publically available by Erik Voeten,[103] and is used as the dependent variable for this study.[104]

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[97] See Nielsen and Simmons 2011.

[98] For a brief discussion of the relationship between foreign aid and BITs, see Salacuse 2010, 442 n.75.

[99] See Erik Voeten, *Data and Analyses of Voting in the UN General Assembly* (2012), *available at* <http://ssrn.com/abstract=2111149> (last visited December 29, 2012).

Of course, it is important to note that UN Affinity scores are a somewhat crude and noisy measure of the strength of relationships. There are many determinants of a country's UN voting; countries decide how to respond to measures by evaluating far more factors than simply whether a country with which they share a strong relationship and high affinity would prefer for them to vote one way or the other. Also, UN voting might be a symbolic action that does not reflect other foreign policy preferences that are more important to a country. Additionally, the Affinity in any given year may be driven in part by the issues presented in that year, making trends a function of the votes and not a function of changes in underlying affinity.

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[100] Voeten 2004; Voeten 2000.

[101] See Gartzke 1998, 14; Gartzke 2000.

[102] It is worth noting as a minor point that UN General Assembly voting is not always completed by the end of the calendar year in which it begins. In these cases, the voting is attributed to the year in which the session began. See Voeten 2012.

[103] See Erik Voeten, *United Nations General Assembly Voting Data*, v.4, *available at* <http://dvn.iq.harvard.edu/dvn/dv/Voeten/faces/study/StudyPage.xhtml?studyId=38311&studyListingIndex=0_dee53f12c760141b21c251525332> (last visited December 8, 2012).

[104] I specifically use the "S2UN" variable as the primary dependent variable for this study. For a robustness check, however, I perform the same analysis presented in Part 5.6.2 using the "S3UN" variable as well.

Although these concerns are valid, these problems with UN Affinity should be randomly distributed between countries that have BITs with the United States and those that do not, so these problems hopefully should not skew the results of using UN Affinity as a dependent variable for this analysis. Also, despite these concerns, the advantages of the measure still outweigh the drawbacks.[105] Specifically, all countries (that are members of the UN) vote in the UN General Assembly in every single year. This makes it possible to look at changes in patterns over time by examining countries' behavior in this arena.[106] Moreover, UN voting occurs on a range of topics in each year, meaning that the UN Affinity measure is a broad-based measure of the similarities in countries' preferences. Finally, it has even previously been hypothesized that BITs may be negotiated in part as a signal about future intentions in UN voting.[107] As a result, UN Affinity scores are actually an excellent measure to examine year-to-year changes in the

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[105] See Gartzke 1998, 14 (explaining the benefits of looking at UN voting as a measure of affinity between countries).

[106] See, e.g., Voeten 2004.

[107] See Alvarez 2010, 621 ("Other [countries] may have concluded BITs with the United States to express solidarity with the United States vis-à-vis other issues—or even to signal that it would now vote with the Unites States should NIEO-type resolutions be proposed in the General Assembly.").

United States' strength of relationships with treaty partners that are a consequence of signing BITs.

The second dependent variable used to test whether the U.S. BITs program has produced political dividends is whether United States troops were deployed in a country in a given year. There has been a line of research examining the effects that the United States's commercial relationships with developing countries have on the likelihood that the country will be willing to allow U.S. troops to be deployed on their soil.[108] As Biglaiser and Rousen (2009) demonstrated, the United States often must provide developing countries with economic incentives to be able to later station troops within their soil.[109] These incentives help to soften domestic opposition to the presence of U.S. troops.[110] Trade concessions are thus a strong predictor that U.S. troops will later be stationed within a given country. As a result, it would be reasonable to hypothesize that signing a BIT with a developing country would make it more likely that that state will later allow U.S. troops on their soil. The variable used to test this is a dummy variable for whether the United States has troops stationed within a given country each year. This was taken from data compiled by the Heritage Foundation that documents all U.S. troop deployments through 2005.[111]

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[108] See, e.g., Biglaiser and De Rousen 2009; Biglaiser and De Rousen 2007.

[109] See Biglaiser and De Rousen 2009, 261.

[110] Id.

[111] See Tim Kane, Global U.S. Troop Deployment, 1950-2005, *available at* <http://www.heritage.org/research/reports/2006/05/global-us-troop-deployment-1950-2005> (last visited April 6, 2013).

The third dependent variable used to test whether the U.S. BITs program has produced political dividends is whether the partner country was a member of the Iraq War Coalition in 2003. At the time of the invasion of Iraq in March 2003, many traditional allies of the United States in the developed world—such as Canada, France, and Germany—were not members of the coalition supporting the war.[112] Instead, the countries comprising the coalition were frequently developing states. Prior research has suggested that these developing states were members of the coalition because of their prior economic and strategic linkages with the United States.[113] Given this, one test of whether BITs have been successful at improving relationships in the developing world would be to examine whether the prior presence of a BIT resulted in a country being more likely to be part of the Iraq War Coalition. This dependent variable is a dummy variable for the year 2003 only, and was based on information released by the White House on March 27, 2003.[114]

6.4.3 Independent & Control Variables

The next step in this process was collecting a set of independent variables for the project. The difficulty in doing so is that previous efforts to theorize and test factors that give rise to the formation of BITs have been limited. Although there has been some research into the factors that predict treaty ratification in general,[115] this author is unaware of any empirical studies that have tried to explain the factors that determine which countries the United States forms BITs with. As a result, this study focused on collecting variables that are widely used by scholars of international law and international relations that may help to explain trends in treaty formation more generally.

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[112] For a complete list of the members of the coalition, see Appendix 5.5.

[113] See Newnham 2008.

[114] White House Press Release, March 27, 2003, *available at* <http://georgewbush-whitehouse.archives.gov/infocus/iraq/news/20030327-10.html> (last visited April 6, 2013).

[115] See Miles and Posner 2008. See also Lupu 2012.

To test whether BITs are a product of a desire to protect investment, three independent variables were collected. The first is *Investment Risk*. This variable is from a proprietary dataset developed by the Political Risk Services Group for business and academic research that measures the annual investment risk for every country on a twelve point scale.[116] The second variable is the annual *US FDI Outflows* of foreign direct investment from the United States to the other member of the dyad each year. This variable was taken from the historical dollars of FDI data maintained by the U.S. Bureau of Economic Research,[117] and was converted to constant dollars.[118] The third variable is the annual *US FDI Inflows* from each dyad partner, which is from the same source as the outflows data.

The control variables collected for this study fell into three categories. The first category of independent variables collected for this study measure the strength of the relationship between the United States and other countries. The first, and most direct, of these variables is whether there is a *Formal Alliance* between the United States and the other member of the dyad.[119] The measure used on this variable for the purposes of this study was whether a country had a formal defensive military alliance in place in a given year.[120] The second relationship measure collected was the *Trade Flows* between the United States and the other member of each dyad.[121] Although both the imports and exports between each country and the United States was collected, given the strong correlation between these two variables, only the value for the goods imported by the United States from each country was used in the empirical analyses presented in this paper. The final relationship measure collected was the amount of *US AID* received by the member of the dyad each year.[122]

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[116] For information on this data, see <http://www.prsgroup.com/prsgroup_shoppingcart/pc-62-7-iris-dataset.aspx> (last visited April 6, 2013).

[117] This data is *available at* <www.bea.gov> (last visited April 5, 2013).

[118] The table of inflation adjustment factors used is *available at* <http://oregonstate.edu/cla/polisci/faculty-research/sahr/sahr.htm> (last visited April 5, 2013).

[119] Gibler and Sarkees 2004; Gibler 2009.

There were two independent variables collected that serve as proxies of a state's political importance to the United States. The first is the whether the country received *Military Aid* from the Untied States in a given year. This data is taken from the Greenbook maintained by USAID.[123] This variable is a strong proxy for political importance to the United States because previous research has suggested that the recipients of military aid are perhaps the states that are most crucial to U.S. foreign policy objectives.[124] The second is the number of times that a country was mentioned each year during State Department briefings. This data was collected by scraping over 3,600 daily briefings from the State Department website that were given between January 2, 1991 and December 23, 2008, and analyzing the documents to determine how many times every country was mentioned.[125] This variable is used as a proxy for political salience to the United States. That is, countries that are frequently mentioned might not be allies like countries that receive military aid, but they are countries are receiving attention from the United States' foreign policy establishment.

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[120] Observations were coded as "1" if there was a "Type 1" alliance in place in a given year. All other observations, including observations with "Type 2" or "Type 3" alliances were coded as zero. For more information on alliance Types, see Gibler and Sarkees 2004.

[121] See Correlates of War, International Trade, 1870-2009, v.3.0, *available at* <http://www.correlatesofwar.org/>. See also Barbieri, Keshk, and Pollins 2009.

[122] See Tierney et al. 2011. This data is available at <http://www.aiddata.org/content/index/Research/research-datasets> (last visited April 5, 2013).

[123] This data is available at <http://gbk.eads.usaidallnet.gov/data/detailed.html> (last visited April 5, 2013).

[124] See, e.g., Meernik, Krueger, and Poe 1998; Poe and Meernick 1995.

In addition to these independent variables, there were three categories of control variables collected. The first category of control variables collected for this study measure the characteristics of the countries other than the United States in each dyad.[126] The first variable is a country's *Polity Score*.[127] A Polity Score is a measure that tries to categorize countries as having an autocratic to democratic government on a -10 to 10 scale.[128] The second variable that measures a country's characteristics is the Gross Domestic Product Per Capita. This variable was coded using the 2012 Penn World Tables.[129] The final variable that captures the dyad partner's characteristics is that country's Composite Index of National Capabilities ("*CINC*

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[125] My thanks to Rich Nielsen and Beth Simmons for sharing the scrapped documents that were analyzed to create this variable.

[126] The justification for collecting data for only one member of each dyad is that the values that capture characteristics of the United States would be constant in each year across all observations in the dataset.

[127] See Marshall and Jaggers 2011.

[128] The measure used for this study is actually the "polity2" variable from the Polity IV dataset. The "polity2" measure tries to correct for missing data problems that are created by regimes in transition or war.

[129] See Alan Heston, Robert Summers, and Bettina Aten, *Penn World Table, v.7.1*, Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania (2012), *available at* <https://pwt.sas.upenn.edu/php_site/pwt_index.php>.

Score").[130] This is a composite variable that tries to measure the relative "power" of countries by including a range of factors such as their population, size of military, and steel production.

Finally, two measures were included that grouped countries in categories. First, the World Bank *Geographic Region* was coded for each country.[131] Second, the World Bank *Income Category* was coded for each country.[132] These variables all helped to ensure that countries were matched with the closest possible pairs of states.

6.5 Results: The Determinants of BIT Formation

To test the theory outlined in Part 6.3, the first step is to analyze whether the set of countries that the United States has chosen to sign BITs with is better explained by investment concerns or political concerns. This section of the paper tests both theories to see which provides a better explanation of the United States' BITs program. First, I explain the empirical strategy used in this section of the paper. Second, I preform a series of tests that demonstrate that measures of investment risk have not been a determinant of which countries the United States has signed BITs with. Third, I perform a series of tests that suggest that the United States instead signed BITs with countries that are recipients of U.S. military aid—that is, politically important states.

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[130] See Correlates of War, National Material Capabilities Data Set, v.3.02, *available at* <http://www.correlatesofwar.org/COW2%20Data/Capabilities/nmc3-02.htm>. See also Singer 1972; Singer 1987.

[131] See World Bank Country Classifications, *available at* <http://data.worldbank.org/about/country-classifications> (last visited December 8, 2012).

