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Research and Teaching Statement
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This statement describes my three main research programs:

- I. Reputation in International Cooperation
- II. Sources of International Credibility: Domestic Audiences and International Law
- III. Models of Voting Behavior

For each program, I describe published and ongoing research. I conclude by briefly discussing my teaching experience.

Research Program I: Reputation and International Cooperation

This research program investigates the role of reputation in international relations. It seeks to explain how reputations form and affect patterns of cooperation and conflict. I explore these issues most extensively in *Reputation and International Cooperation: Sovereign Debt across Three Centuries* (Princeton University Press, 2007). Below, I describe the book and three extensions I am now pursuing.

A. Reputation and Sovereign Debt

My book was motivated by an unsolved puzzle: why do governments repay their debts to private foreign lenders, and what gives bondholders and banks the confidence to lend billions of dollars abroad each year? I address this puzzle by developing a theory of reputation that involves incomplete information, political change, and contextual inference. I then test my theory and the leading alternatives by analyzing a new body of evidence that covers all sovereign debtors in the world across three centuries of financial history.

A Theory of Cooperation through Reputation: According to my theory of reputation, the preferences of governments are heterogeneous: some governments assign greater value than others to maintaining good relations with creditors, and their preferences can change over time. Investors cannot fully know the preferences of a foreign government, but they do have beliefs about the government's "type." Those beliefs, which constitute the government's reputation, evolve as investors observe behavior in context—as they review the government's record of repayment during good times and bad. Borrowers understand this reputational logic and take it into account when deciding whether to default.

My theory explains why investors lend (because they perceive the borrower as a reliable type) and why governments repay (because they want a reputation for reliability, which allows access to foreign capital). Beyond that, the theory generates a wide range of testable implications about the *dynamics* of debtor-creditor relations. It predicts how investors treat first-time borrowers and how risk premiums evolve as borrowers become more seasoned. It explains how debtors ascend or descend the reputational ladder due to the interaction between their behavior and the historical context, and then clarifies how changes in reputation affect access to capital. The theory also helps explain why countries with favorable reputations sometimes default, and why nations with histories of noncompliance suddenly settle with foreign creditors.

Empirical Analysis of the Reputational Theory: The evidence, gathered from archives in nine countries, strongly supports my theory of reputation. From the Amsterdam market of the 1700s, through the rise of London and New York as financial centers, to the Euromarkets of today, reputations have formed and influenced behavior in a remarkably consistent way. The data strongly confirm each of the following predictions of my reputational theory:

- *Uncertainty premiums and seasoning effects:* Investors charge higher interest rates to new borrowers than to more established entities, to cover the risk of lending to a potential “lemon.” As borrowers acquire a record (as they become more seasoned), investors update their beliefs and recalibrate the terms of credit. Countries that pay, thereby distinguishing themselves from bad types, see their risk premiums decline asymptotically toward a risk-free rate.
- *Market exclusion and market reentry:* By defaulting, countries signal that they are less reliable types and lose access to international capital markets. Past defaulters regain access by signaling, through a costly compensation package, that they put a high value on relations with creditors.
- *Contextual inference:* Countries ascend and descend the reputational ladder based not only on their behavior but also—when available—on data about the circumstances they faced. Investors show especially low regard for borrowers that default without a valid economic excuse; do not disparage debtors for defaulting during hard times; and upgrade low-rated debtors that exceed expectations by servicing their debts under extreme hardship.
- *Equilibrium defaults:* In my theory (unlike many others), defaults occur in equilibrium. They arise from incomplete information, political change, and exogenous shocks. Specifically, defaults occur when investors lend to borrowers that have not yet been revealed as lemons; when political change causes bad types to inherit the loans of good-type predecessors; and when adverse economic shocks raise the cost of repayment while lowering the reputational penalty for renegeing.
- *Cooperation with atomized, short-lived actors:* Cooperation can emerge even when investors are short-lived and have little capacity for collective action. The evidence indicates that concerns about reputation have sustained lending and repayment across the centuries, even when capital has come from thousands or millions of disparate bondholders around the world.
- *Reputational rhetoric:* Investors and credit rating agencies consistently identify reputation as the first test of a government bond, and governments regularly articulate a reputational rationale for repaying their loans. Historically under-rated borrowers that paid despite great hardship—epitomized by Argentina in the 1930s—have done so with the aim of raising their reputations.

The same evidence that supports my theory also casts doubt on the leading alternative accounts of reputation in world affairs. For example, I find little support for “desire-based” theories of reputation, according to which people draw systematically biased lessons from history, and “current calculus” theories, which suggest that past behavior has little impact on present beliefs.

