

## INFORMATION TO USERS

This manuscript has been reproduced from the microfilm master. UMI films the text directly from the original or copy submitted. Thus, some thesis and dissertation copies are in typewriter face, while others may be from any type of computer printer.

**The quality of this reproduction is dependent upon the quality of the copy submitted.** Broken or indistinct print, colored or poor quality illustrations and photographs, print bleedthrough, substandard margins, and improper alignment can adversely affect reproduction.

In the unlikely event that the author did not send UMI a complete manuscript and there are missing pages, these will be noted. Also, if unauthorized copyright material had to be removed, a note will indicate the deletion.

Oversize materials (e.g., maps, drawings, charts) are reproduced by sectioning the original, beginning at the upper left-hand corner and continuing from left to right in equal sections with small overlaps.

ProQuest Information and Learning  
300 North Zeeb Road, Ann Arbor, MI 48106-1346 USA  
800-521-0600

**UMI<sup>®</sup>**



INSTITUTIONS, INCENTIVES AND INTERNATIONAL  
MONETARY FUND COMPLIANCE

by

MARTIN S. EDWARDS

A Dissertation submitted to the  
Graduate School-New Brunswick  
Rutgers, The State University of New Jersey  
in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

Graduate Program in Political Science

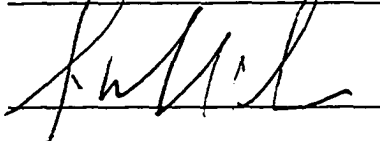
written under the direction of

Professor Jack S. Levy

and approved by

  
\_\_\_\_\_

*Michael D. Burda*  
\_\_\_\_\_

  
\_\_\_\_\_

  
\_\_\_\_\_

  
\_\_\_\_\_

New Brunswick, New Jersey

October 2002

UMI Number: 3077097

UMI<sup>®</sup>

---

UMI Microform 3077097

Copyright 2003 by ProQuest Information and Learning Company.  
All rights reserved. This microform edition is protected against  
unauthorized copying under Title 17, United States Code.

---

ProQuest Information and Learning Company  
300 North Zeeb Road  
P.O. Box 1346  
Ann Arbor, MI 48106-1346

© 2003

Martin S. Edwards

**ALL RIGHTS RESERVED**

ABSTRACT OF THE DISSERTATION

INSTITUTIONS, INCENTIVES AND INTERNATIONAL  
MONETARY FUND COMPLIANCE

by Martin S. Edwards

Dissertation Director:  
Professor Jack S. Levy

A target of criticism from both the left and the right, the International Monetary Fund has come under fire for its policy of conditionality—the act of requiring countries to honor certain conditions as a prerequisite for obtaining financial assistance. Despite the furor over conditionality, the Fund’s conditional lending programs break down frequently. How can we understand these outcomes? Why do some Fund programs succeed and others fail?

I argue that the problem with conditionality is an informational one. The Fund does not know *ex ante* whether a state will honor the commitments that it makes when it signs a letter of intent. Both committed reformers and reform minimizers have incentives to sign Fund agreements. As a result, conditionality does not function as a screening device.

This low information argument brings with it several testable hypotheses, which are addressed in separate chapters of the dissertation through a combination of game

theory, decision theory, and maximum likelihood sample selection techniques. First, the signing of a Fund program does not serve as an endorsement to financial markets, and as a result, there is no evidence that signing a Fund program produces a catalytic response by potential lenders and investors. Second, the Fund "tips the balance" for reform rarely and only under specific domestic institutional conditions. Third, even after controlling for the effects of institutions on economic performance, we see that the Fund's enforcement regime is inefficient, as it consistently sanctions states when there is no evidence that they have violated the bargain of conditionality. These hypotheses are tested using a sample of 126 developing countries that entered a total of 367 Fund programs between 1979 and 1995.

This project suggests that future progress in making Fund programs more credible and more successful can come from taking the effects of political institutions seriously. Moreover, it poses severe challenges to both realist explanations for the behavior of international institutions, as we find limited evidence suggesting that US influence drives Fund behavior, and liberal explanations, as the Fund does not play a role as an information provider.

## Acknowledgments

This project has been germinating in my head for some time. Some would argue too much time. Along the way from tentative ideas to finished product, I have been extraordinarily fortunate to have the help of friends and colleagues who have provided advice, encouragement, and sage counsel.

At Rutgers, my thanks are due to my committee: Jack S. Levy, Bob Kaufman, and Michael Shafer. They were wise enough to try to dissuade me from this project, and yet retained good sense to let me give it a go. Jack deserves special praise. He volunteered to chair my dissertation immediately, and he only asked about the topic after he accepted. Upon hearing the news, he did not rethink his decision. For that I am grateful. Throughout this process, he has been a steadfast supporter and a wellspring of advice. I can only hope to one day be as successful in this business as he is.

Bill Clark graciously served as an External Reader. Much of what follows reflects his thinking, or rather the sort of approach that I thought that he thought I should adopt. Bill was kind enough to spend many hours with me trying to get me to think about the big questions that lie at the heart of this project and develop strategies for getting answers. I hope that I can mentor future graduate students in the same way.

Michael Bordo also generously agreed to serve as an External Reader. He was gracious in bringing me into his workshop on Money, Finance, and History, where I consistently was challenged to think about new questions.



Ed Rhodes, Kerry Haynie, and Beth Leech also played exemplary roles. Ed showed me how to be a better teacher, and was gracious enough to allow an early version of Chapter Five to be published as a working paper from the Center for Global Security and Democracy. His willingness to endorse my project was much appreciated. Kerry and Beth served as my ‘marketing department’ when I was on the job market, and their willingness to endorse a student outside their field was a model for others to follow.

