UNIVERSITY OF CALIFORNIA, SAN DIEGO

International Rules, National Implementation: How Domestic Politics Condition the Effects of International Legal Commitments

A dissertation submitted in partial satisfaction of the
requirements for the degree
Doctor of Philosophy

in

Political Science

by

Yonatan Lupu

Committee in charge:

Professor David A. Lake, Chair
Professor George Downs
Professor James H. Fowler
Professor Emilie M. Hafner-Burton
Professor Miles Kahler
Professor Keith Poole

2012

The dissertation of Yonatan Lupu is approved, and it is acceptable in quality and form for publication on microfilm and electronically:

_____

_____

_____

_____

_____

_____

Chair

University of California, San Diego

2012

DEDICATION

My grandparents – Sara Lupu, Moses Lupu, Sara Fajgenbaum, and Leon Fajgenbaum – did not live to see the completion of this dissertation. It is dedicated to their lives and their memory.

TABLE OF CONTENTS

Signature Page iii

Dedication iv

Table of Contents v

List of Figures vii

List of Tables viii

Acknowledgements ix

Vita and Publications x

Abstract of the Dissertation xi

Chapter 1 The Informative Power of Treaty Commitment: Using the Spatial Model to Address Selection Effects 1
- 1.1 Introduction 2
- 1.2 The Treaty Commitment Selection Effect 4
- 1.3 Estimating Treaty Commitment Preferences 7
- 1.4 Applications: Three Human Rights Treaties 14
- 1.5 Conclusions 20
- 1.6 Appendix 28
  - 1.6.1 Monte Carlo Simulations 28
  - 1.6.2 Treaty Data 31
  - 1.6.3 Robustness Tests 32

Chapter 2 Best Evidence: The Role of Information in Domestic Judicial Enforcement of International Human Rights Agreements 42
- 2.1 Introduction 43
- 2.2 The Effects of International Commitments 46
- 2.3 Domestic Courts and International Commitments 48
- 2.4 Domestic Prosecutions of Human Rights: Information as Evidence 52
  - 2.4.1 Evidence-Production Costs 52
  - 2.4.2 Standards of Proof 57
- 2.5 Research Design 59
  - 2.5.1 The ICCPR 59
  - 2.5.2 Addressing Treaty Commitment Selection Effects . . 60
- 2.6 Results and Discussion 65
- 2.7 Conclusions 70

Chapter 3 Limited Constraints: Veto Players and the Effects of International Human Rights Agreements . . . 80
3.1 Introduction . . . 81
3.2 Veto Players, International Cooperation, and Human Rights . 83
3.3 When Can Veto Players Lock In Human Rights Commitments? 84
3.4 Research Design . . . 90
3.5 Results . . . 95
3.6 Conclusions . . . 96

Chapter 4 Measuring Multilateralism: Ideal Point Estimates of State Preferences over Global Treaties . . . 102
4.1 Introduction . . . 103
4.2 Treaty Commitment Preferences . . . 104
4.3 Methodology . . . 108
4.4 Results . . . 109
4.4.1 Visualizing the Treaty Preference Space . . . 114
4.4.2 Movement in W-NOMINATE Space Over Time . . . 115
4.4.3 Analysis of Normal Vectors . . . 118
4.4.4 Analysis of Treaty-Space Cleavages . . . 119
4.4.5 Regression Analysis . . . 122
4.5 Conclusions . . . 124

Bibliography . . . 141

LIST OF FIGURES

Figure 1.1: Marginal Effects of CEDAW Ratification on Women's Rights . . . 27
Figure 1.2: First comparison of Root Mean Squared Errors 33
Figure 1.3: Second comparison of Root Mean Squared Errors 34
Figure 1.4: Mean Number of Treaties Ratified 35
Figure 1.5: Mean Percentage of Treaties Ratified 36
Figure 1.6: Number of Treaties Ratified as of 2007 37
Figure 1.7: Percentage of Treaties Ratified as of 2007 38

Figure 2.1: Marginal Effects of ICCPR Ratification on Civil Rights 73

Figure 3.1: Marginal Effects of Political Constraints on Empowerment Rights for ICCPR Members 98

Figure 4.1: Scree plots of W-NOMINATE results. 126
Figure 4.2: W-NOMINATE coordinates by region. The locations of the five permanent members of the U.N. Security Council are noted. 127
Figure 4.3: W-NOMINATE coordinates by civilization 128
Figure 4.4: W-NOMINATE coordinates of NATO and Warsaw Pact members in 1980 129
Figure 4.5: Movement of the great powers along the W-NOMINATE dimensions 1960-2008. 129
Figure 4.6: Positions of states with the most movement along the W-NOMINATE dimensions 1960-2008. 130
Figure 4.7: Normal vectors for income, regime type, trade and power in 1980, 1990 and 2000. 131
Figure 4.8: Normal vectors for U.S. Affinity and U.S.S.R. Affinity in 1960, 1970 and 1980 132
Figure 4.9: Cleavages for U.N. Authority and International Human Rights Law 133
Figure 4.10: Cutting lines for treaties perpendicular to the first dimension 134
Figure 4.11: Fits of competing OLS models of W-NOMINATE first dimension . 135
Figure 4.12: Fits of competing OLS models of W-NOMINATE second dimension 135

LIST OF TABLES

Table 1.1: Balance Statistics . . . 23
Table 1.2: Balance Statistics - Controls. Mean values reported for treatment and control groups. . . . 24
Table 1.3: Effects of Treaty Ratification - Ordered Probit Models . . . 25
Table 1.4: Summary of Treaty Commitment Coefficients . . . 26
Table 1.5: Robustness Test Balance Statistics . . . 39
Table 1.6: Robustness Test Balance Statistics - Controls. Mean values reported for treatment and control groups. . . . 40
Table 1.7: Robustness Test: Effects of Treaty Ratification - Ordered Probit Models 41

Table 2.1: Balance Statistics . . . 74
Table 2.2: Additional Balance Statistics . . . 74
Table 2.3: Effects of ICCPR Ratification on Civil Rights . . . 75
Table 2.4: Effects of ICCPR Ratification on Personal Integrity Rights . . . 76
Table 2.5: Robustness Test Balance Statistics . . . 77
Table 2.6: Additional Robustness Test Balance Statistics . . . 77
Table 2.7: Robustness Test: Effects of ICCPR Ratification on Civil Rights . . . 78
Table 2.8: Robustness Test: Effects of ICCPR Ratification on Personal Integrity Rights . . . 79

Table 3.1: Balance Statistics . . . 99
Table 3.2: Additional Balance Statistics . . . 99
Table 3.3: Empowerment Rights . . . 100
Table 3.4: Personal Integrity Rights . . . 101

Table 4.1: Top 10 States by W-NOMINATE Classification Results in 2008 . . . 136
Table 4.2: Measures of Fit for Universal Treaty Data . . . 136
Table 4.3: Measures of Fit for Other Data . . . 136
Table 4.4: OLS Models of First-Dimension W-NOMINATE Coordinates . . . 137
Table 4.5: OLS Models of Second-Dimension W-NOMINATE Coordinates . . . 138
Table 4.6: Summary of Results - Part 1 . . . 139
Table 4.7: Summary of Results - Part 2 . . . 140

ACKNOWLEDGEMENTS

Many of my colleagues, friends and family have contributed to the creation of this dissertation. My advisors – David Lake, George Downs, James Fowler, Emilie Hafner-Burton, Miles Kahler and Keith Poole – have been generous with their time and advice since the beginning of this project. Many other members of the UCSD faculty have contributed to this project, including Lawrence Broz, Gary Cox, Scott Desposato, Jesse Driscoll, Erik Gartzke, Peter Gourevitch, Christina Schneider, Branislav Slantchev, David Victor, Barbara Walter and Langche Zeng. I have also been fortunate to receive helpful feedback on parts of this dissertation from colleagues in other universities, including Brian Greenhill, Daniel Hill, Dan Hopkins, Kosuke Imai, Jon Pevehouse, Paul Poast, Beth Simmons, Richard Steinberg and Erik Voeten. My fellow graduate students have been a consistent source of advice, especially Cameron Brown, Saul Cunow, Chris Fariss, Matt Kearney, Dan Maliniak, Chris O'Keefe, Michael Plouffe, Dan Smith and Devesh Tiwari.

Previous versions of several chapters have benefited from the suggestions of participants in the 2009 Midwest Political Science Association Annual National Conference, 2011 American Political Science Association Annual National Conference, 2011 Peace Science Society North American Meeting, 2011 American Society of International Law Research Forum, 2011 International Studies Association Annual Convention, 2011 Political Methodology Summer Meeting, 2012 American Society of International Law Annual Meeting, and 2012 International Studies Association Annual Convention.

This dissertation would not have been possible without the constant support of my parents, Ruth and Alex, my brother and fellow political scientist, Noam, and my wife, Andrea.

VITA

1999	B.A. in Economics and Government, Georgetown University
2002	J.D., Georgetown University Law Center
2009	M.A. in Political Science, University of California-San Diego
2012	Ph.D. in Political Science, University of California-San Diego

PUBLICATIONS

"International Law and the Waters of the Euphrates and Tigris," *Georgetown International Environmental Law Review* (2001).

"Retroactive Application of the Foreign Sovereign Immunities Act: Landgraf Analysis and the Political Question Doctrine," *UCLA Journal of International Law & Foreign Affairs* (2003) (with Clay Risen).

"The Wiretap Act and Web Monitoring: A Breakthrough for Privacy Rights?," *Virginia Journal of Law & Technology* (2004).

"Rules, Gaps and Power: Assessing Reform of the U.N. Charter," *Berkeley Journal of International Law* (2006).

"Precedent in International Courts: A Network Analysis of Case Citations by the European Court of Human Rights," *British Journal of Political Science* (2012) (with Erik Voeten).

"Trading on Preconceptions: Why World War I Was Not a Failure of Economic Interdependence," *International Security* (2012) (with Erik Gartzke).

"Political Science Research on International Law: The State of the Field," *American Journal of International Law* (2012) (with Emilie M. Hafner-Burton and David G. Victor).

ABSTRACT OF THE DISSERTATION

International Rules, National Implementation: How Domestic Politics Condition the Effects of International Legal Commitments

by

Yonatan Lupu

Doctor of Philosophy in Political Science

University of California, San Diego, 2012

Professor David A. Lake, Chair

How and when do commitments to international institutions affect the behavior of national governments? This dissertation provides substantive and methodological advances in answering this key question in international relations research. Chapter 1 addresses the selection effect problem in this literature, which threatens inferences we can make about the effects of treaty commitments. I argue that in order to address this problem, we first need to estimate the treaty commitment preferences of states. I develop a procedure designed to do so that combines ideal-point estimation and propensity-score matching and apply this procedure to test the effects of three human rights agreements. Chapter 2 analyzes the role of domestic courts in enforcing international commitments. I develop a theory that predicts when courts can be effective enforcers based on the

costs of producing evidence and the legal standards of proof. I test this theory using the procedure developed in Chapter 1. Chapter 3 analyzes the role of domestic legislative veto players in enforcing international commitments. While much of the literature assumes veto players can make commitments more credible in all areas, I argue that in the human rights context their ability to do so depends on the extent to which leaders can effectively violate human rights without legislative approval and in secret. I test this theory using the procedure developed in Chapter 1. Finally, Chapter 4 returns to the estimates of treaty commitment preferences developed in Chapter 1. I analyze the treaty commitment preference space in order to better understand the key predictors of these preferences. I find that economic policy is the clearest and most consistent predictor of treaty commitment preferences, including with respect to non-economic treaties.

Chapter 1

The Informative Power of Treaty Commitment: Using the Spatial Model to Address Selection Effects

1.1 Introduction

The effect of international institutions on state behavior is among the most important issues in international relations scholarship. Theorists have argued that making international commitments allows states to eschew short-term gains for long-term interests, thus facilitating behavior they would not have undertaken absent these obligations (Keohane 1984; Martin 1992). More recently, scholars have extended these theories and empirically tested their implications with respect to the effects of commitment to institutions governing human rights (Poe, Tate and Keith 1999; Keith 1999; Hathaway 2002; Hafner-Burton and Tsutsui 2005; Neumayer 2005; Landman 2005; Hafner-Burton and Tsutsui 2007; Simmons 2009; Hill 2010), the environment (Mitchell 1994; Victor, Raustiala and Skolnikoff 1998; Weiss and Jacobson 1998; Mitchell et al. 2006; Stokke and Hønneland 2007) and international economic relations (Simmons 2000; von Stein 2005; Simmons and Hopkins 2005).

As many scholars have recognized, the inference we can draw from the relationship between treaty commitment and state behavior is threatened by a selection effect. Because governments are free to choose whether or not to commit to treaties, we cannot infer that treaty commitment has a causal effect on their behavior unless we address this selection effect. Several recent studies have adopted sophisticated methods to address this problem (von Stein 2005; Neumayer 2005; Simmons and Hopkins 2005; Hill 2010), the most appropriate and promising of which appears to be the matching approach proposed by Simmons and Hopkins (2005). Yet causal inference is limited even using this approach to the extent variables that affect treaty commitment decisions are omitted from the matching model.

I argue that existing applications of matching in this context (Simmons and Hopkins 2005; Hill 2010) have omitted a key factor: states' treaty commitment preferences. I propose a methodology to estimate these preferences in order include them in these models and make a significant improvement to the ignorability assumption. States' treaty commitment decisions, when analyzed systematically, provide a significant source of information that can reveal their preferences toward individual treaties and predict commitments to those treaties with a high degree of accuracy. I adopt a spatial-model-based method designed for the analysis of legislative roll-call voting, W-NOMINATE

(Poole and Rosenthal 1997), and demonstrate how it can be fruitfully applied in this context. After making a theoretical argument in favor of this methodology, I use Monte Carlo simulations to demonstrate that, in estimating treaty commitment probabilities, it outperforms a traditional model based on observable variables when significant unobservable factors affect treaty commitment decisions.

The paper continues by demonstrating how this methodology can be applied to estimate the effects of commitments to three key international human rights agreements: the Convention on the Elimination of All Forms of Discrimination against Women (CEDAW), the Convention Against Torture (CAT), and the International Covenant on Civil and Political Rights (ICCPR). These are among the most important and frequently analyzed human rights treaties, and empirical findings regarding their effects have found mixed - and often controversial - results. With respect to the CAT and ICCPR, several studies have found that treaty members are more likely to abuse the human rights of their citizens (Hafner-Burton and Tsutsui 2005; Neumayer 2005; Hill 2010), a counter-intuitive result that has created an important puzzle regarding why and how commitment to these treaties might worsen respect for human rights. Using W-NOMINATE on an original data set of commitments to universal treaties, I estimate states' probabilities of commitment to each of these treaties. With these probabilities, I create matched samples that are well-balanced on the probability of selection and use these samples to test the effects of treaty commitment.

My findings demonstrate that these prior results may have been due to an insufficient accounting for treaty commitment selection effects. With respect to both the ICCPR and CAT, I find that treaty commitment has not significantly affected human rights practices. For several possible reasons, human rights abusers appear to be more likely to join such treaties (Goodliffe and Hawkins 2006; Hathaway 2007; Vreeland 2008), yet joining these treaties does not appear to have a causal effect. In other words, there is a selection effect, but not a treatment effect. While this result means that these treaties have not succeeded in improving human rights practices, which is disappointing to proponents of international law, it also means that these treaties have not made matters worse, as prior studies indicate. With respect to CEDAW, I find robust results showing that treaty ratification has led to improvements in respect women's political, economic

and social rights. These findings provide a strong indication, in contrast to prior results (Hill 2010), that commitments to this treaty have improved the lives in women across areas of human rights.

This paper makes three significant contributions to the literature. Most importantly, it points to and provides a methodology for measuring an important factor that predicts treaty commitment decisions and that has been omitted from existing analyses. Including state preferences in the analysis of treaty effects has the potential to significantly improve our ability to empirically test theories of international institutions. Second, this paper demonstrates how ideal point estimation can productively be used outside of the legislative (or quasi-legislative) context and explains how it can be combined with matching to improve ignorability assumptions. To my knowledge, this is the first paper to use these methods in this manner. Finally, by applying this approach to test the effects of the CEDAW, CAT and ICCPR, this paper provides robust empirical results that will hopefully reduce the existing uncertainty regarding the impact of these important treaties.

1.2 The Treaty Commitment Selection Effect

As Downs, Rocke and Barsoom (1996) pointed out, and many social scientists have accepted, international institutions are often endogenous to state interests. The fact that states design and opt in to institutions creates a selection effect that threatens the validity of any inference we might draw from the relationship between institutions and states' behavior. There are two aspects to this problem: (1) both the nature of the underlying problem and state preferences affect the design of an institution; and (2) state behavior with respect to the activities required or proscribed by an international institution may result strictly from underlying state preferences and cannot be assumed to be the result of the institution itself. In this paper, I am concerned primarily with the second of these problems.

Commitment decisions are the key explanatory variables when studying the effects of treaties, and unless we address this selection effect we would be assuming treaty

commitment decisions are random, which they surely are not.[1] With respect to some treaties, for example, a high rate of compliance may not be caused by the treaty but by the fact that compliance requires states to take actions they would take in any event (Downs, Rocke and Barsoom 1996; Simmons 1998), meaning that the treaty has no independent effect on state behavior. In such a scenario, a model that does not account for this effect will tend to overestimate the effect of treaty commitment.

To address this problem, several important studies have adopted Heckman (1979) selection models (von Stein 2005; Neumayer 2005, 2007) or an instrumental variables approach (Simmons 2009). These approaches are subject to several important problems in this context. First, Heckman models are generally highly sensitive to distributional assumptions and the specification of the selection equation (Winship and Mare 1992; Liao 1995). Second, the Heckman sample selection model assumes incidental truncation (i.e., sample selection) and only addresses that truncation mechanism. The model is unnecessary (and often inappropriate) when the outcome variable can be observed for units that did not select into the sample (as is the case for many dimensions of government policy). Third, both the Heckman and instrumental variables approaches require the analyst to specify a variable that is correlated with treaty commitment but is independent of the subsequent outcome. A Heckman model without such a variable is identified based only on assumptions about the distributions of residuals (which are generally not based on theory) and not about the variation in the explanatory variables (about which theories are based)(Sartori 2003). In an instrumental variables model, the need to specify such variables is the core of the approach. This problem becomes acute in the treaty commitment context, in which the decision to commit is made by governments and the outcome variable is also a measure of government practice. Because the same actors often make both decisions, it is more likely that the same factors influence both decisions (Powell and Staton 2009), making finding a good instrument or exclusion restriction a significant problem. As Hill (2010) argues in the human rights context, "States that commit are different from states that do not, and these differences contribute to the decision to select into a 'treatment.' These differences also contribute to their decision to

[1] As Simmons and Hopkins (2005) argue, "Random assignment would imply a theory of frivolous commitment-making, hardly a model on which a useful theory of compliance with legal obligations can be developed" (p. 624).

repress their citizens, which makes it difficult to separate the effect of the treaty from the effect of the institutional features that led them to ratify" (p. 1168).

An alternative approach proposed to address the treaty commitment selection effect is propensity score matching (Simmons and Hopkins 2005). While it is infeasible to randomly assign treaty commitment, we can make causal inference with respect to the relationship between commitment and compliance if we can calculate and account for each state's probability of commitment. By first calculating the probability of selection, it is possible to match states into a sample that consists of sets of units that have and have not made the selection in question, with both sets having equivalent probabilities of having done so. Such a sample is designed to approximate random assignment to treatment (Ho et al. 2007). The propensity score method calculates this probability based on a set of observed pre-treatment characteristics that are theorized to affect the selection decision (Rosenbaum and Rubin 1983, 1984).

Matching has significant advantages over the methods discussed above, although it also has limitations. First, it can be used to control for bias on observable variables without the distributional assumptions required to sustain models such as that proposed by von Stein (2005). Second, it does not require the analyst to specify a factor that affects treaty commitment but not the outcome variable. Finally, matching is a useful method for creating a quasi-experiment by sampling similar treatment and control groups from a larger pool of such units (Morgan and Winship 2007).

Simmons and Hopkins (2005) advocate that propensity score matching in this context be conducted using the following procedure. First, the analyst should theorize about which variables predict treaty commitment and use those to estimate the probability of treaty commitment. As a second stage, they suggest that treated (treaty members) and untreated (non-members) should be matched using methods informed by theory and the first-stage model. Using this procedure, the extent to which we can make causal inference depends in large part on the ability to correctly estimate the probability of treaty commitment. The estimation of the treatment effect in subsequent stages of the research design is highly sensitive to omitted variable bias in the estimated propensity scores (Rubin 1997; Arceneaux, Gerber and Green 2006), and several studies have shown that the choice of underlying variables significantly affects the reliability of propensity score

analysis (Heckman, Smith and Clements 1997; Heckman et al. 1998; Lechner 2000; Smith and Todd 2005). Simmons and Hopkins (2005) therefore recognize that the presence of unobservable determinants of treaty commitment creates a threat to inference, so "Every effort should be made to theorize and to include in the commitment model all observables theory suggests are relevant, and an effort should be made to theorize and measure purported 'unobservables' as well" (p. 627). This paper attempts to do just that.

1.3 Estimating Treaty Commitment Preferences

I argue that the existing applications of matching to overcome the treaty commitment selection effect overlook a key factor that predicts these commitments: states' preferences with respect to treaties. The core of the question regarding the effects of international institutions is whether they have the power to cause governments to undertake actions they would not otherwise undertake. Thus, when we test the effects of treaty commitment on some dimension of state behavior, we can only make causal inference from this finding if we account for the fact that some states have stronger underlying preferences for the treaty than others. This notion is implicit in the existing methods for overcoming the treaty commitment selection effect, yet these methods do not attempt to estimate preferences directly. Existing applications of propensity score matching, for example, use more readily observable characteristics such as regime type and income to estimate the probability of treaty commitment (Simmons and Hopkins 2005; Hill 2010). Part of the intuition behind using these variables is the notion that democracies have different preferences from autocracies, rich countries different preferences from poor countries.

Existing studies have implicitly considered treaty commitment preferences unobservable, yet I argue that we can estimate them. Through their histories of treaty commitment decisions, states reveal significant information regarding their underlying preferences. This information can be used to estimate state preferences with respect to treaties and accurately predict state commitment to individual treaties. In turn, we can use these estimates to calculate predicted probabilities of treaty commitments and use

these in a matching approach similar to that proposed by Simmons and Hopkins (2005).

In order to measure state preferences with respect to treaties, I rely on the spatial model of political choice (Downs 1957; Davis, Hinich and Ordeshook 1970). The basic notion behind implementations of the spatial model is that, by observing the choices political actors make, we can measure their preferences relative to each other and relative to the options with which they are faced. In political science research, this model has largely been used to estimate the preferences of legislators with respect to roll-call voting decisions. By estimating the ideal points of legislators in this way, analysts can determine the probabilities of legislators voting for and against certain bills.

Both the ideal point model (and its implementations using multi-dimensional scaling algorithms) and propensity-score matching are tools for reducing the dimensionality of data, so their use in combination is intuitive to some extent. The following is a more formal explanation of how these tools fit together. Suppose that a government makes treaty commitment decisions based on four sets of factors: (1) observable characteristics of the country (e.g., the country's regime type) (O_1); (2) observable characteristics of the treaty (e.g., the subject-matter of the treaty) (O_2); (3) unobservable characteristics of the country (e.g., a latent preference for multilateral cooperation) (U_1); and (4) unobservable factors that are both country-specific and treaty-specific (U_2). Thus, in a given period of time, the decision Y of country i to ratify treaty j is based on the following model:

$$Y_{ij} = \beta_0 + \beta_1 O_{1i} + \beta_2 O_{2j} + \beta_3 U_{1i} + \beta_4 U_{2ij} + \varepsilon_{ij} \tag{1.1}$$

In a standard matching approach, both U_1 and U_2 will be omitted from the model, resulting in a strong ignorability assumption with respect to both such factors. My approach attempts to incorporate U_1 in order to improve this assumption and reduce omitted variable bias. An ideal-point estimation procedure attempts to reduce O_1, O_2 and U_1 to a specified number of dimensions (usually 2) resulting in a set of ideal point estimates θ. Ideal-point estimation is able to do this because all three of these factors are either country-specific or treaty-specific and thus their impact on treaty commitment decisions is systematically detectable in the treaty commitment data. By contrast, U_2 is both country- and treaty-specific, so its effects on treaty commitment decisions are too ad

hoc to allow for dimensionality reduction. Formally, ideal point estimation models the decision Y of country i ratifying treaty j as follows:

$$Y_{ij} = \alpha_j + \gamma_j \theta_i + \varepsilon_{ij} \tag{1.2}$$

where α_j is an intercept for the treaty, γ_j is a discrimination parameter indicating how well the treaty discriminates between different types of countries, and the error includes the effects of U_{2ij} and any stochastic component. Having estimated the ideal points in this fashion, it is subsequently possible to estimate the probability of each country committing to each treaty, subject to the utility function we choose to impose upon the data (as discussed below).

I use the spatial model to estimate the preferences of states with respect to universal treaties. In my model, the options of committing and not committing to a treaty are represented by points in an n-dimensional policy space. Each state decides whether or not to commit to a treaty by weighing the distance between these points and its ideal point in this space. Simmons (2009) has recently suggested that this logic applies to treaty commitment decisions: "To use the language of spatial models, the nearer a treaty is to a government's ideal point, the more likely that government is to commit." (p. 65, emphasis omitted). I adopt the language of Simmons (2009) in arguing that there are both false-negative and false-positive treaty commitment decisions. False-negatives are occasions in which states do not commit to treaties that seem well-aligned with their preferences. False-positives are occasions on which states do commit to treaties that seem to be contrary to their preferences. Some false-positives may be empty promises, while others may result from factors such as uncertainty over long-term consequences. Yet, I follow Simmons (2009) in arguing that treaty commitments are sincere in the aggregate, and therefore can reveal important information regarding preferences.

Thinking of treaty commitment decisions in this way allows for the use of spatial methods traditionally used to analyze other dichotomous choices, most importantly the methods used to study legislative roll-call voting. Specifically, I use the W-NOMINATE multi-dimensional scaling method to estimate states' treaty commitment preferences (Poole and Rosenthal 1997).[2] W-NOMINATE is a random utility model of Euclidean

[2] An important debate exists in the legislative studies literature regarding the most appropriate method

spatial voting (Enelow and Hinich 1984; Hinich and Munger 1994, 1997) that assumes each actor assigns a utility to each of two options. This utility is determined both by the distance between the actor and the options as well as a stochastic error term.

Poole and Rosenthal (1997) created W-NOMINATE as a tool for analyzing legislatures and used it to study the history of roll-call voting in the U.S. Congress. Other scholars have used W-NOMINATE estimation to study such areas as the repeal of the Corn Laws (Schonhardt-Bailey 2003), the Confederate legislature (Jenkins 1999), the European Parliament (Hix 2001; Noury 2001), and various national legislatures (Londregan 2000; Morgenstern 2003). In addition, many scholars have used the distances between points in the W-NOMINATE space for various purposes, including analyzing party cohesion (Desposato 2008), testing ideological compatibility differentials on party membership (Desposato 2006), measuring party polarization (Howell and Lewis 2002), and measuring the benefits associated with the differences between voting options (Rothenberg and Sanders 2000). W-NOMINATE has also been applied in the international context, particularly to analyze voting by states in the United Nations General Assembly (Voeten 2000; Reed et al. 2008).

Most scholars who use ideal point estimation focus on explaining the latent dimensions of the preference space and any cleavages that exist in that space (Poole and Rosenthal 1997; Voeten 2000). In Chapter 4, I analyze the dimensionality of the treaty commitment space in this fashion. Another important aspect of ideal point estimates is that they can be used to predict actors' choices based on the relative locations of the actors and choices in the preference space. In the legislative context, the closer a bill is to a legislator's ideal point, the more likely the legislator is to vote for the bill. In the treaty context, the closer a state's ideal point is to a treaty, the more likely it is to ratify the treaty. These probabilities are analytically similar to propensity scores, although they

---

to use in that context. In part, the debate revolves around whether it is more appropriate to assume legislators have Gaussian utility functions (Poole and Rosenthal 1997) or quadratic utility functions (Clinton, Jackman and Rivers 2004). This paper is not intended to contribute to that debate. I am not aware of any theories that address the question of whether governments have normal or quadratic utilities or whether we can safely assume some distribution on their errors. As a result, it is not immediately apparent whether one ideal point estimation method is more appropriate than another in the context of treaty commitment decisions. Ultimately, the choice of method may not significantly affect the results as IDEAL estimates are often very similar to W-NOMINATE estimates. Carroll et al. (2009) find, for example, that the ideal points estimated by W-NOMINATE and IDEAL for the 106th U.S. House of Representatives have a correlation coefficient of .996.

are based on direct estimates of state treaty commitment preferences. W-NOMINATE places each treaty in two locations in the preference space: one indicating the location of treaty ratification and one indicating non-ratification. Using these locations, W-NOMINATE calculates the probability that state i ratifies treaty j as follows:

$$P(Ratify)_{ij} = \frac{\exp[u_{ijr}]}{\exp[u_{ijr}] + \exp[u_{ijn}]} \tag{1.3}$$

where u_{ijr} is the deterministic component of the state's utility from ratifying the treaty, and u_{ijn} is the deterministic component of the state's utility from not ratifying the treaty. These utilities are calculated as follows:

$$u_{ijr} = \beta \exp[\frac{-\omega^2 d_{ijr}^2}{2}] \tag{1.4}$$

$$u_{ijn} = \beta \exp[\frac{-\omega^2 d_{ijn}^2}{2}] \tag{1.5}$$

where d_{ijr} is the distance between the state's ideal point and the location of treaty ratification, d_{ijn} is the distance between the state's ideal point and the location of treaty non-ratification, ω is a weight parameter estimated by W-NOMINATE, and β is a signal-to-noise ratio estimated by W-NOMINATE (Poole and Rosenthal 1997).

In the Appendix, I use Monte Carlo simulations to demonstrate that ideal point estimation is an effective method of estimating the probability of states committing to treaties. The simulations demonstrate that, under certain conditions, estimation of treaty commitment probabilities based only on observable variables known to affect those decisions is the more efficient method. One such situation is when we know and can measure all of the predictors of treaty commitment. A second is when the unobservable, unmeasurable or unknown predictors explain relatively little of the variance in those decisions. In the simulations reported above, when the magnitude of the unobservable factors reaches about 25% to 30% of the sum of coefficients, the probabilities estimated using the observable factors become less accurate than those estimated using spatial modeling. Importantly, these results assume we can measure the observables without error or bias.

My argument is that these scenarios will rarely occur in the treaty commitment

context. While scholars have learned a significant amount about the factors that drive treaty commitment, we should not assume that we can fully–or nearly fully–model these decisions. One problem is that factors that have not yet been theorized may be driving certain treaty commitment decisions; it is a strong assumption to argue that there are no "unknown unknowns." The accuracy of treaty commitment probabilities estimated using observable data drops quite rapidly when unobservables become increasingly significant, meaning that, by assuming unobservables do not exist, we take a significant chance of producing unreliable estimates. A second problem is that, even when we can theorize about a particular concept that affects treaty decisions, we may not have access to direct measures of it. Simmons and Hopkins (2005), for example, note that political will is an important factor that explains treaty commitment and which is difficult to observe or measure. They use three proxy measures for political will, yet it is unlikely they have measured this factor completely. Third, even when we have direct measures of a certain concept known to affect treaty commitment, it is often the case in the international relations data that we have reason to question whether the measures are free of error or bias. Finally, correct specification of the model predicting treaty commitments necessitates not only including the correct variables but also specification of the correct functional form, including the inclusion of any polynomial and interaction terms. Yet theory often does not provide sufficient guidance with respect to how to configure these variables (Smith and Todd 2005). In sum, it is unrealistic and risky to assume that we can know, observe and properly measure enough of the factors that explain treaty commitment decisions to meet the conditions under which using these variables to estimate treaty commitment probabilities is more efficient than using the spatial modeling approach.

Both in simulations and in real-world data, ideal point estimation appears to capture both observable and unobservable predictors of treaty commitment. I therefore generally recommend using the ideal point estimation method in this context. Nonetheless, it is also possible to combine matching based on other observables with the procedure outlined here. For example, if we believe that regime type predicts treaty commitments in a way not captured by the ideal point estimates, we could use both factors to match treaty members to non-members. In certain situations, doing so may further reduce

omitted variable bias and improve the ignorability assumption. There are certain risks to this approach. First, an incorrectly specified variable would likely increase the bias in the matching model. Second, any bias in the measurement of this variable would also translate into bias in the matching model. Finally, adding such a variable to a parametric matching model may introduce bias if the functional form is incorrectly specified.

Consequently, I recommend that analysts generally follow a three-step procedure in this context (for a similar procedure in a broader context, see Guo and Fraser (2010)). First, the probability of each country's commitment to a treaty should be estimated for the full sample of applicable country-years using ideal point estimation. Only treaties open to signature by the full set of countries should be included in the analysis. In the second stage, matching should be performed based on these probabilities of treaty commitment. In some situations, it may be preferable to include additional observable variables known to affect commitment to the applicable treaty in the model, keeping in mind the risks of doing so. Third, post-matching analysis should be conducted on the matched sample, generally using multivariate regression analysis. Such a model should include controls for other variables believed to affect the outcome variable.[3]

This methodology can be used with respect to any set of multilateral treaties. The most straightforward application is to universal treaties, which are open to all states, but the method can be used to measure the preferences of states over other multilateral treaties so long as all states in the sample are de jure eligible to join all of the treaties. Multilateral treaties offer a particularly interesting area in which to develop and test theories of international institutions. As many scholars have argued, compliance is more difficult to achieve in the multilateral setting. For example, it is much more difficult to rely on Tit-for-Tat strategies in the multilateral setting because it is unclear which of the many members of the treaty is charged with retaliation (Oye 1986). In such a setting, if one state violates the agreement, others will fear that if they retaliate (and bear the cost of doing so) other states will free ride instead of also retaliating (Goldsmith and Posner 2005). Second, multilateral treaties often include states with many different characteristics, allowing for a comparison of the effects of these treaties on different types of states. Finally, multilateral treaties include many of the most controversial

[3]Some matching algorithms, such as optimal matching, will require special regression adjustments in this stage.

and salient areas of international cooperation, and it is therefore critical for political scientists to better understand their effects.

1.4 Applications: Three Human Rights Treaties

To demonstrate how this methodology can be used, I test the effects of commitment to the three human rights treaties: the CEDAW, CAT and ICCPR. The CEDAW, which was adopted in 1979 by the United Nations General Assembly and came into force in 1981, prohibits discrimination against women with respect to a broad set of political, economic and social rights. The CAT specifically addresses government-sponsored torture. It was adopted in 1984 and came into force in 1987. The ICCPR, by contrast, protects individuals from a broad range of government abuses, including various personal integrity rights. It was adopted in 1966 and came into force in 1976.