Empirical Analysis of Nonreputational Mechanisms: My book not only builds a case for reputation but also questions the importance of the nonreputational mechanisms that are central to the existing sovereign debt literature. Many researchers argue that lenders enforce debt contracts via issue linkage: they threaten to punish defaulters by imposing nonfinancial penalties such as trade sanctions, diplomatic pressure, or military intervention. I find that such issue linkage has proven relatively unimportant in practice.

In particular, my work challenges a common view about the historical importance of gunboat diplomacy. It is widely believed that, before World War I, the fear of gunboat diplomacy motivated countries to repay foreign loans. I show that the apparent correlation between sovereign default and military intervention is mostly spurious. Militarized disputes between creditors and debtors arose not because creditors were using arms to defend bondholders, but because default coincided with other disputes (civil wars, territorial conflicts, and tort claims) that attracted the involvement of foreign powers.

Even the so-called bondholder war against Venezuela in 1902 was really about torts, not bonds. Analyses of diplomatic correspondence, geographic patterns of investment, and default rates against strong and weak creditors are also inconsistent with the gunboat hypothesis.

My book also disputes the relevance of enforcement via trade sanctions. During the interwar period, the alleged heyday of tactical linkage between debt and trade, the countries most exposed to international commerce did not repay at a higher rate than more insular nations. Moreover, defaulters did not discriminate in favor of their major trading partners, as trade sanctions theory would predict. Researchers often cite interwar Argentina as the prime instance of using trade to bully borrowers. Close inspection of archival evidence, however, shows that even in the Argentine case, the government repaid to enhance its reputation, not to avert a trade war. I came across only two references to trade sanctions in more than 96,000 newspaper clippings about sovereign debt, and my systematic survey of investment commentary from the 1920s found almost no evidence for the trade sanctions hypothesis.

Finally, my research disconfirms the common claim that collective action (collective retaliation) is necessary to enforce loan contracts. For centuries, money flowed to sovereign borrowers via atomized bond markets, even though bank lending would have provided more punishment power. When investors finally began to lend through commercial banks in the late twentieth century, neither the terms of lending nor the patterns of repayment matched the predictions of sanctioning theory. Single banks lent on the same terms as well-organized syndicates, and governments that had borrowed from both bondholders and banks did not treat the latter more favorably, despite the enormous disparity in retaliatory power. These patterns, though anomalous from the standpoint of sanctioning theory, are consistent with my reputational argument, which supports lending and repayment even when investors are atomized and short-lived.

Implications for Cooperation under Anarchy: The theory and evidence presented in my book have potentially broad implications for understanding cooperation under anarchy. With an improved theory of reputation—one that emphasizes incomplete information and political change, and posits that observers study behavior in historical context—I was able to discover reputational effects that had previously been overlooked. The concluding chapter discusses how these insights about reputation could apply to other areas of international relations, including foreign direct investment, international trade, military deterrence, and alliance politics.

B. Deepening and Broadening the Study of Reputation

I am now deepening my work on reputation and sovereign debt by examining the determinants of domestic preferences and the effects of economic context. At the same time, I am broadening the research to cover other issue areas, such as foreign direct investment.

Preferences and Government Types: My theory of reputation assumes that domestic preferences about debt and other commitments vary across countries and over time. What accounts for this variation? My manuscript on “Interests, Information, and the Domestic Politics of International Agreements” offers a partial answer. I argue that debt repayment creates domestic winners and losers through two channels: adjustment and reputation. I then show, with polling data from Argentina, that the preferences of citizens depend on their exposure to the adjustment costs and reputational benefits of repayment. This prediction holds mainly for highly informed citizens, however; the preferences of less knowledgeable citizens are only weakly correlated with self-interest. These findings, if replicable in other issue areas, would qualify the common assumption in political economy models that all citizens—regardless of their knowledge and sophistication—lobby and vote according to their rational self-interest.

Context and Economic Shocks: My theory also maintains that investors study behavior in context. I am, therefore, looking more deeply at the effects of economic conditions on compliance with debt contracts. Some of this work, joint with Mark Wright, appears in “Do Countries Default in Bad Times?” (*JEEA* 2007), which uses a new dataset covering the years 1820-2004 to estimate the relationship between

economic output and sovereign default. We are now writing a follow-up article, which shows that the relationship between output and default is conditional on the degree of slack in international capital markets and on political conditions in the borrowing country. These papers not only speak to the theoretical literature about sovereign debt, but also have policy implications for forecasting defaults and designing appropriate institutions to address them.