I was blessed to have a wonderful group of student co-conspirators. Jon DiCicco, Tom Walker, Bill Mabe, Denise Horn, Melissa Brown, and Jeff Becker had the patience to endure my all-too-frequent rants, the willingness to feed me and ply me with beverages, and the wisdom to remind me that there are other valuable uses for my time. Tom deserves special credit for supplying the music at a very critical stage in this process.

Numerous scholars at other universities gave me advice and great comments. Tom Willett, Graham Bird, Lou Pauly, Jim Vreeland, Quan Li, Will Moore, Ashley Leeds, Mischa Taylor-Robinson, Maria Escobar-Lemmon, Erik Wibbels, Jim Morrow, Jude Hays, and Rob Franzese all listened to my ideas and gave me much to think about. Jack Snyder and Ted Hopf played a vital role some years ago by convincing me that I was right for academe.

Others helped me gather the data I needed and helped me to learn what to do with it. Ron Jantz and the staff at the Rutgers Scholarly Communications Center aided this project immensely. The staff at Princeton’s Firestone Library and the University of Michigan’s Kresge Business Administration Library also were essential. At the IMF, thanks are due to Jean Marcouyeux, Kahinde Mbanefo, and Madonna Gaudette for their patience and assistance during my visit. Financial support for my archival research was

generously provided by the Institute for the Study of World Politics. Dean Harvey Waterman also helped support this project. By offering me a Special Study Opportunity Award and a Graduate School Fellowship Fund Scholarship, I was able to take two lengthy trips to Ann Arbor to learn modern political science methods.

I could not have done any of this without the support of my family. My Mom and Dad have lived with this pesky manuscript for years, and they have supported me even when they thought it might never be finished. My sister, Angela Cross, prevented me from spending the past year on the street. My grandmothers were a source of quiet patience and strength. Emily Anna Smith was a latecomer to this enterprise, yet her role was equally important. She has endured more of my muttering and moodswings than anyone, including my pleas to set the alarm clock for 4 am. Despite my curmudgeonly nature, she has been a constant source of affection, a cool voice of reason, and a gentle reminder of what really counts. To all of you, words cannot express the depth of my gratitude for your generosity and grace. Each of you know that this weighty tome means a great deal to me. Each of you, though, mean so much more.

## Table of Contents

Chapter One: Understanding IMF Program Compliance .....	1
The Argument .....	8
The Agenda .....	10
Chapter Two: Literature Review .....	16
Why Keep Commitments? .....	17
Systemic Arguments .....	17
Reputation .....	17
Persuasion .....	20
Enforcement and Endogeneity .....	24
Domestic Arguments .....	28
Partisanship .....	29
Regime Type .....	31
Veto Players .....	32
Why Make Commitments? .....	34
Systemic Explanations .....	35
Realist Theory .....	35
Institutional Theory .....	40

Domestic Sources of the Demand for International Cooperation .....	42
Existing Studies of IMF Agreements .....	46
Implications .....	52
Chapter Three: Information, Inefficiency, and IMF Conditionality .....	53
Conditionality and the Market for Lemons .....	55
Preliminary Evidence .....	63
Program Recidivism .....	69
Conditionality and Program Ownership .....	70
Prior Actions and Screening .....	73
Implications .....	74
Catalytic Finance: What's a "Seal of Approval" Worth? .....	77
Domestic Institutions and Selection: Tipping the Balance? .....	78
Enforcement Costs: Under What Conditions Does the Fund Sanction? ..	81
Compliance as Strategic Interaction .....	84
Conclusion .....	87
Chapter Four: Research Design .....	90
Independent Variables .....	90
Dependent Variables .....	93

Case Selection .....	97
Estimation Techniques .....	101
Coding Criteria .....	103
Reliability Checks .....	104
Appendix One: An Overview of Sample Selection Bias .....	113
Chapter Five: Credibility and Catalytic Finance .....	117
Background .....	121
Past Studies .....	124
A Model of Investor Belief Formation .....	130
Research Design .....	136
Understanding Selection .....	137
Compliance and Catalysis .....	145
Implications .....	152
Chapter Six: Domestic Institutions and IMF Agreements .....	156
The Problem and Its Importance .....	159
Why Demand Fund Agreements? .....	160
Why Supply IMF Agreements? .....	164
From Models to Hypotheses .....	171

Research Design .....	174
Extensions .....	191
Implications for Future Research .....	193
Chapter Seven: Understanding IMF Sanctioning Practices .....	198
Broader Questions .....	199
Argument and Implications .....	201
Empirical Tests .....	206
Determinants of Agreement Selection .....	206
Determinants of Performance Under Fund Agreements .....	207
Determinants of Fund Program Suspensions .....	213
Implications .....	220
Appendix One: Solving The Game .....	227
Appendix Two: Equilibria Under Complete Information .....	230
Chapter Eight: Domestic Institutions and IMF Program Compliance .....	234
Existing Answers to the Puzzle .....	236
Domestic Institutions and the Efficiency of IMF Enforcement .....	242
Which Institutions Matter .....	245
Research Design .....	247

Conditionality as Low Information Equilibrium .....	264
Conclusions .....	270
Appendix One: Alternative Institutional Models .....	275
Chapter Nine: Conclusion .....	277
Objections in Light of Recent Developments .....	278
Evaluation Office .....	279
Good Governance .....	280
Conditionality Reform .....	281
Revising the Rules: Considering Informed Conditionality .....	283
Other Evidence and Implications .....	285
Broader Theoretical Implications .....	289
Implications for the Study of Compliance .....	289
Implications for Paradigmatic Debates .....	292
Rationality and Constructivism .....	295
Domestic Politics and International Outcomes .....	297
Implications for the Study of Domestic Institutions .....	298
Implications for the Study of International Institutions .....	299
Sources Cited .....	301
Vita .....	323