Prior Results. Empirical findings regarding the relationship between commitment to the CEDAW and women's rights have been mixed. Hafner-Burton and Tsutsui (2005) found that commitment to the CEDAW is significantly correlated with an increase in the level of state repression (broadly defined to include other human rights violations). Gray, Kittilson and Sandholtz (2006), however, found that CEDAW commitment is associated with higher levels of female life expectancy and lower levels of female illiteracy. Using the instrumental variables approach, Simmons (2009) found that CEDAW commitment results in a more even ratio of girls to boys in primary and secondary education and in greater access to family planning resources. Most recently, using a matching approach based on observable predictors of treaty commitment, Hill (2010) found that CEDAW commitment has a positive and significant effect on state respect of women's political rights, but no effect on social and economic rights.

Tests of the effects of the CAT have produced arguably the most surprising and controversial findings in this literature. Beginning with Hathaway (2002), several studies have consistently found that CAT ratification is associated with higher rates of torture (Neumayer 2005). Most recently, Hill (2010) confirmed this finding using the measure of torture provided by the Cingranelli-Richards Human Rights Data Project (2009) (CIRI). These counter-intuitive results have led to two puzzles researchers have

attempted to address. The first is why governments of states that conduct torture would choose to ratify the CAT (Goodliffe and Hawkins 2006; Hathaway 2007; Vreeland 2008). The second, far more challenging, puzzle is how, if this relationship is truly causal, ratification of the CAT leads to higher rates of torture (Hollyer and Rosendorff 2011).

Findings with respect to the effects of ICCPR ratification have also been mixed. While Keith (1999) and Hathaway (2002) found no significant effects of ICCPR membership, Hafner-Burton and Tsutsui (2005) found that ICCPR members were more likely to conduct personal integrity rights violations. This finding, indicating that membership in this treaty may actually make human rights practice worse, was supported by Neumayer (2005), which uses a Heckman selection model. Using a matching technique, Hill (2010) likewise finds that ICCPR members become more likely to violate their citizens' personal integrity rights, as measured by the index of Physical Integrity Rights provided by CIRI.

Selection Effects. While part of the reason for these mixed findings may be differing choices regarding estimation model and dependent variable, the results are also due in part to selection effects. Using the approach outlined above, I test the effects of ratification of these treaties. I first collected a data set of states' decisions with respect to commitment to universal treaties. I collected the data from the United Nations Treaty Collection (UNTC), an online database that provides information regarding all treaties deposited with the U.N. Secretary-General. I analyzed the set of treaties hosted by the UNTC to determine which are de jure open to all states and which are limited to a specific set of states, such as on a regional basis. The latter are excluded from the analysis. The UNTC includes conventions, treaties, protocols to treaties and treaty amendments. If a list of signatories is included for any of these types of instruments, I include it in my data set as a separate choice. I do this because each item reflects a separate decision made by states, regardless of whether the item amends a previous choice. For simplicity, I will refer to each such item as a "treaty" in this paper.[4] For each treaty, I have thus created a matrix consisting of all of the states in the international system and an indication of whether or not they ratified the treaty. If a state has ratified a treaty as of a given year,

[4] A full list of these treaties is available from the author upon request.

I have coded that state as a "1" with respect to that treaty; otherwise the state is coded as a "0". Using these data, I created a matrix for each year between 1950 and 2007 that indicates, for each treaty then in effect, which states then in existence had ratified the treaty as of the end of the year. The Appendix presents certain summary statistics of the treaty data set.

With these data, I use a two-dimensional W-NOMINATE model to estimate the probabilities of ratification of the CEDAW, CAT and ICCPR on a country-year basis. For each treaty, I perform nearest-neighbor matching using the MatchIt package in the R programming language (Ho et al. 2009) to match the country-years that ratified the treaty with those that did not do so, following the procedure described by Ho et al. (2007). For the CEDAW and ICCPR, I include in the full sample country-years from 1981 to 2007. 1981 is the first year for which data on the dependent variables are available (as discussed below). For the CAT, I begin the full sample in 1987, which is the year the treaty took effect. Table 1.1 sets forth the results of the matching stage. In the full samples, the mean probability of commitment for the treatment group is much higher than for the control group. In the matched samples, however, these probabilities are significantly more well-balanced.

Dependent Variables. As dependent variables, I follow Hill (2010) in using the measures of human rights practices provided by CIRI. For women's rights, I use the CIRI measures of women's political, economic and social rights. The political rights measure is based on factors such as women's right to vote, run for office and petition government officials. The economics rights measure is coded based on rights such as non-discrimination in the workplace and equality in hiring, promotion and pay. The social rights measure includes rights such as equal inheritance, marriage and divorce rights, and education. These measures are coded on ordinal scales that range from 0 to 3, with higher scores indicating greater levels of respect for rights. A score of 3 indicates the country provides the full set of rights, a 2 indicates the country provides some rights that are effectively enforced, a 1 indicates the country provides some rights that are unenforced, and a 0 indicates the country does not provide these rights. With respect to the CAT, I use the CIRI measure of torture, which is coded on an ordinal scale ranging from 0 to 2. A score of 2 indicates there were no incidents of state-

sponsored torture in the country that year, a 1 indicates there were few such incidents, and a 0 indicates torture was practiced frequently. To test the effects of ICCPR on personal integrity rights, I use the Physical Integrity Index provided by CIRI. This index is an additive scale of measures of four personal integrity rights violations: torture, extrajudicial killings, political imprisonment and disappearances. Each component is coded in a manner analogous to the coding for torture (i.e., an ordinal scale from 0 to 2), and thus the values of the resulting index range from 0 to 8.

Controls. A perfectly balanced sample approximates random assignment to treatment, and therefore simple t-tests can often be used on such samples. My samples are not perfectly balanced with respect to the probability of assignment to treatment, and are not completely balanced with respect to several additional factors that may influence human rights practices, as shown in Table 1.2. To address remaining imbalance between the treatment and control groups, I use ordered probit models to test the effects of treaty commitment, while controlling for these variables. With respect to all three treaties, I control for several factors believed to affect human rights practices. Independent domestic courts can perform important enforcement functions with respect to human rights, particularly when protections have been incorporated into domestic law (Keith 2002*b*; Keith, Tate and Poe 2009; Powell and Staton 2009; Simmons 2009). As a measure of judicial independence, I adopt the data provided by CIRI (JUDICIAL INDEPENDENCE), which are coded as 0 for "not independent," 1 for "partially independent" and 2 for "generally independent." A series of studies has found that democracies are more likely to respect a range of human rights (Davenport 1995, 1999, 2007*a*; Poe and Tate 1994; Poe, Tate and Keith 1999). Using data from the Polity IV project (Marshall and Jaggers 2002), I therefore control for regime type (POLITY). Human rights practices may also vary between old and new regimes, and I follow Hafner-Burton and Tsutsui (2007) by controlling for this factor using the Polity IV data (REGIME DURABILITY). States experiencing either internal or external wars may be more likely to repress human rights (Poe and Tate 1994; Poe, Tate and Keith 1999), so I control for this using data from the Correlates of War Project. The number of international non-governmental organizations (INGOs) active in a country may affect the government's human rights practices, so I control for this factor using the data provided by Hafner-Burton and Tsutsui (2005).

More economically developed states may be less likely to repress their citizens (Poe and Tate 1994; Poe, Tate and Keith 1999), and I control for this using a measure of per capita GDP provided by the World Bank. I use the natural log of this measure because this effect is likely nonlinear (Davenport 2007*a*). To address potential differences among states of different sizes and potential monitoring biases based on this factor, I follow much of the literature in including a control for the natural log of a state's population, using data provided by the World Bank. To address serial correlation, I include lags of the applicable dependent variable for years $t-1$ and $t-2$ for women's rights. With respect to torture and personal integrity rights, I include a lag for year $t-1$. Lagrange multiplier tests indicates that additional lags are not necessary to address serial correlation.[5]

Not surprisingly, there were many observations with missing data among these variables. Because the underlying reasons for the missingness of the data are likely nonrandom, listwise deletion of these observations may result in biased inference (Little and Rubin 1987). I therefore follow Hill (2010) and others in imputing the missing values using the Amelia II Program (Honaker, King and Blackwell 2009).[6] In all models, I report standard errors that are robust toward arbitrary heteroskedasticity and that take into account clustering by country. All of the models also include fixed effects for the year of the observation. Table 1.3 reports the results of these models.

The results of these models differ in important ways from those prior studies. The clearest comparison may be to the results reported by Hill (2010) based on models that use a similar matching procedure but do not balance based on treaty commitment preferences. With respect to the CEDAW, while Hill (2010) found that it improves respect for women's political rights, I find that it also improves respect for women's economic and social rights. These findings are based on the fullest accounting to date for treaty commitment selection effects and thus provide a good indication that the treaty has made an important positive impact on women's lives. Figure 1.1 reports the marginal effects of CEDAW ratification on women's rights, based on the models reported in Table

[5] All of the results reported below with respect to torture and personal integrity rights are robust to the additional inclusion of a lagged dependent variable for year $t-2$.

[6] I conducted the imputation procedure using the full set of country-years (rather than the matched sample) because including the full data allows for more accurate imputation.

1.3. The figure indicates that not only is the impact of CEDAW membership statistically significant, it also has a fairly substantial impact on respect for women's rights. Across all three categories of rights, CEDAW members become much more likely to provide either limited or full and enforceable rights. Similarly, across all categories, CEDAW members become less likely to only provide unenforced rights or no rights at all. The results are therefore encouraging both for those who seek to improve global respect for human rights and those who believe international institutions can be effective tools for accomplishing these goals.

With respect to the CAT, my results contradict those of most previous studies, including Hill (2010). My finding is that ratification of the CAT does not significantly affect torture rates, which stands in sharp contrast to prior findings indicating that such ratification may increase torture. These prior results may have been due to a less complete accounting for selection effects than the procedure I have outlined provides. In other words, prior results appear to have found that governments of states that ratify the CAT are more likely to torture, yet my results indicate that this relationship is not likely to be causal. While this finding certainly leaves us with the puzzle of why states that conduct torture would more often choose to ratify the CAT, it renders moot the more difficult puzzle of how treaty ratification might cause more torture.

On a similar note, my results contradict several recent findings that ICCPR ratifications are associated with increases in personal integrity rights violations (Hafner-Burton and Tsutsui 2005; Neumayer 2005; Hill 2010). Those findings had left us with a similar puzzle as the findings with respect to CAT: why and how could treaty ratification actually cause governments to increase these abuses? My results indicate that these prior findings may have been picking up on a selection effect, rather than a treatment effect. Of course, this still leaves us with a puzzle as to why abusing states tend to select into the treaty more often, but this is arguably a less problematic puzzle than the question of why such commitments would cause increases in abuses.

In the above models, I estimate the probabilities of treaty commitment using only states' treaty commitment preferences (as estimated using W-NOMINATE). It is possible, however, that these preferences do not capture the full model of commitment. Indeed, other observable factors may affect the probability of commitment, including

the factors that ultimately affect states' respect for human rights (Powell and Staton 2009). As a robustness check, I match states using a model that includes the probability of commitment calculated by W-NOMINATE as well as all of the controls listed in Table 1.2. I then estimate a set of ordered probit models on the matched samples that are identical to the models reported in Table 1.3. The balance statistics of this sample are reported in Tables 1.5 and 1.6, and the regression results are reported in Table 1.7, all of which are in the Appendix. These robustness tests produce findings substantively similar to those reported above.

Table 1.4 summarizes my results and compares them with those in Hill (2010). For each treaty and dependent variable, Table 1.4 shows the results from Hill (2010) based on matching on observable treaty commitment covariates, followed by the results reported in Table 1.3 based on matching on treaty commitment preferences, and followed by the results reported in Table 1.7 based on matching on both treaty commitment preferences and observable treaty commitment covariates. In summary, the results of my models differ from previous findings with respect to 5 of the 6 dependent variables. This indicates that a more complete accounting for treaty commitment selection effects can significantly impact the results of subsequent hypothesis tests of the effects of treaty commitment.

1.5 Conclusions

Whether or not international institutions have the power to constrain states has been the subject of a central research agenda in the international relations literature for over two decades. Scholars have especially paid attention to determining the effects of multilateral treaties on state behavior. Yet the fact that states self-select into these treaties threatens our ability to determine whether they act as constraints or simply as screening devices. By using methods created in other substantive areas, scholars have attempted to overcome the selection effect problem. Yet these methods have several important limitations that limit their utility in this context.

In this paper, I have argued that these methods have a significant substantive shortcoming when used to study treaty effects: an insufficient accounting for under-

lying state preferences. Thus, I have outlined an approach to the treaty commitment selection effect problem that estimates states' preferences with respect to treaties by using the spatial model and, subsequently, uses these estimates to match treaty members to comparable non-members. Monte Carlo simulations show that the spatial modeling approach accurately calculates the ex ante probabilities of states committing to treaties, which, in turn, allows us to create balanced samples. This approach has the potential to substantially improve our ability to test theories and advance our knowledge of the causal effects of international institutions.

To my knowledge, this is the first paper to use W-NOMINATE outside of the legislative or quasi-legislative context and the first to combine it with propensity score matching in this manner. I hope that scholars will find the combination of these frequently employed tools useful in other areas. An example might be in the legislative context in which W-NOMINATE is often used. Suppose we are interested in determining whether voting for a particular bill affects legislators' probability of being assigned to a particular committee, seeking re-election, winning re-election or some other outcome. To perform such an analysis, we could use a procedure similar to that outlined above: begin by estimating the probability of legislators voting for the bill, create a matched sample based on these probabilities (and possibly additional factors), and run further statistical tests using the outcome variable.

I have also provided novel empirical findings regarding the effects of commitment to the CEDAW, CAT and ICCPR. The results indicate that ratification of the CEDAW has caused improvements in the lives of women across a broad range of political, economic and social rights. By contrast, commitments to the ICCPR and CAT have not significantly affected human rights practices. These results are an important contribution to the debate on the effects of international institutions, and will hopefully serve to clear up empirical puzzles, especially with respect to existing controversies regarding the possible negative effects of ICCPR and CAT ratifications. Yet the results also point us to new puzzles by contrasting one case, the CEDAW, in which international law has been a useful tool for improving human rights practices with other cases, ICCPR and CAT, where this does not appear to be the case. Potential explanations for these contrasting results, which I hope scholars will analyze in the future, include differences

in treaty design and in the way enforcement mechanism functions in distinct areas of human rights. Regardless of the explanation, the results point to the need for nuanced theories about when international law can be effective and when it cannot.

Table 1.1: Balance Statistics

	CEDAW		CAT		ICCPR	
	Full	Matched	Full	Matched	Full	Matched
Sample Size	4368	1652	3519	1580	4368	1966
Treatment Units	3139	826	2252	790	2947	983
Control Units	1229	826	1267	790	1421	983
Mean Pr(Ratif.) Treat.	0.911	0.662	0.760	0.394	0.882	0.647
Mean Pr(Ratif.) Ctrl.	0.484	0.683	0.230	0.358	0.481	0.654
Improve. in Balance	94.90%		94.49%		98.23%	

Table 1.2: Balance Statistics - Controls. Mean values reported for treatment and control groups.

	CEDAW		CAT		ICCPR	
	Treat.	Control	Treat.	Control	Treat.	Control
Judicial Indep.	1.10	1.18	0.95	1.07	1.03	1.09
Polity	-0.93	-1.34	0.78	0.08	0.17	-0.87
Regime Durab.	20.75	29.34	21.07	15.41	16.53	24.46
Civil War	0.23	0.22	0.25	0.18	0.25	0.19
External War	0.04	0.04	0.01	0.03	0.03	0.04
GDP Per Cap. (log)	7.04	7.69	6.99	7.21	6.84	7.48
Pop. (log)	15.71	15.76	15.98	15.69	15.54	15.88
INGOs	464.16	539.68	484.58	477.50	372.88	543.47

Table 1.3: Effects of Treaty Ratification - Ordered Probit Models

		CEDAW		CAT	ICCPR
	Pol.	Econ.	Soc.	Torture	Phys. Int.
Ratification	0.234**	0.171**	0.265***	-0.091	-0.032
	(0.078)	(0.066)	(0.064)	(0.077)	(0.062)
Judicial Indep.	0.0386	0.114*	-0.0313	0.136	0.195***
	(0.059)	(0.057)	(0.056)	(0.071)	(0.0501)
Polity	0.013*	0.013*	0.030***	0.009	0.024***
	(0.006)	(0.006)	(0.006)	(0.007)	(0.005)
Regime Durab.	0.000	0.004**	0.004**	0.000	0.003
	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)
Civil War	0.024	-0.227**	-0.157*	-0.426***	-0.903***
	(0.072)	(0.085)	(0.075)	(0.099)	(0.099)
External War	-0.085	-0.047	-0.264	0.033	-0.071
	(0.137)	(0.216)	(0.160)	(0.376)	(0.132)
GDP Per Cap. (log)	-0.087*	0.039	-0.001	0.091**	0.062*
	(0.035)	(0.037)	(0.034)	(0.039)	(0.031)
Pop. (log)	-0.022	-0.047	-0.030	-0.186***	-0.127***
	(0.029)	(0.028)	(0.026)	(0.030)	(0.024)
INGOs	0.000*	0.000	0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rights_{t-1}	1.145***	0.765***	0.953***	1.008***	0.481***
	(0.102)	(0.090)	(0.079)	(0.073)	(0.021)
Rights_{t-2}	0.595***	0.480***	0.559***	—	—
	(0.097)	(0.061)	(0.074)		
F.E. for Year	Yes	Yes	Yes	Yes	Yes
n	1652	1652	1652	1580	1966

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.4: Summary of Treaty Commitment Coefficients

Treaty	DV	Matching On	Coef.	s.e.	p	n
CEDAW	Pol. Rights	Observable Cov. (Hill)	0.306	0.159	0.056	1642
CEDAW	Pol. Rights	Treaty Commit. Prefs.	0.233	0.078	0.003	1652
CEDAW	Pol. Rights	Both	0.274	0.074	0.000	1592
CEDAW	Econ. Rights	Observable Cov. (Hill)	0.039	0.119	0.747	1642
CEDAW	Econ. Rights	Treaty Commit. Prefs.	0.171	0.066	0.009	1652
CEDAW	Econ. Rights	Both	0.203	0.065	0.002	1592
CEDAW	Soc. Rights	Observable Cov. (Hill)	0.15	0.12	0.211	1642
CEDAW	Soc. Rights	Treaty Commit. Prefs.	0.265	0.064	0.000	1652
CEDAW	Soc. Rights	Both	0.272	0.070	0.000	1592
CAT	Torture	Observable Cov. (Hill)	-0.65	0.123	0.000	1642
CAT	Torture	Treaty Commit. Prefs.	-0.091	0.077	0.237	1580
CAT	Torture	Both	-0.098	0.079	0.217	1552
ICCPR	Phys. Int.	Observable Cov. (Hill)	-0.405	0.101	0.001	1643
ICCPR	Phys. Int.	Treaty Commit. Prefs.	-0.030	0.062	0.608	1966
ICCPR	Phys. Int.	Both	0.010	0.062	0.869	1870

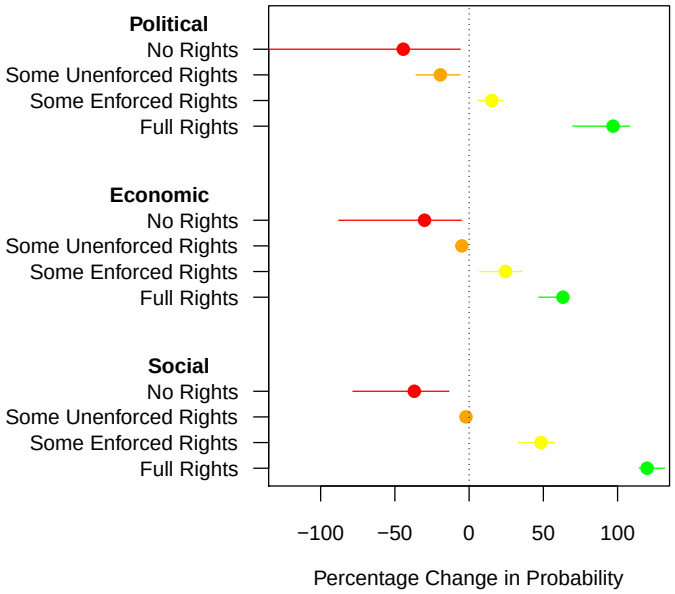


Figure 1.1: Marginal Effects of CEDAW Ratification on Women's Rights

1.6 Appendix

1.6.1 Monte Carlo Simulations

I conduct the simulations according to the following procedure:

Step 1. Create a data set of 100 hypothetical countries and 100 hypothetical universal treaties.

Step 2. Create five random variables with standard normal distributions that determine treaty commitment decisions (X_1 through X_5). X_1, X_2, X_4 and X_5 are observable variables that can be measured without error or bias, while X_3 is unobservable (or unmeasurable). The decision Y of country i to ratify treaty j is given by the following equation:

$$Y_{ij} = \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{3i} + \beta_4 X_{4i} + \beta_5 X_{5i} \tag{1.6}$$

where I arbitrarily assign the following values to the coefficients: $\beta_1 = 1$, $\beta_2 = -1$, $\beta_4 = 1$, $\beta_5 = -1$. The value of β_3 is also fixed in a given simulation, but varies across simulations per Step 9.

Step 3. Assign the true probability P_{ij} of country i to ratify treaty j using the logistic function of Y_{ij}, as follows:

$$P_{ij} = \frac{1}{1 + \exp(-Y_{ij})} \tag{1.7}$$

The function used to calculate P_{ij} here need not be the logistic function so long as the function used here is the same as that used in Step 7.

Step 4. Use the true probabilities of treaty commitment to randomly generate the commitment decisions T_{ij} (where $T \in (0,1)$) of all countries with respect to all treaties.

Step 5. Estimate the country-treaty ideal points using the W-NOMINATE algorithm on the simulated ratification matrix $\mathbf{T}$.[7] To be clear, none of the data on the X variables are used in the W-NOMINATE estimation. Using Equation 1.3, estimate the probability of each country to ratify each treaty $\hat{P}_{ij_{wn}}$. These estimations were performed using the wnominate package in the R programming language (Poole et al. 2008).

Step. 6. For each treaty j, estimate a logit regression model that predicts the simulated

[7] I use a two-dimensional model for these simulations. With additional dimensions, the W-NOMINATE estimates perform better, although this also creates a risk of over-fitting.

ratifications using the observable, measurable variables, as follows:

$$T_j = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_4 X_4 + \beta_5 X_5 + \varepsilon \tag{1.8}$$

Step 7. For each country i, estimate the probability $\hat{P}_{ij_{ob}}$ of ratifying treaty j using the logistic function of the estimates generated by this model, as follows:

$$\hat{P}_{ij_{ob}} = \frac{1}{1 + \exp(\hat{\beta}_0 + \hat{\beta}_1 X_{1i} + \hat{\beta}_2 X_{2i} + \hat{\beta}_4 X_{4i} + \hat{\beta}_5 X_{5i})} \tag{1.9}$$

Step 8. Repeat steps 2 through 7 for a given value of β_3 100 times.
Step 9. Repeat steps 2 through 8 for a range of values of β_3 from 0 to 5.

The results include two sets of estimated treaty commitment probabilities, and the question is which method results in better estimates of P_{ij} for each value of β_3. To determine which method estimates P_{ij} more efficiently, I calculated the root mean squared error (RMSE) of each set of estimated probabilities with respect to the true propensity scores P_{ij} for each simulation. This results in 100 values of the RMSE for each value of β_3 for each method. For each method, Figure 1.2 shows the means of the RMSEs for each value of β_3. To provide a sense of scale, the x-axis shows the value of β_3 divided by the sum of the absolute values of all the βs. The mean RMSEs for the W-NOMINATE estimates appear in blue, while those for the estimates based on observables appear in red.

Predictably, the model based on observable country characteristics performs very well when β_3 is small. When $\beta_3 = 0$, the regression model given by Equation 1.8 is estimating the true model given by Equation 1.6, subject to the stochastic component. Yet, as β_3 becomes larger, the mis-specification in the model given by Equation 1.8 grows in importance. As a result, as β_3 becomes larger, the error in the estimates grows significantly. In substantive terms, this means that as unobservable or unmeasurable variables become more important determinants of treaty ratification decisions, estimating probabilities of treaty commitment based on the observable variables becomes increasingly inefficient.

By contrast, the efficiency of the W-NOMINATE estimates is not made worse

by the presence or importance of unobservable variables. Increasing the magnitude of β_3 does not reduce the extent to which $\hat{P}_{ij_{wn}}$ correctly estimates P_{ij}. This is ultimately because, regardless of the weight given to the unobservable variable, $\hat{P}_{ij_{wn}}$ is estimated based on the simulated treaty ratifications, rather than the other variables that predict treaty ratification. In this simulation, as β_3 becomes larger, the W-NOMINATE estimates actually become more efficient. This is likely the case because, when β_3 grows, its magnitude becomes larger than those of the other βs. When a particular underlying factor explains treaty ratifications more so than others, W-NOMINATE becomes a more efficient estimator specifically because it is designed to find the most important latent dimension. Thus, the improvements in W-NOMINATE estimate efficiency shown in Figure 1.2 as β_3 becomes larger may not occur for all real-world data. The more important point is that the W-NOMINATE estimates are not made worse by unobservable variables, and this should continue to be the case for real-world data.

This simulation, however, relies on the unrealistic assumption that a given country considers the same set of factors for each treaty commitment decision. It is more likely that countries take treaty-specific factors into account, such as the subject matter of the treaty (these are noted as O_2 in Equation 1.1). Thus, I run a second simulation that introduces treaty-specific variables by changing Steps 2 and 6 from the above procedure as follows:

Step 2. Create 11 random variables with standard normal distributions that determine country treaty commitment decisions (X_1 through X_1). All but X_3 are observable variables that can be measured without error or bias. The decision Y of country i to ratify treaty j is given by the following equations:

$$
\begin{aligned}
Y_{ij} \,|\, j \in (0:25) &= \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{3i} + \beta_4 X_{4i} + \beta_5 X_{5i} \\
Y_{ij} \,|\, j \in (26:50) &= \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{3i} + \beta_6 X_{6i} + \beta_7 X_{7i} \\
Y_{ij} \,|\, j \in (51:75) &= \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{3i} + \beta_8 X_{8i} + \beta_9 X_{9i} \\
Y_{ij} \,|\, j \in (76:100) &= \beta_1 X_{1i} + \beta_2 X_{2i} + \beta_3 X_{3i} + \beta_{10} X_{10i} + \beta_{11} X_{11i}
\end{aligned}
$$

where I arbitrarily assign the following values to the coefficients: $\beta_1 = 1$, $\beta_2 = -1$,

$\beta_4 = 1$, $\beta_5 = -1$, $\beta_6 = 1$, $\beta_7 = -1$, $\beta_8 = 1$, $\beta_9 = -1$, $\beta_{10} = 1$, $\beta_{11} = -1$.

Step. 6. Estimate logit regression models that predict the simulated ratifications using the observable, measurable variables, as follows:

$$T_j \mid j \in (0:25) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_4 X_4 + \beta_5 X_5 + \varepsilon$$
$$T_j \mid j \in (25:50) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_6 X_6 + \beta_7 X_7 + \varepsilon$$
$$T_j \mid j \in (51:75) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_8 X_8 + \beta_9 X_9 + \varepsilon$$
$$T_j \mid j \in (76:100) = \beta_0 + \beta_1 X_1 + \beta_2 X_2 + \beta_{10} X_{10} + \beta_{11} X_{11} + \varepsilon$$

Figure 1.3 shows the results of the second simulation. As above, the model based on the observable variables performs relatively well when β_3 is small, but the W-NOMINATE model is more efficient when β_3 is sufficiently large.

1.6.2 Treaty Data

Figures 1.4 through 1.7 present the trends in certain summary statistics of my treaty data set. As is well known, the number of multilateral treaties has increased significantly over recent decades. It is not surprising, therefore, that the average number of ratified treaties has increased consistently since 1950, as Figure 1.4 shows. The only significant drops observed in Figure 1.4 occur in two years, 1960 and 1991, when many new states entered the system. A more striking result is depicted in Figure 1.5, which shows that, accounting for the increase in the total number of treaties in force, the average percentage of treaties ratified by each state has nonetheless increased over time, particularly since the end of the Cold War (with predictable significant drops in 1960 and 1991). Figures 1.6 and 1.7 describe the numbers of states that have ratified given ranges of sums of treaties and percentages of treaties as of 2007. Most states have ratified between 50 and 150 treaties, although a fair number have ratified significantly more. Likewise, most states have ratified between 20% and 50% of the treaties.

1.6.3 Robustness Tests

This section sets forth the results of alternate specifications to the models presented in the main text. Tables 1.5 and 1.6 provide the balance statistics of the matched sample. Table 1.7 provides the results of ordered probit models used to estimate the effects of treaty commitment using the matched samples.

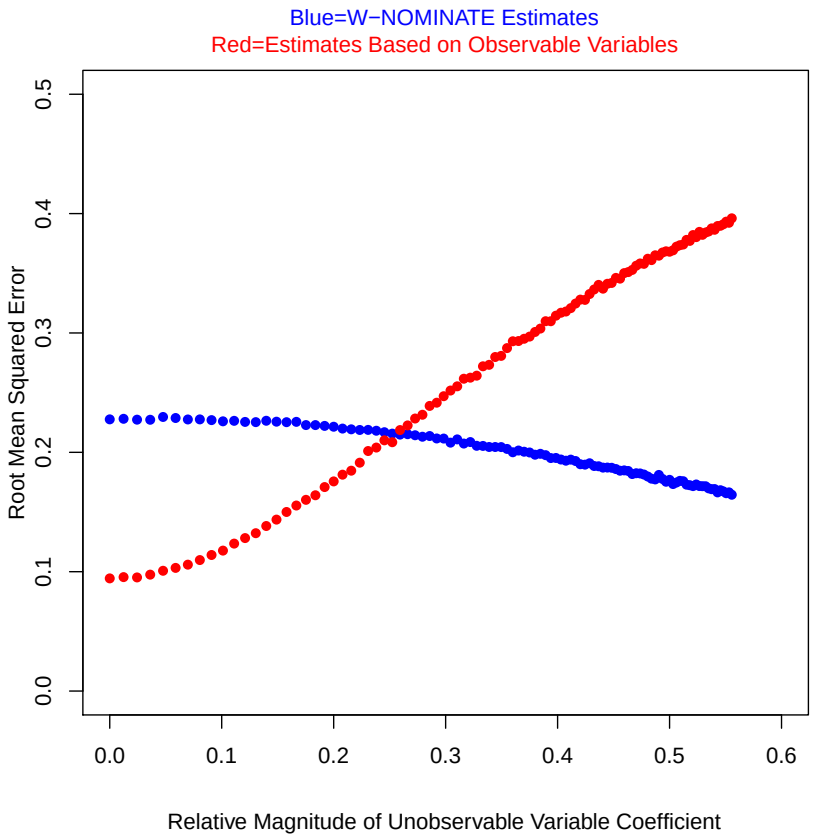


Figure 1.2: First comparison of Root Mean Squared Errors

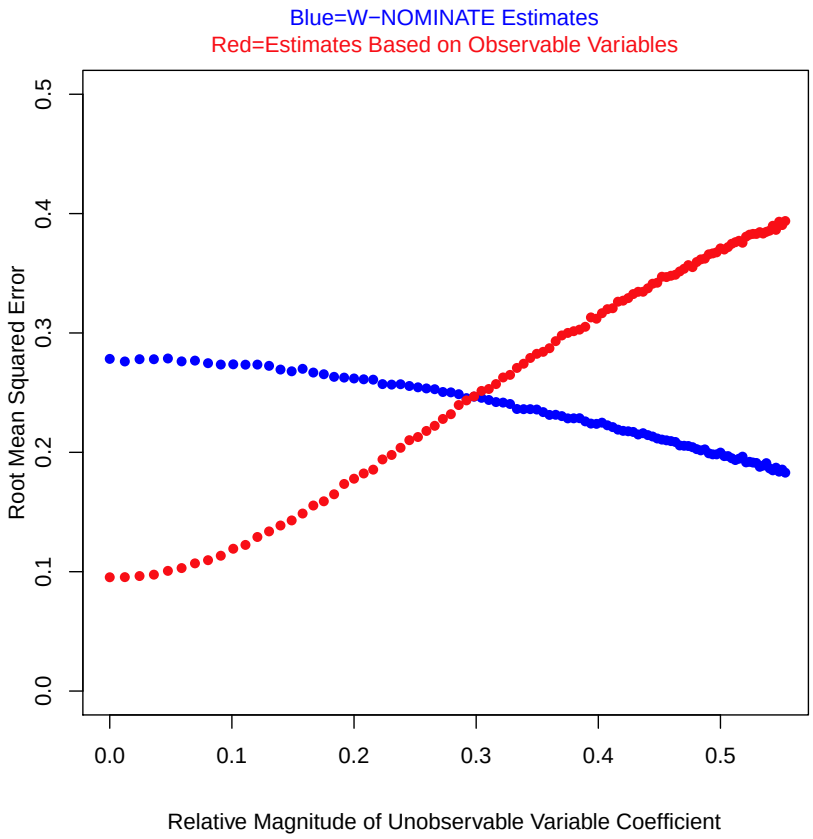


Figure 1.3: Second comparison of Root Mean Squared Errors

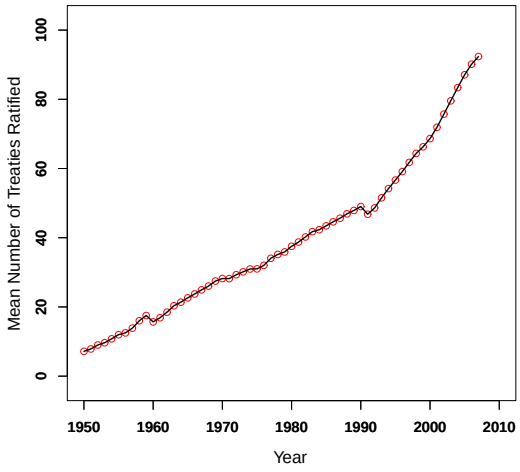


Figure 1.4: Mean Number of Treaties Ratified

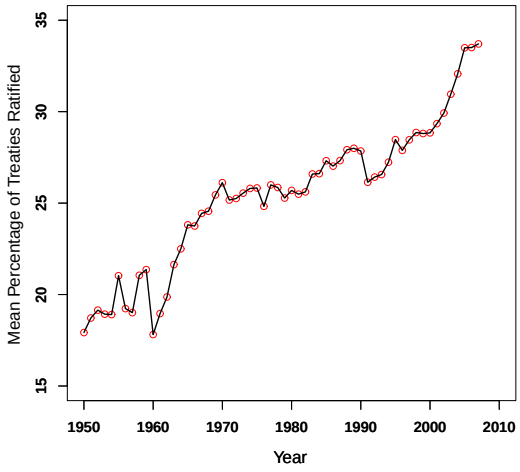


Figure 1.5: Mean Percentage of Treaties Ratified

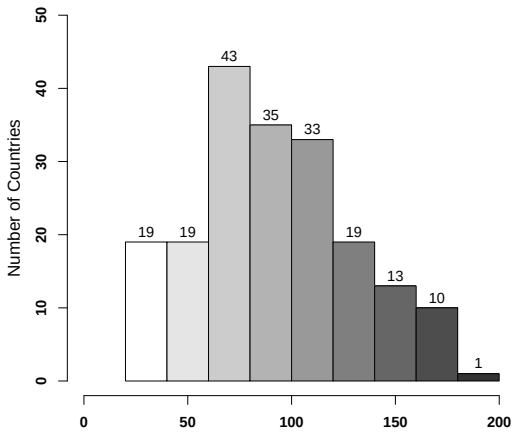


Figure 1.6: Number of Treaties Ratified as of 2007

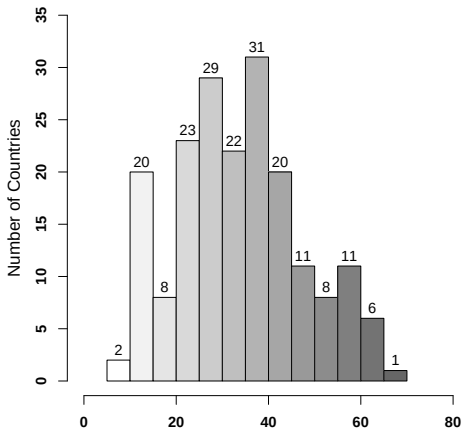


Figure 1.7: Percentage of Treaties Ratified as of 2007

Table 1.5: Robustness Test Balance Statistics

	CEDAW		CAT		ICCPR	
	Full	Matched	Full	Matched	Full	Matched
Sample Size	4368	1592	3519	1552	4368	1870
Treatment Units	3139	796	2252	776	2947	935
Control Units	1229	796	1267	776	1421	935
Mean Pr(Ratif.) Treat.	0.911	0.676	0.760	0.330	0.882	0.656
Mean Pr(Ratif.) Ctrl.	0.484	0.679	0.230	0.358	0.481	0.667
Improve. in Balance	94.92%		98.61%		97.33%	

Table 1.6: Robustness Test Balance Statistics - Controls. Mean values reported for treatment and control groups.