Reputation and Foreign Direct Investment: I am extending my analysis of reputation to other areas of international relations, including foreign direct investment. I have just completed a new database entitled “A Century of Expropriation,” which documents takings of FDI since 1900. I plan to use the data for several purposes: to compare the role of reputation in lending versus direct investment; to study how behavior in one area of international relations, such as expropriation of FDI, spills over to affect reputations in other areas, such as sovereign debt; and to develop and assess more general models of investor-government cooperation, in which investors choose among different types of assets and governments decide which contracts to respect. Wright and I have taken a first step in this direction with our working paper, “Sovereign Theft: Theory and Evidence about Default and Expropriation.”

Research Program II: Sources of International Credibility: Domestic Audiences and International Law

This research program examines the effects of domestic audiences and international law on the credibility of international commitments. Leaders often “go public” by telling their own citizens (domestic audiences) about threats and promises they issue abroad, and they “go legal” by embedding commitments in treaties and other international agreements. Under what conditions do domestic audiences and international law tie the hands of leaders, and how do they shape expectations about behavior? My work on these questions is supported by a five-year, \$400,000 CAREER grant from the National Science Foundation.

A. An Experimental Approach

Previous work on publicity and legalization has been hampered by two obstacles: endogeneity and measurement. In precisely the situations when publicity and legalization would make it costly to renege on an international commitment, rational leaders will either follow through or avoid committing in the first place. This leaves scholars little chance to observe and measure the penalties central to our theoretical models. Research has also been hampered by insufficient data about perceptions. Do key actors *believe* public and legal commitments are more expensive to break?

To address these questions, I am conducting the first-ever experimental analyses of commitments in international relations. The experiments, embedded in surveys of citizens and policymakers, are designed to measure the actual and perceived costs of renegeing on public and legal commitments, while avoiding the difficult endogeneity problems of observational studies. In parallel, I am building new historical datasets that are helping me design experiments and creating opportunities for non-experimental analysis.

B. The Effects of Domestic Audiences

My work on audience costs exemplifies my experimental approach. Recent models of international relations assume that leaders would suffer domestic audience costs if they issued threats or promises and failed to follow through. Citizens, it is claimed, would think less of leaders who backed down than of leaders who never committed in the first place. In a world with audience costs, the prospect of losing domestic support—or even office—could discourage leaders from making empty threats and promises. The concept of domestic audience costs is now central to theories about military crises, and researchers

have incorporated similar ideas into models of other interstate interactions. Despite the prominence of audience costs in international relations theories, though, no previous research had shown whether and when such costs exist in practice.

Initial Experiments: I am using survey experiments to generate direct, endogeneity-free measures of domestic audience costs. As a first cut, I conducted a series of experiments involving military crises. Some citizens were randomly assigned to a control group and told that the president did not get involved in an international crisis. Others were randomly placed in a treatment condition in which the president escalated the crisis but ultimately backed down. All participants in the between-subject design were then asked whether they approved of the way the president handled the situation. I identified audience costs by comparing approval ratings in the “stay out” and “back down” conditions.

My analysis, published in *International Organization* (2007), shows that audience costs exist across a wide range of conditions and increase with the level of escalation. The costs are evident throughout the population, and especially among politically active citizens who have the greatest potential to shape government policy. Finally, preliminary evidence suggests that audience costs arise because citizens care about the international reputation of the country or leader. These findings help identify how, and under what conditions, domestic audiences can make commitments credible. At the same time, they demonstrate the promise of using experiments to answer previously intractable questions about international relations.

Additional Experiments: I have conducted many other experiments that deepen our understanding of audience costs. To see whether policymakers expect that empty threats would be politically costly, I supervised a team of Stanford students that administered experiments to members of the British parliament (N=75). Our data confirm that leaders anticipate domestic audience costs, but also challenge the standard assumption that the perceived costs are higher in democracies. When judging threats by other nations, British leaders believe that the domestic penalty for backing down in a military confrontation would be just as high in autocratic regimes as in democratic ones. They offered many interesting reasons: autocrats depend on a strongman image, cannot share blame as easily as democratic leaders, and are more likely to be killed when ousted. In future experimental and historical work, I plan to disaggregate autocracies based on the presence or absence of institutions for maintaining power, sharing blame, and removing leaders.

Other experiments have led to additional findings: (a) leaders can, through crafted rhetoric, reduce the magnitude of audience costs, but their speeches typically cannot drive audience costs to zero; (b) hostile rhetoric by opposition parties can amplify audience costs and, in some cases, nullify presidential attempts to excuse empty threats; (c) more generally, the magnitude of audience costs varies with the level of elite consensus about foreign policy; and (d) the incidence of audience costs is surprisingly insensitive to party affiliation. I have also confirmed, by running similar experiments in Argentina, that (e) audience costs exist not only in superpowers but also in middle-rank countries, and that (f) it is more costly to back down against a weak adversary such as Paraguay than against a strong adversary such as Great Britain. After reporting some of these findings in articles, I plan to synthesize them in a book.