[132] Id.

6.5.1 Empirical Approach

To test which factors influence whether the United States signs a BIT with a potential partner country, I made two moves. The first was deciding to use logit regression to model BIT formation. The structure of the data for this topic is Binary Time-Series—Cross Sectional (BTSCS). That is to say, the dependent variable (whether a BIT is formed with the U.S. in a given year) is binary, and the data includes observations for every country in the intentional system for all years from 1981, when the United States started negotiating BITs, until after the last BIT with Rwanda was signed, in 2009. Researchers generally, and international relations scholars specifically, have struggled with how to correctly model BTSCS data.[133] The general difficulty is that logit and probit models assume temporal independence between observations, whereas hazard models make an assumption that there is temporal dependence between units, and that selecting the incorrect approach will lead to an incorrect estimation of standard errors. To address this problem, I used the approach suggested by Carter & Signorino and modeled BIT formation using logit regression, but controlled for the possibility that the observations were temporally dependent three variables—time, time^2, and time^3—to account for the time that elapsed since the beginning of the BITs program.[134] The results of using this approach—which was done for every regression presented in Parts 6.5.2 and 6.5.3—did not produce any evidence of temporal dependence,[135] however including the time terms does not substantively effect the results for the variables of interest. This approach thus has the advantages of producing results that are more familiar and easier to interpret than hazard ratios.

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[133] See generally Beck, Katz, and Tucker 1998.

[134] Carter and Signorino 2010. Specifically, for the variable “t”, observations in 1981 are coded as 1, 1983 as 3, etc. Then the variable t, t^2, and t^3 are included in each of the models estimated.

[135] To put this in simpler terms, it appears the number of years that a country has gone without signing a BIT with the United States does not provide evidence as to whether it will in the future.

The second decision made for this analysis was which subsets of countries to examine. I have selected to break the data into three groups for this analysis. First, the United States BITs program has exclusively negotiated treaties with developing countries.[136] As a result, it would be inappropriate to examine the entire universe of countries together to determine which countries the United States has signed a BIT with. As a result, I first subset the data to only include countries that are not members of the OECD. Second, the United States actively pursued BITs with previously communist countries after the end of the Cold War.[137] The determinants of those decisions—that is, which post-communist countries to negotiate with—may have been different than the determinants of which not previously communist states to negotiate with. As a result, I subset the data to only include states that were previously communist. Third, the logical extension of this approach is also to look at developing countries that were never communist. The third category of countries examined was thus non-OECD countries that were not previously communist.

6.5.2 Investment Factors

In this section, I test whether investment considerations can help to explain the pattern of countries that the United States has signed BITs with. To do so, I begin by taking a very simple approach. For each of the three categories of countries just mentioned (Non-OECD, Previously

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[136] See Appendix 5.1.

[137] See Vandevelde 1993, 627-28.

Communist, and Non-OECD & not previously communist), I ran a logit regression estimating the effect of three different variables that serve as proxies for investment considerations.[138] The first, and most important, is the investment risk of a country; the second is the FDI outflows from the United States to a given country; and the third is the FDI inflow from the country to the United States. Additionally, I created a fourth variable—*Capital Stock*—that is the sum of the previous six years of US FDI Outflows to a country. The hope is that this measure can serve as a proxy for the existing U.S. investments within a country.

The results of these twelve regressions are presented graphically in Figure 6.3.[139] Each box represents a different regression, and for each, the line shown is the simulated first differences as the variable moves from its minimum to its maximum value.[140] The black dot is the point estimate for the percent change that the variable has on the likelihood of a BIT being formed in a given year, and the line represents the 95% confidence interval. Statistically significant variables are shown as solid lines, and statistically insignificant variables as dotted lines.[141]

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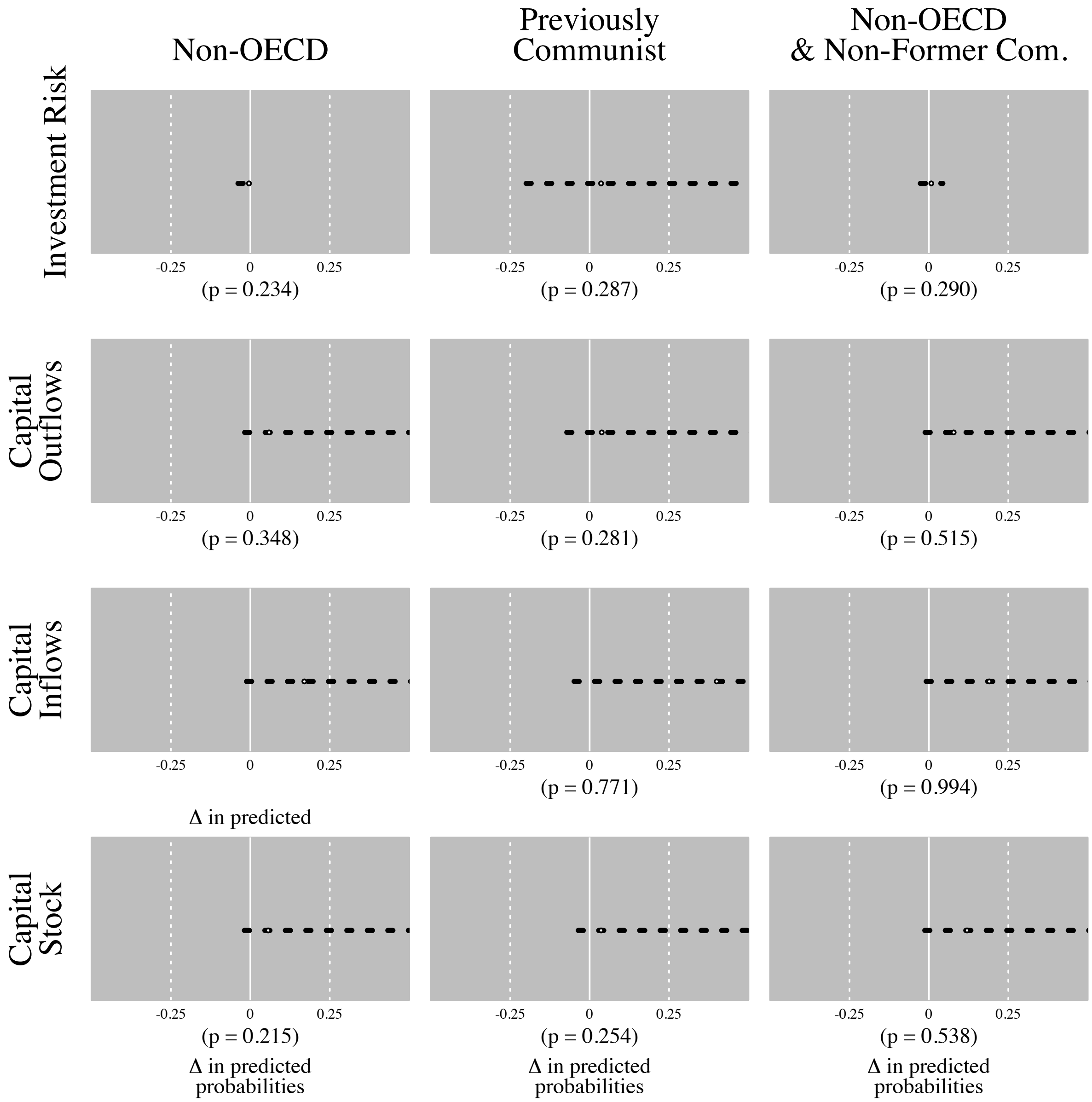
[138] As noted in the previous section, regression presented in this section all included three variables to account for time—t (years elapsed since 1981), t^2, and t^3. However, including them does not have a substantive impact of the results.

.[139] For a defense of presenting regression results graphically, see Kastellec and Leoni 2007.

[140] For an explanation of how, and why, to simulate first differences, see generally King, Tomz, and Wittenberg 2000.

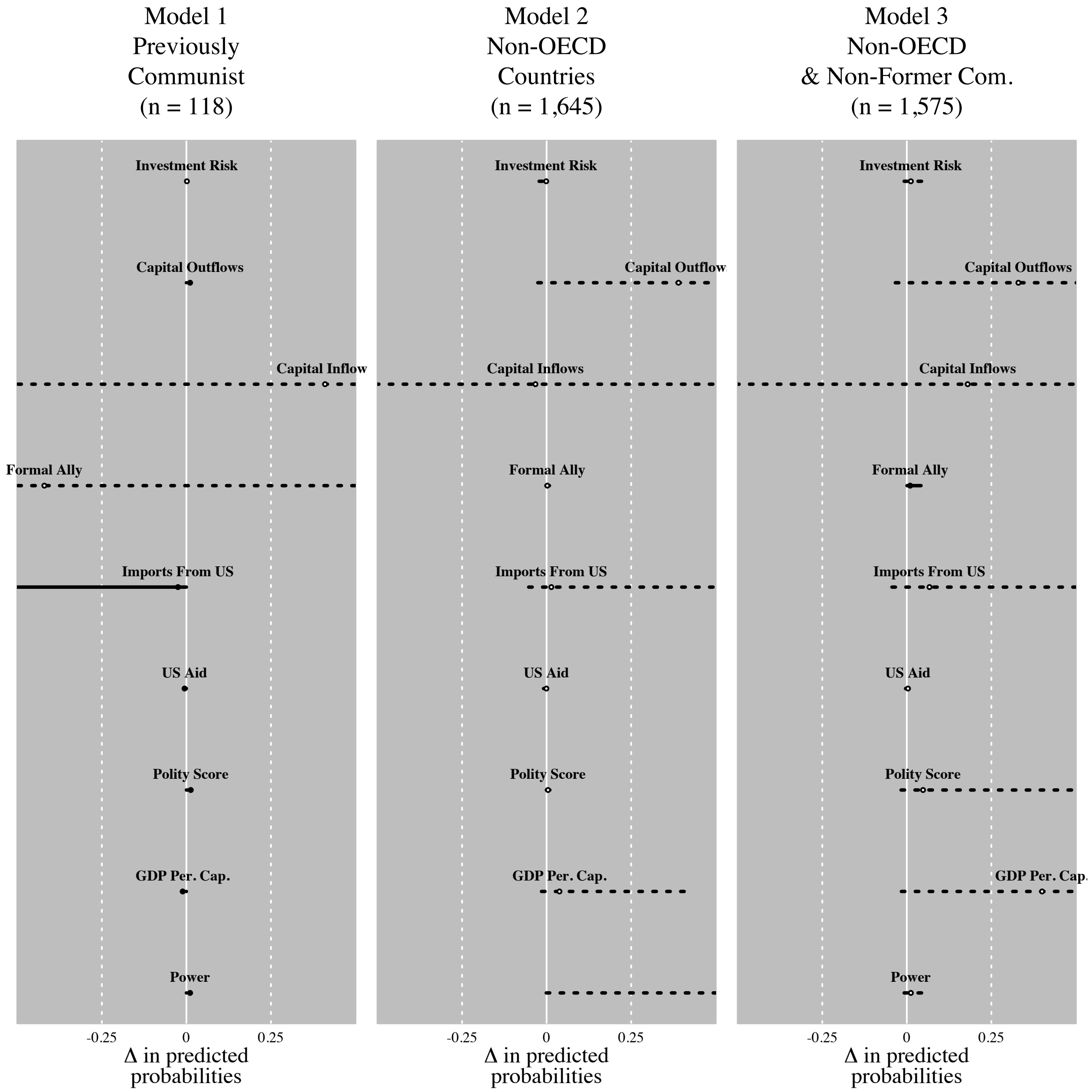
[141] All regressions conducted for this chapter were performed using "Zelig" for R. See Imai, King, and Lau 2008; Imai, King, and Lau 2007.