	CEDAW		CAT		ICCPR	
	Treat.	Control	Treat.	Control	Treat.	Control
Judicial Indep.	1.13	1.14	0.97	1.03	1.00	1.06
Polity	-1.73	-0.72	0.07	0.65	-2.00	-0.56
Regime Durab.	30.15	27.37	17.31	19.82	23.03	22.80
Civil War	0.26	0.21	0.22	0.20	0.22	0.19
External War	0.04	0.03	0.03	0.02	0.03	0.03
GDP Per Cap. (log)	7.63	7.54	7.03	7.20	7.27	7.28
Pop. (log)	15.73	15.75	15.89	15.78	15.94	15.88
INGOs	467.48	547.12	403.51	507.88	413.08	521.37

Table 1.7: Robustness Test: Effects of Treaty Ratification - Ordered Probit Models

	CEDAW			CAT	ICCPR
	Pol.	Econ.	Soc.	Torture	Phys. Int.
Ratification	0.274***	0.203**	0.272***	-0.098	0.010
	(0.074)	(0.065)	(0.070)	(0.079)	(0.062)
Judicial Indep.	0.069	0.122*	-0.042	0.159*	0.171**
	(0.059)	(0.058)	(0.058)	(0.069)	(0.053)
Polity	0.016*	0.013*	0.027***	0.020**	0.030***
	(0.007)	(0.006)	(0.007)	(0.007)	(0.005)
Regime Durab.	-0.000	0.002	0.001	0.003	0.003**
	(0.001)	(0.001)	(0.001)	(0.002)	(0.001)
Civil War	-0.038	-0.301***	-0.203**	-0.545**	-0.763***
	(0.084)	(0.075)	(0.078)	(0.103)	(0.089)
External War	0.200	-0.101	-0.204	-0.014	-0.011
	(0.227)	(0.131)	(0.174)	(0.308)	(0.150)
GDP Per Cap. (log)	-0.115***	0.026	-0.003	0.064	0.020
	(0.032)	(0.032)	(0.033)	(0.033)	(0.027)
Pop. (log)	-0.043	-0.044	-0.045	-0.158***	-0.158***
	(0.029)	(0.027)	(0.027)	(0.031)	(0.028)
INGOs	0.000**	0.000**	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Rights_{t-1}	1.088***	0.749***	0.834***	0.996***	0.505***
	(0.101)	(0.091)	(0.070)	(0.086)	(0.068)
Rights_{t-2}	0.609***	0.530***	0.607***	—	—
	(0.082)	(0.063)	(0.082)		
F.E. for Year	Yes	Yes	Yes	Yes	Yes
n	1592	1592	1592	1552	1870

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 2

Best Evidence: The Role of Information in Domestic Judicial Enforcement of International Human Rights Agreements

2.1 Introduction

Do commitments to international institutions have independent effects on state behavior? This central question in the international relations literature has received critical attention in recent years, resulting in both theory and empirical evidence regarding the effects of commitments to international institutions in a variety of policy areas (Keohane 1984; Mitchell 1994; Victor, Raustiala and Skolnikoff 1998; Simmons 2000; Kelley 2007). The question seems especially complex with respect to human rights, where international enforcement mechanisms thought to work in other areas, such as reciprocity and peer enforcement, do not appear to function (Downs and Jones 2002; Simmons 2009). As a result, scholars have increasingly turned to various other mechanisms by which commitments to human rights treaties might affect government practices, including normative pressure (Finnemore and Sikkink 1998; Keck and Sikkink 1998) and domestic political mobilization (Hafner-Burton and Tsutsui 2005; Conrad 2012). Domestic courts also appear to play a major role in the domestic enforcement of international human rights law. In some countries, the judiciary is sufficiently powerful to prosecute other government actors for violating their international commitments. Knowing this, these governments can use such commitments to constrain their future policy choices (Cross 1999; Keith 2002*b*; Apodaca 2004; Keith, Tate and Poe 2009; Powell and Staton 2009).

Despite the progress scholars have made on explaining the effects of human rights treaties, the existing research has nonetheless left us with two important puzzles. First, independent domestic courts do not appear to improve human rights practices everywhere. Many countries, such as Chile (1972), Costa Rica (1968) and Portugal (1978), have ratified the International Covenant on Civil and Political Rights (ICCPR) and have independent judiciaries, but continue to commit human rights violations, especially torture. Given that the courts of such states are sufficiently independent to enforce these legal obligations, we might expect this enforcement mechanism to deter such violations, yet that does not seem to be the case. Second, the empirical results with respect to the effects of human rights treaties have been mixed. This is especially true for the effects of ICCPR membership: studies have found that it either improves, does not affect or even worsens state respect for human rights (Hathaway 2002; Hafner-Burton and

Tsutsui 2005; Simmons 2009; Hill 2010).

These two puzzles are closely related. We can better understand why human rights treaties appear to be effective in some areas and not others by analyzing which types of legal obligations domestic courts can effectively enforce. I argue that understanding the extent to which domestic courts can enforce international human rights commitments requires accounting for the fact that human rights practices and violations are highly multi-dimensional (McCormick and Mitchell 1997; Hathaway 2002; Davenport 2007*c*). Governments have many potential tools of repression at their disposal, and mechanisms that reduce the use of certain of these tools may not impact others. This paper seeks to address these issues by asking the following question: Does enforcement by domestic courts constrain government practices with respect to all human rights, or does this mechanism only function in certain areas?

Independent courts can be effective enforcers, but enforcement also depends on information. Courts generally have weak monitoring powers and must rely on other actors to produce information regarding violations of law. This paper explains when domestic courts can effectively enforce international human rights law by analyzing when they are likely to have the information necessary to do so. Scholars have analyzed the mechanisms for producing information about human rights violations in much detail. We know NGOs and the media perform crucial roles in monitoring governments, collecting information from victims and educating the domestic and international public about abuses (Keck and Sikkink 1998; Risse, Ropp and Sikkink 1999; Dai 2007; Hafner-Burton 2008). What is missing from this literature, however, is an analysis of the link between these information mechanisms and domestic courts as an enforcement mechanism. These mechanisms are less compatible than much of the literature implicitly assumes.

In the legal context, information consequentially becomes subject to the laws of evidence. Much of the information we may have about violations may be inadmissible in courts. As a result, courts have a different set of information before them than can be used outside the judicial process for "information politics" (Keck and Sikkink 1998). Courts' effectiveness as enforcers of human rights law is systematically affected by the availability of legal evidence, and that availability varies by the type of human rights vio-

lation. Courts are unlikely to be effective enforcers in areas where evidence of violations is especially costly to obtain–even if actors outside the judicial process have significant amounts of information about such violations. In addition, even when a certain amount of admissible evidence can be produced, it may not be sufficient to meet the applicable standard of proof in order to secure a prosecution. The ability of courts to overcome this information asymmetry therefore depends on the cost of producing admissible evidence of violations and on the standard of proof. When evidence-production costs and the standard of proof are low, violators of law are likely to be prosecuted and therefore the prospect of domestic judicial enforcement constrains government actors. When these costs and standards are high, however, violators are not likely to be prosecuted, so potential prosecution by domestic courts will provide little incentive for governments to conform their actions to their international legal commitments.

Applying this argument to the human rights context, I argue that evidence-production costs and standards of proof are high with respect to violations of personal integrity rights, such as torture, extrajudicial killings, political imprisonment and disappearances. Thus, I expect that courts will not be able to constrain governments into reducing these violations, including governments that sign an international commitment to do so. By contrast, both evidence-production costs and standards of proof are relatively low with respect to violations of other civil rights, especially the freedoms of speech, association, assembly and religion. I therefore expect that domestic courts can enforce respect for these rights and, as a result, that commitments to human rights treaties result in improvements in government practices in these areas. I test this theory by analyzing the effects of ratification of the ICCPR on respect for several human rights from 1981 to 2007. To address governments' self-selection into the ICCPR, I use a propensity-score matching technique designed to allow us confidence in our inference regarding this relationship. I find that ratification of the ICCPR has improved government respect for the freedoms of speech, association, assembly and religion, but has not reduced the extent to which governments use torture, extrajudicial killings, political imprisonment and disappearances.

This paper makes several contributions to our understanding of the effects of international institutions. I provide a theory that explains which types of international

commitments are and are not likely to be enforceable by independent domestic judiciaries. Underlying this theory is the notion that these mechanisms depend not only on the types of legal institutions involved, but also on the ways in which different types of legal violations create different information problems. A key insight of this argument is that, in the judicial context, information is highly constrained by the laws of evidence and, therefore, that the information-producing mechanisms analyzed in the literature on human rights may not produce information that can be used by courts as legal enforcers. While both monitoring and enforcement have been thoroughly analyzed in the human rights context, my argument is that while the information-gathering functions of NGOs and the media are crucial for some enforcement mechanisms, we should not assume that the same set of information can be used in the judicial enforcement mechanism. Indeed, information brought before the courts is systematically different. Finally, my analysis provides new empirical results regarding the effects of ICCPR ratification on several areas of human rights. I use disaggregated measures of individual human rights practices in order to examine the specific areas in which the treaty has affected government policy and where it has not. In contrast to several prior studies, I find that ICCPR ratification has not significantly affected government practices with respect to personal integrity rights. My results also include novel findings that the ICCPR has improved respect for several other rights, including the freedoms of speech and association.

2.2 The Effects of International Commitments

States commit to international agreements for various reasons. At times, states ratify treaties that require them to make little or no changes relative to the status quo. Other commitments may simply be forms of cheap talk, especially when compliance is weakly monitored or enforced (Downs, Rocke and Barsoom 1996). In such situations, we can expect that commitment to international law will not significantly change government policy. Yet scholars argue that, on many occasions, governments commit to treaties in order to constrain their behavior and achieve outcomes that may not have occurred otherwise (Moravcsik 2000). The debate regarding the effects of international law has been pushed forward in recent years along at least two fronts. The first is a

line of research that has produced important empirical tests of existing theories. Secondly, scholars have refined existing theory in order to improve our understanding of the mechanisms by which governments can constrain themselves by making international commitments.

The empirical research on the effects of commitments to international law has produced mixed results. I focus this discussion on research that examines the effect of ICCPR ratification, although the foregoing also applies to research on other human rights treaties (Hathaway 2002) as well as other types of international agreements (Mitchell 1994; von Stein 2005). In the first cross-national study of its kind, Keith (1999) found that ICCPR member-states were no more (or less) likely to respect the human rights of their citizens, when controlling for other factors known to affect this behavior. Likewise, Hathaway (2002) finds no significant statistical relationship between ICCPR ratification and respect for trial rights and civil liberties. Using a different model specification, Hafner-Burton and Tsutsui (2005) find that ICCPR members are somewhat more likely than non-members to repress their citizens, especially in terms of personal integrity rights violations. The findings in these papers have been updated by recent efforts that use more sophisticated methods to address treaty commitment selection effects. First, Neumayer (2005) uses Heckman selection models and finds that ICCPR members are more likely to violate the personal integrity rights and civil rights of their citizens, using as dependent variables the pooled measures of both sets of rights provided by the Political Terror Scale and Freedom House, respectively. Second, Simmons (2009) uses an instrumental variables model and finds that ICCPR members are more likely than non-members to abolish the death penalty and respect religious freedom, but not to respect trial rights. Most recently, Hill (2010) matched ICCPR members to non-members based on several observable predictors of ICCPR ratification. He finds that ICCPR members become more likely to violate their citizens' personal integrity rights, as measured by the index of Physical Integrity Rights provided by the Cingranelli-Richards Human Rights Data Project (2009) (CIRI).

Several mechanisms work to improve the human rights practices of governments, including normative persuasion and political pressure (Keck and Sikkink 1998; Lutz and Sikkink 2000; Hafner-Burton 2008). Yet the success of these types of mech-

anisms need not be contingent on a government having made an international legal commitment to uphold respect for human rights. By contrast, the key mechanism by which such commitments can constrain governments and result in improvement in human rights practices is their incorporation into domestic law (Hathaway 2007; Powell and Staton 2009). A key to the enforcement of international law upon its incorporation into domestic law is an actor willing and able to perform this function, most often the judiciary. Particularly in the human rights literature, researchers have argued that respect for human rights is better in countries with domestic judicial systems that are sufficiently autonomous from other branches of government to allow them to check executive and legislative power (Cross 1999; Keith 2002*b*; Apodaca 2004; Keith, Tate and Poe 2009). Both the Universal Declaration of Human Rights and the ICCPR note that an independent judiciary is crucial to maintaining respect for human rights. Others argue that it is not the autonomy of the domestic judiciary that is crucial in this context, but its effectiveness. Powell and Staton (2009) argue that judicial effectiveness is "not only a function of the power of courts to set limits on state behavior, but also of the government's expectations over whether victims of repression will seek legal redress" (p. 151). Thus, while there is an important debate regarding whether the crucial characteristic of the judiciary is its autonomy or its effectiveness (Staton and Moore 2011), there seems to be a consensus that the domestic judiciary is essential to the enforcement of international agreements.

2.3 Domestic Courts and International Commitments

The key to the arguments above is that domestic courts can perform an important enforcement function depending on institutional characteristics and powers. Simmons (2009), for example, has recently argued that the effectiveness of domestic litigation as a treaty enforcement mechanism depends on judicial independence. Building on her work and that of others, this paper argues that the effectiveness of this mechanism depends on much more. The literature on international human rights institutions has analyzed enforcement by domestic courts and the production of information about violations, but has insufficiently linked these two mechanisms. A key contribution of this paper is to

provide this link and demonstrate how it systematically affects when courts can perform their enforcement functions.

When violators are less likely to be punished through domestic judicial action, the power of the courts to bring governments into line with international law decreases. The law-and-economics literature often adopts a statistical terminology and refers to occasions on which a violator of law is not prosecuted as type II errors (Png 1986; Cooter and Rubinfeld 1989). A higher likelihood of type II error creates a lower incentive for actors to obey the law because there is a lower probability that they would face a penalty for not doing so (Polinsky and Shavell 1989). Adopting this terminology, this section sets forth an argument regarding when the probability of type II error is lower and, therefore, domestic courts are more likely to be effective enforcers of international legal commitments.

Information is crucial to the reduction of type II error. Enforcement mechanisms depend on effective monitoring mechanisms. Violators cannot be punished unless their transgressions are observed. While courts have strong enforcement powers, they have relatively weak monitoring powers. "Judicial enforcement is costly and imperfect," argue Sanchirico and Triantis (2007, p. 72), "largely because of limits on the court's ability to detect facts accurately." As U.S. Supreme Court Justice Brennan wrote in Speiser v. Randall, "There is always a margin of error in fact-finding" (357 U.S. 513, 525-26).

Courts face important information asymmetries with respect to enforcing international commitments on the other branches of government. Legislatures and executives often violate legal commitments and have an incentive to keep these violations hidden, especially if they expect judicial prosecution. When guilty parties make greater efforts to prevent prosecution, including by hiding information, the probability of type II error increases (Rubinfeld and Sappington 1987). Unlike legislatures and other institutions, courts generally have little power to directly monitor other actors. In the language of McCubbins and Schwartz (1984), courts cannot use police patrol monitoring mechanisms, but instead must rely on fire alarms. That is, courts depend on other actors to bring information to them regarding alleged violations of international commitments. Overcoming information asymmetries through fire alarm mechanisms is

therefore a crucial component of effective enforcement of international commitments by domestic courts.

Information takes on a specific meaning in the judicial context and is subject to rules governing its acquisition, authentication and admissibility that do not apply in other contexts: the laws of evidence (Bentham 1825; Wigmore 1981). Thus, "court action is a function not of what can be observed by the court but what evidence is actually presented to the court by the ... parties" (Bull and Watson 2004, p. 1). The laws of evidence have two important consequences for the issues addressed in this paper. The first is that, in order to prosecute a violator, the court must have information that is admissible as evidence. Not all of the information (as the term is more broadly used in the international relations literature) that actors may possess regarding violations of international law will be admissible in court. While jurisdictions vary greatly in terms of the rules of evidentiary admissibility, they all have such rules, and there are many similarities among them (Damaska 1973, 1992). Secondly, the amount of evidence that is sufficient to prove an allegation in court varies greatly depending on the type of claim. In legal terms, this is known as the standard of proof. The purpose of a standard of proof is to instruct the fact-finding actor (such as a jury) about the level of confidence it must have in its verdict.

As a result, courts' ability to overcome information asymmetries and enforce international law depends on two factors: the cost of producing admissible evidence and the standard of proof. In some contexts, it is relatively less costly for the parties to locate relevant information that is legally admissible as evidence, while in other contexts doing so will entail significantly greater costs. To be clear, I refer here not to the cost of producing information about violations in the general sense, but specifically to the cost of producing the type of information that is likely to be admissible in court. This is especially pertinent because, in many cases, parties have powerful incentives to hide and tamper with evidence (Sanchirico 2004, 2006), thus raising the cost of discovery. The cost of producing legally admissible evidence is a key determinant of litigants' probability of winning at trial (Tullock 1980; Cooter and Rubinfeld 1989). Higher evidence-production costs generally result in a lower probability of finding against a guilty or responsible party, which increases the probability of type II error (Cooter and Rubinfeld

1989). Because the standard of proof determines the amount of evidence that must be produced at trial, its effect is analogous to that of evidence-production costs. Higher standards of proof require greater amounts of evidence, thus making prosecution more costly and increasing the probability of type II error (Rubinfeld and Sappington 1987).

The foregoing has the following implications for domestic judicial enforcement of international law. When evidence-production costs and standards of proof are relatively low, courts are likely to have sufficient evidence brought before them to overcome information asymmetries and effectively hold other branches of government responsible for breaches of international legal commitments that have been incorporated into domestic law. If rights are violated, victims will be more likely to be able to litigate claims for such violations, resulting in penalties against governments and their agents. We should expect governments to strategically anticipate such prosecutions and therefore become less likely to violate international commitments, so we may not observe such prosecutions. [1] In turn, this means that, under such conditions, commitment to international agreements will have a constraining effect on governments, causing them to change their behavior toward meeting their international legal obligations.

By contrast, in areas where evidence-production costs and standards of proof are high, even courts that are otherwise capable of enforcing international law will not have sufficient evidence before them to allow them to prosecute violators. While violations may be common, evidence of such violations will be scarce, so victims will not be able to bring forward effective claims against the governments. Courts – even highly independent courts – will have few opportunities to rule on such cases and impose penalties on governments and their agents. Knowing this, when governments commit to international agreements in such areas, they will not be constrained by the prospect of future domestic prosecution for violations. As a result, under such conditions, domestic judicial enforcement will not cause governments to conform their behavior to international law.

[1] This is why, generally speaking, levels of prosecutions for violations of law are not good measures of judicial constraints. When actors know that type II errors are less likely, they adjust their behavior accordingly and become less likely to violate the law. In such a scenario, we are unlikely to observe many prosecutions. By contrast, when the probability of type II error is high, actors will violate the law but not be prosecuted. As a result, the two scenarios are observationally equivalent in terms of prosecutions. This observational equivalence explains why statistics on judicial behavior are thought to be weak indicators of the extent to which courts constrain other actors (Johnson, McMillan and Woodruff 2002).

2.4 Domestic Prosecutions of Human Rights: Information as Evidence

The previous section set forth a general argument regarding the conditions under which we can expect domestic courts to be effective enforcers of international commitments. This section applies that argument to international human rights law. It provides an argument regarding which types of human rights violations have low versus high evidence-production costs and standards of proof. I follow Davenport (2007*c*) and others in separating human rights violations into the categories of personal integrity rights and other civil rights and liberties. Personal integrity rights violations include extrajudicial killings and other deprivations of the right to one's life, torture and other inhuman treatment, political imprisonment and forced disappearances. Perhaps because these are often regarded as the worst violations of human rights, they have received the most attention in the academic literature. The other civil rights and liberties I analyze are nonetheless regarded as crucial elements of the international human rights regime. These include the freedom to practice one's religion as well as the freedoms of speech, assembly and association.

2.4.1 Evidence-Production Costs

The processes of producing information about human rights violations have been analyzed by several important studies (Keck and Sikkink 1998; Lutz and Sikkink 2000; Hafner-Burton 2008). In the context of human rights, the analysis of information has focused on the roles of NGOs and the media, as well as the ways in which international institutions diffuse information (Hafner-Burton 2012). Yet these analyses do not link such information to the judicial process. In certain areas, it is significantly more costly to produce legally admissible evidence that can be used in a court of law than to produce information that can be used in "the court of public opinion." We therefore must think about information costs differently when political and normative processes are the mechanisms for improving human rights practices than when the judicial system is the enforcement mechanism. This section examines different areas of human rights to determine where evidence-production costs are relatively high.

Evidence-production costs are high with respect to personal integrity rights violations and low with respect to violations of other civil rights. The first reason for this stems from the availability of the victims. Dai (2007) argues that, in policy areas where the interests of states and their citizens are not aligned, but victims of violations are available as low-cost monitors, we can expect monitoring of international commitments to be conducted by both NGOs and victims. Thus, "human rights regimes rely most critically on the detection of noncompliance by victims and the communication of noncompliance by NGOs" (Dai 2007, p. 60-61). Yet the availability of victims to monitor violations and report them to others varies greatly along with the type of violation. If a victim is either dead or in government custody, this type of monitoring will not facilitate enforcement. In such areas, victims will not perform a meaningful monitoring function, and this responsibility will instead fall to NGOs and other actors.

When personal integrity rights violations occur, the government is often either in possession of the victim for a significant amount of time, the victim is dead, or the victim is too fearful of reprisal to report the violation. The victim is therefore both less likely to bring a case forward and less likely to be in a position to testify or otherwise provide evidence of the abuses. Amnesty International reported in 1977, for example, that Argentine government agents had intimidated and detained individuals who sought to testify about government abuses.[2] In some situations, the victim's family may report a suspected violation, and this can result in an investigation by NGOs or other actors. When the victim is missing, however, the process of confirming the violation can take years (and often requires a regime transition). As Lutz and Sikkink (2000) argue, "Disappearances often are difficult to prove because the accuser must show that the victim was deprived of his or her freedom by government agents notwithstanding government claims to the contrary" (p. 635). This problem is especially acute in the judicial setting because the government can prevent accusers from obtaining the evidence needed to support such claims. In Argentina, for example, many of the names of the victims of the military junta from 1976 to 1983 were not confirmed until years later, and many alleged victims remain unaccounted for. One of the junta's most famous victims was Dagmar Hagelin, a Swedish citizen who was mistakenly shot by an Argentine government death

[2] Amnesty International. 1977. "Report of an Amnesty International Mission to Argentina, 6-15 November 1976."

squad in 1977. She was subsequently imprisoned, and many NGOs inquired as to her status, but the government always denied having her in custody. Her body was never found, and no one has ever been prosecuted for her death (Simpson and Bennett 1985). The case illustrates the key difference between information generally and evidence: the facts may be "known", but nonetheless not be legally provable.

By contrast, when other civil rights are violated, the victims generally remain free and physically unharmed, and are thus more likely to be able to bring their cases to court and to testify as witnesses. This is largely inherent in the nature of these types of repressive activities. Violations of rights other than personal integrity rights, by definition, do not involve physical harm or government custody of the victims. When a government shuts down a newspaper in violation of free speech rights guaranteed under law, agents of the newspaper are physically unimpeded from taking a case to court and testifying as to the events. This type of situation is also significantly less likely to result in plaintiffs being too intimidated by the possibility of additional government abuses to come forward. Being told that one cannot enter one's place of worship can be emotionally jarring, for example, but the psychological impact of this is likely to be far less damaging than that of prolonged detainment or torture.

The second reason for my argument is that the government is generally in a better position to hide evidence of personal integrity rights violations. This, too, stems from the nature of the violations. Personal integrity rights violations often (but not always) occur in situations in which the government has control of the victim and the surrounding evidence, making it less costly for the government to destroy evidence. This can include not only killing the victim and hiding the body, as in cases of disappearances, but also destroying other physical and documentary evidence, including the facilities in which the violations occurred. Very little documentary evidence has been produced, for example, regarding the atrocities committed by the Khmer Rouge in Cambodia, because the regime was able to destroy it (Ratner and Abrams 2001). Victims of such violations are often stripped of identification and identifying physical features, thus complicating the ability to use bodies as evidence. This occurred in Guatemala in the 1970s and 1980s, where only 127 of 3171 unidentified bodies had been identified as of 2008 (Snow et al. 2008). Even evidence of torture can be hidden: "The goal of 'clean' techniques is

plausible deniability by state executives. One cannot plausibly deny the use of scarring techniques in judicial proceedings. 'Clean' techniques, on the other hand, permit state agents to shift debate about their treatment of prisoners from blatant lying to a 'he said, she said' context in which uncertainty exists" (Conrad and Moore 2010) (p. 461).

Where the government is not in possession of the primary evidence, however, the costs of producing evidence of violations will be low. This is much more likely to be the case in violations of rights other than personal integrity rights. Such violations involve government prohibitions on the rights of individuals to exercise freedoms that are often exercised in public or semi-public places. The right to religious freedom, for example, as defined in Article 18 of the ICCPR, explicitly applies "individually or in community with others and in public or private." Because these rights are often exercised in public, their violation is less likely to be conducted in secret. This means that not only are victims more likely to go to court, but they are also likely to be able to produce physical and documentary evidence of the violation. For example, when government agents illegally break up rallies or protests in violation of the right to assembly, victims are often able to produce photographic and video evidence of the events.

The third reason evidence-production costs are higher with respect to personal integrity rights is that, in general, such violations target fewer victims than do violations of other civil rights. In part, this is because the costs of executing personal integrity rights violations are higher than violations of other rights. Personal integrity rights violations typically require the government to hire agents and provide them with resources and facilities, often for the long term. Torture, for example, is both a capital- and labor-intensive process. Fewer victims means that, all else equal, there are fewer potential plaintiffs to bring lawsuits and fewer potential witnesses to testify in those suits. Violations of other civil rights tend to be less resource-intensive, however, on a victim-by-victim basis. This is largely because violation of these rights often does not involve direct or prolonged contact between the government's agents and the victims. A single church can be shut down using a squadron of armed police, for example, denying hundreds the right to freely practice their religion. Shutting down websites can deny the right of free speech to many individuals and organizations, yet often requires relatively few government resources.

This argument may seem counterintuitive given the depth of NGO activities and media coverage of human rights, much of which is focused on abuses of personal integrity rights because they are the most egregious violations. Indeed, the media's preference for reporting on violent incidents has resulted in the common saying that "If it bleeds, it leads." NGOs collect important information about abuses and participate in the mobilization and education of the public (Keck and Sikkink 1998; Davenport 2007*a*). As a result, we often have vast amounts of information about personal integrity rights violations, potentially more so than with respect to other human rights violations.

Yet the information needed for what Keck and Sikkink (1998) call "information politics" differs significantly from that needed for purposes of judicial evidence. For example, NGOs often receive reports from victims' families that their relatives have been kidnapped and use this information to mobilize pressure against governments. But, as the Hagelin case demonstrates, this information often cannot be used to legally prove an offense, particularly when the government hides the relevant evidence. Lawyers have often criticized NGOs for their fact-finding practices (Franck and Fairley 1980; Weissbrodt and McCarthy 1982; Blitt 2004). Much of the information NGOs use does not constitute direct evidence of violations. Winston (2001) argues that "[Amnesty International] does not get into the business of 'naming names' of suspected perpetrators for this reason" (p.37). NGOs "often need to rely upon hearsay statements, documents which are not fully authenticated, and justifiable inference from indirect evidence" (Weissbrodt and McCarthy 1982, p. 203). While many NGOs have developed policies in order to avoid relying excessively on hearsay, they do rely substantially on interviews of witnesses who are neither under an obligation to speak with the NGO nor under an obligation to tell the truth (Orentlicher 1990). Recognizing this, many NGOs have adopted sampling techniques to help gauge the extent of abuses (Orentlicher 1990). Yet while these tools are helpful in creating awareness of violations in the general sense, they are unlikely to be useful as evidence of specific violations of individuals' rights. Unfortunately, the physical evidence needed to corroborate these eyewitness reports is unavailable in many situations, especially those involving personal integrity rights violations (Orentlicher 1990).

Media reports similarly rely on information that is inadmissible as evidence.

While the media certainly have standards regarding authentication and corroboration, many reports are published based on information obtained from anonymous or second-hand sources. This information is unlikely to be useful as legal evidence. Individuals too fearful to be named in a newspaper article, for example, are unlikely to be willing to provide the same information in a legal proceeding. Because personal integrity rights violations often occur in secret, media reports are unlikely to be based on direct evidence of such abuses. As a result, as Ratner and Abrams (2001) argue, "Evidence normally gathered by journalists, academics, and NGOs for historical or reporting purposes is typically different from that needed before ... courts" (p. 256).

2.4.2 Standards of Proof

Standards of proof can be thought of as varying along two dimensions: the type of legal system and the type of suit. Along the first dimension, individual states may impose higher standards of proof for all types of claims than other states. Nonetheless, the argument I make here is that in all types of legal systems, claims of personal integrity rights violations face higher standards of proof than claims of violations of other civil rights. The key reason for this is that personal integrity rights violations tend to be criminal offenses, while violations of other civil rights that do not involve physical harm tend to be civil offenses, although the latter could also be criminal offenses. To my knowledge, no study has compiled a comprehensive list of standards of proof by country and by subject matter. In addition, it would likely be extremely difficult to quantify such standards for the purpose of conducting a statistical analysis. Nonetheless, the literature on the comparative law of evidence indicates that my argument is accurate. In all jurisdictions I am aware of, the standard of proof for criminal claims (and, thus, personal integrity rights violations) is higher than the standard of proof for civil claims (and, thus, other civil rights), albeit the magnitude of this gap likely varies.

The following are examples of the differences between the standards of proof for criminal and civil in two of the world's most common legal systems: the common law and civil law systems. In common law jurisdictions, criminal charges must usually be proved "beyond a reasonable doubt," the highest standard of proof (Wigmore 1981). This standard has sometimes been quantified as requiring a roughly 90% or 95% cer-

tainty in the verdict (Simon and Mahan 1971; McCauliff 1982).[3] By contrast, standards of proof for civil cases are relatively low. In the United States, this standard is usually "the preponderance of the evidence," which is generally taken to mean a greater than 50% probability of the correct verdict (Simon and Mahan 1971; McCauliff 1982). As the court in Livanovitch v. Livanovitch famously wrote, "A bare preponderance is sufficient, though the scales drop but a feather's weight." (99 Vt. 327, 131 A. 799, 1926). Other jurisdictions, such as the UK, the British Commonwealth and Scandinavia, refer to this standard as the "balance of probabilities" and interpret it similarly (Wright 2009). In civil law jurisdictions, the question of whether criminal cases have different standards of proof than civil questions is somewhat more complex. The former conventional wisdom was that similar rules applied to both cases, a conclusion largely reached based on the traditional structure of the French legal system. Yet modern analyses have determined that this is no longer the case. Many civil law jurisdictions (including France) have instituted reforms that place less stringent standards on civil offenses and less serious crimes (Damaska 1973). As a result, while the official standards of proof may be similar in both civil and criminal cases, other rules of evidence make up for this similarity such that the difficulty of proving a civil case in a civil law country is similar to the difficulty of doing so in a common law country (Wright 2009; Engel 2009; Taruffo 2003). Thus, in both common law and civil law countries, the standard of proof for criminal cases is higher than that for civil cases.

In summary, both evidence-production costs and the standard of proof for violations of personal integrity rights are high. Not only is it difficult to obtain information regarding such violations that can be used as legal evidence, but quite often the amount of evidence required for a conviction is large. By contrast, with respect to violations of other civil rights, including the freedoms of speech, association, assembly and religion, evidence-production costs are low and standards of proof are often also lower. This means the probability of type II error is large with respect to personal integrity rights violations, but smaller with respect to other civil rights. Governments that commit to human rights treaties will be likely to face domestic prosecution for violations of the

[3]Quantifying standards of proof is notoriously difficult, and attempts to quantify individual standards have been met with controversy among judges and legal scholars (Tillers and Gottfried 2006; Weinstein and Dewsbury 2006; Newman 2006).

freedoms of speech, association, assembly and religion. If they violate these rights, victims are more likely to e able to successfully litigate claims in domestic courts and impose penalties on governments. Anticipating this, governments that commit to international human rights agreements, will be more likely to improve their respect for these rights. With respect to personal integrity rights, however, this should not be the case. Governments that violate these rights will be unlikely to face prosecution by domestic courts, even if those courts are institutionally independent. Victims of such violations will face high barriers to producing the evidence needed to convict leaders and their agents in domestic courts, and will therefore be unlikely to do so successfully. As a result, when governments commit to international human rights agreements that protect these rights, they will anticipate that they can continue to violate such rights with a low likelihood of prosecution in domestic courts.

This argument leads to the following hypotheses:

> Hypothesis 1: Ratification of applicable international human rights agreements improves government respect for the freedoms of speech, association, assembly and religion.
>
> Hypothesis 2: Ratification of applicable international human rights agreements does not reduce government use of illegal killings, torture, political imprisonment or disappearances.