Historical Analysis: To complement the experiments, I have been building the *MIDipedia*, a new historical database about militarized interstate disputes. Assembled over two summers in collaboration with Jessica Weeks (Stanford Ph.D. student), the database contains detailed narratives and complete citations for more than a thousand disputes. I am using the narratives to design experiments that reflect the kinds of threats leaders make, the forums in which those threats are delivered, and the rhetoric that leaders and opposition groups use both when threats are conveyed and when they are not ultimately carried out. Over time, the database will also open new opportunities for non-experimental analysis—both quantitative and qualitative—about military commitments. We are storing the data in a customized internet wiki, which we plan to open to the public in 2008.

C. The Effects of International Law

I have been using a similar mix of experimental and historical methods to study the effects of international law. According to the United Nations Treaty Series, more than 50,000 bilateral and multilateral international agreements are currently in force. Although these agreements are “legally binding,” their credibility remains a matter of academic and practical debate. Without a higher power that compels sovereign states to respect international law, it is not obvious when—if at all—treaties change the incentives and expectations of key actors in world affairs. My work is shedding new light on this fundamental issue.

Initial Experiments: My manuscript on “The Effect of International Law on Preferences and Beliefs” offers the first experimental analysis of treaty commitments. Some interviewees are given a hypothetical or historical foreign policy situation in which leaders have signed a treaty. Others consider exactly the same situation, *sans* any legal agreement. By comparing the views of participants in the treatment condition (legal agreement) versus the control condition (no agreement), I am able to identify the effect of international law on policy preferences and beliefs.

My experiments, embedded in interviews with U.S. voters and British policymakers, reveal three patterns. First, international law transforms preferences and expectations. Individuals are far more likely to oppose policies that would violate international legal agreements than to oppose otherwise identical policies that would not trammel upon existing pacts. Moreover, many observers, including expert policymakers, anticipate that signatories to treaties will behave differently from non-signatories. Second, treaties exert their effects by publicizing international commitments and embedding them in a legal framework. Publicity raises the reputational costs of renegeing; legalization further puts reputation on the line and, in some countries, enables domestic courts to enforce international commitments. Finally, the effect of international law is additive, not absolute. If the material or moral case for violating international law is sufficiently strong, large proportions of voters and policymakers will advocate breaking the law and will expect foreign leaders to do the same. Thus, my experiments reveal both the power and the limits of international law.

Additional Experiments (and Software): I am now designing and fielding additional experiments with the following objectives: (a) to better understand the mechanisms by which international law ties the hands of leaders; (b) to investigate how the effects of legalization vary by issue and context; and (c) to develop and test theories about the effects of institutional design on preferences and beliefs. At the same time, I am developing a software package, *Treatment*, with Nico Benitez (a Stanford undergraduate) that will make these kinds of experiments more feasible and economical, not only for research but also for teaching. Our software is now in the advanced beta-testing phase. Finally, I am writing easy-to-use software that tests for covariate balance in randomized experiments and observational data.

Historical Analysis: To complement my experimental work on legalization, I have been gathering and analyzing historical data about legal commitments in international trade. One paper, joint with Judy Goldstein and Doug Rivers (*AER* 2007), solves a major puzzle about the effects of the General Agreement on Tariffs and Trade and its successor, the World Trade Organization. We show that previous researchers in economics and political science have overlooked a large proportion of countries to which the GATT applied. By mistakenly classifying many countries as nonparticipants, when in fact they had legal rights and obligations under the agreement, they have systematically underestimated the effect of the GATT on international trade. Through extensive archival research, we identify the full set of GATT participants and, once this institutional detail is understood, show that the GATT did indeed contribute to the substantial growth in postwar trade.

In a follow-up paper, “Institutions in International Relations” (*International Organization* 2007), we broaden the analysis to cover not only the GATT but also many other commercial agreements that have existed since World War II. We show that many of these agreements, like the GATT and the WTO, have

contributed to international trade. Moreover, our evidence suggests that international trade agreements have complemented, rather than undercut, each other.

In the next phase of this research, I plan to study trade treaties that codify the status quo by taking commercial policies that are already in place and translating them into international law. An analysis of such treaties will help isolate the contribution of international law to credibility. With this goal in mind, I have been compiling a new database entitled “Commerce and the Law before World War II.” The database currently includes MFN agreements, direction of trade, and national accounts for the years 1920–40.