Figure 6.3: Bivariate Regression Testing the Influence of Investment Concerns on BIT Formation



In Figure 6.3, not a single one of the four investment variables had a statistically significant influence on the likelihood that any of the groups of countries would sign a BIT with the United States in a given year. A country's investment risk, its levels of capital inflows from the United States, its amount of capital inflows from the Untied States, and the capital stock from the previous six years did not have a statistically significant influence the chance that the country would sign a BIT with the United States in a given year. Of course, the results presented in Figure 6.3 are based on extremely parsimonious models, but they do provide a valuable quick check of whether there is evidence to support an investment protection theory of BIT formation.

Figure 6.4: Influence of Investment Concerns on BIT Formation



In an effort to test whether these variables influenced the formation of BITs, while testing for potential cofounders, I also estimated a series of logit models that include a number of control variables. These models were estimated on the same three subsets of countries that were analyzed in Figure 6.3. For each model, in addition to the variables that measure investment risk, a range of control variables were also included. The results of these regressions are shown in Figure 6.4.

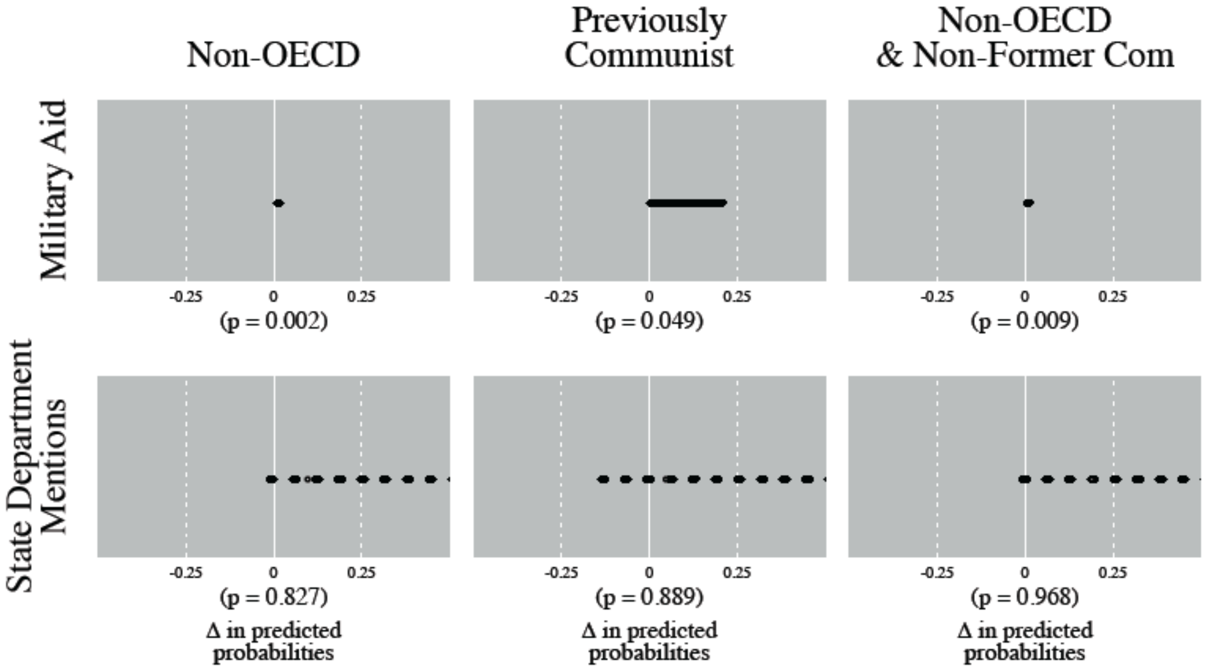
As the results in Figure 6.4 show, even conditioning on a range of control variables does not make any of the variables that measure investment concerns statistically significant for a single model. Instead, these variables are unable to provide a statistically significant prediction for whether a BIT with a country is likely to be formed in a given year for any of the relevant sets of countries. These regressions suggest that when the United States was selecting which countries to form a BIT with, that investment concerns do not help to explain the set of countries that the United States negotiated BITs with.

6.5.3 Political Factors

As the last section has demonstrated, investment considerations do not explain the pattern of countries that the Untied States has negotiated BITs with. This at least gives credence to my theory that the conventional narrative that the U.S. BITs program was primarily a tool for investment protection is incorrect. The second part of testing that theory, however, is exploring whether political considerations can provide a better explanation of which countries the Untied States decided to enter into a BIT with.

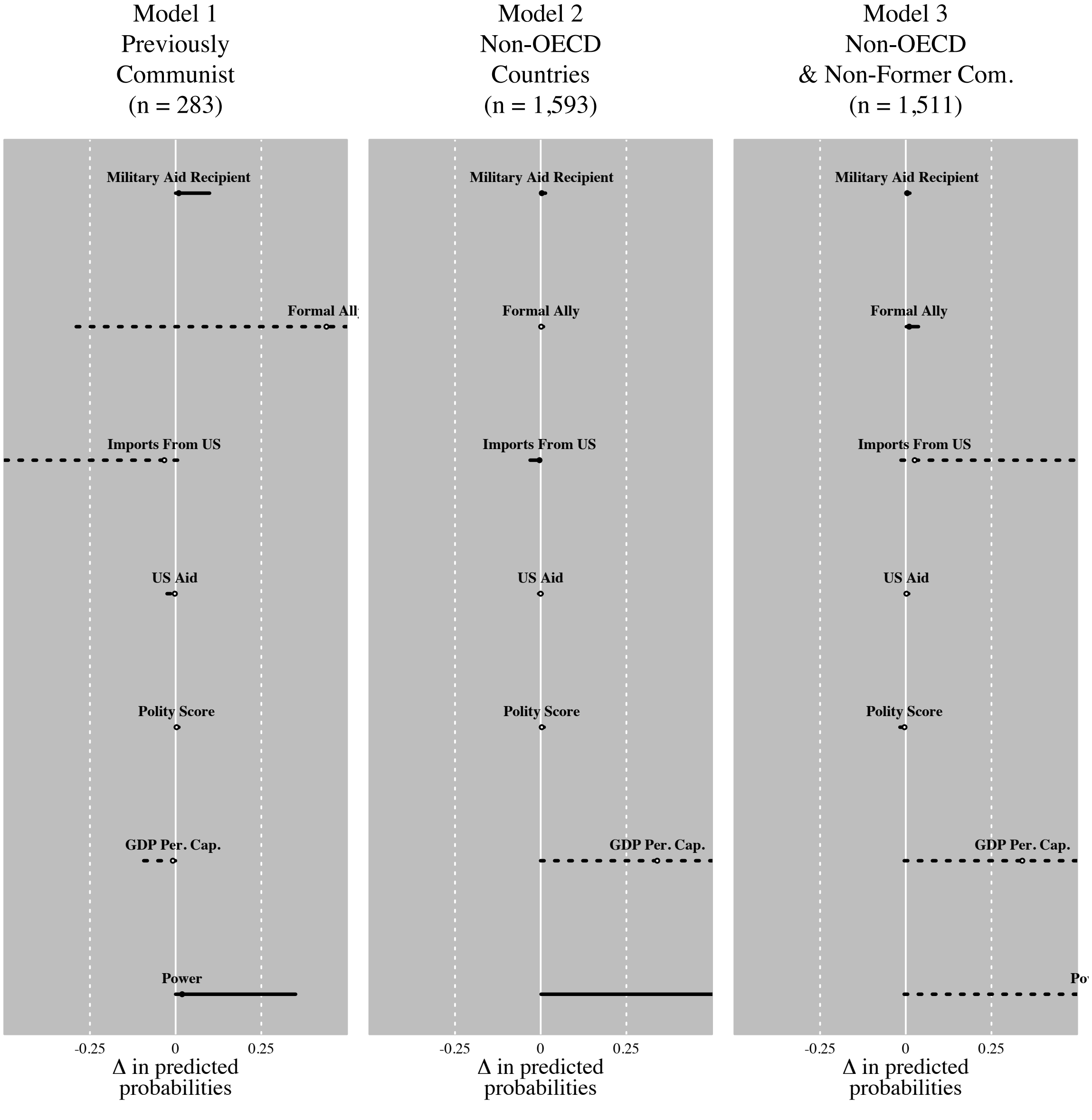
Just like in the previous section, my first step in investigating that possibility was to estimate a series of logit regressions that simply tested whether one variable was a statistically significant predictor of BIT formation. Figure 6.5 tests two independent variables this way. The first is whether a country received military aid from the United States in a given year; which, as previously noted, is a reliable proxy for whether a country is strategically important to the United States' foreign policy priorities. The second is the number of times the State Department mentioned the country in its daily briefings; which serves as a proxy for how politically salient the country is in a given year to the United States.

Figure 6.5: Bivariate Regression Testing the Influence of Political Considerations on BIT Formation



The results in Figure 6.5 shows that recipients of military aid are more likely to sign a BIT with the United States in any given year. This result is statistically significant for all three sets of countries. In contrast, the number of times that a country is mentioned by the State Department in a given year does not provide a statistically significant prediction of whether a BIT is likely to be signed. These results provide preliminary evidence that political importance does help to explain the pattern of BIT formation, but that simple overall political salience does not.

Figure 6.6: Testing the Influence of Political Considerations on BIT Formation



To test this further, I used the same approach as in the previous section and estimated a series of logit regressions that included the military aid variable, along with a series of control variables that measured each country's characteristics and relationship with the United States.

The results of this analysis are presented in Figure 6.6. The results show that Military Aid is a statistically significant predictor of whether a BIT with the United States will be signed in any given year (although for Model 1 the p-value = 0.0505). These results suggest that countries that receive military aid from the Untied States are roughly 5% more likely to form a BIT with the United States in any given year. Although these results do not provide definitive proof that BITs are motivated by foreign policy considerations, they at least suggest that foreign policy considerations are better able to predict BIT formation than investment considerations, thus lending support to the theory I outlined in Part 6.3.2.

6.6 Results: The Political Consequences of BITs

After demonstrating in the last section that investment considerations do not explain which countries the United States has signed BITs with, and that there is better evidence that such decisions were made based on political factors, the second step to testing my theory that the United States' BITs program was a tool used to improve relationships with strategically important developing countries is to examine whether BITs have produced political dividends. To do so, I have performed a series of statistical tests to try and determine whether entering into a Bilateral Investment Treaty with the United States resulted in a country being more likely to support the United States' goals along a number of dimensions. This part of the paper presents the results of those tests. First, I describe the empirical approach that was used for this analysis—Life History matching—to try and produce credible causal estimates on the impact of BITs on a series of dependent variables that are measures of political support for the United States. In the subsequent sections, I present results showing that having a BIT with the United States made the treaty partner more likely to vote similarly to the United States in the United

Nations, more likely to allow the United States to deploy troops on their soil, and more likely to have been a member of the Iraq War Coalition.

6.6.1 Empirical Approach

Perhaps the most difficult task for this project was finding a quantitative method that makes it possible to estimate the influence of BITs on state-to-state relationships while controlling for selection effects. Although the presence of a BIT is clearly associated with increases in the dependent variables evaluated in this section,[142] the more difficult question is whether the BIT *caused* the changes.