## List of Tables

Table 2-1 Some Existing Studies of Selection Into IMF Agreements .....	46
Table 3-1 Selected Studies of Fund Program Compliance .....	64
Table 3-2 Source of ESAF Program Interruptions .....	67
Table 3-3 Hypotheses Testing the Information Failure Argument .....	89
Table 4-1 Contrasts Between Existing Measures of IMF Compliance .....	96
Table 4-2 List of States in No Fund Program Panel, 1979-1995 .....	98
Table 4-3 Fund Stand By and EFF Programs, 1979-1995 .....	98
Table 4-4 Reliability Check Sample .....	104
Table 5-1 Previous Studies on the Catalytic Effect .....	126
Table 5-2 “Naive” Model of Program Selection and Flows .....	127
Table 5-3 Updated Beliefs, Agreement Decision Phase .....	132
Table 5-4 Updated Beliefs, Agreement Compliance Phase .....	134
Table 5-5 Baseline Selection Model .....	138
Table 5-6 Effects of Selection and Fund Programs on Flows .....	140
Table 5-7 Selection-Corrected Estimates of Program Effects on FDI .....	141



Table 5-8 Effects of Selection and Fund Programs on Flows (First Program Years) . .	143
Table 5-9 Heckman Probit Model of Selection and Sanctioning . . . . .	148
Table 5-10 Impact of Program Suspension on Flows . . . . .	150
Table 6-1 A Public Goods Game . . . . .	162
Table 6-2 Potential Forms of Tipping Argument . . . . .	166
Table 6-3 Testable Hypotheses from the Model . . . . .	173
Table 6-4 Sources of IMF Programs, First Program Years . . . . .	178
Table 6-5 Interactive Tests, First Program Years . . . . .	180
Table 6-6 Predicted Probability of Fund Agreement, First Program Years . . . . .	181
Table 6-7 Regime Type, Legislative Fractionalization, and Agreement Selection . . . .	182
Table 6-8 Sources of IMF Programs, All Program Years . . . . .	187
Table 6-9 Interactive Tests, All Program Years . . . . .	189
Table 6-10 Predicted Probability Under IMF Agreement, All Program Years . . . . .	190
Table 6-11 Regime Type, Legislative Fractionalization, and Agreement Persistence . .	191
Table 7-1 Fund Program Selection and Performance . . . . .	209
Table 7-2 Unified Model of Agreement Selection and Sanctioning . . . . .	216
Table 7-3 Predicted Probabilities of Fund Sanctions . . . . .	219

Table 8-1 Contending Approaches to the Study of Compliance .....	241
Table 8-2 Choice and Efficiency in IMF Enforcement .....	244
Table 8-3 Domestic Institutions and Performance Under Fund Programs .....	246
Table 8-4 Domestic Institutions, Program Selection and Performance .....	248
Table 8-5 Interactive Effects of Domestic Institutions on Performance .....	251
Table 8-6 Institutions and Performance Under Fund Programs .....	253
Table 8-7 Domestic Institutions, Selection, and Program Suspension .....	258
Table 8-8 Interactive Tests: Effects of Fractionalization in Democracies .....	259
Table 8-9 Predicted Probability of Fund Sanctioning .....	260
Table 8-10 Democracy, Legislative Fractionalization, and Program Compliance ....	266
Table 8-11 Summary of Alternative Institutional Models .....	275

## List of Figures

Figure 1 Conditionality as Market for Lemons .....	58
Figure 2 Noncompliance Over Time .....	65
Figure 3 Enforcement Game .....	81
Figure 4 Compliance as Strategic Interaction .....	85
Figure 5 Bayesian Model of Investor Beliefs .....	130
Figure 6 Compliance and Updated Beliefs .....	136
Figure 7 Demand for Fund Agreements .....	164
Figure 8 Delegation Game .....	168
Figure 9 Uncertainty and IMF Agreements .....	170
Figure 10 Enforcement Game .....	202
Figure 11 Research Design for Institutional Effects .....	243
Figure 12 Institutions and Monetary Policy .....	252
Figure 13 Institutions and Fiscal Policy .....	253
Figure 14 Fund Sanctions and Democracies .....	262
Figure 15 Hazard Rate for IMF Sanctions .....	269

## **Chapter One: Understanding IMF Program Compliance**

Countries sign letters of intent with the International Monetary Fund (hereafter IMF or Fund) for balance of payments support. In exchange for currency to strengthen a state's reserves, the Fund asks borrowing states to implement policy measures aimed at ameliorating the economic crisis. These policy measures can include fiscal and monetary austerity, trade liberalization, and exchange rate reform. This exchange of policy reform for balance of payments assistance is what is termed conditionality.

While on its face conditionality might be seen as a win-win proposition, in reality, IMF programs often break down. An 1995 IMF study noted that in a sample of 59 conditional lending programs, Fund assistance was suspended in 35 cases. In these instances, the borrowing states were not eligible for the full amount of the loan because they failed to meet the conditions outlined in their letters of intent. Thus, programs intended to address balance of payments problems can fail to achieve their intended aims because cooperation between the IMF and the borrower unravels. In light of the ongoing public debate over the influence and role of the Fund, more attention must be paid to why it is that these programs are not completed as scheduled. This work focuses on the sources of this inefficiency by examining both the Fund's enforcement incentives and the role of domestic institutions.