2.5 Research Design

2.5.1 The ICCPR

My research design focuses on the effects of ICCPR ratification on state human rights practices. Adopted in 1966 and entered into force in 1976, the ICCPR has since been ratified by 167 countries (as of 2010). Unlike many multilateral human rights treaties that have been adopted more recently, the ICCPR covers a broad range of rights. These include the key personal integrity rights discussed in this paper. Article 6 protects individuals' right to life and thus prohibits extrajudicial killings by governments. Likewise, Article 7 prohibits torture and cruel, inhuman or degrading punishment. Article 9 provides that individuals may not be arbitrarily arrested or detained. This, together with

additional prohibitions on the infringement of political rights, is often deemed a prohibition on political imprisonment and other detentions in violation of due process. The ICCPR does not explicitly address forced disappearances, most likely because the term was not used in common parlance until the abuses of the South American regimes of the 1970s became well-known. Yet the elements of a forced disappearance, most importantly arbitrary arrest and summary execution, are explicitly prohibited by the ICCPR. The ICCPR also prohibits governments from infringing on a broad set of additional civil and political rights. Among these are freedoms of speech and expression (Article 19), assembly and association (Articles 21 and 22), and the practice of religion (Articles 18). Importantly, Article 2 requires members to adopt domestic laws, including legislation as necessary, to "give effect to the rights" enumerated in the treaty.

From a research design perspective, focusing on the ICCPR has several advantages in this context. First, this allows me to test the effects of ratification of a single treaty on different dimensions of government human rights practices. This has the advantage of allowing for a direct comparison of treaty commitment effects while minimizing the extent to which findings may be caused by differences in treaty design. In addition, relying on a single treaty allows me to use the same set of units of observation to test all of my hypotheses. As a result, the only difference between the various regression models reported below is the dependent variable, which allows for clean comparisons between the results. Finally, empirical findings regarding the effects of ICCPR membership have produced mixed results, as discussed below. The extent to which existing work allows for causal inference is debatable, and much of the work has used pooled measures of either personal integrity rights and/or other civil rights. The use of methods designed for causal inference regarding the effects of ICCPR ratification on specific measures of human rights practices may shed light on an important empirical puzzle.

2.5.2 Addressing Treaty Commitment Selection Effects

Estimating the effects of treaty commitments is known to be a complex proposition. Governments select the treaties they join in part based on their interests and the extent to which they expect to conform their behavior to the treaties' requirements

(Downs, Rocke and Barsoom 1996). As a result, if we model an outcome on treaty commitments without addressing this problem, we could at best say that treaty members are more likely to experience that outcome, but not that this is a causal relationship. A high rate of treaty compliance among treaty members, for example, may simply mean that states that are more likely to comply are also more likely to join.

Scholars have recently begun taking the treaty commitment selection effect seriously and have employed several methods to address it. Among these are Heckman-based modeling (von Stein 2005; Neumayer 2005) and instrumental variables regression (Simmons 2009). Yet both of these methods are highly sensitive to specification and may not be optimal in this context. To produce reliable estimates, both Heckman (Sartori 2003) and instrumental variables (Heckman 1997; Pearl 2000) models require the specification of a variable that is associated with treaty commitment but not with the outcome policy. Yet, because both of these stages are ultimately decisions of government policy, it is particularly difficult to identify factors that contribute to the decision to join a human rights treaty but not to the decision to repress the human rights of citizens (Hill 2010).

The propensity-score matching approach proposed by Simmons and Hopkins (2005) to address this problem is particularly promising. The first step in this approach is to identify the set of factors that predict treaty commitment. The next step is to match treaty members to treaty non-members based on these underlying factors. The result is a sample that is balanced on the probability of treaty commitment. With respect to this sample, we can think of selection as having been randomly assigned (Ho et al. 2007). The sample can then be subjected to further tests, including simple t-tests and multiple regression, to estimate the effects of treaty commitment. Among the advantages of this approach are that it does not require the analyst to make distributional assumptions nor to specify a factor associated with treaty commitment but not with the outcome policy. More generally, matching has been shown to be an effective tool for creating balanced samples when treatment is not randomly assigned.

A significant threat to inference using this approach is the potential that unobservable (or unmeasured) factors affect treaty commitment decisions and are not included in the matching model (Simmons and Hopkins 2005). The estimation of the

treaty commitment effect is highly sensitive to the propensity score estimates (Rubin 1997), and the choice of underlying variables significantly affects the reliability of propensity score analysis (Heckman, Smith and Clements 1997; Heckman et al. 1998; Lechner 2000; Smith and Todd 2005).

Chapter 1 argues that the key factor that determines treaty commitment decisions – one that is difficult to observe directly – is a state's preference for treaty commitments, i.e., which types of treaties it tends to prefer joining. I propose a methodology to directly estimate these preferences in order to calculate the probability of states committing to specific treaties. This methodology relies on estimating the ideal points of states with respect to universal treaties using the W-NOMINATE algorithm (Poole and Rosenthal 1997), which has traditionally been applied to legislative roll-call voting but has also been used to estimate state preferences (Voeten 2000). In this model, the options of committing and not committing to a treaty are represented by points in an n-dimensional policy space. Each state decides whether or not to commit to a treaty by, among other factors, weighing the distance between these points and its ideal point in this space. The closer a state is to a treaty, the more likely it is to join the treaty (Simmons 2009). Thus, the probability of a particular state ratifying a particular treaty is calculated based on the distance between the state and the treaty in the preference space. Using Monte Carlo simulations, Chapter 1 compares this methodology to a more traditional model (such as those used by Hill (2010)) that calculates treaty commitment probabilities based on more easily observable predictors. He shows that the ideal point model out-performs the observables-based model when significant unobservable (or unmeasured) factors influence treaty commitment decisions, a likely scenario in this context of highly complex decision-making. Ideal point estimation performs better in this context because it is designed to reveal the latent preferences that affect decision-making, regardless of whether the underlying reasons for these decisions are observable or unobservable.

I adopt this methodology to estimate the effects of ICCPR ratification. My research design proceeds in three stages. First I use the W-NOMINATE algorithm on a data set of membership in approximately 300 universal treaties.[4] The results provide

[4]This data set includes all of the universal treaties included in the United Nations Treaty Collection (UNTC). The data include various types of instruments, including protocols and amendments to treaties, all of which are considered separate treaties for purposes of this analysis. The data are coded "1" for

annual estimates of each country's probability of ratifying the ICCPR. These estimates begin in 1976, the first year in which the ICCPR was in force, and continue to 2007. In the second stage, I match treaty members to non-members based on these probabilities using the nearest-neighbor algorithm provided by the MatchIt package in the R programming language (Ho et al. 2009). Table 2.1 sets forth the results of the matching stage. In the full sample, there is a large imbalance between treaty members and non-members in terms of their probabilities of joining the ICCPR. Not surprisingly, ICCPR members are much more likely to have joined the treaty ex ante. After matching, however, these probabilities are much more similar. The treatment group has a 64.7% probability of joining ICCPR, while the control group has a 65.4% probability of doing so. The matching procedure results in a 98.2% improvement in balance on the probability of assignment to treatment.

In a third stage, I use the matched sample to estimate the effects of ICCPR ratification on several dimensions of respect for human rights. As dependent variables, I use the measures provided by CIRI. While other measures of human rights practices are also commonly used in the literature, especially the Political Terror Scale (Gibney and Dalton 1996), the CIRI data are particularly suitable to testing my hypotheses because they disaggregate personal integrity rights violations into several types of violations and they provide data on many other areas of human rights. With respect to personal integrity rights, I use the CIRI measures of Extrajudicial Killings, Torture, Political Imprisonment and Disappearances. All four measures are coded as 0, 1 or 2 for each country-year. A score of 2 indicates that the applicable violation did not occur in that year, while a score of 0 indicates the violation was frequent. I also adopt the CIRI measures of Freedom of Assembly and Association, Freedom of Speech and Freedom of Religion. These measures are also coded as 0, 1 or 2 for each country-year. A score of 2 indicates the applicable freedom was not restricted in that year, while a score of 0 indicates it was severely restricted.

A perfectly balanced sample approximates random assignment to treatment, and therefore simple t-tests can be used on such samples in some contexts. My sample is not perfectly balanced with respect to the probability of assignment to treatment,

country-years that have ratified a treaty and "0" otherwise. A full list of these treaties is available from the author upon request.

and it is not completely balanced with respect to several additional factors that may influence human rights practices, as shown in Table 2.2. To correct for this remaining imbalance, I use multiple regression that controls for other factors that may influence human rights practices. As a measure of judicial independence, I adopt the data provided by CIRI (JUDICIAL INDEPENDENCE), which are coded as 0 for "not independent," 1 for "partially independent" and 2 for "generally independent." Importantly, there is no indication that the laws of evidence were taken into consideration when coding this measure. I include a measure of regime type using the Polity IV data (Marshall and Jaggers 2002) (POLITY) because democracies are more likely to respect human rights (Poe and Tate 1994; Davenport 1995, 1999; Poe, Tate and Keith 1999). Newer regimes and well-established regimes may have different tendencies to respect human rights, so I follow Hafner-Burton and Tsutsui (2007) by controlling for this factor using the Polity IV data (REGIME DURABILITY). Both foreign wars and civil wars may result in periods of increased repression (Poe and Tate 1994; Poe, Tate and Keith 1999). Civil wars, in particular, may result in periods of lawlessness during which even independent courts have a diminished capacity to constrain the other branches of governments. I control for this using data from the Correlates of War Project. As discussed above, NGOs play a key role in political mobilization against oppression and may succeed in improving government practices. I control for the number of international NGOs (INGOS) in a country using the data provided by Hafner-Burton and Tsutsui (2005). The level of economic development is a well-known predictor of human rights practices (Henderson 1991; Poe and Tate 1994; Poe, Tate and Keith 1999), and I control for this using a measure of per capita GDP provided by the World Bank. I use the natural log of this measure because this effect is likely nonlinear (Davenport 2007*a*). To address potential differences among states of different sizes and potential monitoring biases based on this factor, I follow much of the literature in including a control for the natural log of a state's population, using data provided by the World Bank. To address serial correlation, I include lags of the applicable dependent variable for year $t-1$. A Lagrange multiplier test indicates that additional lags are not necessary to address serial correlation.

Not surprisingly, there were many observations with missing data among these variables. Because the underlying reasons for the missingness of the data are likely non-

random, listwise deletion of these observations may result in biased inference (Little and Rubin 1987). I therefore follow Hill (2010) and others in imputing the missing values using the Amelia II Program (Honaker, King and Blackwell 2009). The data on ICCPR ratifications are for the years 1976 to 2007. However, the CIRI data begin in 1981. Rather than attempting to impute the values of the CIRI variables for all countries for the years 1976-1980, which would be subject to a particularly high degree of uncertainty, I begin my analysis in 1981.[5] To test Hypotheses 1 and 2, I estimated a series of ordered probit models using the CIRI human rights measures as dependent variables. In all models, I include fixed effects for the year of the observation and use standard errors that are robust toward arbitrary heteroskedasticity and that take into account clustering by country.

2.6 Results and Discussion

Tables 2.3 and 2.4 report the results of these regression models. The results provide substantial support for the theory presented in this paper. Table 2.3 indicates that commitment to the ICCPR causes governments to improve their respect for the key civil rights of the freedoms of speech, association, assembly and religion. Violations of all of these rights have both relatively low standards of proof and evidence-productions costs. Figure 2.1 reports the marginal effects of ICCPR ratification on respect for civil rights, based on the models reported in Table 2.3. The effect sizes indicate that the impact of ICCPR membership is not only statistically significant, but also has a substantial impact on respect for these rights. For example, ICCPR members are about 39% more likely to provide an unrestricted right to free speech and 20% less likely to severely restrict that right.

By contrast, when violations of human rights have relatively high evidence-production costs and standards of proof, ratification of ICCPR does not appear to significantly impact government respect for rights. As Table 2.4 indicates, ICCPR ratification does not seem to improve government respect for personal integrity rights. These results

[5] I conducted the imputation procedure using the full set of 4368 country-years from 1981 to 2007 (rather than the matched sample of 1966) because including the full data set allows for more accurate imputation.

are starkly different from those reported in Table 2.3, which include the same sample, controls and estimation method, but differ in that they assess the effects of ICCPR ratifications with respect to areas in which victims of human rights violations are more likely to be able to litigate claims against the governments. Not surprisingly, governments of countries experiencing civil war are more likely to oppress their citizens, yet this is significantly more pronounced with respect to personal integrity rights. Such governments are significantly ($p<0.01$) more likely to abuse any of the personal integrity rights than any of the other civil rights.

These results also provide several new findings with respect to ICCPR ratifications. With respect to personal integrity rights, several previous studies have found a relationship between ICCPR ratification and an increase in personal integrity rights violations (Hafner-Burton and Tsutsui 2005; Neumayer 2005; Hill 2010). The methodology I have used in this study builds on their work and is designed to disentangle the selection effects in the treaty ratification process from the treatment effects of such ratifications. My results therefore suggest that prior findings that ICCPR ratification is associated with more violations may have been confounded by selection effects, whereas the treatment effects of such ratifications are not significant for most such rights. These results also provide new evidence regarding the treaty's effects on other important rights. With respect to the freedoms of speech and association, this study provides the first systematic evidence of the effects of ICCPR ratification, indicating that the treaty has substantially improved respect for these rights. My findings also confirm the results in Simmons (2009) that the ICCPR has led to improvements in respect for the freedom of religion.

My results also indicate that commitment to the ICCPR may lead to increases of disappearances (although this finding is significant at $p = 0.066$ so should be viewed as tentative). The argument presented in this paper may help explain this result. Governments often use various forms of repression to accomplish goals such as staying in power and weakening the opposition (Davenport 2007*a*). If the cost of using certain types of repressive techniques increases, governments may become more likely to use other, less costly options (Moore 2000; Poe 2004). My results provide empirical evidence that such substitution may occur. Because ICCPR commitment constrains governments'

ability to restrict the freedoms of speech, association, assembly and religion, they may turn to harsher methods, for which evidence is less costly to hide, to accomplish what they no longer can with less egregious human rights violations. Disappearances may be preferable to certain governments in such situations than other personal integrity rights violations because, as discussed above, evidence of such actions is often particularly difficult to produce. This is an argument I hope to explore in greater depth in future work.

To test the robustness of these results, I estimated several additional models. In the above model, I estimate the probability of commitment to the ICCPR using states' treaty commitment preferences (using W-NOMINATE). It may be the case, however, that these preferences do not capture the full model of treaty commitment. Other factors may affect the probability of ICCPR commitment, most importantly the factors that ultimately affect states' respect for human rights (Powell and Staton 2009). The benefit of including these in the estimate of propensity scores is a potential reduction in omitted variable bias. Including these factors, however, may also introduce new bias if they are measured with bias, if they do not actually affect the probability of treaty commitment, or if the functional form is mis-specified. In an alternate specification, I match states using a model that includes the probability of commitment calculated using W-NOMINATE as well as all of the controls listed in Table 2.3. I then run a set of ordered probit models on the matched sample that are identical to the models reported in Tables 2.3 and 2.4. The balance statistics of this sample are reported in Tables 2.5 and 2.6, and the regression results are reported in Tables 2.7 and 2.8. This alternate specification results in findings that also support my argument. The main difference between the results of this model and those reported above is that, in the former, ICCPR ratification does not have a significant relationship with disappearances.

In addition, I test the robustness of the results using alternative measures of judicial independence. This is a particularly difficult concept to measure, and no single data set has gained universal acceptance. In alternative specifications, I replace the CIRI measure with the judicial independence measure developed by Tate and Keith (2007) and the International Country Risk Guide (ICRG) measure of Law and Order. These alternate specifications yield similar results to those reported above. As the discussion

above suggests, the gap between the standards of proof for criminal and civil cases may be especially large in certain common law countries. To check whether the cross-national differences in the size of this gap between these two standards are driving my results, I conduct additional robustness tests in which I add an indicator variable for common law jurisdictions, and obtain similar results to those reported above. Other characteristics of individual legal systems may also affect the judicial process, so I also estimate additional robustness tests that include indicators for each of the world's five most common legal systems (using the common law as a baseline). These results do not substantially differ from those reported above. Finally, the results are also robust to an alternate specification that uses as its dependent variable the CIRI index of the four separate personal integrity rights measures.

To be clear, my results do not rely on the notion that independent domestic courts are the *only* mechanism for change in human rights practices, but instead that they are the key mechanism for doing so contingent upon commitment to international law. There is significant evidence that normative and political pressure on governments can promote improvements in human rights practices at least in certain situations (Keck and Sikkink 1998; Lutz and Sikkink 2000; Hafner-Burton 2008). I have included NGOs in my model to account for the important work they do in creating these types of pressure. One might argue that an alternative explanation for my results is that such pressure is not effective at reducing abuses of personal integrity rights but is effective at improving respect for other civil rights. Such an argument would be an effective alternative explanation if it could be shown that the effectiveness of such normative and political pressure is contingent upon commitment to international human rights law. If pressure on governments is more effective in some areas of human rights and not others, that alone would not explain why states that commit to the ICCPR become more likely to respect civil rights than ICCPR non-members. Yet it is not clear why the ability of activists, NGOs and others to use normative arguments to convince governments to respect human rights should be contingent upon commitment to international law. To make a normative argument against torture and other egregious abuses of human rights relies primarily on the notion that such practices are immoral, not merely that they are illegal.

There are, however, scholars who see a direct connection between law-based

and norm-based persuasion. Franck (1990), for example, argues that international law exercises a "normative pull" over states, causing them to alter their practices toward legal requirements independently of the prospect of judicial or other forms of enforcements. Relying on this theory, one might argue that my results demonstrate not that judicial enforcement varies between areas of human rights, but rather that the law's normative pull varies. Yet sustaining this argument requires us to believe that international law has a normative pull with respect to the freedoms of speech, association, assembly and religion, but not with respect to personal integrity rights. In other words, this would mean that the law does not have a normative pull with respect to what are generally regarded as the normatively worst abuses of human rights.

Finally, it could be argued that these results can be explained by the relationship between government leaders and their agents. Many human rights abuses are conducted by government agents, and this may especially be the case with respect to personal integrity rights violations (Conrad and Moore 2010). Thus, one might argue that the mechanism behind my results is that government leaders are often unable to exercise sufficient control over their agents to prevent such abuses. Under this argument, if leaders cannot prevent their agents from committing personal integrity rights abuses, commitment to international law would not reduce such abuses even if courts are just as powerful in constraining leaders' incentives to commit both types of abuses. While it is certainly likely that principal-agent problems of this type exist in this context, this argument overlooks the extent to which judicial enforcement should constrain both government agents and leaders. Government agents are generally just as liable for human rights abuses as are leaders. Not only does the "only following orders" defense famously not apply in human rights law, the scenarios envisioned by this argument involve agents acting on their own accord in violation of executive orders. When agents expect the probability of type II error to be low they should become less likely to violate the law. Thus, the theory presented in this paper should apply to both government agents and leaders.

2.7 Conclusions

Under certain conditions, domestic courts can perform important roles in the enforcement of international human rights law. Scholars have analyzed and debated which characteristics of judicial institutions are necessary in order for them to function in this manner. Yet the literature has not focused on whether certain violations are more likely than others to be deterred through potential domestic prosecution. The ways in which the domestic judiciary shapes the effects of international law on national governments depend not only on institutional characteristics, but are also contingent on the characteristics of the legal issues at stake. This paper has provided a framework that predicts when enforcement by domestic courts is likely to create a sufficient constraint on governments to prevent violations of their international obligations.

The extent to which domestic courts can enforce international obligations depends on their ability to overcome crucial information problems. While information problems have been analyzed in detail with respect to human rights, the theory developed in this paper explains how the laws of evidence affect those problems in the judicial context. I focus on two factors that affect the probability that violators will be prosecuted: the cost of producing evidence and the standard of proof. These factors are key determinants of the information problem facing the courts, and therefore the effects of independent judiciaries are contingent on them. When evidence-production costs and standards of proof are low, I argue, governments will be more likely to face prosecution and can therefore constrain themselves by making international law. By contrast, when evidence-production costs and standards of proof are high, even independent domestic courts will not be able to overcome their information problems and successfully enforce international legal commitments.

I apply this framework to human rights to determine where we can expect courts to help turn international commitments into meaningful domestic change. I argue that governments that commit to international human rights law will improve their practices with respect to many crucial civil rights, but not with respect to personal integrity rights. A key link that is missing from the human rights literature is that between the well-documented monitoring functions performed by NGOs and other actors and the use of information as evidence in court. While these actors are crucial in the human rights

information system, we should not assume that all of the information they produce will be useful in the judicial setting. Much of it is not. Indeed, for several reasons discussed above, legal admissible evidence of the worst types of abuses is often most difficult to obtain.

This argument and the empirical results that support it provide new insights into the debate of whether human rights agreements – and international institutions more generally – affect government policy. Unlike several recent studies, I find that ratification of the ICCPR has not significantly affected the rate at which governments abuse most of the personal integrity rights of their citizens. These are the violations for which legally admissible evidence is most costly to obtain and for which standards of proof are high in all legal systems. By contrast, ICCPR ratifications do improve governments' respect for fundamental freedoms when rights violations are relatively less costly to provide evidence of in court and when the standards of proof for such claims are relatively low. These include the rights to practice religion freely, the right to free speech and the right to free association, results that should be encouraging to those who argue that international legalization can improve respect for human rights.

My analysis also suggests several potential areas of future research. The first is an application of the underlying theory developed here to policy areas other than human rights. To the extent domestic courts play an important role in enforcing other international commitments, their ability to do so may also depend on factors similar to those I have pointed to in the human rights area. Second, while my discussion has focused on domestic courts, international courts likely face similar information problems. The literature on international courts has debated the extent to which they are independent from the interests of their member-states (Garrett and Weingast 1993; Alter 2008). Yet the theory developed here suggests that the extent to which these courts can constrain their member-states may be limited by additional factors that we have yet to explore in depth. Finally, my empirical results also suggest that ratification of the ICCPR may cause governments to increase their abuse of certain personal integrity rights, although this finding should be viewed as tentative. I have suggested that this may be the result of strategic substitution by governments that lose the option to repress their citizens in less egregious ways after joining the ICCPR. This may also be an indication that international

human rights law has become over-legalized (Helfer 2002), leading to consequences unintended by the actors that created it.

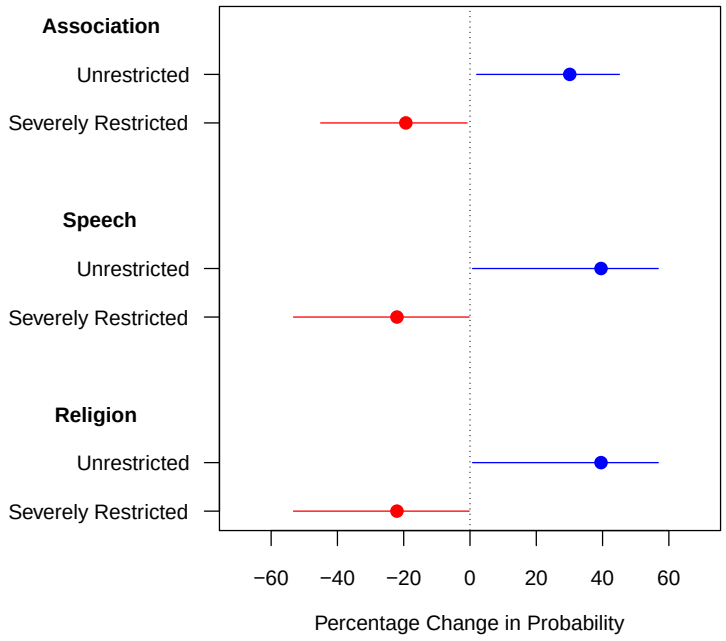


Figure 2.1: Marginal Effects of ICCPR Ratification on Civil Rights

Table 2.1: Balance Statistics

	Full Sample	Matched Sample
Total Sample Size	4368	1966
Treatment Units (ICCPR Members)	2947	983
Control Units (Non-ICCPR Members)	1421	983
Mean Pr(ICCPR Ratification) - Treatment Group	0.8819	0.6465
Mean Pr(ICCPR Ratification) - Control Group	0.4810	0.6536
Percentage Improvement in Balance	98.23%	

Table 2.2: Additional Balance Statistics

	Treatment Group Mean	Control Group Mean
Judicial Independence	1.03	1.09
Polity	0.17	-0.87
Regime Durability	16.53	24.46
Civil War	0.25	0.19
External War	0.03	0.04
GDP Per Capita (logged)	6.84	7.48
Population (logged)	15.54	15.88
INGOs	372.88	543.47
n	983	983

Table 2.3: Effects of ICCPR Ratification on Civil Rights

	(1) Freedom of Association	(2) Freedom of Speech	(3) Religious Freedom
ICCPR Ratification	0.171**	0.182**	0.157**
	(0.081)	(0.089)	(0.080)
Judicial Independence	0.160***	0.347***	0.278***
	(0.056)	(0.060)	(0.054)
Polity	0.074***	0.078***	0.041***
	(0.007)	(0.008)	(0.007)
Regime Durability	-0.001	0.002	0.001
	(0.002)	(0.002)	(0.002)
Civil War	-0.095	-0.172**	0.015
	(0.096)	(0.085)	(0.108)
External War	-0.043	-0.167	-0.005
	(0.154)	(0.177)	(0.176)
GDP Per Capita (logged)	-0.030	0.014	-0.112***
	(0.036)	(0.035)	(0.033)
Population (logged)	-0.103***	-0.011	-0.134***
	(0.032)	(0.032)	(0.034)
INGOs	0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)
Rights_{t-1}	1.309***	0.711***	1.019***
	(0.067)	(0.070)	(0.062)
Fixed Effects for Year	Yes	Yes	Yes
n	1966	1966	1966

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.4: Effects of ICCPR Ratification on Personal Integrity Rights

	(1) Killings	(2) Torture	(3) Imprison.	(4) Disapp.
ICCPR Ratification	-0.099	-0.032	0.087	-0.143*
	(0.080)	(0.074)	(0.080)	(0.078)
Judicial Independence	0.176***	0.213***	0.287***	0.273***
	(0.060)	(0.059)	(0.059)	(0.064)
Polity	0.002	0.018***	0.059***	-0.005
	(0.006)	(0.006)	(0.006)	(0.006)
Regime Durability	0.004*	0.003*	0.003	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Civil War	-0.843***	-0.643***	-0.574***	-0.882***
	(0.096)	(0.101)	(0.110)	(0.078)
External War	-0.358**	-0.314	-0.372*	0.024
	(0.182)	(0.222)	(0.207)	(0.174)
GDP Per Capita (logged)	0.111***	0.060*	0.034	0.035
	(0.037)	(0.033)	(0.036)	(0.037)
Population (logged)	-0.107***	-0.088***	-0.133***	-0.095***
	(0.028)	(0.027)	(0.037)	(0.028)
INGOs	-0.000	-0.000	-0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Rights_{t-1}	0.910***	0.965***	0.979***	0.877***
	(0.061)	(0.064)	(0.060)	(0.056)
Fixed Effects for Year	Yes	Yes	Yes	Yes
n	1966	1966	1966	1966

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.5: Robustness Test Balance Statistics

	Full Sample	Matched Sample
Sample Size	4368	1970
Treatment Units (ICCPR Members)	2947	985
Control Units (Non-ICCPR Members)	1421	985
Mean Pr(ICCPR) - Treatment Group	0.8819	0.6879
Mean Pr(ICCPR) - Control Group	0.4810	0.6605
Percentage Improvement in Balance	93.17%	

Table 2.6: Additional Robustness Test Balance Statistics

	Treatment Group Mean	Control Group Mean	% Improvement in Balance
Judicial Independence	1.11	1.06	62.54
Polity	0.09	-0.48	88.63
Regime Durability	23.89	23.93	94.37
Civil War	0.21	0.19	30.7
External War	0.03	0.03	79.89
GDP Per Capita (logged)	7.44	7.36	72.06
Population (logged)	15.88	15.86	94.72
INGOs	603.24	550.15	84.41
n	985	985	

Table 2.7: Robustness Test: Effects of ICCPR Ratification on Civil Rights

	Freedom of Association	Freedom of Speech	Religious Freedom
ICCPR Ratification	0.115**	0.137**	0.196**
	(0.047)	(0.057)	(0.090)
Judicial Independence	0.130**	0.352***	0.235***
	(0.052)	(0.061)	(0.059)
Polity	0.071***	0.071***	0.037***
	(0.007)	(0.009)	(0.009)
Regime Durability	-0.002	0.001	-0.000
	(0.002)	(0.002)	(0.002)
Civil War	-0.114	-0.216**	-0.101
	(0.095)	(0.103)	(0.111)
External War	0.066	-0.124	-0.035
	(0.150)	(0.170)	(0.203)
GDP Per Capita (logged)	0.003	0.007	-0.084*
	(0.039)	(0.037)	(0.045)
Population (logged)	-0.095***	-0.032	-0.066
	(0.032)	(0.031)	(0.042)
INGOs	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)
Rights_{t-1}	1.401***	0.815***	1.081***
	(0.071)	(0.073)	(0.062)
Fixed Effects for Year	Yes	Yes	Yes
n	1970	1970	1970

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 2.8: Robustness Test: Effects of ICCPR Ratification on Personal Integrity Rights

	(1) Killings	(2) Torture	(3) Imprison.	(4) Disapp.
ICCPR Ratification	-0.047	-0.040	0.083	-0.120
	(0.073)	(0.072)	(0.079)	(0.081)
Judicial Independence	0.117*	0.135**	0.206***	0.288***
	(0.061)	(0.059)	(0.055)	(0.059)
Polity	0.007	0.021***	0.065***	0.001
	(0.006)	(0.006)	(0.007)	(0.008)
Regime Durability	0.005***	0.001	0.001	0.000
	(0.002)	(0.001)	(0.002)	(0.002)
Civil War	-0.797***	-0.524***	-0.553***	-0.842***
	(0.089)	(0.109)	(0.127)	(0.089)
External War	-0.225	-0.321	-0.374*	-0.082
	(0.203)	(0.224)	(0.211)	(0.168)
GDP Per Capita (logged)	0.086**	0.071**	-0.007	0.027
	(0.034)	(0.033)	(0.034)	(0.036)
Population (logged)	-0.175***	-0.152***	-0.180***	-0.123***
	(0.026)	(0.025)	(0.039)	(0.028)
INGOs	0.000	0.000	0.000	0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Rights_{t-1}	0.965***	0.976***	1.025***	0.907***
	(0.066)	(0.066)	(0.063)	(0.063)
Fixed Effects for Year	Yes	Yes	Yes	Yes
n	1970	1970	1970	1970

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Chapter 3

Limited Constraints: Veto Players and the Effects of International Human Rights Agreements

3.1 Introduction

When and how do international commitments constrain national governments? Over the last ten years, scholars have increasingly analyzed the specific mechanisms for international institutional effects in various substantive areas (Simmons 2009; Mitchell et al. 2006; Kelley 2007; Dai 2007). Human rights present an especially difficult problem for those who argue that international commitments are effective, because the international reciprocity, reputation and peer enforcement mechanisms that generally facilitate cooperation do not appear to affect human rights practices (Downs and Jones 2002; Simmons 2009). Nonetheless, several domestic mechanisms may be able to turn commitments to international human rights agreements into meaningful improvements in human rights practices. Domestic actors use both normative arguments and political mobilization (Finnemore and Sikkink 1998; Keck and Sikkink 1998; Hafner-Burton and Tsutsui 2005; Simmons 2009; Conrad 2012) to pressure governments to honor their international commitments.

Domestic political institutions are also important mechanisms for making international commitments credible. Moravcsik (2000) argues that treaty ratification is an act of delegation that provides institutions the authority to enforce the government's promises. While his study focuses on international enforcement, the logic applies to domestic enforcement as well: human rights agreements, once ratified and incorporated into domestic law, delegate enforcement to domestic political institutions. Leaders continue to have incentives to violate human rights (Davenport 1995), and this process of domestic legalization can be an important constraint on such leaders (Hathaway 2007; Powell and Staton 2009). A key mechanism for domestic lock-in of international commitments is enforcement by independent domestic courts (Cross 1999; Keith 2002*b*; Apodaca 2004; Keith, Tate and Poe 2009), although this mechanism is not effective for all types of human rights violations, as demonstrated in Chapter 2.

Unlike existing work that has focused on the domestic judiciary, this paper analyzes the extent to which the domestic legislature can serve as a lock-in mechanism for international human rights agreements. I build on veto player theory (Tsebelis 1995, 1999, 2002), which predicts that more legislative veto players should make international cooperation less likely but more successful once established. I examine the situa-

tions in which legislative veto players can prevent leaders from cheating on their human rights commitments and thus make international human rights agreements more effective. Veto players can raise the cost of violating human rights de jure, but leaders seeking to weaken the opposition can nonetheless choose to violate rights de facto. Thus, the extent to which legislative veto players play a role in this context depends on which types of rights leaders constrained by the legislature would choose to violate in practice.

I argue that leaders in such situations will be more likely to pursue violations that are relatively less costly to hide and more likely to be successfully implemented. I distinguish between personal integrity rights violations, such as murder and torture, and empowerment rights, such as the freedoms of speech and religion. Because violating empowerment rights requires the government to prevent individuals from exercising their positive rights, it is more difficult to hide such violations and more difficult to make those violations effective. By contrast, personal integrity rights are freedoms from government actions. Violating such rights is within the government's control and does not require the government to prevent action by other actors. In addition, the government is better able to conduct such activities in secret. As a result, veto players are likely to increase the extent to which ratifications of human rights agreements improve respect for empowerment rights, but will be ineffective enforcers of such commitments with respect to personal integrity rights.

This paper makes three contributions to the literature. Although scholars have argued that domestic legislatures have important effects on international cooperation (Mansfield, Milner and Pevehouse 2007) and on human rights (Conrad and Moore 2010; Conrad 2011), this is the first study to assess how and when domestic legislatures affect whether international cooperation improves human rights practices. Theories that rely on domestic enforcement of international commitment depend, in large part, on the effectiveness of domestic constraints, such as veto players. This paper demonstrates that, as a mechanism for making international human rights commitments credible, legislative veto players are limited. Second, this paper contributes to our understanding of what some call the "Domestic Democratic Peace," or the finding that democracies are less likely to violate human rights. Much of the debate has focused on which aspect of democracy is most important in creating this effect, e.g., free elections, public pref-

erences and norms, civil society, and institutional constraints within the governments (Henderson 1991; Bueno De Mesquita et al. 2005; Davenport 2007*b*; Conrad and Moore 2010). My argument suggests that legislative veto players do play a role in this process, albeit a limited one. Finally, the argument developed here suggests an important limitation on veto player theory. While veto players may generally promote policy stability, the extent to which they do so depends on leaders' ability to implement changes to policy without turning to the legislature. The respect for human rights may not be affected by such policy stability to the extent other government behavior is.