Roughly a decade ago, scholars began to empirically study the effect of treaty ratification on state behavior.[143] Although these early efforts were an important step forward that have helped to improve our understanding of international legal agreements, many of these early efforts also suffered from a number of problems with their research design.[144] One specific problem was that these efforts were not able to adequately control for selection effects.[145] That is to say, countries that had chosen to ratify treaties were systematically different than countries that had not, making it impossible to attribute observed differences between these countries to the treaties themselves.

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[142] The results of parsimonious regression models that examine the dependent variables considered in Parts 5.6.2 - 5.6.4 of this paper that do not take steps to move towards causal analysis are presented in Appendix 5.6.

[143] See, e.g., Hathaway 2002.

[144] See Goodman and Jinks 2003 (criticizing elements of the research design used by Oona Hathaway in her widely cited study of the impact of human rights regime).

[145] For a general discussion of this issue, see Hill 2010, 1161-62.

In an effort to help correct for these problems, a second wave of empirical scholarship on international law has employed a variety of quantitative methods that help to correct for endogeneity issues inherent to studying treaty ratification.[146] One method that has been particularly popular as a way to correct these problems is the use of matching.[147] Matching is the process of pre-processing data so that treated units are paired with observations that have not been treated.[148] Although there are a variety of matching methods available,[149] the basic intuition is that, if two units are paired together that are as similar as possible in every way except that one has received a particular treatment—for example, ratified a particular treaty—then systematic differences in a dependent variable of interest can be attributed to that treatment.

Although matching has allowed scholars to make considerable methodological improvements over earlier efforts to study the rights of treaties,[150] finding appropriate ways to match time-series cross sectional datasets has still been a challenge for researchers.[151] The challenge is that, when looking at time series data, it might not be appropriate to match observations based on the values that variables take in a particular year because they may be trending in opposite directions. For example, two countries might have the same value for a variable measuring democracy in a given year, but one country could be in the process of becoming more democratic while the other is sliding towards autocracy. One recently developed solution to this problem is life history matching.[152] The concept is that, for each unit of observation, variables are collected for the observation year as well as for several prior years. For example, if a research design used country-years as the level of observation, for the observation of "Haiti 1999", Haiti's GDP might be collected for 1999, 1998, and 1997 for the single observation. Then For "Haiti 2000," the GDP data would be collected for 2000, 1999, and 1998. Using this process, countries can then be easily matched on the values of a given variable for several years so that observations have similar life histories. Although this method has only recently been developed, it has already shown its promise as a way to study treaty ratification in a study that examined the rewards that developing countries receive for ratifying human rights treaties.[153] As a result, using Life History Matching is the ideal way to attempt to estimate whether the United States' BITs program has caused countries to be more supportive of the American foreign policy goals.

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[146] See Simmons 2009 (using instrumental regression); von Stein 2005 (using Heckman Selection Models); Hill 2010 (using matching).

[147] See Beth Simmons and Hopkins 2005.

[148] Ho et al. 2007.

[149] *See* King et al. 2012.

[150] Compare Hill 2010 with Hathaway 2002.

[151] See generally Nielsen 2011.

6.6.2 The Effect of BITS on UN Voting

To estimate the impact of having a BIT with the United States on UN Affinity, the first step was pre-processed the data using life history matching. To do so, I first omitted any observations that contained missing values for variables that would be later used to match the data. Next, I subset the data to exclude "treated" observations that were outside of the scope for a specific regression. For example, for the regressions estimating the impact of the first year after a BIT has gone into effect, I excluded all of the observations from countries two years after a BIT had gone into effect.

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[152] See id. See also Nielsen 2012.

[153] See Nielsen and Simmons 2012.

After sub-setting the data in this way, I then used "propensity score" matching to pair a treated observation with an untreated observation.[154] This was done separately for each regression that was performed.[155] The observations were matched based on: *Investment Risk*; *US FDI Outflows*; *UD Military Aid*; *Formal Alliance*; *Trade Flows*; *US Aid*; *Polity Score*; *GDP Per Capita*; *CINC Score*; *Geographic Region*; *Income Category*; and *Year*.[156] Using these variables, a dataset was created that contained one untreated observation for each treated observation. In all cases, the use of matching in this way dramatically reduced the covariate imbalance between the two groups—and thus made it possible to produce less biased estimates of the treatment effect of having a BIT with the United States. The balancing statistics produced by matching are shown in Table 6.2.

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[154] All matching in this paper was performed using the "MatchIT" function for R. See Ho et al. 2009.

[155] For example, for each of the four regressions presented in Figure 6.7, a separate dataset was first matched in this way.

[156] For each of the models estimated in this section, a different number of previous years data was used during the life history matching. This was to explicitly deal with the possibility of post-treatment bias. This is further explained in notes 163 – 166.

Table 6.2: Balance Statistics for UN Affinity Models

	Model 1 BIT Ratified		Model 2 BIT 3 Years		Model 3 BIT 5 Years		Model 4 BIT All Years	
	Full	Matched	Full	Matched	Full	Matched	Full	Matched
Saple Size	1,754	58	1,869	168	1,924	278	2,104	758
Treatment Units	29	29	84	84	139	139	379	379
Control Units	1,725	29	1,785	84	1,785	139	1,725	379
Mean Distance – Treatment Group	0.152	0.152	0.204	0.204	0.252	0.252	0.507	0.507
Mean Distance – Control Group	0.014	0.101	0.038	0.172	0.058	0.227	0.108	0.327
% Balance Improvement	70%		81%		87%		55%	

After matching, the next step was to run a series of linear regressions to estimate the causal effect of BITs on UN voting.[157] For each matched dataset, a model was fit to the data that included a variable for the presence of a BIT, as well as the variables used to match the data (which helped to correct for any remaining imbalance).[158] After each model was ran, I then simulated the first differences for whether or not a BIT was present while holding all other values at their mean.[159] The advantage of this approach is that it provides an estimate of the quantity of interest within a given confidence interval, which in turn makes for easy interpretation.[160]

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[157] This analysis was conducted using "Zelig" for R. See Imai, King, and Lau 2007.

[158] The variables specifically included were the same as those used for matching with the exception that *Geographic Region* was dropped.

[159] Non-numerical variables, such as *Income Category*, are set at their median.

[160] See generally King, Tomz, and Wittenberg 2000.

Figure 6.7: The Effects of BITs of UN Affinity

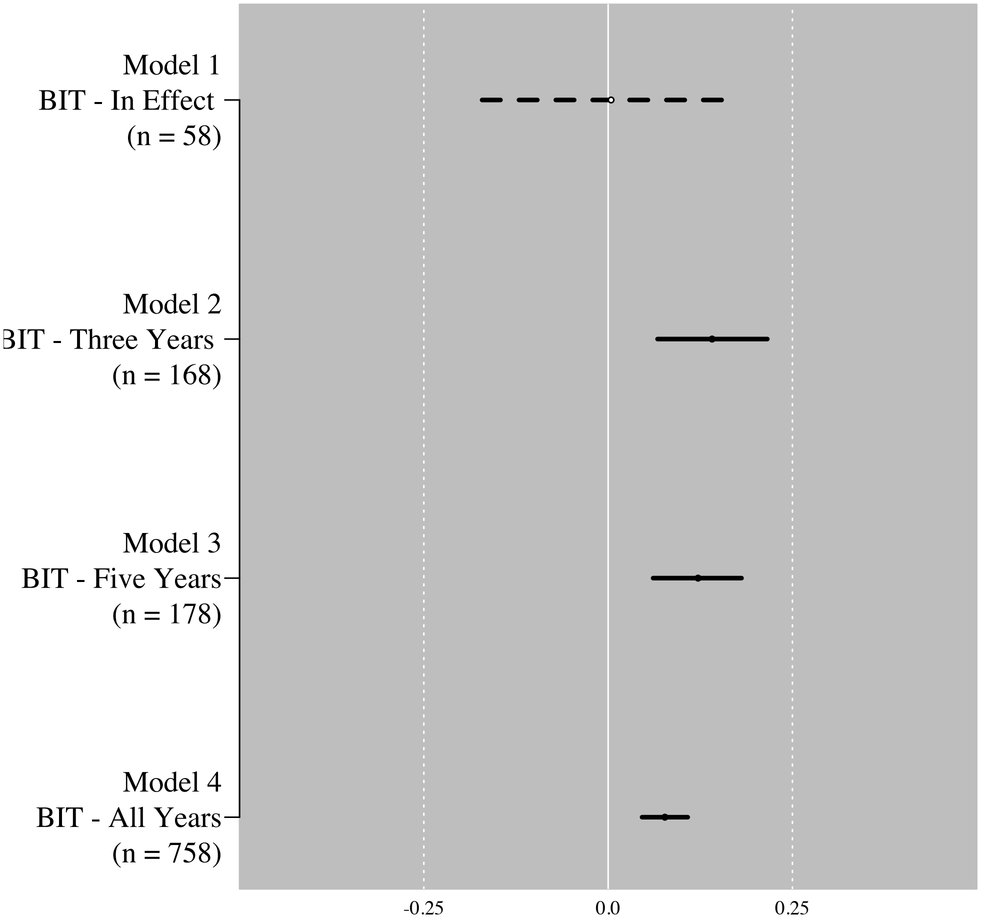

Figure 6.7 presents the results of four different regressions that were performed using this method to estimate the effect of different stages of the development of BITs on UN Voting.[161] The figure was created by first creating four separate datasets based on matched pairs of

observations from: (1) the year after a BIT went into effect;[162] (2) the first three years after a BIT was in effect;[163] (3) the fifth year the BIT had been in effect;[164] and (4) all years in which BITs were in effect.[165] Each line is then the simulated first differences from a regression performed on these datasets, and estimates to the right of zero mean that the BIT increased UN Affinity. Dotted lines represent regressions where the impact of a BIT was not statistically significant, and solid lines where the result was significant.

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[161] Like the earlier Figures in this Chapter, Figures 6.7, 6.8, & 6.9 were programed manually in R.

There are several interesting results in Figure 6.7. First, it appears that a BIT going into effect does not have an effect on UN Affinity in the following year (Model 1). In fact, the estimated effect is almost exactly zero. This suggests that developing states may not become closer to the US immediately after a BIT has taken effect, but instead only after time has elapsed and the relationship has improved. Second, having a BIT with the United States in effect for three years does produce an increase in UN Affinity the year after the BIT goes into effect that is substantively large and statistically significant (Model 2). Moreover, this remains true during the fifth year that a BIT is in effect (Model 3). Finally, the final line on the graph shows that the effect of having a BIT with the United States in all years still has a positive effect, but that this effect is smaller than the effect for BITs in the year after it has gone into effect and over the first five years of its existence (Model 4). This suggests that the impact of negotiating a BIT might not be linear or constant over time. Instead, the effect may dissipate. That said, the key result of this section is that having a BIT with the United States does result in a country being significantly more likely to vote similarly to the U.S. in the United Nations General Assembly.

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[162] This dataset was matched based on the prior three years of data for the time-variant variables, lagged one year. That is t-1, t-2, and t-3.

[163] Each observation is matched on three years of prior values for the time variant variables. To avoid the possibility of post-treatment bias, the matching was done on t-3, t-4, and t-5 for each of the time-variant variables. This is so that observations in the third year that the BIT was in effect do not contain any variable with post-treatment values.

[164] To avoid post-treatment bias, this data was matched on the data for the year before the BIT went into effect—which is the t-5 values for all time-variant observations.

[165] This data was matched using the three prior year values for each observation. In other words, this model explicitly introduces the possibility of post-treatment bias. This is because in the 10th year a BIT is in effect, that observation was matched on variables with values after the BIT had been signed.