The problem of Fund compliance is profound. The evidence that I have gathered for this project suggests that the noncompliance rate for Fund programs approaches 40%. This finding is confirmed by Mussa and Savastano (1999), who note that between 1973 and 1997, more than a third of all Fund arrangements ended with disbursements of less

than half of the original support. Other studies that focus on specific categories of Fund arrangements report similar results. For example, of the thirty Extended Fund Facility (EFF) programs initiated prior to January 1, 1985, twenty-four were either renegotiated or had payments interrupted.<sup>1</sup> Sixteen of the twenty-four were suspended outright by the Fund (Haggard 1986:157-158). The recent review of the Extended Structural Adjustment Facility noted that only one-quarter of these arrangements have been completed without interruption (International Monetary Fund 1997:42).<sup>2</sup> Thus, regardless of the timeframe studied or the type of program, it is clear that the ‘medicine’ of IMF conditionality is not always taken by the patients, and as a result the doctor discontinues the treatment.

Unpacking the sources of Fund program compliance thus helps us answer both policy relevant and broader theoretical questions. In order to make future Fund programs more credible and more successful, we need to understand the factors that cause them to fail. Numerous arguments exist on what the Fund should do, but these are seldom rooted in a clear depiction of the strategic problem that the Fund faces. This project aims to fill this gap.

Moreover, understanding why IMF agreements break down helps shed light on broader debates within IR scholarship. On paper, the IMF is one of the most influential international institutions. Not only are its programs believed to serve as seals of approval to international markets, but the Fund also has the ability to discipline states by

---

<sup>1</sup> EFF is a larger lending program intended to last 24-36 months in duration.

<sup>2</sup> ESAF (now known as the Poverty Reduction and Growth Facility) is intended for very low income countries.

suspending programs. Unpacking the sources of program compliance helps us to understand the actual degree of influence that the Fund has over both states and markets.

Previous attempts to explain variations in IMF program compliance generally make two sorts of arguments. The first suggests that the Fund relationship with developing countries fails because of principal-agent problems (Killick 1996, 1998). In economic parlance, this relationship is one in which a principal, such as a manager, has to design a contract to get an agent, such as a traveling salesman, to perform. Killick argues that the failure of conditionality stems from a failure of the Fund to design appropriate incentives for politicians to honor their commitments. This holds in two senses. First, Fund programs do not deliver the goods that they promise. While it is often thought that conditionality serves as a 'seal of approval' to international markets, this effect has been difficult to establish empirically (Bird and Rowlands 1997, 1999). Second, the Fund has a vested interest in lending, and as a result its own monitoring behavior is inconsistent. It responds to noncompliance by terminating the loan, but it eventually goes back to the bargaining table with countries. Because this is common knowledge, the Fund's threats to suspend programs carry little weight. As a result, politicians can wait out the Fund and attempt to secure better deals through delay.

While this argument helps to clarify the links between choices and outcomes, it leaves a great deal unresolved. First, it raises a larger question of where this inefficiency comes from and why this system persists over time. Applying the principal-agent analogy is a helpful device, but it raises further questions. The claim that Fund programs fail because the contracts are imperfect requires that we better understand the sources of this imperfection. Thus, we need more sophisticated arguments.

An example will help clarify this point. Some suggest that the Fund explicitly prefers to lend, and they attribute the sources of its behavior to "pushing money." However, it clearly also cares about securing policy reforms. After all, if it was solely interested in lending, then it would not suspend programs, since this entails cutting off the flow of loans. Thus, the Fund's preferences need to be more clearly spelled out, because it clearly values policy reform in its utility function. Moreover, a better understanding of Fund preferences clarifies why ineffective conditionality—a pattern of repeated program failures over time—appears to be an equilibrium outcome.<sup>3</sup>

A second problem with Killick's argument closely follows. Suggesting that IMF agreements are imperfect contracts raises the broader question of why any Fund agreements are honored. Indeed, by suggesting that the Fund is not a credible enforcer, and arguing that it does not uphold its end of the bargain, we would expect that the noncompliance rate would be much higher than it is in practice. Given a structure of incentives that allows for cheating, why would any government comply? In other words, this argument cannot explain variation.

Not only is it the case that this argument overpredicts noncompliance, but it raises a further question. Killick's argument does not help us understand the conditions under which states choose Fund agreements in the first place. If Fund programs offer few benefits, and promise certain costs in the form of implementing austerity measures, why sign the agreement in the first place? Would a government not seek bilateral aid or commercial loans, which do not entail conditions? Thus, if we take Killick's argument to

---

<sup>3</sup> In game theoretic terminology, an equilibrium exists if no players can gain from switching their strategies.

its logical limits, there are two critically unresolved issues: why enter the agreement in the first place, and why comply with it at all?

The problems with Killick's argument resembles the early critiques of systemic theory in international politics, in that in order to explain variations, it often required consideration of the domestic level of analysis. Theoretically, however, domestic politics was treated by systemic theory as 'residual variance' (Moravcsik 1993). Just as neorealist theory needed to increasingly incorporate the domestic sources of preferences to answer questions, we cannot understand the decisions to enter or honor IMF agreements without a better understanding of the preferences of politicians, which necessitates sustained attention to domestic politics.

If the essence of Killick's argument is that international incentives best explain variations in compliance, another literature suggests that domestic incentives are more important than the international ones. In the eighties and nineties, a substantial research program emerged on the politics of economic policy reform. This literature sought to explain the sources of different policy responses to economic crisis across LDCs. The arguments that grew out of this literature, which focus on factors such as the relations between executives and legislatures (Krueger and Turan 1993; Lal and Maxfield 1993), party organization and electoral rules (Bates and Collier 1993; Grindle and Thoumi 1993), federalism (Wibbels 1999), and electoral cycles (Nelson 1990), appear frequently in recent scholarship.