3.2 Veto Players, International Cooperation, and Human Rights

Two key predictions of veto player theory are that more veto players should increase the difficulty of making new policies, but that, once made, such policies will be more difficult to change (Tsebelis 1995, 1999, 2002). These predictions have been tested with respect to many areas of international cooperation, most often with respect to economic policy. Domestic veto players have significant effects on international cooperation, most importantly by making commitments more credible (Milner 1997; Milner and Rosendorff 1997; Martin 2000). In governments with more veto players, there are fewer changes to tariff rates and non-tariff barriers (O'Reilly 2005), monetary policy is more rigid (Hallerberg 2002), independent central banks have a greater impact on inflation rates (Keefer and Stasavage 2003), and fewer changes are made to capital controls (Kastner and Rector 2003). Similarly, states with more veto players are less likely to conclude preferential trade agreements (Mansfield, Milner and Pevehouse 2007) and ratify European Union environmental directives (Perkins and Neumayer 2007).

Although the literature analyzing the relationship between veto players and international cooperation has focused on economic policy, there are reasons to expect veto players to matter with respect to human rights. Several arguments from the human rights literature suggest that the relationship between the legislature and the executive has important effects on human rights. First, Bueno De Mesquita et al. (2005) argue that party competition is a key to reducing human rights violations. Such competition may be par-

ticularly important when opposition groups control institutional veto points. Likewise, Conrad (2011) argues that when dictators face threats from opposition groups within the legislature, they are more likely to respond with rights concessions than when facing threats from outside the legislature. Conrad and Moore (2010) demonstrate that when more veto players are present, governments that conduct torture are less likely to stop doing so. Finally, when there are more veto players in the legislature, governments are more likely to make reservations when ratifying human rights agreements (Neumayer 2007; Kearney and Powers 2011).

While there are reasons to think veto players can make legalized human rights protections credible, other findings seem to put this argument into question. Several studies have examined whether constitutional protections of human rights affect respect for those rights, generally finding they do not. Davenport (1996) finds that most constitutional protections have no effect on human rights practices, although protections for the freedom of the press are associated with fewer negative sanctions, and restrictions on such rights are associated with increased negative sanctions. Cross (1999) also finds that most constitutional protections are not associated with better human rights practices. Keith (2002*a*) finds that protections for fair and public trials are associated with fewer human rights violations, but that prohibitions on torture are not. Most recently, Keith, Tate and Poe (2009) examined the effects of a wide range of constitutional provisions, including those protecting individual freedoms, providing for independent courts and allowing the government to declare states of emergency. They find that trial rights are associated with fewer violations of personal integrity rights, but other provisions, most notably prohibitions on torture, are not.

3.3 When Can Veto Players Lock In Human Rights Commitments?

The existing work discussed above presents somewhat contradictory results. If veto players can constrain executives, then human rights commitments that have become domestic law should result in fewer violations of these rights. On the other hand, if constitutional protections for human rights tend not to result in such improvements, then

perhaps legalized commitments to international agreements may be similarly ineffective. In addition, if human rights violations are the norm, then more veto players may result in the continuation of such practices. The argument developed in this paper attempts to reconcile these results by developing a more nuanced understanding of the extent to which veto players can enforce legalized human rights commitments.

I first consider a simple and direct application of veto player theory to international human rights agreements. If more veto players decrease the probability of policy change and the probability of states entering into treaties, then they should decrease states' willingness to commit to human rights agreements. There is already some evidence for this (Neumayer 2007; Kearney and Powers 2011). Once commitments to respect human rights are ratified and become domestic law, executives will nonetheless have a continued incentive to violate them, especially when seeking to weaken the opposition (Davenport 1995). Yet, the opposition, to the extent it controls veto points, will be able to prevent the executive from undoing the states' newly granted legal protections. This analysis, therefore, would suggest that when states ratify human rights treaties, the extent to which such ratifications result in improvements to human rights practices increases with the number of veto players.

Such an analysis, however, has significant shortcomings in the context of human rights. In the context of capital controls, for example, such an analysis may be valid. Once implemented, it would be more difficult for the executive to undo changes to capital control policy when veto players support such policies. In part, however, this is because it may be difficult for executives to violate capital control policies de facto. The distinction between de facto and de jure violations is the key to understanding the relationship between the potential effects of human rights commitments and veto players. Generally speaking, veto player theory focuses on de jure policy, rather than de facto practices. The simple analysis above assumes that to the extent executives cannot undo de jure protections for human rights, respect for human rights should increase de facto following treaty commitments. Yet in many contexts executives can and do violate human rights protections de facto, without regard for the legality of such violations – and this is perhaps why many constitutional protections do not appear to improve respect for human rights.

The extent to which veto players can enforce commitments to international human rights agreements – and turn those commitments into improvements in the respect for human rights – therefore depends on the extent to which executives can violate those commitments de facto without turning to the veto players for de jure approval. This suggests that veto players may be able to make international human rights commitments credible in some areas but not others. Human rights are multi-dimensional (McCormick and Mitchell 1997; Hathaway 2002; Davenport 2007*c*), and legalization strategies that work with respect to some rights may not work for others, as argued in Chapter 2. The factors that drive leaders' choices between de jure repression or extra-legal repression are key to understanding when veto players matter.

Leaders weigh the potential costs and benefits of repressing domestic dissent as compared with alternative strategies. The potential costs of repression include political backlash, the loss of legitimacy both domestically and internationally and the resources needed to execute repressive activity. By contrast, repression can benefit leaders by eliminating or weakening opponents and increasing the cost of dissent. Alternative strategies for achieving these goals include persuasion, material concessions and simple neglect. Leaders make decisions regarding repressive behavior by comparing the costs and benefits of such alternatives and their relative probabilities of success (Dahl 1966; Lichbach 1984, 1995; Gurr 1986; Gartner and Regan 1996; Moore 1998, 2000; Davenport 2004, 2007*a*).

This calculus affects not only the choice of whether or not to repress, but also the repression strategy itself. Consider the leader of a state that has ratified international human rights agreements and modified its domestic law accordingly. The leader faces a threat from a domestic group and has decided that responding with repression is the optimal strategy. One option for doing so is to attempt to convince the legislature to change the domestic law such as to take away the opposition group's rights. In some situations, this may be the less costly option. De jure repression generally requires leaders to expend fewer material resources. Also, changes to domestic law, especially if they are implemented in the national constitution, are less likely to result in a domestic prosecution, even by an effective judiciary, than extralegal violations. As mentioned above, however, if the opposition controls veto points in the legislature, this de jure op-

tion may be very costly or entirely impractical. In such a situation, the costs of obtaining legislative approval may far exceed the costs of extra-legal repression of the opposition group.

Now the leader must decide which type(s) of repressive behavior to conduct, i.e., which rights to violate. Conditional on the leader having chosen to pursue repression in a de facto manner, two considerations become crucial. First, a key risk leaders face when violating human rights – and the law, more generally – is being caught and punished. Punishments could take many forms: political costs, such as a loss of supporters; decreased legitimacy for the regime; and legal sanctions. Leaders would therefore prefer to maximize the probability of hiding their violations. They are likely to choose to repress in ways that are less costly to hide. Second, leaders want to maximize the probability that the repressive behavior will be executed successfully. In other words, they consider the extent to which the resources they spend to execute violations – and the risks they take in doing so – will result in actual repressive behavior that weakens the opposition and fulfils its purpose.

Thus, in a state with legalized human rights commitments and legislative veto players, leaders choosing to repress will be more likely to do so in ways that are less costly to hide and more likely to be successful. Applying this argument to specific human rights, I argue that violations of personal integrity rights are easier to hide and more likely to succeed, while violations of empowerment rights are costlier to hide and less likely to succeed. Personal integrity rights violations include extrajudicial killings and other deprivations of the right to one's life, torture and other inhuman treatment, political imprisonment and forced disappearances. Empowerment rights are those "which provide the individual with control over the course of his or her life, and in particular, control over (not merely protection against) the state" (Donnelly and Howard 1988, p. 215). These include the freedoms of religion, speech, assembly and association.

Governments can and often do conduct violations of personal integrity in situations that allow the government to hide information. Torture, for example, often occurs in facilities under government control, which allows the government to control the situation, prevent access to outsiders, and destroy evidence of the abuses. While released victims of torture often bear the scars of the events, governments increasingly use 'clean'

torture techniques that leave little scarring and, consequently, decrease the cost of hiding torture (Conrad and Moore 2010). Political imprisonments, by definition, occur in government facilities. In many cases, these are made public. Yet it is also possible for the government to detain prisoners in secret when they choose to. In some such cases, the victims may be reported as missing, and the government may be suspected of having detained them, but because the government is in possession of the victim it can hide this information well. Likewise, by their very nature, disappearances are abuses in which the government explicitly seeks to hide information by not disclosing a detention and/or a murder. As has been well-documented in a several cases, its often very difficult for other actors to obtain information about disappearances (Simpson and Bennett 1985; Lutz and Sikkink 2000; Snow et al. 2008). Government-sponsored killings sometimes occur in public, which indicates that leaders may not always seek to hide such abuses, but governments also have the ability to conduct such killings in secret, particularly in combination with detentions and disappearances.

By contrast, violations of empowerment rights are much more costly for the government to hide. By their nature, these are abuses in which the government is not in control of the victims. Empowerment rights are often exercised in public or semi-public places. Leaders can send their agents to close places of worship or shut down newspapers, for example, but such events are relatively easy for other actors to observe. Likewise, while the government can shut down protests, thus placing limits on free speech and free association rights, such shut downs are often well-documented by victims, especially in the electronic age.

With respect to repressing empowerment rights, the probability of a leader achieving his goals is also lower. Empowerment rights are often conceived of as positive rights (Berlin 1969), i.e., they provide individuals control over rather than protection from the state. As Donnelly and Howard (1988, p. 234) argues, "Empowerment rights provide what we referred to above as "external" checks on state power." Restrictions on the freedom of speech and religion amount to forms of government regulation (Coase 1974; Posner 1987; McConnell and Posner 1989). Empowerment rights, therefore, are rights that are within the control of individuals. Violating these rights requires the government to prevent individuals from exercising them, which is a different and significantly more

difficult proposition than the violation of protections from government action. The nature of these rights thus makes them relatively costly for the government to effectively violate, especially in situations in which the rights have already been granted and the government seeks to take them away. Leaders seeking to weaken the opposition by violating these rights could, as mentioned above, shut down media outlets and houses of worship or attempt to break up public gatherings. Yet these strategies are costly to implement effectively. While the leader may able to shut down some opposition gatherings or media, opposition groups have often proved resilient in finding alternatives. In many cases, governments have resorted to more serious violations, especially imprisonments, in part in order to prevent key opposition figures from exercising their empowerment rights.

Personal integrity rights are often thought of as negative rights: they are freedoms from government action rather than freedoms to perform certain actions. Violating these right requires the government to perform certain actions rather than preventing individuals from doing so. Leaders of governments are therefore in a better position to effectively violate these rights. Conversely, it is more costly for the opposition to circumvent such repression strategies than to avoid limits on, for example, the freedom of speech. This is not to say that personal integrity rights violations are always likely to accomplish their purpose. Studies have shown, for example, that torture is an ineffective technique for gathering information. The argument made here, however, is that a leader seeking to violate such rights can be relatively more confident that such violations will be effectively implemented than a leader seeking to violate empowerment rights.

I now return to the question of when legislative veto players can make commitments to international human rights agreements credible. While such veto players are likely to be able to deter de jure violations, leaders may be able to violate other human rights de facto without seeking legislative approval. As a result, legislative veto players can turn commitments to international human rights agreements into improvements to actual practices when it is relatively more costly for leaders to violate rights de facto. These arguments lead to the following hypotheses:

> Hypothesis 1: The effect of the ratification of international human rights agreements on government respect for empowerment rights increases with the number of legislative veto players.

Hypothesis 2: The effect of the ratification of international human rights agreements on government respect for personal integrity rights does not increase with the number of legislative veto players.

3.4 Research Design

The first issue to address in my research design is the choice of international human rights agreement. I focus on the ICCPR because it covers both empowerment rights and personal integrity. Adopted in 1966 and entered into force in 1976, the ICCPR has since been ratified by 167 countries (as of 2012). Unlike many multilateral human rights treaties that have been adopted more recently, the ICCPR covers a broad range of rights. These include the key personal integrity rights discussed in this paper. Article 6 protects individuals' right to life and thus prohibits extrajudicial killings by governments. Likewise, Article 7 prohibits torture and cruel, inhuman or degrading punishment. Article 9 provides that individuals may not be arbitrarily arrested or detained. This, together with additional prohibitions on the infringement of political rights, is often deemed a prohibition on political imprisonment and other detentions in violation of due process. The ICCPR does not explicitly address forced disappearances, most likely because the term was not used in common parlance until the abuses of the South American regimes of the 1970s became well-known. Yet the elements of a forced disappearance, most importantly arbitrary arrest and summary execution, are explicitly prohibited by the ICCPR. The ICCPR also prohibits governments from infringing on a broad set of additional civil and political rights. Among these are freedoms of speech and expression (Article 19), assembly and association (Articles 21 and 22), and the practice of religion (Articles 18). Importantly, Article 2 requires members to adopt domestic laws, including legislation as necessary, to "give effect to the rights" enumerated in the treaty.

Analyzing the joint effects of ICCPR ratification and legislative veto players has several advantages in this context. First, this allows me to analyze the effects of a single treaty on different dimensions of government human rights practices, thus minimizing the extent to which findings may be caused by differences in treaty design. In addition, relying on a single treaty allows me to use the same set of units of observation to test all of my hypotheses. As a result, the only difference between the various regression

models reported below is the dependent variable, which allows for relatively simple comparisons between the results. Finally, there is an important debate about the effects of ICCPR to which my results contribute.

Estimating the effects of treaty commitment is known to be difficult. Governments select the treaties they join in part based on their interests and the extent to which they expect to conform their behavior to the treaties' requirements (Downs, Rocke and Barsoom 1996). As a result, if we model an outcome on treaty commitments without addressing this problem, we could at best say that treaty members are more likely to experience that outcome, but not that this is a causal relationship. A high rate of treaty compliance among treaty members, for example, may simply mean that states that are more likely to comply are also more likely to join. Scholars have recently begun taking the treaty commitment selection effect seriously and have used several methods to address it. Among these are Heckman-based modeling (von Stein 2005; Neumayer 2005) and instrumental variables regression (Simmons 2009). Yet both of these methods are highly sensitive to model specification and may not be optimal in this context. To produce reliable estimates, both Heckman (Sartori 2003) and instrumental variables (Heckman 1997; Pearl 2000) models require the specification of a variable that is associated with treaty commitment but not with the outcome policy. Yet, because both of these stages are ultimately decisions of government policy, it is particularly difficult to identify factors that contribute to the decision to join a human rights treaty but not to the decision to repress the human rights of citizens (Hill 2010).

The propensity-score matching approach proposed by Simmons and Hopkins (2005) to address this problem is particularly promising. The first step in this approach is to identify the set of factors that predict treaty commitment. The next step is to match treaty members to treaty non-members based on these underlying factors. The result is a sample that is balanced on the probability of treaty commitment. With respect to this sample, we can think of selection as having been randomly assigned (Ho et al. 2007). The sample can then be subjected to further tests, including simple t-tests and multiple regression, to determine the causal effects of treaty commitment. Among the advantages of this approach are that it does not require the analyst to make distributional assumptions nor to specify a factor associated with treaty commitment but not with the

outcome policy. More generally, matching has been shown to be an effective tool for creating balanced samples when treatment is not randomly assigned.

A significant threat to inference using this approach is the potential that unobservable (or unmeasured) factors affect treaty commitment decisions and are not included in the matching model (Simmons and Hopkins 2005). The estimation of the treaty commitment effect is highly sensitive to the propensity score estimates (Rubin 1997), and the choice of underlying variables significantly affects the reliability of propensity score analysis (Heckman, Smith and Clements 1997; Heckman et al. 1998; Lechner 2000; Smith and Todd 2005). Chapter 1 argues that the key factor that determines treaty commitment decisions – one that is difficult to observe directly – is a state's preference for treaty commitments, i.e., which types of treaties it tends to prefer joining. I propose a methodology to directly estimate these preferences in order to calculate the probability of states committing to specific treaties. This methodology relies on estimating the ideal points of states with respect to universal treaties using the W-NOMINATE algorithm (Poole and Rosenthal 1997), which has traditionally been applied to legislative roll-call voting but has also been used to estimate state preferences (Voeten 2000). In this model, the options of committing and not committing to a treaty are represented by points in an n-dimensional policy space. Each state decides whether or not to commit to a treaty by, among other factors, weighing the distance between these points and its ideal point in this space. The closer a state is to a treaty, the more likely it is to join the treaty (Simmons 2009). Thus, the probability of a particular state ratifying a particular treaty is calculated based on the distance between the state and the treaty in the preference space.

My research design proceeds in three stages. First I use the W-NOMINATE algorithm on a data set of membership in approximately 300 universal treaties. This data set includes all of the universal treaties included in the United Nations Treaty Collection (UNTC). The data include various types of instruments, including protocols and amendments to treaties, all of which are considered separate treaties for purposes of this analysis. The data are coded "1" for country-years that have ratified a treaty and "0" otherwise. A full list of these treaties is available from the author upon request. The results provide annual estimates of each country's probability of ratifying the ICCPR. These

estimates begin in 1976, the first year in which the ICCPR was in force, and continue to 2007.

In the second stage, I match treaty members to non-members using the nearest-neighbor algorithm provided by the MatchIt package in the R programming language (Ho et al. 2009). I include in the matching model the W-NOMINATE estimated probabilities as well as several other factors that may affect the probability of ICCPR commitment, most importantly the factors that ultimately affect states' respect for human rights (Powell and Staton 2009). As a measure of legislative veto players, I follow existing studies of the relationship between veto players and treaties (Mansfield, Milner and Pevehouse 2007; Perkins and Neumayer 2007; Neumayer 2007) in using the measure developed by Henisz (2002) (POLITICAL CONSTRAINTS). The measure is especially useful for purposes of testing my theory because it is designed to quantify the difficulties executives face when making policy changes. Based on a spatial model of interaction between political actors, the measure takes into account three factors: (1) the extent to which there are legislative veto points outside of the executive; (2) the extent to which these veto points are controlled by different parties from the executive's; and (3) the extent to which the majority controlling each veto point is cohesive. As a measure of judicial independence, I adopt the data provided by CIRI (JUDICIAL INDEPENDENCE), which are coded as 0 for "not independent," 1 for "partially independent" and 2 for "generally independent." I include a measure of regime type using the Polity IV data (Marshall and Jaggers 2002) (POLITY) because democracies are more likely to respect human rights (Poe and Tate 1994; Davenport 1995, 1999; Poe, Tate and Keith 1999). Newer regimes and well-established regimes may have different preferences, so I follow Hafner-Burton and Tsutsui (2007) by controlling for this factor using the Polity IV data (REGIME DURABILITY). Both foreign wars and civil wars may result in periods of increased repression (Poe and Tate 1994; Poe, Tate and Keith 1999). Civil wars, in particular, may result in periods of lawlessness during which even independent courts have a diminished capacity to constrain the other branches of governments. I control for this using data from the Correlates of War Project. NGOs play a key role in political mobilization against oppression and may succeed in improving government practices. I control for the number of international NGOs (INGOS) in a country using the data

provided by Hafner-Burton and Tsutsui (2005). The level of economic development is a well-known predictor of human rights practices (Henderson 1991; Poe and Tate 1994; Poe, Tate and Keith 1999), and I control for this using a measure of per capita GDP provided by the World Bank. I use the natural log of this measure because this effect is likely nonlinear (Davenport 2007*a*). To address potential differences among states of different sizes and potential monitoring biases based on this factor, I follow much of the literature in including a control for the natural log of a state's population, using data provided by the World Bank.

There are many units with missing data among these variables. Because the underlying reasons for the missingness of the data are likely non-random, listwise deletion of these observations may result in biased inference (Little and Rubin 1987). I therefore follow Hill (2010) and others in imputing the missing values using the Amelia II Program (Honaker, King and Blackwell 2009). The data on ICCPR ratifications are for the years 1976 to 2007. However, the CIRI data begin in 1981. Rather than attempting to impute the values of the CIRI variables for all countries for the years 1976-1980, which would be subject to a particularly high degree of uncertainty, I begin my analysis in 1981.[1]

In the third stage, I use the matched sample to test my hypotheses. As dependent variables, I use the measures provided by CIRI. While other measures of human rights practices are also commonly used in the literature, especially the Political Terror Scale (Gibney and Dalton 1996), the CIRI data are particularly suitable to testing my hypotheses because they disaggregate personal integrity rights violations into several types of violations and they provide data on many other areas of human rights. With respect to personal integrity rights, I use the CIRI measures of Extrajudicial Killings, Torture, Political Imprisonment and Disappearances. All four measures are coded as 0, 1 or 2 for each country-year. A score of 2 indicates that the applicable violation did not occur in that year, while a score of 0 indicates the violation was frequent. I also adopt the CIRI measures of Freedom of Assembly and Association, Freedom of Speech and Freedom of Religion. These measures are also coded as 0, 1 or 2 for each country-year. A score of 2 indicates the applicable freedom was not restricted in that year, while a score of 0

[1] I conducted the imputation procedure using the full set of 4368 country-years from 1981 to 2007 (rather than the matched sample) because including the full data set allows for more accurate imputation.

indicates it was severely restricted. I estimate a series of ordered probit models using these measures as dependent variables. To test Hypotheses 1 and 2, I create an interaction term of POLITICAL CONSTRAINTS and ICCPR RATIFICATION. In all models, I include fixed effects for the year of the observation and use standard errors that are robust toward arbitrary heteroskedasticity and that take into account clustering by country. To address serial correlation, I include lags of the applicable dependent variable for year $t-1$. A Lagrange multiplier test indicates that additional lags are not necessary to address serial correlation.

3.5 Results

Tables 3.1 and 3.2 set forth the results of the matching stage. In the full sample, there is a large imbalance between treaty members and non-members in terms of their probabilities of joining the ICCPR. Not surprisingly, ICCPR members are much more likely to have joined the treaty ex ante. After matching, however, these probabilities are much more similar. The matched sample is not perfectly balanced, however, so multiple regression is required for hypothesis testing. Tables 3.3 and 3.4 report the results of the regression models. These results substantially support the theory presented in this paper. In all three models of empowerment rights, the coefficient on the interaction between POLITICAL CONSTRAINTS and ICCPR RATIFICATION is significant and positive. This indicates that the extent to which commitment to the ICCPR improves respect for the freedoms of speech, association, assembly and religion increases with the extent to which executive powers are constrained by legislative veto players. Figure 3.1 reports marginal effects based on the models reported in Table 3.3. The marginal effects reported are the expected percentage changes in the values of the dependent variables for ICCPR members based on a one-standard-deviation increase in the interaction term. For each right, I report the expected percentage change in the country providing an unrestricted right and of severely restricting that right. Thus, for example, the effect of one-standard-deviation increase in POLITICAL CONSTRAINTS on ICCPR Members results in a 38% increase in the probability of providing an unrestricted right to free speech and a 20% decrease in the probability of severely restricting that right. By con-

trast, the results with respect to personal integrity rights indicate that the extent to which ICCPR membership affects respect for these rights is not significantly affected by the extent to which the executive is constrained by legislative veto players.

3.6 Conclusions

Much of the literature on the relationship between domestic political institutions and international cooperation has focused on the features of institutions. Veto player theory, in addition, suggests that international cooperation can be enhanced when the opposition controls the legislative veto points required to renege on international commitments. This paper has focused on the limitations of these analyses. I have provided a framework that explains when legislative veto players can act as a lock-in mechanism for international commitments and when they cannot. In the human rights context, leaders often choose to sidestep these constraints and repress opposition groups without legal backing. As a result, legislative veto players are effective enforcers of international human rights agreements with respect to those rights that are more difficult for leaders to violate de facto.

These results may help explain the puzzling findings in previous studies that national constitutional provisions do not appear to reduce violations of human rights, especially those of personal integrity rights. If veto players are ineffective enforcers of such rights because leaders can effectively violate them de facto, then constitutional provisions may prove ineffective for similar reasons. Among the few constitutional provisions that appear to result in fewer violations is the freedom of press, a finding that is consistent with the argument that leaders constrained by legalized protections often choose to violate other rights.

Another key implication of this study is that veto players play an important, but limited, role in the domestic democratic peace. Scholars have debated, for example, whether the "Voice" aspects of democracy, such as the right to vote, contribute more to relatively few violations of human rights in democracies than the "Veto" characteristics of democracy, such as legislatures and the judiciary (Davenport 2007*b*; Conrad and Moore 2010). My analysis suggests that different aspects of the domestic democratic

peace may be more the product of different mechanisms. Legislative veto players can result in fewer illegal violations of empowerment rights, but not of personal integrity rights. This suggests that, to the extent democracies violate personal integrity rights less than other types of regimes, the mechanism for this restraint lies in other democratic characteristics.

The relationship explored in this paper between veto player theory and international cooperation suggests additional areas for future research. Tsebelis (2002) argues that a greater number of legislative veto players leads to greater policy stability, which in turn leads to judicial independence. Many scholars believe independent courts are more effective at enforcing international human rights commitments that have been implemented into domestic law. While the effects of independent courts and legislative veto players on the effects of human rights agreements have been studied separately, the argument provided by Tsebelis (2002) suggests that these constraints on executive power may take effect sequentially, which bears further analysis. In addition, one of the implications of veto player theory is that the ideological distance between veto players should enhance policy stability. It may be the case, however, that ideology may be more salient with respect to some human rights than others and, therefore, that this factor is more important in some areas.

The argument developed in this paper, as well as the results, also supports the notion that leaders strategically substitute some human rights violations for others, as previous studies have suggested (Moore 1998, 2000; Poe 2004). A leader blocked from violating empowerment rights de jure because of veto players in the legislature may turn to an alternative strategy of violating personal integrity rights de facto. This suggests, in turn, that absent the political and institutional constraints that make empowerment rights violations more costly, some such leaders may have found it less costly to violate those rights. Given that personal integrity rights violations are often considered the gravest violations of human rights, it would be an unfortunate consequence of legalization if this were the case. I hope to explore this question in greater depth in future work.

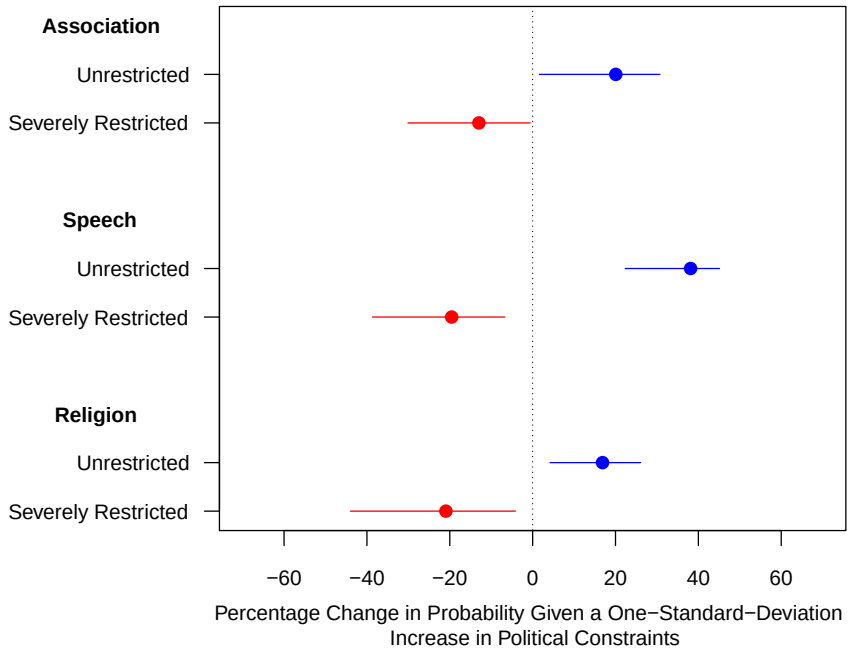


Figure 3.1: Marginal Effects of Political Constraints on Empowerment Rights for IC-CPR Members

Table 3.1: Balance Statistics

	Full Sample	Matched Sample
Sample Size	4368	1980
Treatment Units (ICCPR Members)	2947	990
Control Units (Non-ICCPR Members)	1421	990
Mean Pr(ICCPR Ratification) - Treatment Group	0.8819	0.6648
Mean Pr(ICCPR Ratification) - Control Group	0.4810	0.6444
Percentage Improvement in Balance	94.91%	

Table 3.2: Additional Balance Statistics

	Treatment Group Mean	Control Group Mean	% Improvement in Balance
Political Constraints	0.12	0.17	69.82
Judicial Independence	0.99	1.07	36.63
Polity	-1.92	-0.48	73.19
Regime Durability	22.21	23.04	32.22
Civil War	0.21	0.19	26.21
External War	0.04	0.04	93.49
GDP Per Capita (logged)	7.29	7.32	91.12
Population (logged)	15.89	15.79	79.68
INGOs	394.13	497.81	76.16
n	990	990	

Table 3.3: Empowerment Rights

	(1) Freedom of Association	(2) Freedom of Speech	(3) Religious Freedom
ICCPR Ratification	0.091	-0.129	-0.035
	(0.092)	(0.097)	(0.086)
Political Constraints	-0.499	-0.823**	-0.386
	(0.326)	(0.306)	(0.307)
Political Constraints X	0.811*	1.234**	1.174**
ICCPR Ratification	(0.325)	(0.385)	(0.443)
Polity	0.076***	0.079***	0.044***
	(0.009)	(0.010)	(0.008)
Judicial Independence	0.180**	0.350***	0.256***
	(0.058)	(0.063)	(0.054)
Regime Durability	-0.002	-0.000	0.000
	(0.002)	(0.002)	(0.002)
Civil War	-0.163	-0.168	0.001
	(0.096)	(0.098)	(0.086)
External War	-0.093	0.039	-0.145
	(0.138)	(0.155)	(0.174)
GDP Per Capita (logged)	-0.055	-0.018	-0.091**
	(0.032)	(0.032)	(0.031)
Population (logged)	-0.118***	-0.054	-0.131***
	(0.032)	(0.030)	(0.031)
INGOs	0.000*	0.000	0.000
	(0.000)	(0.000)	(0.000)
Rights_{t-1}	1.303***	0.919***	1.087***
	(0.072)	(0.070)	(0.056)
Fixed Effects for Year	Yes	Yes	Yes
n	1980	1980	1980

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 3.4: Personal Integrity Rights

	(1) Killings	(2) Torture	(3) Imprison.	(4) Disapp.
ICCPR Ratification	-0.099	-0.231*	-0.082	-0.100
	(0.093)	(0.104)	(0.086)	(0.096)
Political Constraints	-0.194	-0.448	-0.838*	0.055
	(0.340)	(0.343)	(0.356)	(0.307)
Political Constraints X	0.415	0.781	0.681	-0.162
ICCPR Ratification	(0.315)	(0.590)	(0.446)	(0.427)
Polity	0.013	0.027***	0.068***	0.008
	(0.008)	(0.008)	(0.011)	(0.009)
Judicial Independence	0.110	0.126*	0.275***	0.307***
	(0.064)	(0.063)	(0.057)	(0.062)
Regime Durability	0.005*	0.004**	0.002	0.003
	(0.002)	(0.001)	(0.003)	(0.002)
Civil War	-0.915***	-0.585***	-0.576***	-0.928***
	(0.104)	(0.102)	(0.115)	(0.081)
External War	-0.379*	-0.471*	-0.242	-0.074
	(0.170)	(0.195)	(0.210)	(0.152)
GDP Per Capita (logged)	0.068	0.025	-0.009	0.017
	(0.037)	(0.036)	(0.034)	(0.036)
Population (logged)	-0.118***	-0.188***	-0.192***	-0.035
	(0.030)	(0.032)	(0.035)	(0.029)
INGOs	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Rights_{t-1}	0.939***	0.927***	0.932***	0.904***
	(0.059)	(0.068)	(0.073)	(0.063)
Fixed Effects for Year	Yes	Yes	Yes	Yes
n	1980	1980	1980	1980

Ordered Probit Models

Robust standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Chapter 4

Measuring Multilateralism: Ideal Point Estimates of State Preferences over Global Treaties

4.1 Introduction

International institutions are a key area of research in international relations. Scholars have analyzed why states create institutions (Keohane 1984) and how they design them (Koremenos, Lipson and Snidal 2001), and, in turn, the extent to which those institutions affect state behavior (Simmons 2000; Hafner-Burton and Tsutsui 2005; Hill 2010). Both institutional design and the effects of institutions are closely connected with institutional membership (Downs, Rocke and Barsoom 1996). Analysts have therefore studied membership patterns across various institutional forms and substantive areas, including human rights treaties (Vreeland 2008), environmental treaties (von Stein 2008), international courts (Simmons and Danner 2010), and alliances (Morrow 1991).

Preferences are crucial to the research agenda on international institutions – and especially crucial to understanding institutional membership. States design institutions based on their preferences over possible solutions to cooperation problems. Uncertainty about other states' preferences often affects design choices and may impede negotiations (Koremenos, Lipson and Snidal 2001; Kydd 2001; Morrow 2001). Preferences can also shape the effects of international institutions. Institutions with weak enforcement mechanisms may not be able to elicit compliance from states with an interest in cheating on their commitments. Conversely, observed compliance may result from underlying state preferences rather than mechanisms created by the institution itself (Downs, Rocke and Barsoom 1996). Finally, international institutions, once put in place, may also shape state preferences through the spread of norms and acculturation (Moravcsik 1997; Risse, Ropp and Sikkink 1999; Thompson 2006), potentially resulting in a convergence of preferences among member-states (Bearce and Bondanella 2007).

Despite the attention paid to questions about international institutions and the recognition that preferences are crucial in answering those questions, few studies have systematically analyzed the factors that shape states' preferences with respect to international institutions. Several scholars have analyzed the demand for international cooperation, but this literature tends to focus only on economic relations (Bagwell and Staiger 1997*a*,*b*; Downs, Rocke and Barsoom 1998). Treaty-making and treaty-joining varies tremendously, and to argue that states join when they have a preference for certain gains from cooperation does not explain this variation. Instead, it raises the question of

what are the most important determinants of states' treaty commitment preferences.

This paper attempts to answer this question by systematically analyzing states' decisions to join multilateral treaties. I estimate states' treaty commitment preferences by using a spatial-modeling approach often used to estimate legislative ideal points. I then analyze the results of the ideal point estimation to determine which characteristics of states are most important in shaping their treaty commitment preferences. This analysis reveals that the key factor underlying state preferences in this area is an interest in economic cooperation: the number and type of treaties states join depends in large part on their interest in being integrated into the global economy. This finding is consistent both during and after the Cold War. While other factors, including cultural ties and domestic regime types, appear to also play minor roles, economic interests generally overshadow these other factors.

The results of this paper have important implications for the continuing study of international institutions. First, if states' preferences with respect to universal treaties are shaped primarily by their economic interests, then it may be the case that these interests also affect the joining of other institutions, including regional organizations, bilateral treaties and informal institutions. Second, to the extent that uncertainty about preferences can be an impediment to international cooperation, this paper indicates that this problem may be most severe when states cannot reliably determine each others' economic preferences. The problem may be exacerbated when competing domestic interests have differing economic preferences, and thus there is a possibility that states' economic preferences may change in the future when the domestic balance of power changes. As Grieco, Gelpi and Warren (2009) argue, relative shifts in domestic partisan power change states' economic preferences and, as a result, affect treaty compliance.