6.6.3 The Effects of BITs on US Troop Deployments

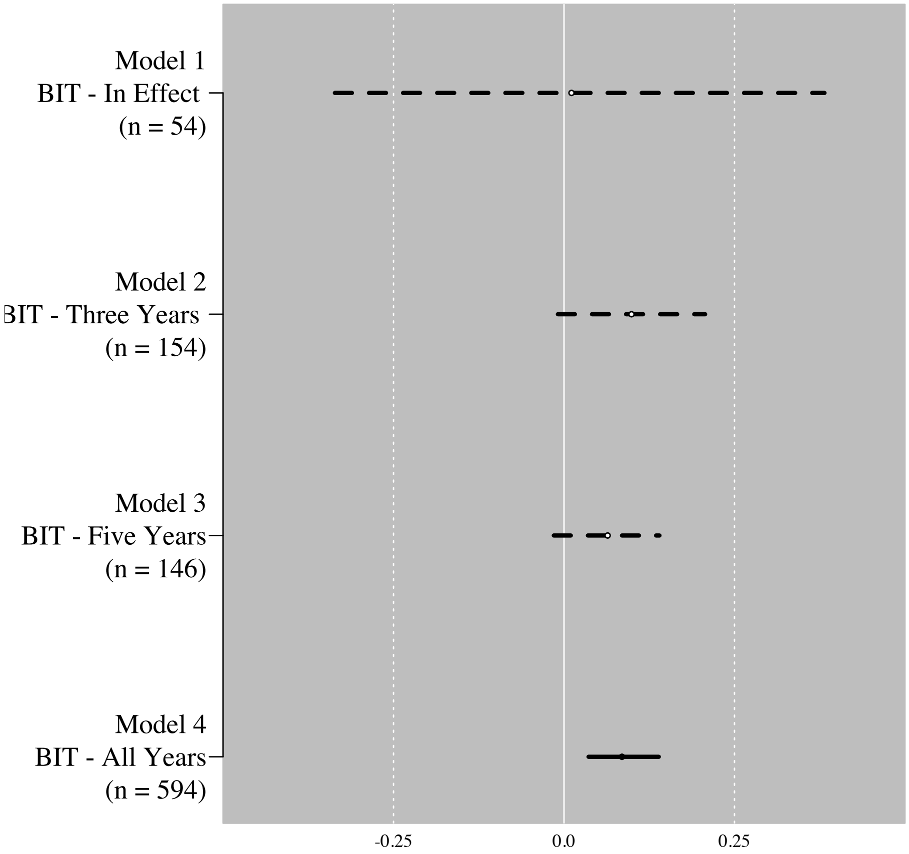
Table 6.3: Balance Statistics for Troop Deployment Models

	Model 1 BIT Signed		Model 2 BIT Ratified		Model 3 BIT 5 Years		Model 4 BIT All Years	
	Full	Matched	Full	Matched	Full	Matched	Full	Matched
Saple Size	1,623	54	1,561	154	1,607	246	1,893	594
Treatment Units	27	27	77	77	123	123	297	297
Control Units	1,596	27	1,484	77	1,484	123	1,596	297
Mean Distance – Treatment Group	0.163	0.163	0.225	0.225	0.266	0.266	0.488	0.488
Mean Distance – Control Group	0.014	0.114	0.040	0.187	0.061	0.229	0.095	0.319
% Balance Improvement	67%		80%		82%		57%	

The next test was to examine if in addition to influencing UN Voting, whether having a BIT with the United States made a treaty partner more likely to allow U.S. troops on their soil in future years. This was tested using the same method outlined in the previous section. To do so, observations were first matched using the exact same method and the same variables used to match observations to test UN Voting.[166] Once again, doing so significantly reduced covariate imbalance for all of the samples matched. This information is summarized in Table 6.3.

Once again, after matching the next step was to run a series of linear regressions to estimate the effect of BITs on U.S. troop deployment. For each of the four matched datasets presented in Table 6.3, a regression was run using the same variables that were used to match the data. I then simulated differences for the effect of a BIT. The results of this process are presented in Figure 6.8.

Figure 6.8: The Effects of BITs on US Troop Deployment



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[166] See notes 163 – 166 for information on how many years of data was used to match each model.

Figure 6.8 presents the results of four different regressions that estimate the effect of having a BIT with the United States on the likelihood that the treaty partner will allow U.S. troops to be stationed on their soil. There are several interesting results in Figure 6.8. The first is that in the year after a BIT with the United States goes into effect (Model 1) or in the first three year after the BIT goes into effect (Model 2), the treaty partner is not more likely to allow U.S. troops to be stationed on their soil. In the fifth years after that a BIT has gone into effect, this result remains true (Model 3). When looking at all the years that the BIT is in effect, however, having a BIT with the United States does make the country roughly 9% more likely to allow U.S. troops to be deployed on their soil (Model 4). These results are both substantively and statistically significant. That said, Model 4 is the only model that has the possibility of post-treatment bias because the observations are matched based on data after the BIT has been in effect. As a result, it is unclear whether a BITs eventually helps contribute to countries allowing the United States to station troops on their soil at a higher rate. Given that previous research suggests that this is important to the United States' foreign policy, and that the United States has traditionally been willing to make substantial concessions to achieve this objective, this suggests that BITs might have the potential important political dividends with countries in the developing world.

6.6.4 The Effects of BITs on Support for the Iraq War

As a third test of whether the United States' BITs program has helped to produce foreign policy benefits for the United States, I examined whether developing countries that the United States signed BITs with were more likely to be members of the Iraq War Coalition. To do so, I matched each BIT in the year it went into using the same variables described in the previous sections. One important difference, however, is that, given the small sample sixe of this analysis, I was only able to match on one year's worth of data instead of using Life History matching as I had in the proceeding two sections. The observations were still matched, however, on one year's worth of values for the variables: *Investment Risk*; *US FDI Outflows*; *UD Military Aid*; *Formal Alliance*; *Trade Flows*; *US Aid; Polity Score*; *GDP Per Capita*; *CINC Score*; *Geographic Region*; *Income Category*; and *Year*. The results of this matching are presented in Table 6.4.

Table 6.4: Balance Statistics for Iraq Coalition Model

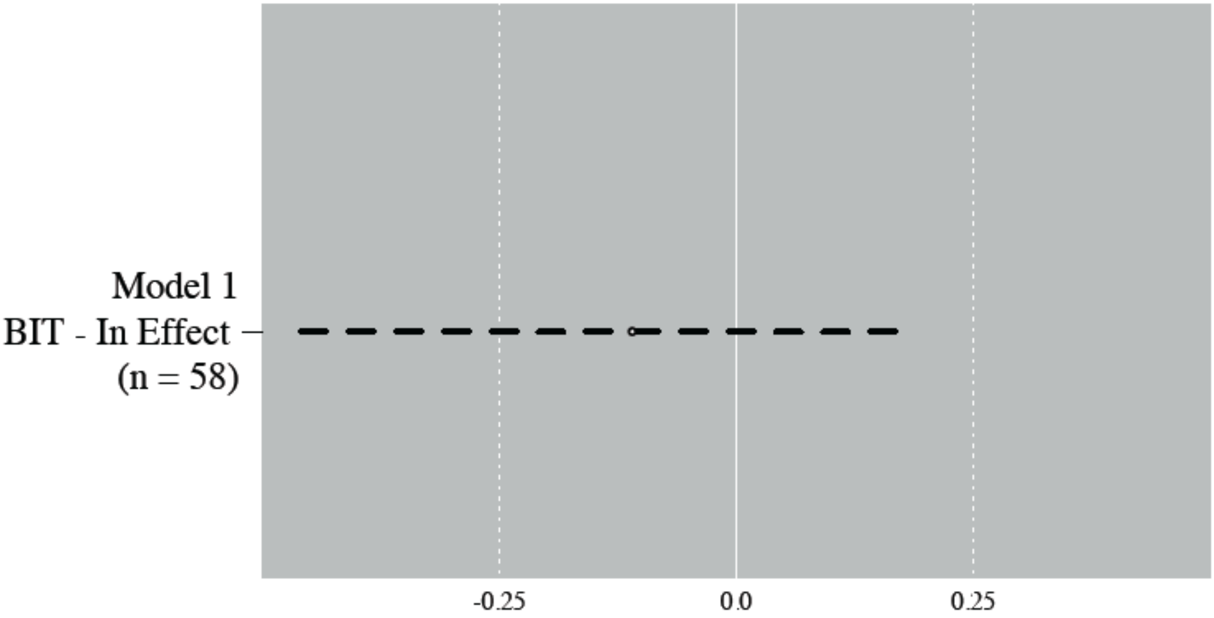
	Model 1 BIT Previously Ratified	
	Full	Matched
Saple Size	1,796	58
Treatment Units	29	29
Control Units	1,767	29
Mean Distance – Treatment Group	0.106	0.106
Mean Distance – Control Group	0.015	0.084
% Balance Improvement	76%	

After matching the data in this way, I then estimated a model testing whether a country that had a BIT in effect with the United States influenced whether that country would be a member of the Iraq War Coalition in 2003. It is worth noting that, in 2003, there had only been a few countries that had previously signed a BIT with the United States where the BIT was not yet in effect. As a result, including countries that had signed a BIT previously but where the BIT was not yet in effect did not substantively alter the results. The results of this regression are reported in Figure 6.9.

The regression presented in Figure 6.9 shows that although having a BIT with the United States is a statistically significant predictor of membership in the Iraq War Coalition (see Appendix 5.6), this result does not hold after countries are matched based on data the year the

BIT went into effect. In other words, even though Poland was in the Iraq War Coalition in 2003, it was not more likely to do so than the country it was matched with from the year its BIT went into effect with the United States. The important implication of this result, of course, is that having a BIT with the United States might be a sufficient carrot to change UN Voting, but not sufficient to change willingness to go to war.

Figure 6.9: The Effects of BITs on Support for the Iraq War



6.7 Conclusion

Scholars studying the United States' BITs program have essentially uniformly argued that this program was motivated by a desire to help promote the development of international law that is friendly to investment, and to help protect the American investments abroad. This paper argues that this conception of the program is mistaken. Instead, based on careful analysis of the historical record of America's BITs program, I argue that BITs have been a foreign policy tool that the United States has used when it wanted a low cost way to signal that it would like to improve its relationship with a specific developing country. The treaty partners, in turn, cared more about the signaling effect of BITs than they did about the investment potential. As a result, I argue that investment considerations should not help to explain the pattern of BITs that have been negotiated. Instead, political factors should help to explain BIT formation, and if the BIT program has been effective, BITs should have produced political benefits.

This paper rigorously tested that theory by both showing that investment considerations do not helped to predict BIT formation, and that political importance (as measured by military aid) does. Moreover, BITs have resulted in countries being more likely to vote similarly to the United States in the UN, may have allowed U.S. troops to be deployed on their soil, but have did not cause treaty partners to be more likely to be a member of the Iraq War Coalition. In other words, the United States' BITs program has produced modest political dividends, but has not been a magic tool to grow alliances strong enough to cause a country to follow America to war.

Of course, it is important to note that it would be a mistake to interpret these results as strictly causal. In other words, although these results show that having a BIT with the United States in effect is *associated* with a statistically significant increase in these measures of political support, this might not be solely *caused* by the formation of the BIT. The point of using matching and linear regression is to try and control for other possible factors that may have caused the change in the dependent variable, but it is always possible that there are other variables that were not accounted for in the model—and which may not even be observable—that are in part driving the changes in relationship. As a result, it would be too strong to say that the BIT alone is driving a given result. It would be fair to say, however, that this is one of the more rigorous attempts to date to try and control for these selection effects when trying to determine the consequences of BITs, and that the result is robust to a variety of alternative model specifications.