Though these studies excelled at developing causal arguments regarding the effects of domestic institutions, the role of the Fund was often relegated to the background. In focusing on domestic politics, issues such as a state's level of influence



with the Fund were either minimized or omitted as explanatory factors. While the decision to focus on some variables at the expense of others is at some level sensible, this decision has important consequences for what we can learn about compliance with a Fund agreement. Since compliance emerges through a process of strategic interaction between the Fund and the LDC, an argument that focuses solely on domestic factors to the detriment of international factors is incomplete.<sup>4</sup> After all, what we see as compliance with an agreement could represent two outcomes: either states meet their commitments and are not punished, or states do not meet their commitments, but also are not punished. Thus, one could attribute compliance with the agreement to the presence of certain domestic institutions, but it could also be the case that the state has a high degree of leverage over the Fund, and is not punished for this reason. Failing to address the alternative explanation of lax enforcement means that the findings may be spurious. In other words, there are inferential reasons for taking both domestic and international explanations seriously, and any approach to theory building has to address this domestic international nexus (Frieden and Martin 2001).

In addition to safeguarding against spuriousness, a more balanced appraisal promises answers to new questions. Focusing on one level of analysis to the detriment of the other means that some of the most compelling questions at the nexus of international and comparative politics remain unanswered, such as: under what conditions can the Fund effectively ‘tip the balance?’ Does the Fund treat certain types of states differently, or is it impartial? Why does the Fund tolerate substantial spells of noncompliance, and

---

<sup>4</sup> Levy (1989) makes a similar argument about explanations couched at specific levels of analysis and their ability to explain the outbreak of war.

does it devise letters of intent strategically so as to reduce the incidence of compliance problems? How does the Fund's behavior help or harm the ability of domestic reformers to set the agenda? These sorts of questions bring the insights from the first literature, which focused on the agreement and enforcement incentives of the Fund, to bear on the second, which focuses on the role of domestic politics. If we seek to answer these questions, we need to build on the insights of previous works and take both domestic and international incentives seriously. After all, any finding that suggests that domestic institutions help explain compliance outcomes confronts the issue of why the Fund does not know about this and devise programs appropriately. Again, we have to ask why inefficiencies in conditionality persist over time.

If our goal is to develop a deeper understanding of the Fund's relationship with developing countries, how can we best accomplish this? In the pages that follow, I attempt to develop testable hypotheses across levels of analysis. I unpack incentives on both the domestic and international levels through decision-theoretic and game-theoretic models and focused empirical tests.<sup>5</sup> Given the large number of Fund programs, the research design for this dissertation is therefore a large-N one, which is designed to help us understand the interaction of international and domestic factors in the aggregate, and strengthen our knowledge base about the Fund's behavior and the effects of its programs.

---

<sup>5</sup> This follows Lake and Powell's (1999) exhortation to think of relationships initially in a partial equilibrium framework, and then integrate them to develop a more complete picture.

## **The Argument**

To recap, this project answers two related questions. First, why do IMF programs fail? Second, why are repeated program failures (and the inefficiency this implies) an equilibrium? My argument traces the inefficiencies in conditionality to the Fund's information environment. While Fund conditionality is intended to solve a state's balance of payments disequilibrium, whether a state can or will implement the austerity measures in the letter of intent is a political issue, not an economic one. Unfortunately, the borrower's degree of commitment to a Fund program is known only to the borrower. Thus, the problem with conditionality is that the Fund makes loans and enforces them under uncertainty about whether a borrower is committed or not.

Conditionality continues to produce such meager results because this low information environment is an equilibrium outcome. Because the status quo for countries in balance of payments crises is a continued deterioration of the economy, they have incentives to seek Fund programs whether they are committed reformers or not. Thus, the mere fact that a country wishes to sign a letter of intent does not convey any information about whether it will honor it.

Of course, this is a justification for program monitoring. Installments of an IMF loan, known as tranches, are released to the country if it continues to honor its promises in the letter of intent to implement austerity measures. States that breach their commitments find that their programs are suspended by the Fund. Unfortunately, the potential for Fund sanctions also does not screen out marginal reformers. Indeed, politicians in these countries can benefit from some reform, since implementing programs that break with the status quo, but do not fully reflect the Fund's wishes, is a better

outcome than doing nothing to solve a country's economic problems. Thus, partial reform can be an equilibrium, and politicians can have incentives to 'gamble' and enter agreements that they may not be able to honor.

Thus, both committed reformers and marginal reformers face incentives to enter Fund programs, even if the potential for sanctions exists. In other words, political leaders that sign Fund programs do not *reveal* any information to the Fund about their ability or willingness to implement the adjustment program. At the same time, however, the Fund faces very weak incentives to *acquire* information about whether their programs will be implemented or not. This is because the Fund faces a perennial tension between lending and selectivity. On one hand, its resources are intended to be available to all states to aid balance of payments problems, and on the other, it is supposed to act to safeguard its resources. The need for safeguards was one of the justifications behind conditionality, but since politicians know the Fund is not going to permanently abandon them, they have incentives to challenge the Fund and deviate from their promises.

Much of the Fund's recent policy changes regarding conditionality can be seen in an informational context. The content of conditions has expanded in many countries to encompass structural reforms. Some argue that this mission creep hurts program implementation because the Fund asks too much, but this raises a larger question of why more conditions are added. We can think about this as an attempt by the Fund to make states reveal their degree of commitment to the program. By increasing the burden of conditionality, presumably only committed reformers would sign letters of intent. Again, once we understand that even partial reform is better than the status quo, it becomes clear that more conditionality is unlikely to be better conditionality.