4.2 Treaty Commitment Preferences

Existing work on international institutional membership shows the importance of preferences, yet does not explain which characteristics of states are more important in shaping them. To argue that states with homogenous preferences tend to cooperate raises the question of which states tend to have similar preferences as well as the question of

what similarity means in this context. For example, states tend to create institutions with restricted membership so that they can cooperate with others that have similar interests, which may increase the probability of compliance (Downs, Rocke and Barsoom 1996; Koremenos, Lipson and Snidal 2001). Yet this type of reasoning does not tell us which factors drive the underlying interests in the first place. Likewise, states may attempt to minimize distributional and enforcement problems by working with specific partners, but this begs the question of which types of states generally prefer to work together. States are complex actors, and their characteristics vary along many dimensions. The goal of this paper is to determine which of those characteristics are most important in shaping their commitments to international institutions.

Economic Factors. The growth of global economic activity over the last several decades has been facilitated and institutionalized in part through the creation of multilateral agreements. Some treaties explicitly cover economic policies, such as those related to trade liberalization, while others facilitate international exchange indirectly by coordinating activities and expectations with respect to issues such as container shipping, the transport of hazardous materials and road signage. Functionalists often argue that governments cooperate with each other because of increasing material demands from domestic actors (Shanks, Jacobson and Kaplan 1996). Rich states tend to join more intergovernmental organizations (IGOs) (Jacobson, Reisinger and Mathers 1986; Shanks, Jacobson and Kaplan 1996; Beckfield 2003), and pairs of states that trade heavily with each other are more likely to join the same IGOs (Boehmer and Nordstrom 2008). Analyzing voting behavior in the United Nations General Assembly (UNGA), Kim and Russett (1996) find that, after the Cold War, voting preferences were generally defined based on states' level of economic development. Krasner (1995) also argues that international institutional joining may be based on economic grounds, and specifically that smaller economies may seek to join institutions to protect their interests from larger, more powerful economies. This suggests that economic characteristics such as income, trade and a more general interest in international economic cooperation may be significant determinants of states' treaty commitment preferences.

Regime Type. There are several reasons to believe democracies may have different treaty commitment preferences from autocracies. The processes of aggregation

of multiple domestic preferences into unified national decisions by governments differ when governments must appeal to voting constituents. International cooperation is less likely when domestic state authority to ratify treaties is allocated to multiple branches of government, as it is within democracies (Milner 1997). Democratic dyads are more likely to join the same IGOs (Boehmer and Nordstrom 2008), which indicates that regime type affects states' choices of international institutions. Democracy is also a key determinant of treaty compliance (Simmons 2000); the intertwined relationship between treaty joining and compliance suggests democracy may also be a determinant of joining. Regime type is thought to affect a wide range of other international outcomes, including the ability of states to win wars (Reiter and Stam 2002) and the extent to which states make reliable allies (Lipson 2005). Finally, democratic peace theory suggests that regime type affects states preferences in ways that shape conflictual and cooperative behavior.

Power and the Cold War. States' treaty commitment preferences may also depend on their relative capabilities. If states make decisions based on the distribution of capabilities (Ikenberry and Doyle 1997; Waltz 1993), then we might expect them to choose treaties and treaty partners based on this factor. Waltz (1993), in particular, predicted that European States would balance against the United States in the post-Cold-War period, a notion that could be extended to treaty commitment preferences. Along similar lines, Iida (1988) argues that weak states may band together into blocs to counter more powerful states. During the Cold War, these power dynamics may have been especially stark. Voeten (2000) finds that the Cold War was a key factor in determining UNGA voting preferences. If the underlying factors that shaped these votes were similar to the factors shaping treaty-making, it may be the case that the Cold War was a key dimension of treaty commitment preferences.

Civilization and Region. Another key factor that may affect treaty commitment preferences, espoused most prominently by Huntington (1997), is the state's "civilization." Huntington argues that both international conflict and cooperation are shaped by cultural factors, independently of concerns over power and economics. In his view, each state's civilization shapes its preferences, and the world consists of eight such civilizations with various degrees of similarity and difference from each other. If Huntington is

correct, then civilization may be a key factor in treaty commitment preferences. Some recent evidence indicates that international institution membership may be affected by such factors. Beckfield (2003) finds that Western states tend to join more IGOs, and Greenhill (2010, Ch. 6) shows that many joint memberships in IGOs can be explained by states having shared linguistic and colonial ties. Underlying these results is the additional possibility that states in different geographic regions may have distinct treaty commitment preferences. Conflictual behavior varies considerably by region (Bennett and Stam 1999; Lemke 2002, 2003), which suggests that cooperative behavior may likewise vary. Accordingly, several studies of UNGA voting have found that geography was among the key determinants of state preferences (Russett 1966; Kim and Russett 1996; Voeten 2000).

Supranationalism and Legalization. States guard their sovereignty carefully, but some may be more willing to relinquish sovereignty in order to benefit from cooperation. In part, this difference could be because some states stand to gain more from cooperation, but part of the difference may be that some states have lower "sovereignty costs" than others. Studies of UNGA voting have consistently found that preferences for or against the ceding of authority to the U.N. have formed important aspects of state preferences (Alker and Russett 1965; Voeten 2000). Because states often grant authority to the U.N. and other international organizations via treaty instruments, their preferences on this point may also shape their treaty commitments. On a related issue, states may differ on the extent to which they are willing to accept the international legalization of their commitments. While differences along this line may inform all types of treaty commitments, the extent to which this issue is crucial may be best tested with respect to commitments to international human rights treaties. Human rights treaties are often though to be among the forms of international law that require states to give up the most sovereignty because they govern relations between states and their citizens, rather than interstate relations.

4.3 Methodology

A relatively simply approach to determining treaty commitment preferences may be to regress treaty joining decisions on the various independent variables thought to affect those decisions. Problems with such an approach include the fact that it ignores the extent to which many treaties are similar (and thus have similar members) and assumes that all treaties are equally informative about underlying preferences. By analogy, such a procedure would amount to attempting to estimate legislator preferences based on a count of bills they vote for, a procedure long recognized in the legislative studies literature as being misleading. We certainly may not know in advance which treaties can best discriminate among state preferences, but methods have been developed that do so. The literature on the UNGA voting, for example, has long recognized that methods designed to reduce the dimensionality of choice behavior are appropriate for estimating state preferences (Alker 1964; Alker and Russett 1965; Russett 1966; Voeten 2000).

In order to estimate directly state preferences with respect to treaties, I rely on the spatial model of political choice (Downs 1957; Davis, Hinich and Ordeshook 1970). The basic notion behind implementations of the spatial model is that, by observing the choices political actors make, we can measure their preferences relative to each other and relative to the options with which they are faced. In this model, the options of committing and not committing to a treaty are represented by points in an n-dimensional policy space. Each state decides whether or not to commit to a treaty by weighing the distance between these points and its ideal point in this space. Simmons (2009) has recently suggested that this logic applies to treaty commitment decisions: "To use the language of spatial models, the nearer a treaty is to a government's ideal point, the more likely that government is to commit." (p. 65, emphasis omitted). Thinking of treaty commitment decisions in this way allows for the use of methods traditionally used to analyze other dichotomous choices, most importantly those used to study legislative roll-call voting. Specifically, I use the W-NOMINATE multi-dimensional scaling method to estimate states' treaty commitment preferences (Poole and Rosenthal 1997). W-NOMINATE is a random utility model of Euclidean spatial voting (Enelow and Hinich 1984; Hinich and Munger 1994, 1997) that assumes each actor assigns a utility to each of two options. This utility is determined both by the distance between the actor and the options as well

as a stochastic error term. Poole and Rosenthal (1997) created W-NOMINATE as a tool for estimating legislator preferences and used it to analyze roll-call voting in the U.S. Congress. It has since been used to study other legislatures (Schonhardt-Bailey 2003; Desposato 2006, 2008) and to analyze voting in the UNGA (Voeten 2000; Reed et al. 2008).

I focus on universal treaties, which are open to all states. This is because, in order to be able to use the spatial modeling approach to analyze treaty decisions, we must be able to infer that any non-joining behavior is the choice of states in question. I collected treaty ratification data from the United Nations Treaty Collection (UNTC), an online database that provides information regarding all treaties deposited with the U.N. Secretary-General. The data set includes a broad range of substantive areas, including human rights, trade, transportation, the environment, communications and arms control. I analyzed the set of treaties included in the UNTC to determine which are de jure open to all states and which are limited to a specific set of states, such as on a regional basis. The latter are excluded from the analysis, leaving 280 universal treaties. The UNTC includes conventions, treaties, protocols to treaties and treaty amendments. If a list of members is included for any of these types of instruments, I include it in my data set as a separate treaty-commitment choice. I do this because each item reflects a separate decision made by states, regardless of whether the item amends a previous choice. For simplicity, I will refer to each such item as a "treaty" in this paper. For each treaty, I have thus created a matrix consisting of all of the states in the international system and an indication of whether or not they ratified the treaty. If a state has ratified a treaty as of a given year, I have coded that state as a "1" with respect to that treaty; otherwise the state is coded as a "0". Using these data, I created a matrix for each year between 1950 and 2008 that indicates, for each treaty then in effect, which states then in existence had ratified the treaty as of the end of the year.

4.4 Results

For each year from 1950 to 2008, W-NOMINATE produces a set of coordinates indicating the locations of each state and treaty within the space. The first issue to ad-

dress is the correct number of dimensions to include in the model. The crux of this question is as follows: While the underlying data can be analyzed in n-dimensions, how many of these dimensions are sufficiently substantively meaningful? Poole (2005) suggests that a preliminary determination can be made by plotting the normalized eigenvalues of the double-centered agreement score matrix produced by W-NOMINATE. Because the eigenvalues measure the fit of the underlying data, they are likely to flatten out when the dimensions are no longer meaningful. Figure 4.1 shows plots of eigenvalues against their dimensions for 1980, 1990, 2000 and 2008. The eigenvalues begin to flatten out after the third (or fourth in 2008) dimension, which suggests that there two meaningful dimensions are in the data. I continue the analysis using two-dimensional models.

Before analyzing the results, I first discuss the fit of the model to the treaty commitment data. The algorithm converges on estimated state and treaty locations when the probability of the observed treaty ratifications is maximized. The extent to which the model fits the data is therefore based on how well it predicts actual treaty ratification behavior. The model will not correctly predict all treaty ratifications, resulting in both false positives (i.e., cases where a country is predicted to ratify a treaty but does not do so) and false negatives (i.e., cases where a country is predicted not to ratify a treaty but nonetheless does so). Table 4.1 lists, for 2008, the ten countries with the most predicted ratifications, false positives, predicted non-ratifications and false-negatives. European states dominate the list of states with the most correctly predicted treaty ratifications. Note that these are not simply states that ratify many treaties, but also ones that *predictably* ratify such treaties. This is intuitively not surprising, especially as these countries are often at the forefront of setting the agenda for international lawmaking. States that predictably ratify few treaties include small island states, extremely poor states such as Somalia and Equatorial Guinea, and relatively new states such as East Timor. These states may have little incentive to participate in the institutions created by many of these treaties. Interestingly, several states appear to have both many false positives and false negatives, including the United States, Switzerland and Cuba. This suggests that the model is relatively weak at predicting the treaty ratification behavior of these states, i.e., these states tend to be the most idiosyncratic in terms of treaty ratification.

Three standardized measures of fit have been developed to compare results using multidimensional scaling methods (Poole and Rosenthal 1997; Poole 2005). The first is the percentage of choices included in the underlying data that are correctly classified by the model. This measure gives an overall sense of how well treaty ratifications fit each of the dimensions and provides an indication of the extent to which the second dimension is significant relative to the first. One limitation of this measure, however, is that it does not take into account the underlying distribution of 1s and 0s in the data, which is likely to be uneven. This problem is addressed by a second measure, the aggregate proportional reduction in error (APRE), which provides the percentage reduction in classification errors provided by W-NOMINATE relative to a model that assumes all states ratify the same treaties as the majority of states. The APRE is calculated by dividing (1) the sum of all minority choices subtracted by classification errors; by (2) the sum of minority choices. A highly effective method of determining the effect of adding dimensions to the model involves subtracting the APRE for a one-dimensional model from the APRE for a two-dimensional model, which controls for the size of the majority and provides a measure of the net benefit of adding the second dimension. Finally, the geometric mean probability (GMP) reflects how well each state's actual choices reflect those predicted by the model. The GMP is calculated by taking the exponential of the average log-likelihood of observed decisions.

Table 4.2 provides the measures of fit for several years of the treaty data. For comparison, Table 4.3 provides comparable measures provided by Poole and Rosenthal (1997) for the U.S. House of Representatives, Hix, Noury and Rolan (2006) for the European Parliament, and Voeten (2000) for the UNGA. The first dimension appears to explain about 82% of the variance in the treaty, whereas the second dimension explains only an additional 2%. Most importantly, this suggests the first dimension is meaningful and predicts treaty commitment to a significant extent. This also suggests that the second dimension is not especially significant and minor differences in states' locations along that dimension may not be particularly meaningful. The measures of fit are comparable to those of the other data, especially the Congressional data. It is notable that the APRE2-APRE1 statistic is significantly larger for the treaty data than for the U.S. House and UNGA, which means that adding the second dimension to the model does

more to improve fit with respect to the treaty commitment data. Nonetheless, the decline in this statistic over time suggests the second dimension has become less important over time.

Interpreting ideal point estimation results can be complex. There are two related aspects to doing so. The first is interpreting the meanings of the dimensions. Second, there may be particularly interesting cleavages in the space that cross multiple dimensions. The coordinates themselves do not directly reveal the answers to these questions, although there are several techniques that can be used to answer them.

Because W-NOMINATE is a spatial model, and the coordinates it produces are only meaningful relative to each other, it is useful to begin by analyzing the results visually. This approach is to some extent a subjective interpretation, but it must rely on an underlying knowledge of the states and treaties. Consider the analogy to the U.S. Congress: simple visual inspection reveals that the key cleavage in the preference space is between Democrats and Republicans. While it is unlikely that the treaty preference space will be so clearly defined, it may be possible to see specific patterns emerge. This is especially true of categorical variables, such as region and civilization. The notion that culture determines states' preferences does not offer specific predictions regarding where different regions or civilizations should be in relation to each other in the treaty space, but rather that a state's location will be significantly determined by its culture. This suggests that states should be clustered by region or civilization, and this clustering may be apparent visually. Likewise, clustering among the key rivals during the Cold War would support the notion that this conflict was crucial in shaping treaty commitment preferences.

A second way to interpret the W-NOMINATE results is to analyze the movement of states within the space over time. If states known to have transitioned in important ways over this period move significantly along a given dimension, this might suggest that the particular form of transition is correlated with the dimension. For example, if states were to move along the first dimension in the years after transitioning to democracy, this would support the notion that the first dimension is defined by regime type. A third statistical method for analyzing the W-NOMINATE dimensions is by plotting a known characteristic of the states as a normal vector in the preference space (Kruskal

and Wish 1976; Borg and Groenen 1997; Poole 2005). Extending a vector from the origin through the coordinates of the normal vector yields a line that indicates that, as states move along that line through the two-dimensional space, they tend to have larger values of the characteristic in question. Likewise, upon reflecting that line over the origin, states that are further from the origin tend to have lower values of that characteristic. If a normal vector is close to parallel to one of the axes, this indicates that the underlying characteristic is correlated to the W-NOMINATE estimates for the that dimension.

For each treaty, W-NOMINATE estimates the position of a line in the two-dimensional space that best divides the states expected to ratify the treaty from those not expected to ratify it. In cases of false-negatives and false-positives, states will end up on the incorrect side of the cutting line. The closer a state is to the cutting line, the more uncertainty there is about whether or not the state would ratify the treaty. Analyzing the locations and angles of these cutting lines in the preference space is a useful way to interpret the space. First, cutting lines that are perpendicular to a dimension are useful for interpreting that dimension. If several treaties on a related subject are perpendicular to a dimension that means that states along one end of that dimension are likely to make different choices with respect to that subject matter than states on the other end of the dimension. Second, cutting lines are also useful for understanding cleavages in the space that are cut across multiple dimensions. Treaties with cutting lines along these cleavages can give a strong indication of what the cleavage means.

A final way to interpret the W-NOMINATE results is to regress the state coordinates on variables thought to predict treaty commitment. If, hypothetically, income is the first W-NOMINATE dimension, then a model that regresses the state first-dimension coordinates on income should find a significant relationship and have a relatively large model fit. This analysis is also useful for testing possible predictors of treaty commitment against each other.

As the discussion above suggests, some of these methods may be more useful with respect to certain variables. For continuous variables with reliable existing measures, such as income and population, the normal vector and regression analysis may be especially useful. For categorical variables, however, it may be more useful to visually determine whether there is any clustering of states in accordance with the categories.

This is likely to be the case for analyzing the effects of region and civilization. Other possible meanings of the space, including support for U.N. supranationalism, for which we do not have external measures, may be best analyzed using the treaty cutting lines.

4.4.1 Visualizing the Treaty Preference Space

Figure 4.2 shows the locations of states in a two-dimensional treaty preference space in 1980, 1990, 2000 and 2008. Because the first dimension explains much more of the variance than the second dimension, it is important to note that small differences along the second dimension may not be especially meaningful. The plots depict each state by region, which allows us to see any regional clustering. The plots also specify the locations of the five permanent members of the UN Security Council. Because region is a categorical variable, effects of this variable on the locations of states within the preference space may be most effectively analyzed visually. Several aspects of these results are worth noting. First, there is some regional clustering, especially among the European states, which tend to be in the northeast of the space in 1980 and 1990 and move toward the east more recently. Other regions are less tightly clustered, except for a group of Asian states toward the western area of the space in 2000 and the southwest in 2008. Nonetheless, the regional clustering appears to be less stark than that found by Voeten (2000) for the UNGA. Most regions overlap significantly with each other.

Second, there are important changes in the structure of the preference space over time, although the changes are not especially dramatic. As of 2000, the European states have moved in the space from the northeast to the east, suggesting there may have been a change in the meaning of the second dimension during the 1990s. In addition, the great powers have generally moved southward along the second dimension over time, which suggests a change in the meaning of this dimension. Third, a significant cleavage existed in the space during 1980 and 1990 running at approximately the $x = y$ line. This cleavage does not appear to divide states along regional lines. The cleavage appears to be weaker in 2000 and to have dissipated as of 2008. Finally, during the Cold War the Western great powers do appear to be separated from the U.S.S.R. and China along the second dimension. This provides only minor support for the importance of the Cold War in the treaty preference space, however, because these states are not at opposite poles of

the space.

Because civilization is also a categorical variable, its effects on the location of states in the treaty preference space can also be analyzed for visual clustering. Figure 4.3 shows the locations of states in the treaty preference space coded by civilization. The civilizations are coded according to the categories and map provided by Huntington (1997, p.xx). There appears to be some civilization-based clustering, but it is not stark. There are two clusters of Latin American states, for example, one on each side of the $x = y$ cleavage in 1980, 1990 and 2000. Western states also cluster in two groups on either side of this cleavage. As of 2008, states from most civilizations are spread fairly widely across the space. Overall, these plots provide little support for the notion that treaty commitment preferences are based on civilization.

An additional aspect that can be analyzed graphically is the possible division between members of NATO and the Warsaw Pact during the Cold War. Figure 4.4 shows the locations of the members of these alliances in 1980. NATO members are mostly clustered together, as are most Warsaw Pact members. Yet the two clusters are not particularly far apart in the space, especially along the first dimension. The results for other years during the Cold War are fairly similar. If the Cold War were a primary determinant of treaty commitment preferences, we might expect to see the members of the two alliances in opposite sides of the space (e.g., Democrats and Republicans in the U.S. Congress). The results therefore suggest the Cold War was not a major factor in determining treaty commitment preferences.

4.4.2 Movement in W-NOMINATE Space Over Time

I continue the analysis of the treaty preference space by examining the movement of key states along both dimensions over time. Figure 4.5 shows the movement of the five permanent members of the U.N. Security Council since 1960. In terms of the first dimension, most of the great powers are relatively stable over time. China, however, moves rapidly along the first dimension in the late 1970s. This period in China's history witnessed the takeover by Deng Xiaoping and the beginning of the reforms intended to modernize China's economy. During this era, China ratified many treaties intended to facilitate trade and other economic cooperation. Its movement along the first dimen-

sion toward the more developed economies during this era suggests the first dimension may reflect the extent of a state's interest in international economic cooperation. It is also remarkable how closely correlated the movements of the United States and China have been along the first dimension since 1980. This means that the United States and China have had similar preferences along the first dimension since 1980, which further suggests the dimension is more likely to be one of economic interests than factors such as regime type, region and civilization, along which the two powers clearly differ. Finally, the fact that the U.S. and U.S.S.R. are consistently on the same side of the first dimension weighs against the Cold War being a key determinant of treaty commitment preferences. Indeed, the lack of significant movement by the great powers after the end of the Cold War suggests this change in the structure of international relations did not have a significant impact on treaty commitment preferences. The great powers have moved significantly more along the second dimension over time. First, China moved far along this dimension in the late 1970s, again during the beginning of its economic reforms. Interestingly, this moved China away from the United States (and the other Western powers), in contrast with its movement along the first dimension. In the 2000s all of the great powers moved in the same direction along the second dimension, which may mean that the meaning of the second dimension changed during this time.

Other states have also moved significantly in the treaty preference space. Figure 4.6 shows, for each dimension, the movement over time of the four states that have moved the furthest along that dimension. The states that have moved further along the first dimension are Uruguay, Paraguay, Mali and South Korea. All of these states moved in the same direction, although at different times. Analyzing these periods in these states' histories may help to explain the first dimension. South Korea's major movement occurred in the late 1970s, during the Fourth Republic and the lead-up to the assassination of President Park Chung-hee. This was period of significant domestic oppression, but also of increased interest on the part of the South Korean government in multilateral economic cooperation, especially leading up to the normalization of relations between China and the United States. Uruguay's treaty preference shift also occurred in the late 1970s, in the middle of a period of civil-military dictatorship. The president during this period, Aparicio Méndez, instituted domestic economic reforms and began opening up

Uruguay's economy to cooperation with other states. Mali's significant movement along the first dimension occurred in 1967, the year in which Mali reformed many domestic economic policies and rejoined the Franc currency zone. This also immediately preceded a military coup the following year. In 1967, Mali ratified many treaties intended to facilitate economic cooperation. Paraguay's movement along the first dimension has been more gradual. During this period, Paraguay experienced a transition to democracy as well as significant increases in trade and income, so the Paraguay case is less informative as to the meaning of the first dimension. Nonetheless, the cases of Uruguay, South Korea and Mali indicate that as states seek to open up economic relations with the world their treaty commitment preferences tend to change such that they move in a single direction along the first dimension.

With respect to the second dimension, the most significant movement has been by China, Iran, Australia and Bahrain. The changes with respect to China's treaty commitment preferences are discussed above. Iran moved significantly along the second dimension in the mid 1970s and again in the early 2000s. The first change coincides with the Shah's abolition of opposition parties. Among other things, this made it easier for the Shah to obtain legislative approval of treaties he had previously signed, and thus Iran ratified many treaties immediately after the creation of one-party rule. In the second period, Iran was ruled by the reformer Mohammad Khatami, who initiated many economic and political reforms, including opening up Iran to international cooperation. Australia is an interesting case, as its most significant movement occurred in 1988 under a newly elected government. The prior election had been called early and featured an unusual double dissolution, such that all seats in the legislature were up for election. The Labor Party consolidated power during the election, which likely means it was able to pass the ratifications of many treaties it did not have the votes to pass earlier. Bahrain is perhaps the oddest case because it moves significantly in one direction, then immediately back in the other direction. This occurred during the 1980s, a particularly tumultuous period that included an attempted Islamist coup and ongoing political uncertainty caused by the Iran-Iraq war. It may be the case that, as a result, Bahrain's treaty ratification behavior was relatively erratic during this period.

4.4.3 Analysis of Normal Vectors

In this section, I interpret the treaty preference space by using normal vector analysis. The first step is to estimate the following ordinary least squares model:

$$Y = \beta_1 X_1 + \beta_2 X_2 + \varepsilon \tag{4.1}$$

where Y is a vector of country-year data (e.g., trade, GDP), X_1 is the vector of first-dimension W-NOMINATE coordinates and X_2 is the vector of second-dimension W-NOMINATE coordinates. The coordinates (x, y) of the normal vector are obtained using the following equations:

$$x = \frac{\beta_1}{\sqrt{\beta_1^2 + \beta_2^2}} \tag{4.2}$$

$$y = \frac{\beta_2}{\sqrt{\beta_1^2 + \beta_2^2}} \tag{4.3}$$

I calculate the coordinates of the normal vectors for several variables that may illuminate the meaning of the preference space. For regime type, I use the data from the Polity IV project (Marshall and Jaggers 2002). As a measure of state power, I use the Correlates of War capabilities index (CINC). For trade and GDP per capita, I use the data provided by Gleditsch (2002). I take the natural logarithm of these three measures. Finally, I use the measures of Affinity toward the U.S. and U.S.S.R. developed by Gartzke (2006) as indicators of preference similarity to the U.S. and U.S.S.R. These measures are not all available after the year 2000, so this method cannot be used to analyze the preference space after that.

Figure 4.7 shows the normal vectors for several variables in 1980, 1990 and 2000. The vectors demonstrate that states toward the northeast of the space tend to be rich, powerful, democratic and heavily engaged in international trade. By contrast, states that are poor, weak, autocratic and relatively autarkic tend to be in the southwest of the space. That the vectors are so close to each other is likely due to these variables being highly correlated with each other. This trend is fairly consistent over time, but as of 2000 the vectors for power, regime type and trade move away from the $x = y$ line and flatten out. There are several implications of these results. First, these two types

of states tend to prefer different types of multilateral treaties, which suggests that all of these underlying variables may be important, although because of their correlation with each other we cannot determine their independent effects using this method. Second, the flattening out of several of the vectors in 2000 suggests that these factors, and especially trade, are now closer to the meaning of the first dimension and may play more important roles in determining treaty commitment preferences.

To further test the effects of the Cold War, Figure 4.8 shows the normal vectors for the U.S. and U.S.S.R. Affinity scores in 1960, 1970 and 1980. The vectors point in opposite directions because states that tended to have high Affinity scores for the U.S. tended to have low Affinity scores for the U.S.S.R., and vice versa. In 1960, the vectors are close to parallel to the second dimension, with states closer to the U.S. toward the north of the space and those favoring the U.S.S.R. toward the south. This suggests the Cold War may have been the second dimension of treaty commitment preferences during this era. As the Cold War went on, however, the normal vectors move toward the $x = y$ line, which suggests that, as of 1980, Cold War dynamics were less important in determining treaty commitment preferences.

4.4.4 Analysis of Treaty-Space Cleavages

I begin the cutting-line analysis by focusing on the main cleavage in the treaty preference space before 2000. The treaty cutting lines that fit this cleavage best are those for the Convention on the Privileges and Immunities of the Specialized Agencies and several annexes to that convention relating to specific U.N. bodies. This convention and its applicable annexes must be ratified by states that join the relevant U.N. organizations. That is, if a state joins UNESCO, it must ratify the convention and the UNESCO annex. As a result, these treaties function as proxies for membership in these organizations. The states in the north-western portion of the space were not members, while those in the south-eastern potion were members (subject to classification error). Certain of these organizations–particularly UNESCO–were controversial during the 1970s and 1980s (Buehrig 1976). This suggests this line represents a cleavage between states generally in favor of broad U.N. authority and those opposed to it. Figure 4.9 plots the cutting line for this convention, with states expected to oppose U.N. authority to the northwest

of it and those expected to favor it to the southeast. Over time, the line moves toward the northwest of the space with more states joining the camp that favors broad U.N. authority. In addition, more states are close to the cutting line in more recent years, which means the cleavage resulting from differing preferences with respect to U.N. authority is less salient now that it previously was.

The cutting-line analysis is a useful tool for evaluating whether the legalization of human rights is a key factor in shaping treaty commitment preferences. Figure 4.10 shows the cutting line for the International Covenant on Civil and Political Rights (ICCPR), one of the key instruments of international human rights law. The cutting line for the International Covenant on Economic, Social and Cultural Rights (ECOSOC) is very similar. States expected to ratify the ICCPR are to the right, while those expected not to ratify are to the left. For 1990, 2000 and 2008, Figure 4.10 also shows the cutting line for the Optional Protocol to the ICCPR. The Optional Protocol establishes an individual complaint mechanism for government abuses of human rights, so ratification of the protocol is generally seen as a deeper commitment to international human rights law. The Optional Protocol cutting line is always to the right of the ICCPR cutting line. States to the right of the Optional Protocol line are those expected to ratify it, meaning they have a strong preference for international human right law. States between the two lines are expected to ratify the ICCPR, but not the Optional Protocol, and thus have a middle-range commitment to international human rights law. States to the far left are those not in favor of legalizing commitments to respect human rights. This analysis reveals two points regarding the relevance of human rights legalization to the treaty preference space. First, the cutting lines for the ICCPR and the Optional Protocol do not fall within significant cleavages in the space, which means preferences toward the legalization of human rights is not a key determinant of treaty commitment preferences more generally. Second, the leftward movement of both the ICCPR and Optional Protocol cutting lines from 1990 to 2008 means that many states changed their preferences in favor of human rights legalization during this time. As of 1990, only a set of European states, which are generally the strongest supporters of human rights law, could be expected to ratify the Optional Protocol. By 2008, however, the cutting line for the Optional Protocol divides states fairly evenly.

I continue this analysis by determining which treaty cutting lines are closest to perpendicular to the first dimension. Doing so may reveal a pattern that explains the meaning of the first dimension. I focus here only on the first dimension because the second dimension explains relatively little variance, and there are few treaties close to perpendicular to it. In 1980, the treaties with cutting lines closest to perpendicular are the Convention on the Privileges and Immunities of the United Nations (CPIUN), the Convention for the Suppression of the Traffic in Persons and of the Exploitation of the Prostitution of Others (CSTE), the Convention on Consent to Marriage, Minimum Age for Marriage and Registration of Marriages (CMMR), and the Convention on the Political Rights of Women (CPRW). In 1990, these treaties were the Vienna Convention on Diplomatic Relations (VCDR), the Convention on the High Seas (CHS), the Convention on Road Traffic (CRT), and the Convention on the Elimination of Discrimination Against Women (CEDAW). In 2000, the treaties are the ECOSOC, CMMR, the International Convention on the Elimination of All Forms of Racial Discrimination (CERD), the Convention on the Taxation of Road Vehicles for Private Use in International Traffic (RVIT) and the International Convention to Facilitate the Importation of Commercial Samples and Advertising Material (CSAM). Finally, in 2008 they are the CSTE, Customs Convention on the International Transport of Goods under Cover of TIR Carnets (TIR), and the Convention on the Recognition and Enforcement of Foreign Arbitral Awards (CRFAA).

What do these treaties tell us about the meaning of the first dimension? For all of them, predicted ratifiers are on the right, and predicted non-ratifiers are on the left of the space. Thus, generally speaking, the first dimension separates states more likely to ratify these treaties from those less likely to do so. It may not be immediately obvious what these treaties substantively have in common, but they do seem to have two areas of overlap. The first is human rights, and especially women's rights, which are covered under the CPRW, CEDAW, CMMR and CSTE. Secondly, many of these treaties were created to facilitate various aspects of international economic cooperation and exchange, including the CRT, CHS, CSAM, RVIT and CRFAA. The Premable to the CSAM states, for example, that the treaty is intended to "promote the expansion of international trade." Thus, this analysis suggests that the preferences toward the legalization of women's

rights and international economic cooperation may be crucial to the meaning of the first dimension.

4.4.5 Regression Analysis

The final method I use to analyze the treaty preference space includes a series of regression models. Regression analysis can be useful for comparing the extent to which multiple variables that may explain treaty commitment fit the W-NOMINATE results. I begin by running OLS models using the state coordinates along each dimension as dependent variables. In each of these models, only one type of variable is included, and each model is run separately for each year from 1960 to 2000. The extent to which these variables fit the data can be analyzed by comparing the R^2 statistics of these models. Figure 4.11 shows the R^2 statistics of these models using the first dimension as the dependent variable. The models that include trade alone have a significantly better fit than any others, and this becomes increasingly so starting in the early 1970s. This indicates that, as globalization has increased and economic cooperation has become more important, states' preferences with respect to economic cooperation increasingly explain their treaty commitment preferences. Interestingly, capabilities are by far the weakest predictors of treaty preferences along this dimension. Several other factors, including regime type and income, fit the data fairly well, but because these variables are correlated with each other (and with trade), it is difficult to discern their individual impact from this analysis.

The second dimension also follows interesting patterns, as shown in Figure 4.12. In the 1960s, it is highly correlated with trade, although this correlation declines in the early 1970s, which is also the era in which the fit of trade with the first dimension improves. Civilization has the best fit with the second dimension. This can be difficult to interpret because it is a categorical variable, but based on the plots in Figure 4.3, it seems that Western, Orthodox and Latin American states tend to be on the positive end of this dimension, whereas African, Sinic, Islamic and Buddhist states are mostly on the other end. This suggests that the meaning of the second dimension, especially during the 1960s and 1970s, may be related to cultural issues. As with the first dimension, capabilities are a poor predictor of treaty commitment preferences. To examine the

independent relationships between these variables and treaty commitment preferences, I estimate several additional OLS models. These models also use the state coordinates along each dimension as dependent variables, but include all of the variables analyzed in the models above. As above, I use the natural logarithms of per capita GDP and trade. For each dimension, I estimate models for 1960, 1970, 1980, 1990 and 2000 separately. The results of these models are shown in Tables 4.4 and 4.5.

For the first dimension, trade is the only consistent predictor of states' ideal points. This is the clearest evidence yet that the first dimension is closely related to states' preferences toward trade, as well as economic cooperation more broadly. The result is especially striking because treaties explicitly focused on issues such as reductions to barriers to trade or the opening of financial markets are generally not universal so are not included in the UNTC data. Nonetheless, the UNTC does include many universal treaties that facilitate economic cooperation, as discussed above. Another factor that consistently predicts treaty commitment preferences along the first dimension is the Sinic civilization. The result suggests that in earlier eras these states likely had significantly different treaty commitment preferences from the rest of the world. The magnitude of this coefficient decreases over time, and it is no longer significant in 2000, implying that the preferences of China and the other Sinic states may have changed during their increasing integration into the global economy. The democracy variable becomes significant as of 2000, which means that in the current era regime type may also play a key role in shaping treaty commitment preferences. Consistent with the results above, state capabilities are not significant in these models, which confirms that power is not a key driver of treaty commitment preferences.

With respect to the second dimension, the two variables most consistently significant are the Western and Latin-American civilizations. This is consistent with the visual finding that these states tend to cluster on the north side of the preference space. Interestingly, Voeten (2000) finds that membership in these civilizations is significantly correlated with the first dimension of UNGA voting in the 1990s, whereas with respect to treaty preferences these variables are only significant for the second dimension. This suggests that the determinants of UNGA voting preferences differ substantially from those of treaty commitment preferences.