That said, even despite the limitations that exist on the ability to interpret these results causally, there are still several important implications from the findings in this study. The first implication is that these findings lend strong support to the theory that the United States has used BITs as a strategy to improve relationships.[167] The one thing that is unambiguously clear from the results presented in this paper is that variables that measure investment consideration do not help to explain BITs at all, whereas political concerns do at least in part. This suggests that it is time scholars change the way they describe, discuss, and evaluate the United States' BITs program.

The second implication of these findings is that looking solely at the effect of BITs on FDI flows and investor protection may be the wrong way to measure their effectiveness. As an increasingly diverse body of evidence has suggested that BITs might not directly increase investment,[168] scholars have become increasingly skeptical of the value of these treaties. If, however, BITs are treaties that have been signed for political reasons and they have produced at least some political divided, there may be value to the program that has previously not been understood. After all, as this study has shown, BITs may produce many benefits beyond simply improving investment climates abroad. Only after scholars internalize the message that developing countries use BITs for more than protecting investment will we begin to see the complete picture.

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[167] See Salacuse 2010, 442 (arguing that capital importing states may be motivated to sign BITs by the desire to improve their relationships with capital exporting states).

[168] See generally Yackee 2010.

Chapter 7

Conclusion

This dissertation has engaged in important ongoing debates about the efficacy of the laws of war, the potential of human rights treaties to change domestic policies, the drivers of compliance with international trade law, and the motivations of the United State's Bilateral Investment Treaty program. In each of these substantive areas, the empirical results demonstrated influences that international agreements have on domestic and foreign policy that have not previously either not been explored or demonstrated.

Of course, there are obviously limitations to the results that have been presented in this dissertation. For starters, the chapters using observational data—2, 5, and 6—all confront barriers to inference that make it difficult to perform reliable causal analysis. Although efforts were taken to carefully confront selection and endogeneity issues, there will always be remaining sources of bias. Additionally, despite the evidence of statistically significant treatment effects in the chapters using experimental evidence—3 and 4—whether these treatment effects result in outcome consequential changes in policy remains an open question. As a result, I am not under the mistaken belief that these essays will be the final word on any of the substantive debates they address.

Instead, it is my hope that the research presented in this dissertation will help contribute to the growing body of scholarship taking international agreements seriously. And maybe by doing so, we can gain a better understanding of how law can be used to make the world more sane, safe, and fair.

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Appendix 1

Supporting Material For Chapter 2

Appendix 1.1 Regression Results for Figure 2.1

	Model 1: All Countries	Model 2: All Countries	Model 3: Stable Autocracy	Model 4: Transitioning Democracy
AP II Rat.	-0.97***	-0.86**	0.37	-2.56***
	(0.27)	(0.32)	(0.96)	(0.61)
Polity		-0.10***	0.39	-0.08*
		(0.03)	(0.34)	(0.04)
Polity^2		-0.01	0.10	-0.01
		(0.01)	(0.06)	(0.01)
Population (ln)		0.38**	0.92	0.35
		(0.13)	(0.48)	(0.26)
GDPPC (ln)		0.17	-0.11	-0.32
		(0.18)	(0.68)	(0.28)
Ethnic Frac.		2.79	-26.58	0.58
		(2.58)	(17.67)	(3.62)
Ethnic Frac.^{2}		-2.27	29.29	3.73
		(2.66)	(18.07)	(3.82)
Rough Terrain		-0.18	-0.11	-0.12
		(0.15)	(0.43)	(0.25)
OIL		-0.58	-2.25	3.18**
		(0.48)	(1.55)	(1.06)
Intercept	-0.20	-7.64**	-12.04	-4.47
	(0.16)	(2.42)	(10.68)	(3.83)
N	279	279	62	190
AIC	356.9	346.6	78.77	195.8

Note: * significant at $p < 0.05$; **$p < 0.01$; ***$p < 0.001$. S.E.s in parentheses.

Appendix 1.2 Regression Results for Figure 2.2

	Model 5: Countries that Switched	Model 6: Countries that Switched	Model 7: Countries that Switched
AP II Rat.	-2.07***	-1.56*	-3.46*
	(0.54)	(0.62)	(1.48)
Polity		-0.19***	-0.65
		(0.05)	(0.63)
Population (ln)			7.76*
			(3.24)
GDPPC (ln)			-2.09
			(1.37)
Ethnic Frac.			-13.41
			(11.49)
Rough Terrain			-3.18
			(1.99)
OIL			3.31
			(2.24)
Intercept	0.78*	0.47	-103.04*
	(0.36)	(0.44)	(43.45)
N	72	72	72
AIC	86.21	72.71	54.69

Note: * significant at $p < 0.05$; **$p < 0.01$; ***$p < 0.001$. S.E.s in parentheses.

Appendix 1.3: Regression Results for Figure 2.3

	Model 8: All Countries	Model 9: Stable Auto.	Model 10: Trans. Demo.
AP II Rat.^	-0.05	0.11	-0.31*
	(0.10)	(0.16)	(0.14)
Polity	-0.03***	0.04	-0.02**
	(0.01)	(0.04)	(0.01)
Polity^2	-0.00	0.01	0.00
	(0.00)	(0.01)	(0.00)
Population (ln)	0.08**	0.15*	0.08
	(0.03)	(0.07)	(0.05)
GDPPC (ln)	0.03	0.02	-0.04
	(0.04)	(0.12)	(0.05)
Ethnic Frac.	0.63	-3.57	-0.34
	(0.49)	(2.56)	(0.52)
Ethnic Frac.^{2}	-0.60	3.97	0.84
	(54)	(2.61)	(0.62)
Rough Terrain	-0.03	-0.01	0.01
	(0.03)	(0.07)	(0.04)
OIL	-0.15	-0.31	0.29
	(0.10)	(0.21)	(0.14)
Intercept	-1.26**	-1.86	-0.75
	(0.49)	(1.76)	(0.63)
N	279	62	190
Adj. R^2	0.08	0.14	0.23

Note: * significant at $p < 0.05$; **$p < 0.01$; ***$p < 0.001$. Standard Errors in parentheses.

Appendix 1.4: Countries by Regime Type Since World War II

Stable Autocracy	Transitioning Democracy
Afghanistan	Colombia
Angola	Croatia
Azerbaijan	Democratic Republic of Congo
Bosnia-Herzegovina	El Salvador
Cote D’Ivoire	Georgia
Djibouti	Haiti
Ethiopia	Indonesia
Iraq	Laos
Morocco	Liberia
Mozambique	Mexico
Papua New Guinea	Moldova
Yemen	Myanmar
	Nepal
	Niger
	Pakistan
	Philippines
	Russia
	Senegal
	Serbia
	Sierra Leone
	Somalia
	Sri Lanka
	Sudan
	Uganda

Note: Stable Autocracies are countries that never scored above a 5 on the Polity IV Democracy variable between 1945 and 2010. Transitioning Democracies are countries that have scored above a 5 during the same period, but were not Stable Democracies that remained above 8 for the entire period.

Appendix 1.5: Regressions on Ratification of AP II

	Model 1	Model 2	Model 3	Model 4
Common Law	-2.13*** (0.33)			-2.05*** (0.39)
Regional Density		4.96*** (0.65)		4.74*** (0.80)
Ratification Process			1.78*** (0.26)	1.36*** (0.28)
Intercept	0.23 (0.15)	-3.30*** (0.42)	-3.77*** (0.52)	-5.17*** (0.77)
N	279	279	279	279
AIC	32.1	302.8	312.4	238.3

Note: * significant at $p < 0.05$; **$p < 0.01$; ***$p < 0.001$. S.E.s in parentheses.

Appendix 1.6: Countries by Regime Type Year Prior to Conflict Year

Autocratic	Democratic
Afghanistan (10)	Colombia (22)
Angola (6)	Cote D'Ivoire (1)
Azerbaijan (1)	Democratic Republic of Congo (2)
Bosnia-Herzegovina (3)	El Salvador (3)
Cote D'Ivoire (2)	Georgia (3)
Croatia (1)	India (19)
Democratic Republic of Congo (1)	Indonesia (6)
Djibouti (4)	Israel (8)
Ethiopia (18)	Moldova (1)
Haiti (1)	Nepal (8)
Indonesia (3)	Niger (1)
Iraq (6)	Philippines (2)
Laos (2)	Russia (7)
Liberia (3)	Senegal (2)
Mexico (1)	Sierra Leone (1)
Morocco (1)	Sudan (1)
Mozambique (2)	
Myanmar (21)	
Nepal (4)	
Niger (3)	
Pakistan (1)	
Papua New Guinea (7)	
Russia (7)	
Senegal (7)	
Serbia (2)	
Sierra Leone (6)	
Somalia (8)	
Sudan (20)	
Uganda (4)	
Yemen (1)	

Note: Autocratic countries that scored below a 5 on the Polity Project Democracy variable the year prior to the conflict year. Democratic countries scored a five or above the year prior to the conflict year.

Appendix 2

Supporting Material For Chapter 3

Appendix 2.1: Exact Wording of the Experiment

Vignette

“In a country that is a strategic ally of the United States, a rebel group has controlled an outlying region of the country for a long time. As a result of recent instability in the country, the rebels have left the areas they control and launched an attack on the country's capital in an effort to overthrow the government.”

“The U.S. president responded by launching air strikes in support of our ally. After suffering initial casualties from the air strikes, the rebel forces took shelter in areas heavily populated with civilians. This made the U.S. military unable to continue air strikes while distinguishing rebel targets from civilian targets. Any continued bombing would result in excessive civilian casualties. This forced the U.S. president to consider whether to continue the bombing campaign.”

“If the U.S. were to halt the bombing campaign, it is likely that the rebel forces would overthrow the government, and that the country would no longer be an ally of America.”

[Treatment 1]

[Treatment 2]

“Ultimately, the president decided to continue the bombing campaign against the rebel forces because failing to do so would result in the loss of a strategic ally. Do you approve, disapprove, or neither approve nor disapprove of the president's decision to continue the bombing campaign?”

Treatment 1

1. **Null Treatment**: (nothing)

2. **International Law Treatment:** “On the other hand, continuing the bombing of civilians would be immoral. It is immoral to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained.”

3. **Morality Treatment:** “On the other hand, continuing the bombing of civilians would violate international law. It is a violation of international law and treaties that the United States has signed to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained.”

4. **Combined Treatment:** “On other hand, continuing the bombing of civilians would violate international law. It is a violation of international law and treaties that the United States has signed to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained. Additionally, continuing to bomb civilians is not only a

violation of international law, it is immoral. It is also immoral to continue a bombing campaign when the expected loss of civilian life is excessive relative to the military advantage gained."

Treatment 2

1. **Null Treatment:** (nothing)

2. **Reciprocity Treatment:** "The rebel forces have publicly committed to comply with international law and not intentionally kill civilians, and there is not any evidence that they have broken that commitment."

Mechanism Questions

Morality: "Would it be morally wrong for the U.S. president to continue air strikes when he or she knew that it would be difficult to distinguish between civilians and combatants?"

International Commitment: "The U.S. often makes international commitments with other countries and the international community by signing treaties. Do you agree that the U.S. should never break its international commitments?"

International Response: "What do you think the likelihood is that the U.S. bombing of civilians would result in other countries taking actions against the U.S. in response that harmed America's interests?"