The above depiction of the low information environment in which the Fund operates is one in which domestic institutions have no effect on the demand for or the ability to commit to an agreement. Of course, we know that this is not the case. What I do throughout this project is compare our expectations regarding the effects of low information conditionality to real world data taken from the experiences of 106 developing countries from 1979 to 1995. Using the existing literature on institutions and their effects, I derive hypotheses consistent with the low information conditionality argument. If my claim is correct, then we expect to see evidence corroborating this account on a number of fronts. First, we would see evidence that private markets would not respect the Fund's endorsement, since they do not believe that the Fund has an informational advantage in ascertaining a borrower's degree of commitment to reform. Second, the Fund fails to design letters of intent around the domestic constraints that leaders face, so that it 'tips the balance' only very rarely and under specific institutional conditions. Finally, we would expect to see mismatches between crime and punishment. That is, even after controlling for a state's performance under the program, we see certain types of states more likely to be sanctioned by the Fund. I discuss each of these hypotheses briefly below, and elaborate the argument more fully in Chapter Three.

### **The Agenda**

As noted, three implications follow from the claim that the failure of conditionality is an informational one. The first of these is a straightforward empirical question: what are the effects of Fund programs, and of program noncompliance, on the behavior of international markets? My evidence suggests that the Fund's 'seal of

approval' is nonexistent. Flows of new loans and investments are not enticed by a state's decision to enter a Fund letter of intent. Outside observers learn that a state that signs a Fund program is a risky investment rather than a sure one. Thus, the signal that is sent confirms that a state's investment climate is a poor one. This null finding regarding the so called 'catalytic' effect of Fund programs holds even when we control for the degree of program compliance. Whether or not a state is honoring its commitment conveys added information to the investor only under rare conditions. These findings strongly question the neoliberal notion (Keohane 1984) that international institutions can serve as information providers. Moreover, it is not at all surprising, given these findings, that politicians are loath to enter Fund agreements. Given the effects of these programs on market behavior, they indeed promise certain costs and uncertain benefits.

The second stage in the project is to understand the decision of the Fund to offer agreements and sanction noncompliance. This question is critically important for linking the international and domestic levels. It is also one for which existing theory provides numerous answers. My approach is first based on an incomplete information model of the enforcement process. In this model, the Fund makes decisions whether to offer an agreement and whether to sanction noncompliance under uncertainty; it does not know whether a given borrowing state is a committed reformer or a reform minimizer. A state (treated for modeling convenience as a unitary actor) chooses to enter agreements and honor them based on its assessment of the costs and benefits of reform. One of the results from the model is that the Fund treats states differently based on its assessment of the costs of enforcing the letter of intent. If the Fund faces a state for which the enforcement costs are high, cheating is common, and the Fund is much less likely to sanction it. Thus,

in this model, the international influence that states have with the Fund creates incentives to cheat that can trump the domestic commitment to reform. Significantly, weak results exist for the proposition that US influence affects Fund behavior, providing further support for the enforcement costs hypothesis. This model thus helps us to understand the boundaries of the Fund's influence given that its clients are not all equally influential, and thus seeks to clarify the conditions under which Fund enforcement is credible.<sup>6</sup>

While the results from this model point out how the Fund's leverage can be limited, we still need to open the black box of the state to understand the role played by domestic institutions. The crucial insight from the economic policy reform literature is that implementing reform involves a decision by political actors to coordinate on actions that they may find individually costly. We start by focusing on reform as a public good, and then assess how domestic institutions: specifically regime type, legislative organization, and electoral rules, affect whether this good is provided. One reason the IMF assumes importance, then, is that conditionality can provide resources to help domestic actors build a pro-reform coalition (Fischer 1997; James 1998). This, of course, is a common argument. However, the assumption underlying this argument; namely that the Fund devises conditionality differently across polities so as to 'tip' the balance, has not been noticed. I find that even after controlling for economic (and geopolitical) factors, as a state's degree of legislative fractionalization increases, a state is more likely to be under an IMF program. However, this only holds for nondemocracies. The higher costs of adjustment in democratic regimes (due to both distributional and competence concerns)

---

<sup>6</sup> Thus, in contrast to Killick, we have an argument at the systemic level of analysis that explains variations in performance and program suspension.

do not produce a greater demand for IMF conditionality, and so in these states, increasing a state's degree of legislative fractionalization does not produce a higher likelihood of entering a Fund program. Thus, because the Fund is uninformed about the political constraints that its clients face, it does not design conditions attached to its loans so as to appreciate the higher costs of adjustment associated with accountability. As a result, these states are less likely to enter its programs.

In order to understand compliance, we need to understand both performance under the agreement as well as enforcement of it. If the Fund's account of program monitoring has merit, then we would expect to see that those states that exhibit poorer performance are those that are sanctioned. Our evidence suggests that this is indeed the case, but the Fund's low information environment produces mismatches between performance and sanctioning when we control for the effects of institutions. To the point, we see little evidence that democracies with highly fractionalized legislatures exhibit poorer performance under these programs, but after controlling for performance, they are more likely to be sanctioned. The Fund seems to operate with a belief that these states are poor reformers, thus sanctioning them more frequently. This belief, however, is not supported by our evidence. Again, the notion that Fund program suspension is inefficient stems from low information. Thus, the Fund needs to take the politics of adjustment seriously in the design of its programs, and better appreciate the domestic constraints that leaders face.

Our evidence to some extent exonerates the IMF. It is not the puppet of the US Treasury as suggested by some observers, nor is it the mindless loan-pushing bureaucracy depicted by others. However, it is an exaggeration to dismiss all the claims advanced by



its critics. Fund program noncompliance stems from the Fund's inability to acquire better information about the politics of adjustment; namely how domestic institutions shape the demand for IMF programs as well as performance under them. Developing more credible and more successful programs requires the Fund to take politics seriously. At the same time, the evidence here suggests that the Fund is not uniformly influential. States with high enforcement costs are able to hold it hostage, and thus we see more 'crime' and less 'punishment' in these states. Thus, one road ahead lies in the Fund increasingly developing the ability to just say no to large borrowing clients. Whether this selectivity will obtain in the future is an issue open for debate.