4.5 Conclusions

This paper has shown that the key determinant of multilateral treaty commitment preferences is the extent of states' interest in international economic cooperation. Tables 4.6 and 4.7 summarize the results of my analysis. While no single method can interpret the W-NOMINATE results conclusively, economics is by far the best and most consistent predictor of states' position along the key dimension of treaty commitment preferences. This finding would perhaps be less noteworthy if it were based on a data set consisting of preferential trade agreements, bilateral investment treaties and other instruments explicitly related to economics. Yet that is far from the case. My data set includes a wide range of treaties, including those governing human rights, the environment and arms control. The results therefore indicate that states' economic interests affect (and can often predict) their commitments to international cooperation in other substantive areas as well.

Several other factors do affect commitments to multilateral treaties, but none do so as clearly and consistently as economic interests. I have found some evidence, for example, that in the current era treaty commitment preferences are also shaped in part by regime type. This supports a wide array of literatures that argue that democratic regimes are likely to have different preferences than autocracies and that democratic dyads are more likely to cooperate successfully. Likewise, cultural differences do appear to affect treaty commitment preferences, although this appears to be on a second dimension that explains relatively little behavior as compared to economics. Finally, states' varying degrees of willingness to commit to the legalization of human rights, and especially to women's rights, appear to significantly affect states' ideal points with respect to treaty commitments.

Other factors we might expect to be key determinants of treaty commitment preferences – and international cooperation more generally – do not appear to have significantly influenced which treaties states ratify. Cold War alignment appears to vary along the second dimension, yet members of both NATO and the Warsaw Pact tend to be on the positive end of this dimension. The broader meaning of this dimension, especially during the Cold War era, appears to have been a distinction between states with a Judeo-Christian background and others. An important cleavage persisted in the treaty

preference space defined by conflicting views with respect to the granting of increased authority to the U.N. Yet, in the recent era, the significance of this cleavage appears to have dissipated. This is not to say that states are now in agreement on this point, but rather that the evidence indicates their differences on this point do not affect treaty commitment choices generally. Finally, it is striking how little national capabilities appear to affect treaty commitment preferences. I have found no evidence that indicates that more powerful states systematically prefer to ratify different treaties than weaker states. Indeed, there is little in the data to suggest either that states use multilateral treaties to balance against powerful states or to bandwagon with individual major powers.

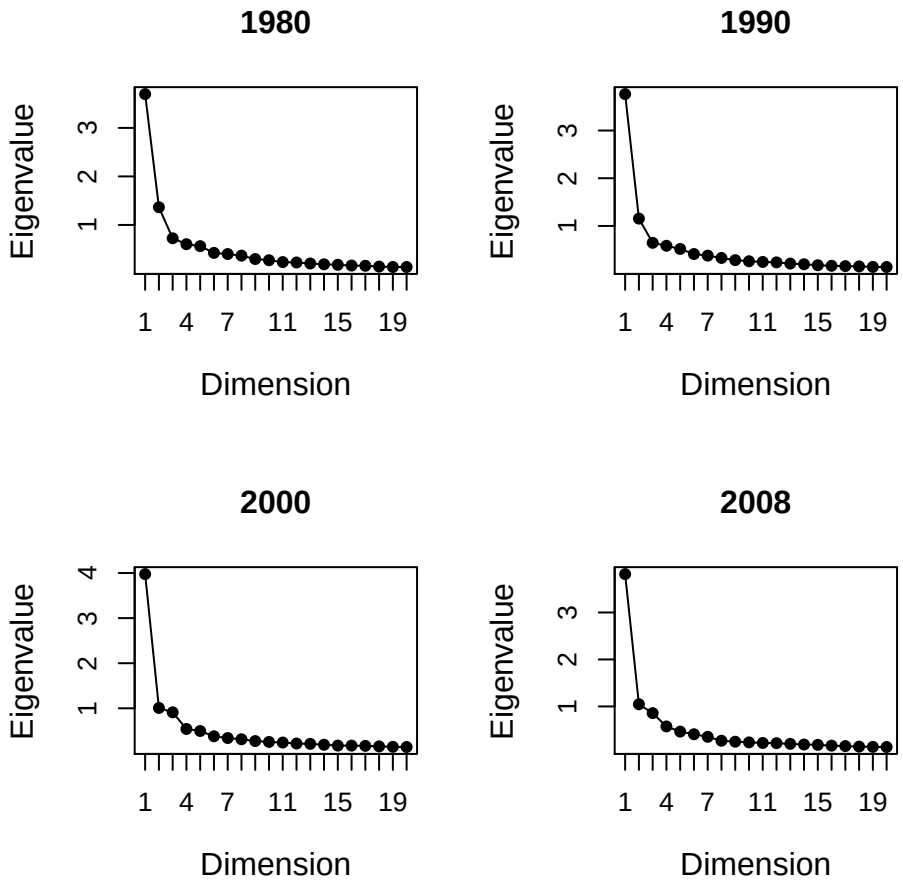
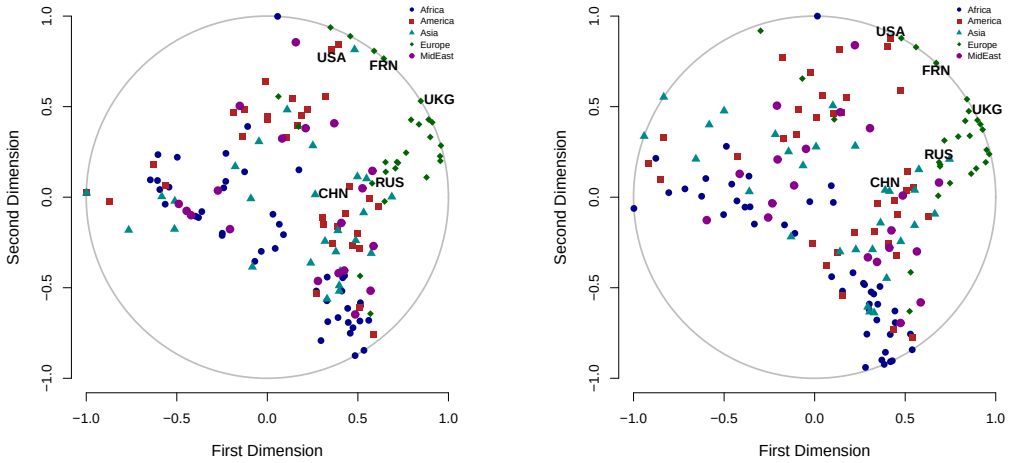
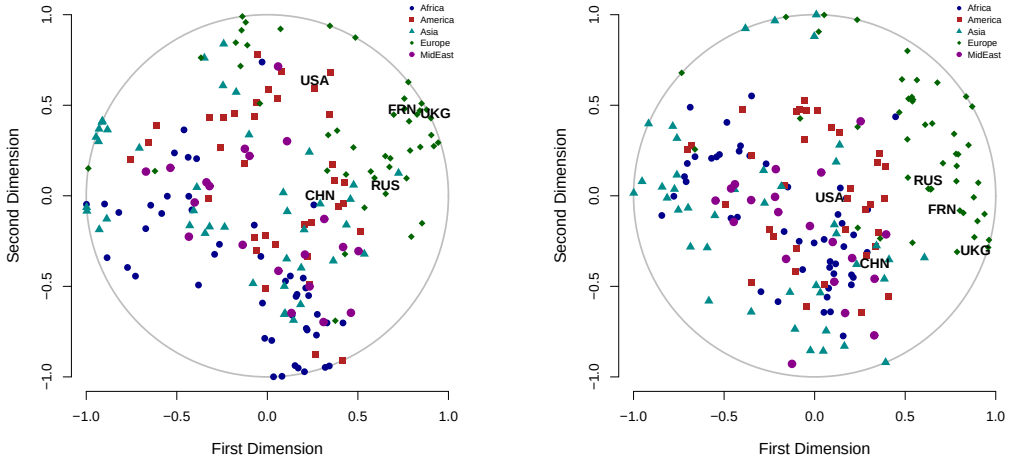


Figure 4.1: Scree plots of W-NOMINATE results.

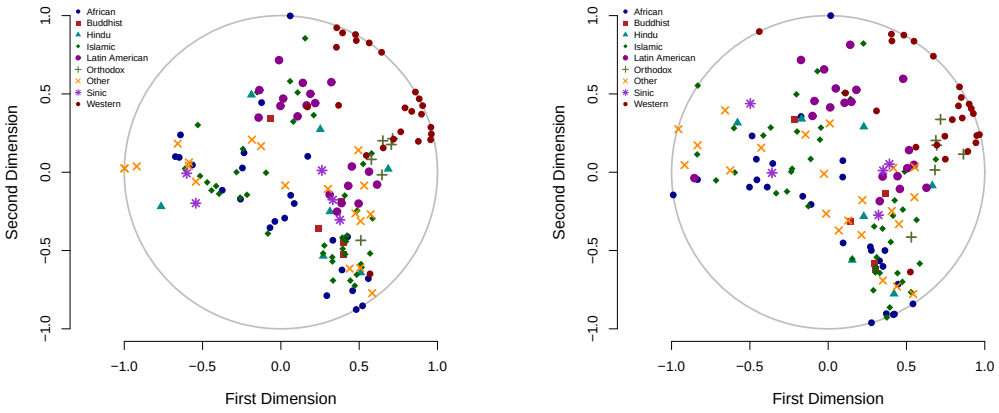


(a) 1980 (b) 1990

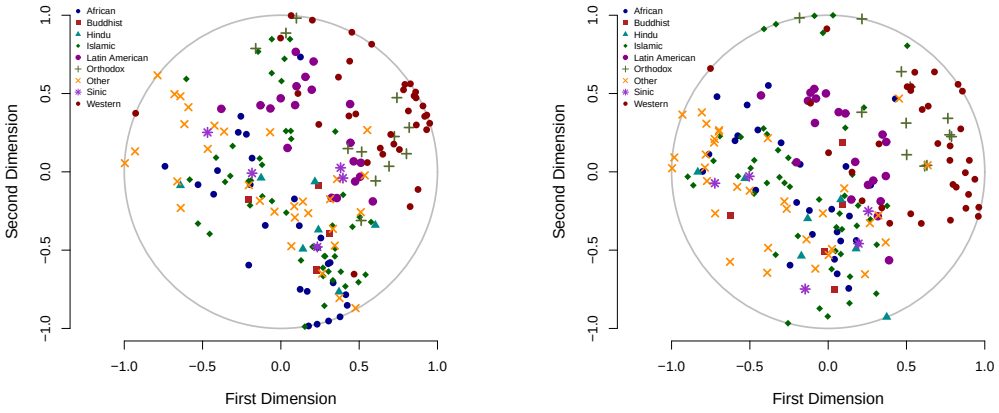


(c) 2000 (d) 2008

Figure 4.2: W-NOMINATE coordinates by region. The locations of the five permanent members of the U.N. Security Council are noted.



(a) 1980 (b) 1990



(c) 2000 (d) 2008

Figure 4.3: W-NOMINATE coordinates by civilization

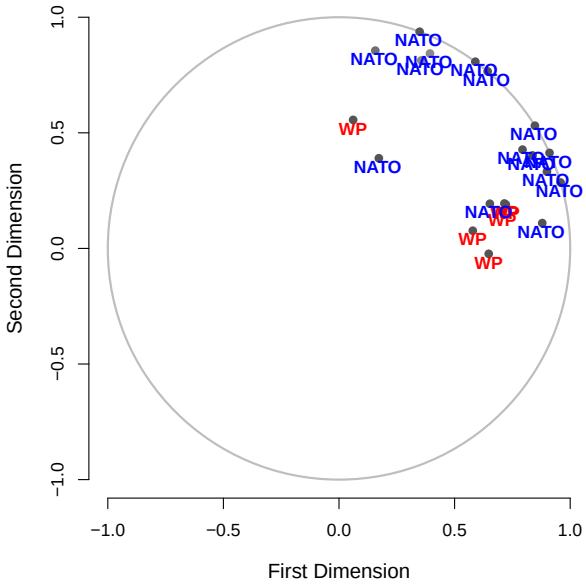


Figure 4.4: W-NOMINATE coordinates of NATO and Warsaw Pact members in 1980

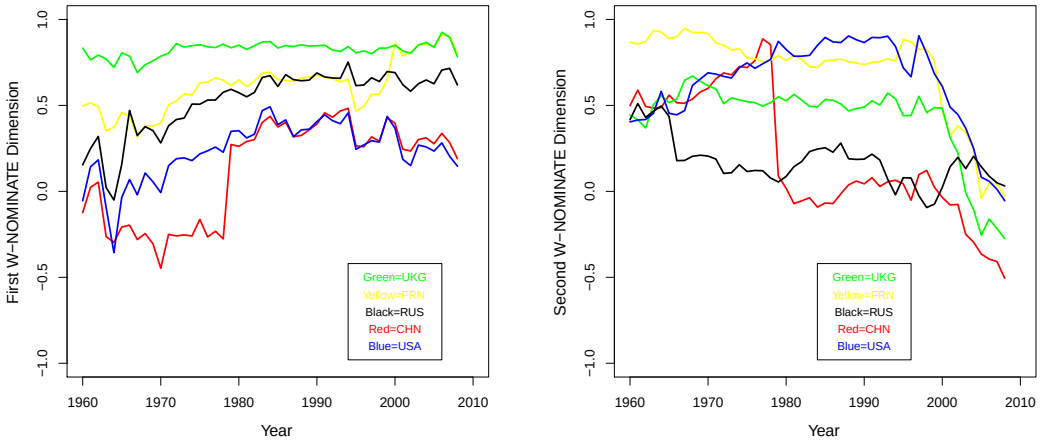


Figure 4.5: Movement of the great powers along the W-NOMINATE dimensions 1960-2008.

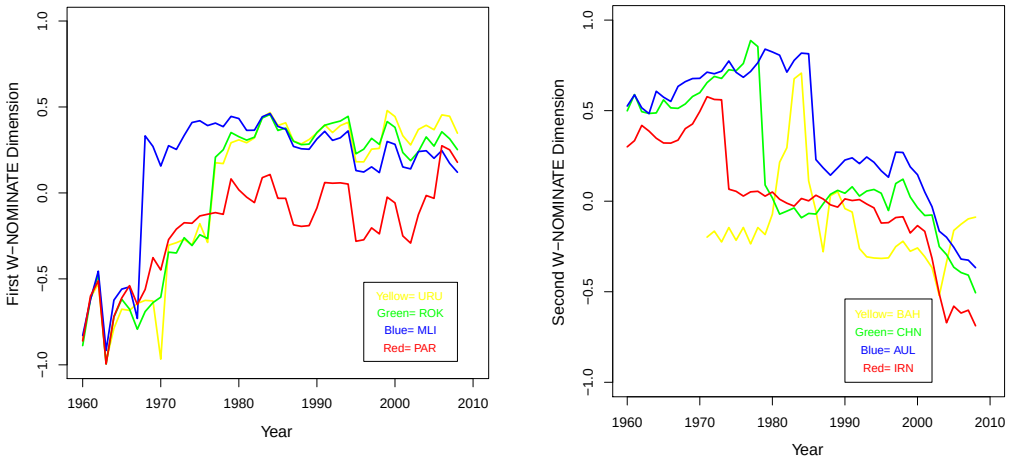
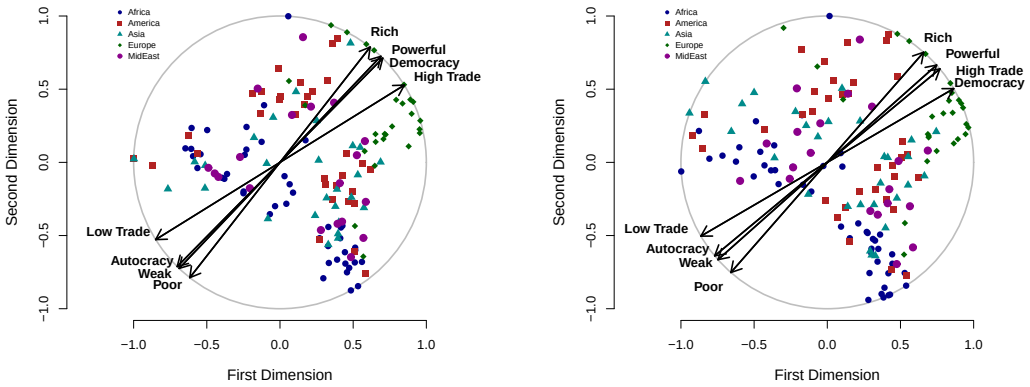
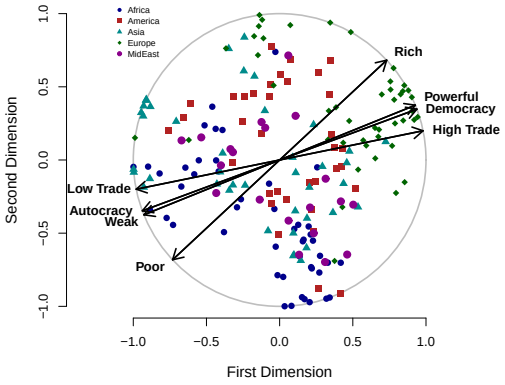


Figure 4.6: Positions of states with the most movement along the W-NOMINATE dimensions 1960-2008.



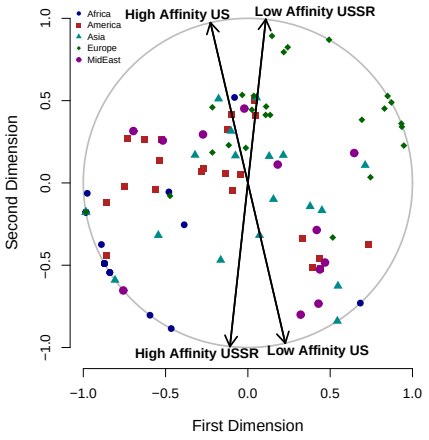
(a) 1980

(b) 1990

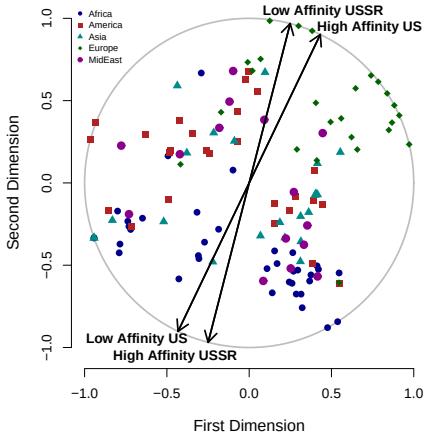


(c) 2000

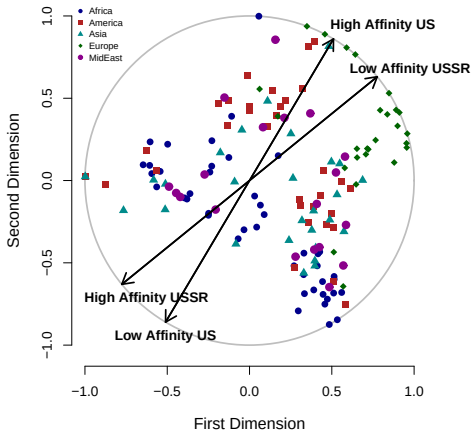
Figure 4.7: Normal vectors for income, regime type, trade and power in 1980, 1990 and 2000.



(a) 1960

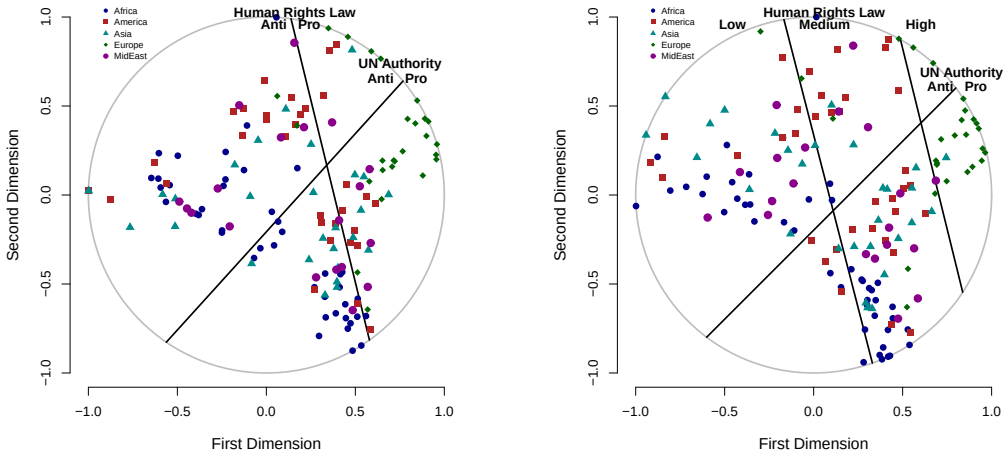


(b) 1970



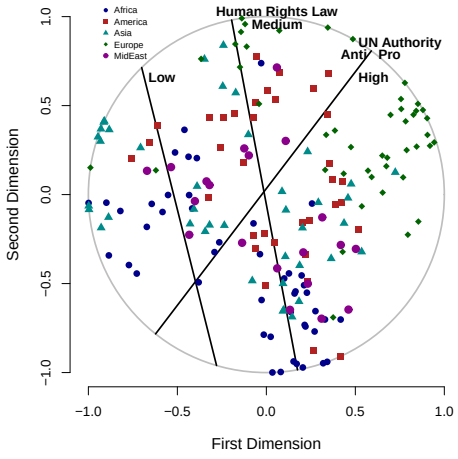
(c) 1980

Figure 4.8: Normal vectors for U.S. Affinity and U.S.S.R. Affinity in 1960, 1970 and 1980

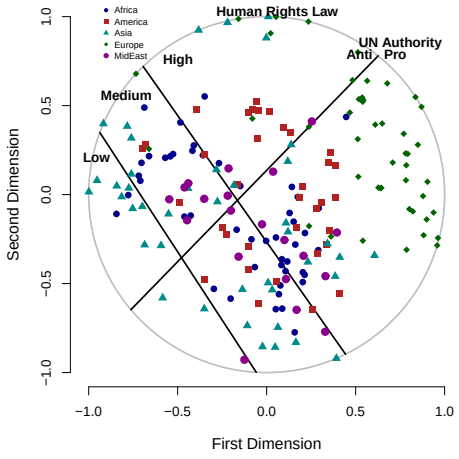


(a) 1980

(b) 1990



(c) 2000



(d) 2008

Figure 4.9: Cleavages for U.N. Authority and International Human Rights Law

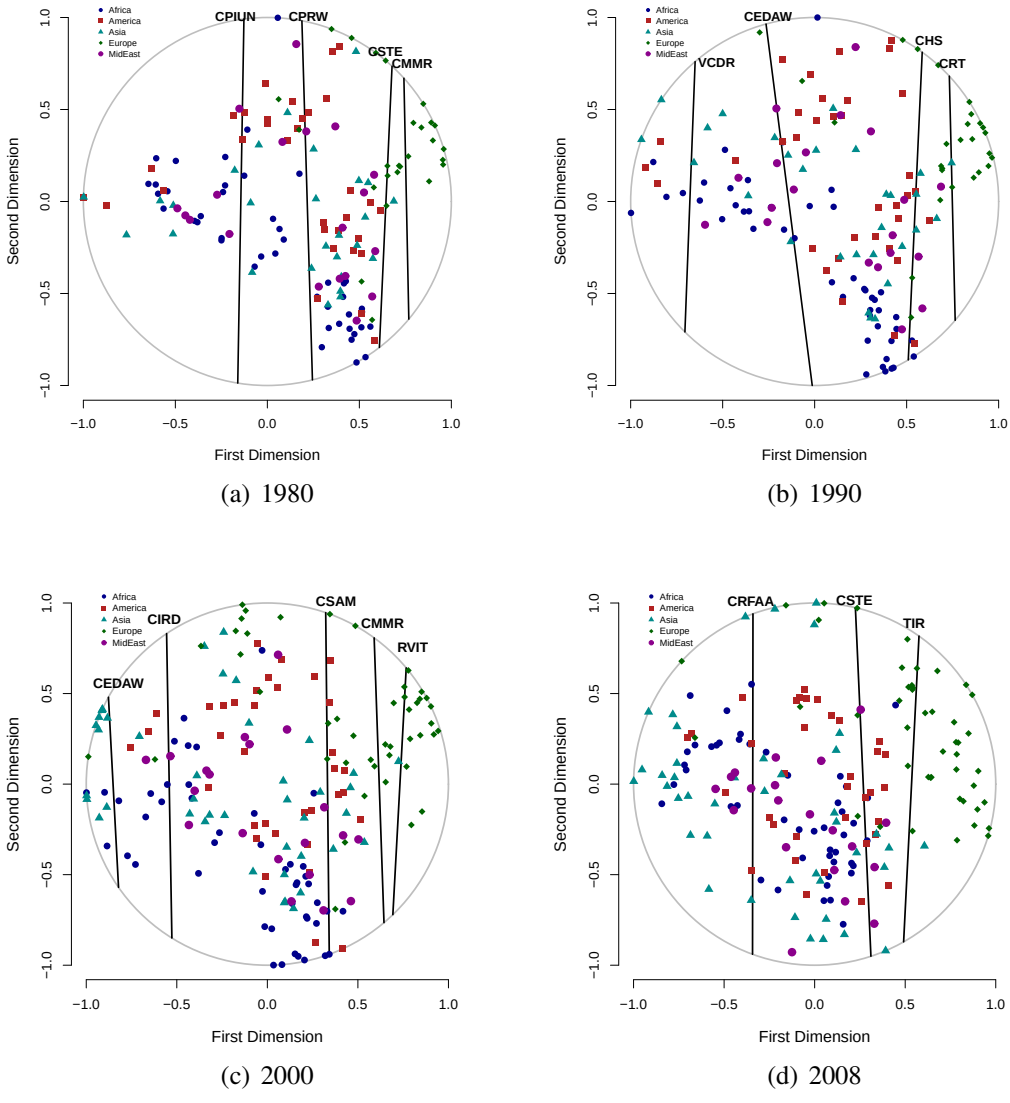


Figure 4.10: Cutting lines for treaties perpendicular to the first dimension

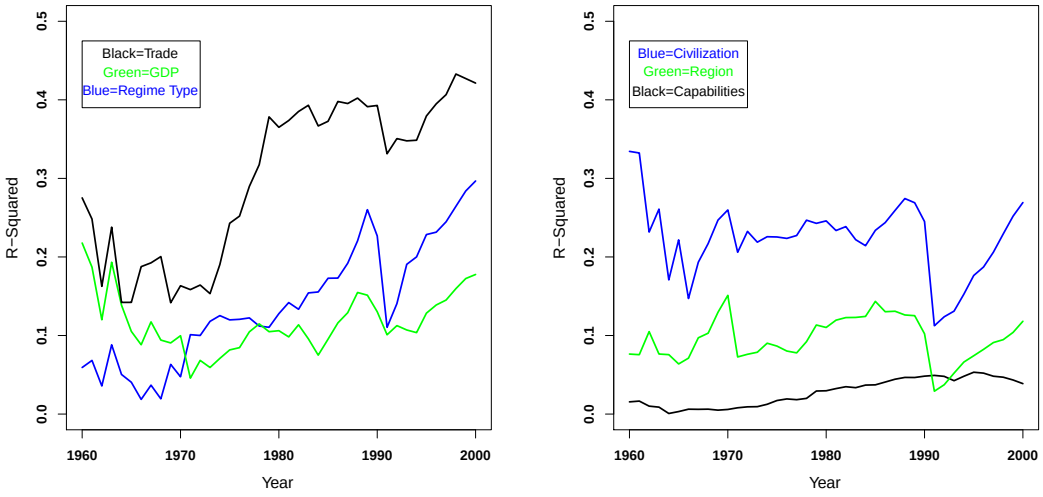


Figure 4.11: Fits of competing OLS models of W-NOMINATE first dimension

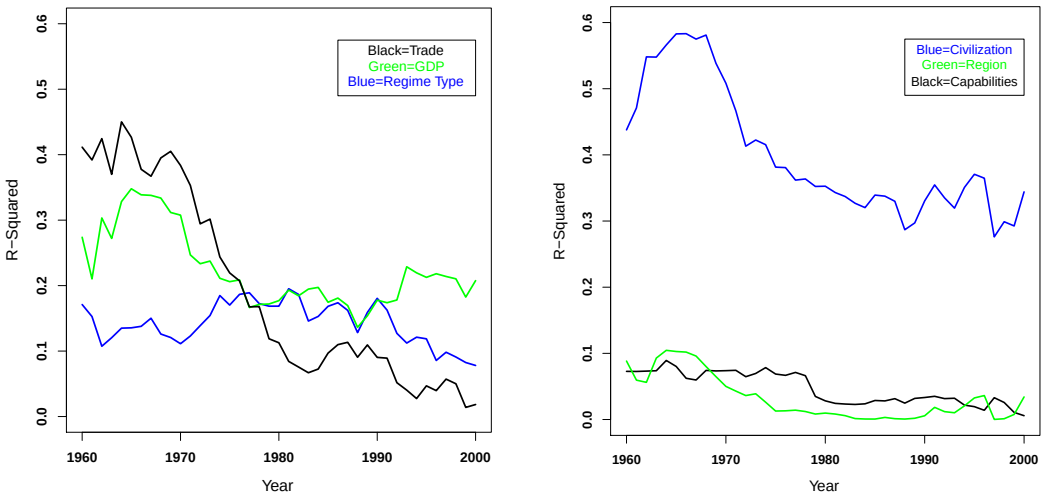


Figure 4.12: Fits of competing OLS models of W-NOMINATE second dimension

Table 4.1: Top 10 States by W-NOMINATE Classification Results in 2008

Most Ratifications Correctly Predicted	**Most False Positives**	**Most Non-Ratifications Correctly Predicted**	**Most False Negatives**
Netherlands	Montenegro	Tuvalu	Liberia
Norway	United States	Palau	Mexico
Denmark	Luxembourg	Bhutan	United States
Sweden	Switzerland	Micronesia	Cuba
Finland	Liberia	East Timor	Sri Lanka
Germany	Russia	Marshall Islands	Switzerland
Belgium	Cuba	Brunei	Montenegro
Austria	Bosnia	Somalia	Panama
United Kingdom	Austria	Eritrea	Canada
Slovakia	Czech Republic	Equatorial Guinea	Uzbekistan

Table 4.2: Measures of Fit for Universal Treaty Data

Year	**Classification % 1 dim**	**Classification % 2 dim**	**APRE1**	**APRE2**	**APRE2-APRE1**	**GMP1**	**GMP2**
1960	86.5	89.5	25.3	41.9	16.6	0.71	0.78
1970	81.8	84.4	26.9	37.5	10.6	0.66	0.71
1980	82.5	84.6	27.4	36.1	8.7	0.68	0.71
1990	82.6	84.2	27.3	34.2	6.9	0.67	0.70
2000	83.2	84.7	24.6	31.5	6.9	0.67	0.70
2008	82.5	84.3	21.8	29.7	7.9	0.67	0.70

Table 4.3: Measures of Fit for Other Data

Data	**Class. % 1**	**Class. % 2**	**APRE1**	**APRE2**	**APRE2-APRE1**	**GMP1**	**GMP2**
U.S. House 1960	82.7	84.4	47.9	53.1	5.2	.68	.70
Eur. Parl. 1979-84	86.0	91.5	46.9	67.6	20.7	—	—
UNGA 1991-1996	91.8	93.0	62.1	67.7	5.6	—	.83

Table 4.4: OLS Models of First-Dimension W-NOMINATE Coordinates

Variable	**1960**	**1970**	**1980**	**1990**	**2000**
Democracy	-0.010	-0.003	0.003	0.007	0.013**
	(0.008)	(0.007)	(0.005)	(0.006)	(0.005)
Capabilities	-0.540	-1.465	-2.237	-1.850	-1.071
	(1.852)	(1.814)	(1.541)	(1.791)	(1.403)
Per Capita GDP (logged)	0.092	-0.007	-0.112*	-0.106*	-0.027
	(0.100)	(0.076)	(0.047)	(0.053)	(0.034)
Total Trade (logged)	0.084*	0.095**	0.134***	0.125***	0.098***
	(0.041)	(0.035)	(0.024)	(0.025)	(0.019)
Asia	-0.018	0.047	-0.252	-0.134	-0.077
	(0.306)	(0.207)	(0.154)	(0.154)	(0.122)
Europe	0.245	0.430	0.153	0.196	0.238
	(0.303)	(0.246)	(0.183)	(0.185)	(0.139)
MidEast	0.104	-0.004	-0.246	-0.167	-0.062
	(0.339)	(0.256)	(0.187)	(0.194)	(0.144)
Africa	-0.548	0.144	-0.131	-0.153	-0.069
	(0.333)	(0.246)	(0.182)	(0.184)	(0.141)
Western	-0.233	-0.176	-0.110	-0.109	-0.091
	(0.286)	(0.234)	(0.164)	(0.165)	(0.120)
Islamic	-0.349	-0.160	-0.107	-0.104	-0.041
	(0.201)	(0.179)	(0.128)	(0.130)	(0.090)
African	-0.246	-0.308	-0.163	-0.120	-0.023
	(0.255)	(0.204)	(0.151)	(0.153)	(0.112)
Latin	-0.512	-0.316	-0.226	-0.201	-0.138
	(0.327)	(0.214)	(0.160)	(0.161)	(0.128)
Orthodox	-0.670*	-0.315	-0.063	-0.017	-0.108
	(0.314)	(0.289)	(0.213)	(0.212)	(0.137)
Sinic	-0.873***	-0.718**	-0.371*	-0.365*	-0.243
	(0.254)	(0.227)	(0.174)	(0.178)	(0.144)
Constant	-0.834	-0.479	0.226	0.197	-0.397
	(0.655)	(0.514)	(0.354)	(0.392)	(0.274)
n	99	122	134	134	154
R^2	0.544	0.352	0.462	0.492	0.585

Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.5: OLS Models of Second-Dimension W-NOMINATE Coordinates

Variable	**1960**	**1970**	**1980**	**1990**	**2000**
Democracy	0.003	-0.001	0.009	0.007	-0.002
	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
Capabilities	0.614	1.909	2.914	4.013	3.494
	(1.339)	(1.219)	(1.667)	(2.083)	(1.988)
Per Capita GDP (logged)	-0.121	-0.072	-0.018	-0.005	0.090
	(0.072)	(0.051)	(0.051)	(0.062)	(0.048)
Total Trade (logged)	0.090**	0.031	-0.030	-0.032	-0.065*
	(0.030)	(0.023)	(0.026)	(0.029)	(0.026)
Asia	0.026	0.108	0.206	0.259	0.262
	(0.221)	(0.139)	(0.167)	(0.179)	(0.172)
Europe	0.253	0.239	0.169	0.288	0.285
	(0.219)	(0.165)	(0.197)	(0.215)	(0.197)
MidEast	-0.044	0.200	0.246	0.310	-0.072
	(0.245)	(0.172)	(0.202)	(0.225)	(0.204)
Africa	-0.120	-0.161	-0.089	-0.040	-0.247
	(0.240)	(0.165)	(0.197)	(0.214)	(0.199)
Western	0.477*	0.638***	0.731***	0.611**	0.710***
	(0.207)	(0.157)	(0.178)	(0.192)	(0.169)
Islamic	0.110	0.001	0.149	0.118	0.403**
	(0.145)	(0.120)	(0.139)	(0.151)	(0.127)
African	0.026	0.010	0.189	0.105	0.341*
	(0.184)	(0.137)	(0.163)	(0.178)	(0.159)
Latin	0.276	0.430**	0.641***	0.693***	0.821***
	(0.237)	(0.143)	(0.173)	(0.187)	(0.182)
Orthodox	0.146	0.178	0.256	0.135	0.558**
	(0.227)	(0.194)	(0.231)	(0.247)	(0.195)
Sinic	0.039	-0.010	0.039	0.186	0.215
	(0.184)	(0.152)	(0.188)	(0.207)	(0.204)
Constant	-0.062	0.016	-0.002	-0.128	-0.714
	(0.474)	(0.345)	(0.383)	(0.455)	(0.388)
n	99	122	134	134	154
R^2	0.572	0.637	0.455	0.430	0.516

Standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4.6: Summary of Results - Part 1

Method	**Economics**	**Regime Type**	**Power**	**Cold War**
Visual Inspection	—	—	—	Minor clustering by alignment
State Movement	States move along this dimension when opening up international economic relations	No evidence in favor	No evidence in favor	No evidence in favor
Normal Vectors	Trade is the closest normal vector to the first dimension in 2000	Regime type has moved closer to the first dimension in 2000	No evidence in favor	Affinity toward U.S. and U.S.S.R. divided states along second dimension during Cold War
Treaty Cut Points	Treaties that facilitate economic cooperation divide states along first dimension	—	—	—
Regression	Trade levels predict placement along the first dimension	Regime type predicts placement along first dimension in 2000	No evidence in favor	—

Table 4.7: Summary of Results - Part 2

Method	**Civilization**	**Region**	**UN Authority**	**Human Rights Law**
Visual Inspection	Clustering among Western and Latin states	Clustering among European states	—	—
State Movement	—	—	—	—
Normal Vectors	—	—	—	—
Treaty Cut Points	—	—	The key cross-dimensional cleavage in the space until the 1990s	Not a key cleavage. Preferences have changed in favor of legalization over time. Support for women's rights remains associated with the first dimension.
Regression	The second dimension tends to separate Western and Latin American states from others	No evidence in favor	—	—

Bibliography

Alker, Jr., Hayward. 1964. "Dimensions of Conflict in the General Assembly." *American Political Science Review* 59(3):642–57.