Continued Death: "If the U.S. decided to continue bombing, resulting in civilian casualties, what is the likelihood that the rebel troops would kill civilians?"

Quit Death: "If the U.S. decided NOT to continue bombing and risk civilian lives, what is the likelihood that the rebel troops would kill civilians?"

Appendix 2.2: Effect on Pre-Treatment Covariates on Treatment Probabilities

	1	**2**	**3**	**4**
Male	-0.019	-0.291*	-0.068	0.341*
	(0.146)	(0.145)	(0.148)	(0.156)
Age	-0.005	-0.004	-0.000	0.005
	(0.007)	(0.007)	(0.007)	(0.007)
College Degree	0.101	0.013	0.190	0.044
	(0.142)	(0.144)	(0.145)	(0.146)
Republican	0.336*	-0.035	-0.227	0.009
	(0.149)	(0.157)	(0.163)	(0.159)
U.S. Citizen	0.326	0.130	0.282	-1.674
	(0.511)	(0.557)	(0.511)	(1.028)
Black	0.202	0.527	0.078	-0.469
	(0.339)	(0.339)	(0.352)	(0.350)
Hispanic	0.522	-0.476	0.376	-0.370
	(0.351)	(0.485)	(0.370)	(0.383)
White	-0.108	0.151	-0.028	-0.537*
	(0.237)	(0.250)	(0.241)	(0.221)
Other Race	-0.290	-0.113	-0.169	-0.044
	(0.577)	(0.582)	(0.241)	(0.495)

	5	**6**	**7**	**8**
Male	0.88	-0.011	0.009	-0.012
	(0.154)	(0.153)	(0.156)	(0.152)
Age	0.004	-0.001	0.002	0.000
	(0.007)	(0.007)	(0.007)	(0.007)
College Degree	-0.269	-0.011	-0.069	-0.030
	(0.153)	(0.150)	(0.153)	(0.149)
Republican	-0.310	-0.168	0.295	0.045
	(0.168)	(0.165)	(0.159)	(0.159)
U.S. Citizen	0.315	-0.253	0.582	-0.559
	(0.560)	(0.626)	(0.514)	(0.747)
Black	-0.145	-0.561	0.126	0.086
	(0.438)	(0.415)	(0.386)	(0.386)
Hispanic	0.751	-0.605	-0.230	-0.583
	(0.395)	(0.481)	(0.467)	(0.524)
White	0.408	-0.038	0.087	0.202
	(0.278)	(0.243)	(0.264)	(0.263)
Other Race	-0.480	0.318	-0.000	0.516
	(0.776)	(0.504)	(0.587)	(0.514)

Note: * significant at $p < 0.05$; ** $p < 0.001$; *** $p < 0.001$. Standard errors are in parentheses.

Appendix 3

Supporting Material For Chapter 4

Appendix 3.1: Exact Wording of the Experiment

Vignette

“The United States often subjects prisoners to solitary confinement for extended periods of time. These periods can last years. When in solitary confinement, prisoners are held in their cell for up to twenty-three hours a day and are deprived of human contact.”

“Supporters of the use of solitary confinement argue that its use is necessary to maintain prison discipline and ensure the safety of prisoners and guards alike.”

[Treatment]

“American lawmakers have been considering reforms that would eliminate the use of solitary confinement except in extreme circumstances where keeping the prisoner in the general population would pose immediate safety risks.”

“Do you approve, disapprove, or neither approve nor disapprove of these reforms?”

Treatments

1. **Null Treatment**: (nothing)

2. **Placebo Treatment:** “Critics of the use of solitary confinement argue that it should be eliminated except in the most extreme cases because it violates the human rights of the prisoners held in solitary confinement.”

3. **International Law Treatment:** “Critics of the use of solitary confinement argue that it should be eliminated except in the most extreme cases because it violates international human rights treaties that the United States has signed.”

Mechanism Questions

Commitment: “The United States often makes international commitments with other countries and the international community by signing treaties. Do you believe that the Untied States should change its domestic policies to honor international legal commitments?”

Morality: “Is the use of solitary confinement immoral?”

International Standards: “Should the United States’ treatment of prisoners conform to international standard?”

Appendix 3.2: Effect of Demographic Covariates on Treatment Probability

	Null	**Placebo**	**International Law**
Male	0.008	-0.035	0.027
	(0.112)	(0.110)	(0.112)
Age	-0.002	-0.002	0.004
	(0.005)	(0.005)	(0.005)
College Degree	0.099	0.017	-0.116
	(0.111)	(0.108)	(0.110)
Republican	-0.002	-0.074	0.077
	(0.125)	(0.123)	(0.123)
U.S. Citizen	0.601	-0.105	-0.607
	(0.423)	(0.445)	(0.513)
Black	0.209	-0.071	-0.144
	(0.280)	(0.279)	(0.291)
Hispanic	0.034	0.038	-0.074
	(0.296)	(0.289)	(0.302)
White	0.020	-0.038	0.021
	(0.190)	(0.185)	(0.191)
Other Race	-0.474	-0.068	0.470
	(0.440)	(0.396)	(0.387)

Note: * significant at $p < 0.05$; ** $p < 0.001$; *** $p < 0.001$. Standard errors are in parentheses.

Appendix 3.3: Ordered Logit Analysis of Treatment Effects

Placebo Treatment	-0.013
	(0.112)
International Law Treatment	0.265*
	(0.114)
Republican	-1.042***
	(0.108)
Male	-0.073
	(0.094)
Age	-0.010*
	(0.004)
Education Level	0.023
	(0.037)
U.S. Citizen	0.731
	(0.393)
Black	0.104
	(0.243)
Hispanic	0.012
	(0.240)
White	0.351*
	(0.155)
Other Race	1.009**
	(0.352)
Observations	1,535

Note: * significant at $p < 0.05$; **$p < 0.01$; ***$p < 0.001$. S.E.s in parentheses. Cutpoints for ordered logit model are not shown.

Appendix 4

Supporting Material For Chapter 5

Appendix 4.1: Sources of Information on U.S. Compliance with WTO Consultations

DS #	Short Title	Complainant(s)	Consultation Request	Compliance Date	WTO Source	US Gov. Source
DS002, DS004	US _ Gasoline	Venezuela, Brazil	1/24/95	8/19/97	DSB Meeting Minutes 25 September 1997	NA
DS024	US _ Underwear	Costa Rica	12/22/95	3/28/97	DSB Meeting 10 April 1997	NA
DS033	US _ Wool Shirts and Blouses	India	3/14/96	12/4/96	WTO Panel Report 6 January 1997	61 Fed. Reg. 64342
DS058, DS061	US _ Shrimp	India; Malaysia; Pakistan; Thailand; Philippines	10/8/96	7/8/99	WTO Appellate Body Report 22 October 2001	64 Fed. Reg. 36946
DS099	US _ DRAMS	Republic of Korea	8/14/97	10/20/00	WTO Panel Report 7 November 2000	65 Fed. Reg. 59391
DS108	US _ FSC	European Communities	11/18/97	5/11/06	DSB Meeting Minutes 17 May 2006	Public Law 109-222
DS136, DS162	US _ 1916 Act	European Communities, Japan	6/4/98	12/3/04	DSB Meeting Minutes 17 December 2004	Public Law 108-429
DS138	US _ Lead and Bismuth II	European Communities	6/12/98	3/14/00	DSB Meeting Minutes 5 July 2000	65 Fed. Reg. 13713
DS160	US _ Section 110(5) Copyright Act	European Communities	1/26/99	4/12/03	NA	H.Rept. 108-77 at 33.

Appendix 4.1: Sources of Information on U.S. Compliance with WTO Consultations (Continued)

DS #	Short Title	Complainant(s)	Consultation Request	Compliance Date	WTO Source	US Gov. Source
DS165	US _ Certain EC Products	European Communities	3/4/99	3/3/99	WTO Appellate Body Report 11 December 2000	March 4, 1999 Letter Director, Trade Compliance Division
DS166	US _ Wheat Gluten	European Communities	3/17/99	4/9/01	Statement by the US and EU 10 April 2001	66 Fed. Red. 18510
DS176	US _ Section 211 Appropriations Act	European Communities	7/8/99		NA	CRS Report RL32014
DS17, DS178	US _ Lamb	New Zealand, Australia	7/16/99	11/14/01	DSB Meeting Minutes 21 November 2001	Proclamation 7502
DS179	US _ Stainless Steel	Republic of Korea	7/30/99	8/28/01	DSB Meeting Minutes 10 September 2001	66 Fed. Reg. 45278
DS184	US _ Hot-Rolled Steel	Japan	11/18/99	11/8/02	NA	67 Fed. Reg. 69186
DS192	US _ Cotton Yarn	Pakistan	4/3/00	11/9/01	DSB Meeting Minutes 21 November 2001	66 Fed. Reg. 56805
DS202	US _ Line Pipe	Republic of Korea	6/13/00	3/1/03	DSB Meeting Minutes 19 June 2006	68 Fed. Reg. 19578

Appendix 4.1: Sources of Information on U.S. Compliance with WTO Consultations (Continued)

DS #	Short Title	Complainant(s)	Consultation Request	Compliance Date	WTO Source	US Gov. Source
DS212	US _ Countervailing Measures on Certain EC Products	European Communities	11/10/00	5/26/06	DSB Meeting Minutes 18 March 2003	72 Fed. Reg. 6519
DS213	US _ Carbon Steel	European Communities	11/10/00	4/1/04	DSB Meeting Minutes 20 April 2004	69 Fed. Reg. 17,131
DS217, DS234	US _ Offset Act (Byrd Amendment)	Australia, Canada, Brazil, Chile, European Union, India, Indonesia, Japan, Rep. of Korea, Thailand, Mexico	12/21/00	2/8/06	NA	Public Law 109-171, Sec. 7601
DS236, DS247, DS257, DS264, DS277, DS311	US _ Softwood Lumber III	Canada	8/21/01	9/12/06	Statement By US and Canada 12 October 2006	Comprehensive Agreement 12 September 2006
DS248, DS249, DS251, DS252, DS253, DS254, DS258, DS259	US _ Steel Safeguards	European Communities, Japan, Republic of Korea, China, Switzerland, Norway, New Zealand, Brazil	3/7/02	12/4/03	DSB Meeting Minutes 10 December 2003	Proclamation 7741
DS267	US _ Upland Cotton	Brazil	9/27/02	4/6/10	Statement by the US and Brazil 25 August 2010	USTR Press Release (April 6, 2010); Public Law 112-55
DS268	US _ Oil Country Tubular Goods Sunset Reviews	Argentina	10/7/02	6/22/07	DSB Meeting Minutes 31 August 2007	72 Fed. Reg. 34442

Appendix 4.1: Sources of Information on U.S. Compliance with WTO Consultations (Continued)

DS #	Short Title	Complainant(s)	Consultation Request	Compliance Date	WTO Source	US Gov. Source
DS285	US _ Gambling	Antigua and Barbuda	3/13/03		NA	CRS Report RL32014
DS294	US _ Zeroing (EC)	European Communities	6/12/03	6/20/12	NA	77 Fed. Reg. 29875; USTR Press Release (February 6, 2012)
DS322	US _ Zeroing (Japan)	Japan	11/24/04	6/20/12	NA	77 Fed. Reg. 29875; USTR Press Release (February 6, 2012)
DS335	US _ Shrimp (Ecuador)	Ecuador	11/17/05	8/15/07	DSB Meeting Minutes 31 August 2007	72 Fed. Reg. 48257
DS343, DS345	US _ Shrimp	Thailand, India	4/24/06	4/1/09	DSB Meeting Minutes 20 April 2009	74 Fed. Reg. 14809
DS344	US _ Stainless Steel (Mexico)	Mexico	5/26/06	6/20/12	NA	77 Fed. Reg. 29875; USTR Press Release (February 6, 2012)
DS350	US _ Continued Zeroing	European Communities	10/2/06	6/20/12	NA	77 Fed. Reg. 29875; USTR Press Release (February 6, 2012)
DS379	US _ Anti-Dumping and Countervailing Duties (China)	China	11/19/08		NA	CRS Report RL32014