Broader implications emerge from this analysis for the role of governance. "Good governance" has become an increasingly important mantra in Fund operations, but exactly what governance is is rarely defined. The analysis here suggests that the Fund needs to not only develop the means to understand the politics of adjustment, but incorporate that knowledge into its operations. We see that democracies with high degrees of legislative fractionalization are not more likely to enter Fund agreements, and even though they exhibit mean levels of performance under the programs, they are more likely to be sanctioned. Thus, not only does the Fund need to better understand governance, but it needs to incorporate that knowledge into its operations in order to aid those countries for whom conditionality was envisioned.

Through unpacking the compliance problem in a step-by-step fashion, these results help us to better understand the strategic environment in which the Fund operates, and on a deeper level, how difficult its task is. Several lessons emerge from this analysis. First, my work points to a need to take politics 'out of the error term.' The Fund's

rhetoric presently focuses on the importance of ‘borrower ownership’ as a key indicator of whether a letter of intent is likely to be honored. Lacking a theory about program ownership however, guarantees that this phrase will certainly not form the basis for designing credible adjustment programs. This project offers the promise of building strong theoretical arguments that can be used to derive policy-relevant lessons about the politics of adjustment. Second, the work suggests an approach to the study of domestic institutions that differs substantially from existing work. Many arguments about the links between institutions and credible commitments exist in the literature. However, in order to test these accurately, we need to consider their effects both on the decision to enter agreements as well as honor them. Through linking theory and data, this project advances new answers to old questions regarding the effects of domestic institutions. Finally, this work develops a much clearer understanding of the policy choices that international institutions make, and the constraints under which they operate. Numerous arguments have been advanced as to why the IMF does what it does, but there has been very little head-to-head testing. As a result, the Fund is toothless to some observers and omnipotent to others. By developing and testing clear arguments about the Fund’s behavior I advance our understanding of the sources of the IMF’s influence and its limits in a world of global markets and sovereign states.

## Chapter Two: Literature Review

The evidence suggests that compliance with IMF programs is low, and that conditionality fails because states do not honor their agreements. How can we explain these outcomes? The existing literature offers a myriad number of answers for why commitments are kept. I discuss these in the pages below to orient this project in a broader research community. In order to better understand the genesis of the present project, I organize this literature review around two basic questions: what are the conditions under which commitments are made, and what do we know about when and how commitments are kept? I argue below throughout that these two questions are intimately linked, and that the answers to one question bring empirical implications for how the second should be answered. I close each section by addressing the notable silences or unanswered questions in the literature, and close the chapter by outlining the big questions that underpin the argument that appears in the following chapter.

The central argument of this chapter is that previous studies of compliance, which regard the decision to keep commitments as separate from the decision to make commitments, are prone to inferential confounds from selection bias. In order to understand why states honor international agreements, we have to understand how and why they are made. Failure to understand this can result in faulty causal inferences. Thus, to fully understand compliance, we have to broaden our theoretical scope. While this argument appears in other contexts in the IR scholarship, the implications have not been studied by scholars working in the area of international cooperation. For example, scholars studying deterrence or alliance formation note the potential for selection effects

(Morrow 1989; Fearon 1994). My claim is that compliance with international agreements is not analytically distinct from these other research areas. Thus, below I outline how existing scholarship has answered two questions in international relations theory: why keep commitments, and why make them?<sup>7</sup> I then go on to outline how these questions are linked, and develop the consequences that emerge as a result of studying these questions separately.

### **Why Keep Commitments?**

Under anarchy, states often make and break agreements with little consequence. Thus, the puzzle of compliance is how to account for the robustness of cooperation in an environment in which continued commitment can be costly and incentives to defect exist. What causal mechanisms keep states committed to agreements? The literature offers a number of potential candidates from the systemic and domestic levels of analysis. At the systemic level, I address the role of reputation, persuasion, and enforcement. On the domestic level, I discuss partisanship, regime type, veto players, and two level games.

#### *Systemic Arguments*

##### *Reputation*

From the first applications of game theory that stressed the iterated nature of cooperation (Axlerod 1984), early institutionalist works viewed reputational considerations as essential to support compliance. A reputation based on a state's past

---

<sup>7</sup> In Chapter Eight, I address additional lines of argument stemming directly from the literature on IMF programs.

behavior becomes an important signal about its future behavior. Because a state's reputation signals its trustworthiness to others (even in areas unrelated to the original agreement), states comply with commitments in order to guard their reputations (Keohane 1984:257-258). States that habitually violate agreements produce bargaining problems in the future because agreements become difficult to reach. Moreover, concern over the consequences of violation also helps to "lock" states into costly agreements.

Noncompliance can create a precedent and invoke a spiral of defection by other actors that unravels the institution. This creates a sub-optimal situation, since the goods that were sought by initially forming the institution will not be delivered. Fear of these consequences can create incentives to remain in compliance. International institutions thus serve an important role. By disseminating information and providing standards of good conduct against which state behavior can be assessed, they lower the cost of preserving a good reputation (Axelrod and Keohane 1985:250).

The link between reputation and compliance is problematic in the case of the IMF for several reasons. First, it is difficult to say who or what has the reputation. Does it accrue to governments or states? Institutional theory is under-specified because it fails to elaborate the conditions under which governments will invest in reputations.<sup>8</sup> The strategic setting that governments confront when they seek assistance from the Fund—namely, a balance of payments crisis—may serve to shorten government time horizons. Santaella (1992) suggests that the pressures of balance of payments crises mitigate against attempting to establish a reputation for credible debt reduction or

---

<sup>8</sup> Keohane does suggest, in a prelude to more sophisticated domestic arguments, that outgoing governments may want to "lock-in" successive ones (1984:116-118).

inflation fighting. Since states face adverse conditions when they approach the Fund, they also may be prone to ‘overshooting’ as a means to signal credibility (Rodrik 1989). In other words, Fund programs are a case where the strategic setting may undermine the importance of reputations. The economic crisis governments face can render time horizons short, and reputational considerations can be moot as a result (North and Weingast 1989:807; Shepsle 1991:253; Simmons 1993:45,285-286).