Alker, Jr., Hayward and Bruce M. Russett. 1965. *World Politics in the United Nations General Assembly*. Yale University Press.

Alter, Karen J. 2008. "Agents or Trustees? International Courts in their Political Context." *European Journal of International Relations* 14:33–63.

Apodaca, Clair. 2004. "The Rule of Law and Human Rights." *Judicature* 87:292–99.

Arceneaux, Kevin, Alan S. Gerber and Donald P. Green. 2006. "Comparing Experimental and Matching Methods Using a Large-Scale Voter Mobilization Experiment." *Political Analysis* 14:37–62.

Bagwell, Kyle and Robert W. Staiger. 1997*a*. "Multilateral Tariff Cooperation During the Formation of Customs Unions." *Journal of International Economics* 4:91–123.

Bagwell, Kyle and Robert W. Staiger. 1997*b*. "Multilateral Tariff Cooperation During the Formation of Regional Free Trade Areas." *International Economic Review* 28:291–319.

Bearce, David H. and Stacy Bondanella. 2007. "Intergovernmental Organizations, Socialization, and Member-State Interest Convergence." *International Organization* 61:703.

Beckfield, John. 2003. "Inequality in the World Polity: the Structure of International Organization." *American Sociological Review* 68(3):401–424.

Bennett, D. Scott and Allan C. Stam. 1999. "A Universal Test of an Expected Utility Theory of War." *International Studies Quarterly* 44(3):451–80.

Bentham, Jeremy. 1825. *A Treatise on Judicial Evidence*.

Berlin, Isaiah. 1969. Two Concepts of Liberty. In *Four Essays on Liberty*. Oxford University Press.

Blitt, Robert Charles. 2004. "Who Will Watch thew Watchdogs? Human Rights Nongvoernmental Organizations and the Case for Regulation." *Buffalo Human Rights Law Review* 10:261–398.

Boehmer, Charles and Timothy Nordstrom. 2008. "Intergovernmental Organization Memberships: Examining Political Community and the Attributes of International Organizations." *International Interactions* 34(3):282–309.

Borg, Ingwer and Patrick J.F. Groenen. 1997. *Modern Multidimensional Scaling: Theory and Applications*. New York: Springer.

Buehrig, Edward H. 1976. "Review: The Tribulations of UNESCO." *International Organization* 30:679–685.

Bueno De Mesquita, Bruce, George W. Downs, Alastair Smith and Feryal Marie Cherif. 2005. "Thinking inside the Box: A Closer Look at Democracy and Human Rights." *International Studies Quarterly* 49(3):439–57.

Bull, Jesse and Joel Watson. 2004. "Evidence Disclosure and Verifiability." *Journal of Economic Theory* 118:1–31.

Carroll, Royce, Jeffrey B. Lewis, James Lo, Keith T. Poole and Howard Rosenthal. 2009. "Comparing NOMINATE and IDEAL: Points of Difference and Monte Carlo Test." *Legislative Studies Quarterly* 34:555–91.

Clinton, Joshua, Simon Jackman and Douglas Rivers. 2004. "The Statistical Analysis of Roll Call Data: A Unified Approach." *American Political Science Review* 98:355–70.

Coase, R.H. 1974. "The Market for Goods and the Market for Ideas." *American Economic Review* 64(2):384–91.

Conrad, Courtenay R. 2011. "Constrained Concessions: Beneficent Dictatorial Responses to the Domestic Political Opposition." *International Studies Quarterly* 55:1167–87.

Conrad, Courtenay R. 2012. "Divergent Incentives for Dictators: Domestic Institutions and (International Promises Not to) Torture." *Journal of Conflict Resolution* .

Conrad, Courtenay Ryals and Will H. Moore. 2010. "What Stops the Torture?" *American Journal of Political Science* 54:459–76.

Cooter, Robert D. and Daniel L. Rubinfeld. 1989. "Economic Analysis of Legal Disputes and Their Resolution." *Journal of Economic Literature* 27:1067–97.

Cross, Frank B. 1999. "The Relevance of Law in Human Rights Protection." *International Review of Law and Economics* 19:87–98.

Dahl, Robert. 1966. *Political Opposition in Western Democracies*. New Haven, CT: Yale University Press.

Dai, Xinyuan. 2007. *International Institutions and National Policies*. New York: Cambridge University Press.

Damaska, Mirjan. 1973. "Evidentiary Barriers to Conviction and Two Models of Criminal Procedure: A Comparative Study." *University of Pennsylvania Law Review* 121:506–89.

Damaska, Mirjan. 1992. "Of Hearsay and Its Analogues." *Minnesota Law Review* 76:425–58.

Davenport, Christian. 1995. "Multi-Dimensional Threat Perception and State Repression: An Inquiry into Why States Apply Negative Sanctions." *American Journal of Political Science* 39:683–713.

Davenport, Christian. 1996. ""Constitutional Promises" and Repressive Reality: A Cross-National Time-Series Investigation of Why Political and Civil Liberties are Suppressed." *Journal of Politics* 58(3):627–54.

Davenport, Christian. 1999. "Human Rights and the Democratic Proposition." *Journal of Conflict Resolution* 43:92–116.

Davenport, Christian. 2004. "The Promise of Democratic Pacification: An Empirical Assessment." *International Studies Quarterly* 48(3):539–60.

Davenport, Christian. 2007*a*. "State Repression and Political Order." *Annual Review of Political Science* 10:1–23.

Davenport, Christian. 2007*b*. *State Repression and the Domestic Democratic Peace*. New York: Cambridge University Press.

Davenport, Christian. 2007*c*. "State Repression and the Tyrannical Peace." *Journal of Peace Research* 44(4):485–504.

Davis, Otto A., Melvin J. Hinich and Peter C. Ordeshook. 1970. "An Expository Development of the Mathematical Model of the Electoral Process." *American Political Science Review* 64:426–48.

Desposato, Scott. 2006. "Parties for Rent? Ambition, Ideology, and Party Switching in Brazil's Chamber of Deputies." *American Journal of Political Science* 50:62–80.

Desposato, Scott. 2008. "The Impact of Electoral Rules on Legislative Parties: Lessons from the Brazilian Senate and Chamber of Deputies." *Journal of Politics* 68:1018–30.

Donnelly, Jack and Rhoda E. Howard. 1988. "Assessing National Human Rights Performance: A Theoretical Framework." *Human Rights Quarterly* 10(2):214–48.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. Boston: Addison Wesley.

Downs, George W., David M. Rocke and Peter N. Barsoom. 1996. "Is the Good News About Compliance Good News About Cooperation?" *International Organization* 50:397–406.

Downs, George W., David M. Rocke and Peter N. Barsoom. 1998. "Managing the Evolution of Multilateralism." *International Organization* 52(2):397–419.

Downs, George W. and Michael A. Jones. 2002. "Reputation, Compliance, and International Law." *Journal of Legal Studies* 33:S94–S114.

Enelow, James M. and Melvin J. Hinich. 1984. *The Spatial Theory of Voting*. New York: Cambridge University Press.

Engel, Christoph. 2009. "Symposium: Emotions In Context: Exploring The Interaction Between Emotions And Legal Institutions: Preponderance of the Evidence versus Intime Conviction: A Behavioral Perspective on a Conflict between American and Continental European Law." *Vermont Law Review* 33:435–67.

Finnemore, Martha and Kathryn Sikkink. 1998. "International Norm Dynamics and Political Change." *International Organization* 52:887–917.

Franck, Thomas M. 1990. *The Power of Legitimacy Among Nations*. New York: Oxford University Press.

Franck, Thomas M. and H. Scott Fairley. 1980. "Procedural Due Process in Human Rights Factfinding by International Agencies." *American Journal of International Law* 74:304–345.

Garrett, Geoffrey and Barry R. Weingast. 1993. Ideas, Interests, and Institutions: Constructing the European CommunitySs Internal Market. In *Ideas and Foreign Policy: Beliefs, Institutions, and Political Change*, ed. Judith Goldstein and Robert O. Keohane. Cornell University Press p. 173.

Gartner, Scott Sigmund and Patrick M. Regan. 1996. "Threat and Repression: The Non-Linear Relationship between Government and Opposition Violence." *Journal of Peace Research* 33(3):273–87.

Gartzke, Erik. 2006. "The Affinity of Nations Index.".

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

Gleditsch, Kristian. 2002. "Expanded Trade and GDP Data." *Journal of Conflict Resolution* 46(5):712–24.

Goldsmith, Jack and Eric Posner. 2005. *The Limits of International Law*. New York: Oxford University Press.

Goodliffe, Jay and Darren G. Hawkins. 2006. "Explaining Commitment: States and the Convention Against Torture." *Journal of Politics* 68:358–71.

Gray, Mark M., Miki Caul Kittilson and Wayne Sandholtz. 2006. "Women and Globalization: A Study of 180 Countries, 1975-2000." *International Organization* 60:293–333.

Greenhill, Brian. 2010. *Norm Transmission in Networks of Intergovernmental Organizations*. Ph.D. Thesis, University of Washington.

Grieco, Joseph M., Christopher F. Gelpi and T. Camber Warren. 2009. "When Preferences and Commitments Collide: The Effect of Relative Partisan Shifts on International Treaty Compliance." *International Organization* 63(2):341–355.

Guo, Shenyang and Mark W. Fraser. 2010. *Propensity Score Analysis: Statistical Methods and Applications*. Thousand Oaks, CA: Sage.

Gurr, Ted. 1986. Persisting Patterns of Repression and Rebellion: Foundations for a General Theory of Political Coercion. In *Persistent Patterns and Emergent Structures in a Waning Century*, ed. Margaret Karns. Praeger pp. 79–106.

Hafner-Burton, Emilie M. 2008. "Sticks and Stones: Naming and Shaming the Human Rights Enforcement Problem." *International Organization* 62:689–716.

Hafner-Burton, Emilie M. 2012. "International Regimes for Human Rights." *Annual Review of Political Science* 15:1–24.

Hafner-Burton, Emilie M. and Kiyoteru Tsutsui. 2005. "Human Rights in a Globalizing World: The Paradox of Empty Promises." *American Journal of Sociology* 110:1373–1411.

Hafner-Burton, Emilie M. and Kiyoteru Tsutsui. 2007. "Justice Lost! The Failure of International Human Rights Law to Matter Where Needed Most." *Journal of Peace Research* 44:407–25.

Hallerberg, Mark. 2002. "Veto Players and the Choice of Monetary Institutions." *International Organization* 56(4):775–802.

Hathaway, Oona. 2002. "Do Human Rights Treaties Make a Difference?" *Yale Law Journal* pp. 1935–2042.

Hathaway, Oona. 2007. "Why Do Countries Commit to Human Rights Treaties?" *Journal of Conflict Resolution* 51:588–621.

Heckman, J., J. Ichimura, J. Smith and P.E. Todd. 1998. "Characterizing Selection Bias Using Experimental Data." *Econometrica* 66:1017–98.

Heckman, J., J. Smith and N. Clements. 1997. "Making the Most out of Social Experiments: Accounting for Heterogeneity in Program Impacts." *Review of Economic Studies* 64:487–536.

Heckman, James. 1997. "Instrumental variables: A study of implicit behavioral assumptions used in making program evaluations." *Journal of Human Resources* 32:441–62.

Heckman, James J. 1979. "Sample Selection Bias as a Specification Error." *Econometrica* 47:153–61.

Helfer, Laurence R. 2002. "Overlegalizing Human Rights: International Relations Theory and the Commonwealth Caribbean Backlash Against Human Rights Regimes." *Columbia Law Review* 102:1834–1911.

Henderson, Conway. 1991. "Conditions Affecting the Use of Political Repression." *Journal of Conflict Resolution* 35:120–142.

Henisz, Witold J. 2002. "The Institutional Environment for for Infrastructure Investment." *Industrial and Corporate Change* 11(2):355–89.

Hill, Daniel W. 2010. "Estimating the Effects of Human Rights Treaties on State Behavior." *Journal of Politics* 72:1161–74.

Hinich, Melvin J. and Michael C. Munger. 1994. *Ideology and the Theory of Political Choice*. Ann Arbor: University of Michigan Press.

Hinich, Melvin J. and Michael C. Munger. 1997. *Analytical Politics*. Ann Arbor: University of Michigan Press.

Hix, Simon. 2001. "Legislative Behaviour and Party Competition in the European Parliament: An Application of Nominate to the EU." *Journal of Common Market Studies* 29:663–88.

Hix, Simon, Abdul Noury and Gérard Rolan. 2006. "Legislative Behaviour and Party Competition in the European Parliament: An Application of Nominate to the EU." *American Journal of Political Science* 50(2):494–511.

Ho, Daniel, Kosuke Imai, Gary King and Elizabeth A. Stuart. 2009. "MatchIt: Nonparametric Preprocessing for Parametric Causal Inference." *Journal of Statistical Software* 42(8).

Ho, Daniel, Kosuke Imai, Gary King and Elizabeth Stuart. 2007. "Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference." *Political Analysis* 15:199–236.

Hollyer, James R. and B. Peter Rosendorff. 2011. "Why Do Authoritarian Regimes Sign the Convention Against Torture? Signaling, Domestic Politics and Non-Compliance." *Quarterly Journal of Political Science* 6:275–327.

Honaker, James, Gary King and Matthew Blackwell. 2009. "Amelia II: A Program for Missing Data.".

Howell, Wiliam G. and David E. Lewis. 2002. "Agencies by Presidential Design." *Journal of Politics* 64:1095–1114.

Huntington, Samuel P. 1997. *The Clash of Civilizations and the Remaking of World Order*. Pocket Books.

Iida, Keisuke. 1988. "Third World Solidarity: The Group of 77 in the United Nations General Assembly." *International Organization* 42(2):375–95.

Ikenberry, G. John and Michael W. Doyle. 1997. Conclusion: Continuity and Innovation in International Relations Theory. In *New Thinking in International Relations Theory*, ed. Michael W. Doyle and G. John Ikenberry. Boulder, Colo.: Westview Press pp. 266–80.

Jacobson, H.K., W.M. Reisinger and T. Mathers. 1986. "National Entanglements in International Governmental Organizations." *American Political Science Review* 80(1):141–159.

Jenkins, Jeffrey. 1999. "Examining the Bonding Effects of Party: A Comparative Analysis of Roll-Call Voting in the U.S. and Confederate Houses." *American Journal of Political Science* 43:1144–65.

Johnson, Simon, John McMillan and Christopher Woodruff. 2002. "Courts and Relational Contracts." *Journal of Law, Economics, and Organization* 18:221–277.

Kastner, Scott L. and Chad Rector. 2003. "International Regimes, Domestic Veto-Players, and Capital Controls Policy Stability." *International Studies Quarterly* 47:1–22.

Kearney, Patrick and Ryan Powers. 2011. "Veto Players and Conditional Commitment to U.N. Human Rights Agreements." Presented at the American Political Science Association Annual Meeting in Seattle, Washington.

Keck, Margaret E. and Kathryn Sikkink. 1998. *Activists Beyond Borders: Advocacy Networks in International Politics*. Ithaca: Cornell University Press.

Keefer, Philip and David Stasavage. 2003. "The Limits of Delegation: Veto Players, Central Bank Independence, and the Credibility of Monetary Policy." *American Political Science Review* 97(3):407–423.

Keith, Linda Camp. 1999. "The United Nations International Covenant on Civil and Political Rights: Does It Make a Difference in Human Rights Behavior?" *Journal of Peace Research* 36:95–118.

Keith, Linda Camp. 2002*a*. "Constitutional Provisions for Individual Human Rights (1977-1996): Are They More than Mere "Window Dressing?"." *Political Research Quarterly* 55(1):111–43.

Keith, Linda Camp. 2002*b*. "Judicial Independence and Human Rights Protection around the World." *Judicature* 85:195–200.

Keith, Linda Camp, C. Neal Tate and Steven C. Poe. 2009. "Is The Law a Mere Parchment Barrier to Human Rights Abuse?" *Journal of Politics* 71:644–60.

Kelley, Judith. 2007. "Who Keeps International Commitments and Why? The International Criminal Court and Bilateral Nonsurrender Agreements." *American Political Science Review* 101:573–589.

Keohane, Robert O. 1984. *After Hegemony: Cooperation and Discord in the World Political Economy*. Princeton: Princeton University Press.

Kim, Soo Yeon and Bruce Russett. 1996. "The New Politics of Voting Alignments in the United Nations General Assembly." *International Organization* 50(4):629–652.

Koremenos, Barbara, Charles Lipson and Duncan Snidal. 2001. "The Rational Design of International Institutions." *International Organization* 55(04):761–799.

Krasner, Stephen D. 1995. *Structural Conflict: The Third World Against Global Liberalism*. Berkeley: University of California Press.

Kruskal, Joseph B. and Myron Wish, eds. 1976. *Multidimensional Scaling*. Beverly Hills and London: Springer.

Kydd, Andrew. 2001. "Trust Building, Trust Breaking: The Dilemma of NATO Enlargement." *International Organization* 55:801.

Landman, Todd. 2005. *Protecting Human Rights: A Comparative Study*. Washington, DC: Georgetown University Press.

Lechner, M. 2000. "An Evaluation of Public Sector Sponsored Continuous Vocational Training Programs in East Germany." *Journal of Human Resources* 35:347–75.

Lemke, Douglas. 2002. *Regions of War and Peace*. Cambridge: Cambridge University Press.

Lemke, Douglas. 2003. "African Lessons for International Relations Research." *World Politics* 56(4):114–38.

Liao, Tim Futing. 1995. "The Nonrandom Selection of Don't Knows in Binary and Ordinal Responses: Corrections with the Bivariate Probit Model with Sample Selection." *Quality and Quantity* 29:87–110.

Lichbach, Mark. 1984. "An Economic Theory of Governability: Choosing Policy and Optimizing Performance." *Public Choice* 307–37:44.

Lichbach, Mark. 1995. *The Rebel's Dilemma*. Ann Arbor: University of Michigan Press.

Lipson, Charles. 2005. *Reliable Partners: How Democracies Have Made a Separate Peace*. Princeton, NJ: Princeton University Press.

Little, R.J.A. and D.B. Rubin. 1987. *Statistical Analysis with Missing Data*. New York: Wiley and Sons.

Londregan, John. 2000. *Legislative Institutions and Ideology in Chile*. Cambridge: Cambridge: Cambridge University Press.

Lutz, Ellen L. and Kathryn Sikkink. 2000. "International Human Rights Law and Practice in Latin America." *International Organization* 54:633–659.

Mansfield, Edward D., Helen V. Milner and Jon C. Pevehouse. 2007. "Vetoing Cooperation: The Impact of Veto Players on Preferential Trading Arrangements." *British Journal of Political Science* 37(3):403–32.

Marshall, Monty and Keith Jaggers. 2002. "Polity IV Project: Political Regime Characteristics and Transitions, 1800-2002.". University of Maryland.

Martin, Lisa L. 1992. *Coercive Cooperation*. Princeton: Princeton University Press.

Martin, Lisa L. 2000. *Democratic Commitments: Legislatures and International Cooperation*. Princeton, NJ: Princeton University Press.

McCauliff, C.M.A. 1982. "Burdens of Proof: Degrees of Belief, Quanta of Evidence, Or Constitutional Guarantees." *Vanderbilt Law Review* p. 1293.

McConnell, Michael W. and Richard A. Posner. 1989. "An Economic Approach to Issues of Religious Freedom." *University of Chicago Law Review* 56(1):1–60.

McCormick, James M. and Neil J. Mitchell. 1997. "Human Rights Violations, Umbrella Concepts, and Empirical Analysis." *World Politics* 49:510–25.

McCubbins, Mathew and Thomas Schwartz. 1984. "Congressional Oversight Overlooked: Police Patrols versus Fire Alarms." *American Journal of Political Science* 28:165–79.

Milner, Helen V. 1997. *Interests, Institutions, and Information*. Princeton, N.J.: Princeton University Press.

Milner, Helen V. and B. Peter Rosendorff. 1997. "Democratic Politics and International Trade Negotiations." *Journal of Conflict Resolution* 41(1):117–46.

Mitchell, Ronald B. 1994. *Intentional Oil Pollution at Sea: Environmental Policy and Treaty Compliance*. Cambridge, MA: MIT Press.

Mitchell, Ronald B., William C. Clark, David W. Cash and Nancy M. Dickson. 2006. *Global Environmental Assessments: Information and Influence*. Cambridge, MA: MIT Press.

Moore, Will H. 1998. "Repression and Dissent: Substitution, Context, and Timing." *American Journal of Political Science,* 42(3):851–73.

Moore, Will H. 2000. "The Repression of Dissent: A Substitution Model of Government Coercion." *Journal of Conflict Resolution* 44(1):107–27.

Moravcsik, Andrew. 1997. "Taking Preferences Seriously: A Liberal Theory of International Politics." *International Organization* 51:513.

Moravcsik, Andrew. 2000. "The Origins of Human Rights Regimes: Democratic Delegation in Postwar Europe." *International Organization* 54:217–252.

Morgan, Stephen L. and Christoher Winship. 2007. *Counterfactuals and Causal Inference: Methods and Principles for Social Research*. New York: Cambridge University Press.

Morgenstern, Scott. 2003. *Patterns of Legislative Politics: Roll-Call Voting in Latin America and the United States*. Cambridge: Cambridge University Press.

Morrow, James D. 1991. "Alliances and Asymmetry: An Alternative to the Capability Aggregation Model of Alliances." *American Journal of Political Science* 35:904–933.

Morrow, James D. 2001. "The Institutional Features of the Prisoners of War Treaties." *International Organization* 55:971.

Neumayer, Eric. 2005. "Do International Human Rights Treaties Improve Respect for Human Rights?" *Journal of Conflict Resolution* 49:925–53.

Neumayer, Eric. 2007. "Qualified Ratification: Explaining Reservations to International Human Rights Treaties." *Journal of Legal Studies* 36:397–430.

Newman, Jon O. 2006. "Quantifying the Standard of Proof Beyond a Reasonable Doubt: A Comment on Three Comments." *Law, Probability and Risk* 5:267–69.

Noury, Abdul G. 2001. "Ideology, Nationality and Euro-Parliamentarians." *European Union Politics* 3:33–58.

O'Reilly, Robert F. 2005. "Veto Points, Veto Players, and International Trade Policy." *Compaative Political Studies* 38(6):652–75.

Orentlicher, Diane F. 1990. "Bearing Witness: The Art and Science of Human Rights Fact-Finding." *Harvard Human Rights Journal* 3:83–135.

Oye, Kenneth A. 1986. Explaining Cooperation Under Anarchy: Hypotheses and Strategies. In *Cooperation Under Anarchy*, ed. Kenneth A. Oye. Macmillan pp. 1–24.

Pearl, Judea. 2000. *Causality: Models, Reasoning, and Inference*. Cambridge: Cambridge University Press.

Perkins, Richard and Eric Neumayer. 2007. "Implementing Multilateral Environmental Agreements: An Analysis of EU Directives." *Global Environmental Politics* 7(3):13–41.

Png, I.P.L. 1986. "Optimal Subsidies and Damages in the Presence of Judicial Error." *International Review of Law and Economics* 6:101–05.

Poe, Steven C. 2004. The Decision to Repress: An Integrative Theoretical Approach to the Research on Human Rights and Repression. In *Understanding Human Rights Violations: New Systematic Studies*, ed. S. Carey and S. Poe. Aldershott: Ashgate pp. 16–42.

Poe, Steven and C. Neil Tate. 1994. "Repression of Human Rights to Personal Integrity in the 1980s: A Global Analysis." *American Political Science Review* 88:853–72.

Poe, Steven, C. Neil Tate and Linda Camp Keith. 1999. "Repression of the Human Right to Personal Integrity Revisited: A Global Cross-National Study Covering the Years 1976-1993." *International Studies Quarterly* 43:291–313.

Polinsky, A. Mitchell and Steven Shavell. 1989. "Legal Error, Litigation, and the Incentive to Obey the Law." *Journal of Law, Economics and Organization* 5:99–108.

Poole, Keith, Jeffrey Lewis, James Lo and Royce Carroll. 2008. "Scaling Roll Call Votes with wnominate in R." *Journal of Statistical Software* .

Poole, Keith T. 2005. *Spatial Models of Parliamentary Voting*. New York: Cambridge University Press.

Poole, Keith T. and Howard Rosenthal. 1997. *Congress: A Political-Economic History of Roll Call Voting*. New York: Oxford University Press.

Posner, Richard A. 1987. "The Law and Economics Movement." *American Economic Review* 77(2):1–13.

Powell, Emilia Justyna and Jeffrey K. Staton. 2009. "Domestic Judicial Institutions and Human Rights Treaty Violation." *International Studies Quarterly* 53:149–74.

Ratner, Steven R. and Jason S. Abrams. 2001. *Accountability for Human Rights Atrocities in International Law: Beyond the Nuremberg Legacy*. New York: Oxford University Press.

Reed, William, David H. Clark, Timothy Nordstrom and Wonjae Hwang. 2008. "War, Power, and Bargaining." *Journal of Politics* 70:1203–1216.

Reiter, Dan and Allan C. Stam. 2002. *Democracies At War.* Princeton, NJ: Princeton University Press.

Risse, Thomas, Stephen C. Ropp and Kathryn Sikkink, eds. 1999. *The Power of Human Rights: International Norms and Domestic Change*. Cambridge: Cambridge University Press.

Rosenbaum, Paul R. and Donald B. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70:41–55.

Rosenbaum, Paul R. and Donald B. Rubin. 1984. "Reducing Bias in Observational Studies Using Subclassification on the Propensity Score." *Journal of the American Statistical Association* 79:516–24.

Rothenberg, Lawrence S. and Mitchell S. Sanders. 2000. "Legislator Turnout and the Calculus of Voting: The Determinants of Abstention in the U.S. Congress." *Public Choice* 103:259–70.

Rubin, Donald B. 1997. "Estimating Causal Effects from Large Data Sets Using Propensity Scores." *Annals of Internal Medicine* 127:757–63.

Rubinfeld, Daniel L. and David E. M. Sappington. 1987. "Efficient Awards and Standards of Proof in Judicial Proceedings." *RAND Journal of Economics,* 18:308–15.

Russett, Bruce M. 1966. "Discovering Voting Groups in the United Nations." *American Political Science Review* 60(2):327–39.

Sanchirico, Chris William. 2004. "Evidence Tampering." *Duke Law Journal* 53:1215–1336.

Sanchirico, Chris William. 2006. "Detection Avoidance." *New York University Law Review* 81:1331–99.

Sanchirico, Chris William and George Triantis. 2007. "Evidentiary Arbitrage: The Fabrication of Evidence and the Verifiability of Contract Performance." *Journal of Law, Economics, and Organization* 27:72–94.

Sartori, Anne E. 2003. "An Estimator for Some Binary-Outcome Selection Models Without Exclusion Restrictions." *Political Analysis* 11:111–38.

Schonhardt-Bailey, Cheryl. 2003. "Ideology, Party and Interests in the British Parliament of 1841-1847." *British Journal of Political Science* 33:581–605.

Shanks, C., H.K. Jacobson and J.H. Kaplan. 1996. "Inertia and Change in the Constellation of International Governmental Organizations, 1981-1992." *International Organization* 50(4):593–627.

Simmons, Beth A. 1998. "Compliance with International Agreements." *Annual Review of Political Science* 1:75–93.

Simmons, Beth A. 2000. "International Law and State Behavior: Commitment and Compliance in International Monetary Affairs." *American Political Science Review* 94:819–34.

Simmons, Beth A. 2009. *Mobilizing for Human Rights: International Law in Domestic Politics*. Princeton: Princeton University Press.

Simmons, Beth A. and Allison Danner. 2010. "Credible Commitments and the International Criminal Court." *International Organization* 64:225–256.

Simmons, Beth A. and Daniel J. Hopkins. 2005. "The Constraining Power of International Treaties: Theory and Methods." *American Political Science Review* 99:623–31.

Simon, Rita James and Linda Mahan. 1971. "Quantifying Burdens of Proof: A View from the Bench, the Jury, and the Classroom." *Law and Society Review* 5:319–330.

Simpson, John and Dana Bennett. 1985. *The Disappeared and the Mothers of the Plaza: The Story of the 11,000 Argentinians Who Vanished*. New York: St. Martins Press.

Smith, J.A. and P.E. Todd. 2005. "Does Matching Overcome LaLonde's Critique of Non-experimental Estimators?" *Journal of Econometrics* 125:305–53.

Snow, Clyde Collins, Fredy Armando Peccerelli, Jose Samuel Susanavar, Alan G. Robinson and Jose Maria Najera Ochoa. 2008. Hidden in Plain Sight: X.X. Burials and the Desaparecidos in the Department of Guatemala, 1977-1986. In *Statistical Methods for Human Rights*, ed. Jana Asher, David L. Banks and Fritz Scheuren. Springer.

Staton, Jeffrey K. and Will H. Moore. 2011. "Judicial Power in Domestic and International Politics." *International Organization* 65(3):553–87.

Stokke, Olav Schram and Geir Hønneland. 2007. *International Cooperation and Arctic Governance: Regime Effectiveness and Northern Region Building*. London: Routledge.

Taruffo, Michele. 2003. "Rethinking the Standards of Proof." *American Journal of Comparative Law* 51:659–677.

Tate, C. Neal and Linda Camp Keith. 2007. "Conceptualizing and Operationalizing Judicial Independence Globally." Paper prepared for the Annual Meeting of the American Political Science Association, Chicago, Illinois, August 30-September 1, 2007.

Thompson, Alexander. 2006. "Coercion Through IOs: The Security Council and the Logic of Information Transmission." *International Organization* 60:1.

Tillers, Peter and Jonathan Gottfried. 2006. "Case Comment: United States v. Copeland, 369 F. Supp. 2d 275 (E.D.N.Y. 2005): A Collateral Attack on the Legal Maxim That Proof Beyond A Reasonable Doubt Is Unquantifiable?" *Law, Probability and Risk* 5:167–73.

Tsebelis, George. 1995. "Decision Making in Political Systems: Veto Players in Presidentialism, Parliamentalism, Multicameralism, and Multipartyism." *British Journal of Political Science* 25:289–326.

Tsebelis, George. 1999. "Veto Players and Law Production in Parliamentary Democracies: An Empirical Analysis." *American Political Science Review* 93(3):591–608.

Tsebelis, George. 2002. *Veto Players: How Political Institutions Work*. Princeton, NJ: Princeton University Press.

Tullock, Gordon. 1980. *Trials on Trial, The Pure Theory of Legal Procedure*. New York: Columbia University Press.

Victor, David G., Kal Raustiala and Eugene B. Skolnikoff. 1998. *The Implementation and Effectiveness of International Environmental Commitments: Theory and Practice*. Laxenburg, Austria and Cambridge, MA: MIT Press.

Voeten, Erik. 2000. "Clashes in the Assembly." *International Organization* 54:185–215.

von Stein, Jana. 2005. "Do Treaties Constrain or Screen? Selection Bias and Treaty Compliance." *American Political Science Review* 99:611–22.

von Stein, Jana. 2008. "The International Law and Politics of Climate Change: Ratification of the United Nations Framework Convention and the Kyoto Protocol." *Journal of Conflict Resolution* 52:243–268.

Vreeland, James R. 2008. "Political Institutions and Human Rights: Why Dictatorships Enter into the United Nations Convention Against Torture." *International Organization* 62(1):65–101.

Waltz, Kenneth W. 1993. "The Emerging Structure of International Politics." *International Security* 18(2):44–79.

Weinstein, Jack B. and Ian Dewsbury. 2006. "Comment on the Meaning of 'Proof Beyond a Reasonable Doubt'." *Law, Probability and Risk* 5:167–73.

Weiss, Edith Brown and Harold K. Jacobson. 1998. *Engaging Countries: Strengthening Compliance with International Accords*. Cambridge, MA: MIT Press.

Weissbrodt, D. and J. McCarthy. 1982. Fact-Finding by Nongovernmental Organizations. In *International Law and Fact-Finding in the Field of Human Rights*, ed. B.G. Ramcharan. Martinus Nijhoff pp. 186–230.

Wigmore, John H. 1981. *Evidence in Trials at Common Law*. New York: Little, Brown.

Winship, Christopher and Robert D. Mare. 1992. "Models for Sample Selection Bias." *Annual Review of Sociology* 18:327–50.

Winston, Morton. 2001. Assessing the Effectiveness of International Human Rights NGOs: Amnesty International. In *NGOs and Human Rights*, ed. Claude E. Welch Jr. University of Pennsylvania Press.

Wright, Richard W. 2009. Proving Facts: Belief versus Probability. In *European Tort Law*, ed. Helmut Koziol and Barbara C. Steininger. European Centre of Tort and Insurance Law pp. 79–106.