Research Program III: Models of Voting Behavior

Beyond my research in the field of international relations, I have sought to understand how citizens think about political issues and choose which candidates to support. Few questions are as fundamental to democratic politics. In this section, I describe several interconnected lines of research.

A. Experimental Analysis of Spatial Theories

I am collaborating with Robert Van Houweling and Paul Sniderman on a research program to study spatial theories of voting. Our work, funded by a \$177,000 grant from the National Foundation, utilizes survey experiments (like the ones I am conducting to study domestic audiences and international law) to overcome problems of endogeneity and measurement that have impeded previous voting research. We have generated many papers; here, I describe two that I co-authored with Van Houweling in 2007. Both papers demonstrate the promise of combining formal theory, statistical modeling, and experiments to answer previously intractable questions about voting.

The Microfoundations of Issue Voting: Three theories—proximity voting, discounting, and directional voting—are at the center of a rich theoretical and empirical debate about how voters evaluate the policy positions of candidates. In “The Microfoundations of Issue Voting,” Van Houweling and I explain why existing data and methods are insufficient to estimate the prevalence of these decision logics in the electorate. We then formally derive an exhaustive set of critical tests: situations in which the theories predict different vote choices. Through survey experiments concerning federal health care policy, we administer the tests to a nationally representative sample of adults. Our unique algorithm for assigning experimental conditions allows us to disentangle all three theories for the first time. We find that proximity voting is about twice as common as discounting and four times as common as directional voting. Our analysis further shows that discounting is most prevalent among ideological centrists and non-partisans, suggesting that centrists make sophisticated judgments to bring policy in line with their preferences. These findings have important implications for understanding political competition and representation in democracies.

The Electoral Implications of Candidate Ambiguity: In a related study, Van Houweling and I investigate how voters respond to candidates that take ambiguous positions in political campaigns. In theory, ambiguity affects how voters make choices and who wins or loses elections. In practice, measurement and endogeneity problems have hamstrung efforts to study the consequences of ambiguity. In “The Electoral Implications of Candidate Ambiguity,” we design and administer survey experiments that overcome these obstacles by manipulating the precision of candidate positions. We find that, on average, ambiguity does not repel and may in fact attract voters. The preference for vague versus precise candidates depends partly on individual attitudes toward risk, and on the tendency to see ambiguous candidates from one’s own party in a favorable light. Our new experimental methods, developed in this paper and the previous one, open many avenues for research about electoral politics.

B. Electoral Surprise and Voting Behavior

In related work, Kenneth Scheve and I have studied one of the most consistent features of American politics: the regular loss of votes experienced by the president's party in midterm congressional elections. Our article in the *BJPS* (1999) establishes that electoral surprise contributes to midterm loss: the more surprised moderate voters are about the outcome of a presidential election, the less likely they are to support the president's party in the subsequent midterm contest.

C. Statistical Tools for Analyzing Elections (and other applications)

Beyond these substantive papers, I have contributed a set of statistical tools for studying elections. I coauthored "An Easy and Accurate Regression Model for Multiparty Electoral Data" (*Political Analysis* 2002) which proposes a seemingly unrelated regression model for multiparty electoral data. I also collaborated with Gary King and Jason Wittenberg to write "Making the Most of Statistical Analyses" (*AJPS* 2000) and develop *Clarify*, which won the Okidata award for best research software in Political Science. *Clarify* is widely used in the study of elections and other work involving limited dependent variables.

Undergraduate and Graduate Teaching

At Stanford, I regularly teach Introduction to International Relations (enrollment of 300 undergraduates) and a freshman seminar on "The Politics of Economic Development," which is based on a book I edited with Jeffrey Frieden and Manuel Pastor. I also advise undergraduate honors theses, teach graduate seminars in international relations theory and international political economy, and serve on numerous Ph.D. dissertation committees.

In 2002 I founded the Political Science Program for Undergraduate Research, which involves undergraduates in faculty-led research. The program provides stipends for 20 students to work full-time with faculty during the summer, and it supports 30–40 students on a part-time basis during the academic year. As director of the program, I raise about \$150,000 annually for student stipends; help match students with appropriate faculty mentors; run a research seminar that meets twice per week throughout the summer; and work closely with my own undergraduate mentees.

I enjoy teaching and have received several honors, including the Dean's Award for Distinguished Teaching (2003) and the Cox Medal for Faculty Excellence in Fostering Undergraduate Research (2005), a university-wide award given to one faculty member at Stanford each year.