Appendix 4.1: Sources of Information on U.S. Compliance with WTO Consultations (Continued)

DS #	Short Title	Complainant(s)	Consultation Request	Compliance Date	WTO Source	US Gov. Source
DS382	US _ Anti-Dumping Measures on Orange Juice (Brazil)	Brazil	11/27/08	4/16/12	NA	77 Fed. Reg. 23659; USITC 4311
DS383	US _ Anti-Dumping Measures on PET Bags	Thailand	11/26/08	7/28/10	DSB Meeting Minutes 31 August 2010	75 Fed. Reg. 48940
DS392	US _ Poultry (China)	China	4/17/09	9/30/09	WTO Panel Report 20 September 2010	NA
DS402	US - Zeroing in Anti-Dumping Measures	Republic of Korea	11/24/09	10/24/11	WTO Communiqué 9 December 2011	76 Fed. Reg. 66892
DS404	Anti-dumping Measures on Certain Shrimp	Viet Nam	2/1/10	4/16/12	Statement by the US 24 May 2012	77 Fed. Reg. 8101

Appendix 5

Supporting Material For Chapter 6

Appendix 5.1: BITs Signed by the United States

BIT Partner	Signed	Into Effect	BIT Partner	Signed	Into Effect
Panama	10/27/82	05/30/91	Ecuador	08/27/93	05/11/97
Senegal	12/06/83	10/25/90	Belarus	01/15/94	NA
Haiti	12/13/83	NA	Jamaica	02/04/94	03/07/97
D.R. Congo	08/03/84	07/28/89	Ukraine	03/04/94	11/16/96
Morocco	07/22/85	05/29/91	Georgia	03/07/94	08/17/97
Turkey	12/03/85	05/18/90	Estonia	04/19/94	02/16/97
Cameroon	02/26/86	04/06/89	Trinidad & Tobago	09/26/94	12/26/96
Egypt	03/11/86	06/27/92	Mongolia	10/06/94	01/01/97
Bangladesh	03/12/86	07/25/89	Uzbekistan	12/16/94	NA
Grenada	05/02/86	03/03/89	Albania	01/11/95	01/04/98
Rep. Congo	02/12/90	08/13/94	Latvia	01/13/95	12/26/96
Poland	03/21/90	08/06/94	Honduras	07/01/95	07/11/01
Tunisia	05/15/90	02/07/93	Nicaragua	07/01/95	NA
Sri Lanka	09/20/91	05/01/93	Croatia	07/13/96	06/20/01
Czech Republic	10/22/91	12/19/92	Jordan	07/02/97	06/12/03
Slovakia	10/22/91	12/19/92	Azerbaijan	08/01/97	08/02/01
Argentina	11/14/91	10/20/94	Lithuania	01/14/98	11/22/01
Kazakhstan	05/19/92	01/12/94	Bolivia	04/17/98	06/06/01
Romania	05/28/92	01/15/94	Mozambique	12/01/98	03/03/05
Russia	06/17/92	NA	El Salvador	03/10/99	NA
Armenia	09/23/92	03/29/96	Bahrain	09/29/99	05/30/01
Bulgaria	09/23/92	06/02/94	Uruguay	11/04/05	11/01/06
Kyrgyzstan	01/19/93	01/12/94	Rwanda	02/19/08	01/01/12
Moldova	04/21/93	11/25/94			

Note: This table lists all Bilateral Investment Treaties signed by the United States as of December 29, 2012. The label "NA" is used for BITs that have been signed but have not gone into effect. This data is taken from the U.S. Department of State Bilateral Investment Treaty List, *available at* <http://www.state.gov/e/eb/ifd/bit/117402.htm#7> (last visited December 29, 2012).

Appendix 5.2: PTAs Signed by the United States

PTA Partner	Signed	Into Effect
Israel	04/22/85	09/01/85
Mexico	12/01/92	01/01/94
Canada	12/01/92	01/01/94
Jordan	10/24/00	12/17/01
Singapore	05/06/03	01/01/04
Chile	06/06/03	01/01/04
Australia	05/18/04	01/01/05
Costa Rica	05/28/04	01/01/09
Guatemala	05/28/04	07/01/06
Nicaragua	05/28/04	04/01/06
Honduras	05/28/04	04/01/06
El Salvador	05/28/04	03/01/06
Morocco	06/15/04	01/01/06
Dominican Republic	08/01/04	03/01/07
Bahrain	09/14/04	08/01/06
Oman	01/19/06	01/01/09
Peru	04/12/06	02/01/09
Colombia	11/22/06	05/15/12
Panama	06/28/07	10/31/12
South Korea	06/30/07	03/15/12

Note: This table lists all Preferential Trade Agreements signed by the United States as of December 29, 2012. This data is taken from the U.S. Department of State Existing Free Trade Agreements List, *available at* <http://www.state.gov/e/eb/tpp/bta/fta/fta/index.htm> (last visited December 29, 2012).

Appendix 5.3: Congressional Consideration of BITs Signed by the United States

Country	Introduced	Passed	Country	Introduced	Passed
D.R. Congo	3/25/1986	10/20/88	Jamaica	9/19/1994	6/27/96
Morocco	3/25/1986	10/20/88	Belarus	9/23/1994	6/27/96
Senegal	3/25/1986	10/20/88	Estonia	9/26/1994	6/27/96
Turkey	3/25/1986	10/20/88	Ukraine	9/26/1994	6/27/96
Cameroon	5/28/1986	10/20/88	Mongolia	6/26/1995	6/27/96
Bangladesh	5/30/1986	10/20/88	Georgia	7/10/1995	6/27/96
Egypt	6/2/1986	10/20/88	Latvia	7/10/1995	6/27/96
Grenada	6/3/1986	10/20/88	Trinidad & Tobago	7/11/1995	6/27/96
Panama	3/25/1986	10/28/90	Albania	11/6/1995	6/27/96
Poland	6/19/1990	10/28/90	Uzbekistan	2/28/1996	10/18/00
R. Congo	2/19/1991	8/11/92	Bahrain	5/23/2000	10/18/00
Tunisia	5/17/1991	8/11/92	Bolivia	5/23/2000	10/18/00
Sri Lanka	8/20/1991	8/11/92	Croatia	5/23/2000	10/18/00
Czech Republic	6/2/1992	8/11/92	El Salvador	5/23/2000	10/18/00
Slovakia	6/2/1992	8/11/92	Honduras	5/23/2000	10/18/00
Russia	7/28/1982	8/11/92	Jordan	5/23/2000	10/18/00
Kazakhstan	9/7/1993	10/21/93	Mozambique	5/23/2000	10/18/00
Romania	8/3/1992	11/17/93	Lithuania	9/5/2000	10/18/00
Argentina	1/19/1993	11/17/93	Azerbaijan	9/12/2000	10/18/00
Bulgaria	1/19/1993	11/17/93	Uruguay	4/4/2006	9/12/06
Armenia	9/8/1993	11/17/93	Rwanda	11/20/2008	9/26/11
Kyrgyzstan	9/8/1993	11/17/93	Haiti	3/26/1986	NA
Moldova	9/8/1993	11/17/93	Nicaragua	6/26/2000	NA
Ecuador	9/10/1993	11/17/93			

Note: This table lists all Bilateral Investment Treaties signed by the United States as of December 29, 2012. The label "NA" is used for BITs that have been signed but have not gone into effect. The date "Introduced" is the date that the treaty was introduced to the Senate, and the "Passed" date is the date that each BIT was approved by the Senate. All BITs were passed by voice votes.

Appendix 5.4: Congressional Consideration of PTAs Signed by the United States

PTA Partner	Introduced	Passed
Israel	4/29/1985	5/23/1985
Mexico	11/4/1993	11/22/1993
Canada	11/4/1993	11/22/1993
Jordan	4/4/2001	12/7/2001
Singapore	7/15/2003	7/31/2003
Chile	7/15/2003	7/31/2003
Australia	7/6/2004	7/22/2004
Morocco	7/15/2004	7/22/2004
Costa Rica	6/23/2005	6/27/2005
Guatemala	6/23/2005	6/27/2005
Nicaragua	6/23/2005	6/27/2005
Honduras	6/23/2005	6/27/2005
El Salvador	6/23/2005	6/27/2005
Dominican Republic	6/23/2005	6/27/2005
Bahrain	11/16/2005	12/13/2005
Oman	6/26/2006	7/20/2006
Peru	9/27/2007	12/4/2007
Colombia	10/3/2011	10/12/2011
Panama	10/3/2011	10/12/2011
South Korea	10/3/2011	10/12/2011

Note: This table lists all Preferential Trade Agreements signed by the United States as of December 29, 2012. The date “Introduced” is the date that the treaty was introduced to the Congress, and the “Passed” date is the date that each BIT was approved by the Congress.

Appendix 5.5: Members of the Iraq War Coaltion, March 2003

Afghanistan
Albania
Angola
Australia
Azerbaijan
Bulgaria
Colombia
Czech Republic
Denmark
Dominican Republic
El Salvador
Eritrea
Estonia
Ethipia
Georgia
Honduras
Hungary
Iceland
Italy
Japan
Kuwait
Latvia
Lithuania
Macedonia
Marhall Islands
Micronesia
Mongolia
Netherlands
Nicaragua
Palua
Panama
Philippines
Poland
Portugal
Romania
Rwanda
Singapore
Slovakia
Solomon Islands
South Korea
Spain
Tonga
Turkey
Uganda
Ukraine
United Kingdom
United States
Uzbekistan

Note: This table lists all of the members of the Iraq War Coalition reported by the White House on March 27, 2003. This list is available at < http://georgewbushwhitehouse.archives.gov/infocus/iraq/news/20030327-10.html> (last visited April 6, 2013).

Appendix 5.6: Parimonious Models Estiamting the Effect of BITs

BIT Signed Regressed on Dependent Variables

	Model 1 United Nations Voting	Model 2 Troop Deployment	Model 3 Iraq War Coalition
BIT Signed	0.211***	0.108*	1.125**
	(0.055)	(0.049)	(0.368)
Intercept	-0.460***	1.402***	-1.439***
	(0.006)	(0.046)	(0.210)
N	3,796	2,966	192

Note: * significant at p < 0.05; **p < 0.01; ***p < 0.001. S.E.s in parentheses. Model 1 was estimated using OLS and Models 2 and 3 using Logit

BIT Ratified Regressed on Dependent Variables

	Model 1 United Nations Voting	Model 2 Troop Deployment	Model 3 Iraq War Coalition
BIT Ratified	0.048**	0.436**	1.106**
	(0.017)	(0.158)	(0.388)
Intercept	-0.460***	1.418***	-1.378***
	(0.005)	(0.042)	(0.201)
N	4,948	3,932	192

Note: * significant at p < 0.05; **p < 0.01; ***p < 0.001. S.E.s in parentheses. Model 1 was estimated using OLS and Models 2 and 3 using Logit