We can take this line of argument to its logical conclusion. States enter Fund programs when they face balance of payments problems. If it is the case that the source of the problem is attributable to the government, such as excessive spending, then it is difficult to say that a “reputation” existed before entering the agreement. If governments have bad reputations when they enter the agreement, what is it about the agreement that changes their valuation of reputations? This line of explanation resurfaces when we discuss credibility and signaling in a subsequent section, but it should be clear that this changes the question from why honor the commitment to why make it in the first place. In sum, the strategic setting for governments clearly matters by shaping the demand for a reputation as well as whether it can be preserved. How strategic settings matter, however, is unaddressed in institutional theory.

Even if we disregard the notion that reputations may fit IMF agreements rather poorly, it is clear that politicians may make tradeoffs that reflect personal benefits rather than international ones. Thus, a reputation can be underproduced, especially if preserving it requires politicians to sacrifice personal survival. Some evidence suggests that the degree of cooperation with the IMF closely follows electoral cycles. One reason IMF aid suspensions occurred in Mexico and Poland is because of election-year budget-busting

(Kaufman, Bazdresch, and Heredia 1994:367, Kearns 1994:386-387). Again, the institutional approach does not explain when and how these tradeoffs take place, which is vitally important. In short, whether we focus on constraints produced by either the economic crisis or the electoral calendar, we require a more nuanced theoretical understanding of how governments value and produce international reputations in order to assess their effect on compliance.

### *Persuasion*

Another set of causal mechanisms thought to explain variations in compliance with international agreements has been recently put forward by the managerial school (Chayes and Chayes 1993, 1995; Mitchell 1994). In the eyes of the managerialists, noncompliance is a no-fault problem that is resolved by diplomatic dialogue. States essentially sign treaties that reflect their interests (Chayes and Chayes 1993:179,186), and noncompliance, when it happens, results from a state's capacity problems in implementation or from ambiguities in the agreement itself. It does not reflect willful calculation or an intentional flouting of the agreement by the signatory state. Targeted assistance is also essential for states that are unable to keep their promises because of capacity problems. In short, in this approach, compliance problems, which are rare by definition, are managed away. It should not be surprising, then, that the managerialists regard the record of state compliance with international agreements as a rather positive one.

The managerial argument is that states are predisposed to comply with agreements, which is why they sign them in the first place. Evidence from IMF programs

challenges their interpretation of the sources of noncompliance. First, it is difficult to argue that noncompliance results from ambiguity. This can be easily shown from the letters of intent, which specify the performance criteria for fiscal and monetary variables in a contract-like fashion. The performance criteria are benchmarks for the state to achieve in order to reach its targeted change in the balance of payments. Predicting where specific macroeconomic aggregates will be in 12 months is certainly an exercise prone to error, so these targets are sometimes negotiated and changed over the course of the program. The fundamental point remains, however: politicians know what they need to do in order to remain in compliance with the agreement.

This brings us to Chayes and Chayes' second explanation for noncompliance: capacity problems. While issues of capacity may be important for environmental treaties, they have decidedly less explanatory power for implementing stabilization agreements, since here we are talking about a state's ability to implement policies of fiscal and monetary restraint. Moreover, relying on capacity problems as an explanation of noncompliance is somewhat befuddling if we think about the agreement design phase. This argument suggests that the capacity problem was either somehow unknown when the agreement was signed or evolved over the course of implementing the agreement. Why it is that parties would design an agreement to solve a collective problem (such as Greenhouse Gases) and not design provisions to aid parties that may have weak institutional structures remains unexplained. It would be more logical to assume that negotiators sought to change the agreement so as to allow for targeted benefits to weak states in order to gain their adherence. In other words, the existence of capacity problems should be a barrier that is solved at the time that the agreement is signed.



Chayes and Chayes base the claim that noncompliance is “no fault” on the assumption that states have an interest in complying with the agreements they sign. We know that this is not necessarily the case. Defection can be both “voluntary” in the sense that a leader made a conscious decision at time zero to break an agreement, or “involuntary” in that the cooperation of domestic groups needed to implement the agreement was not obtained (Putnam 1988). In either case, we can imagine scenarios in which noncompliance involves a willful decision, especially given that these authors concede that international agreements are often ambiguous and incomplete. Of course, Chayes and Chayes cannot legitimately address the potential for involuntary defection, because their implicit model of the state is that of a billiard ball with no domestic politics (Koh 1997). Compliance involves an ongoing process rather than a one-shot decision, and it is difficult in this situation to suggest that domestic incentives are not influential.

Existing qualitative evidence suggests that compliance is indeed a mixed motive game, and that domestic politics does affect a state’s ability to commit to a Fund program. Numerous case study-based accounts suggest the mere presence of agreement between the IMF and a developing country says little about whether groups that were not party to the negotiations will also adhere to it (Killick 1995:115; Nelson 1984).<sup>9</sup> Moreover, since noncompliance problems result from an inability to meet fiscal and monetary performance criteria, this suggests that the agreement goes offline between the signatories and the institutions in charge implementing austerity measures (Sachs 1989; Schadler 1995; Mecagni 1999). If it is the case that compliance requires political management,

---

<sup>9</sup> Mosely, Harrigan, and Toye (1991) make a similar argument focusing on the World Bank.

then this tells us that securing the cooperation of domestic veto actors is essential to understanding compliance. It also suggests that a view of compliance that is based on the assumption that politicians in the country implementing the agreement have mixed motives with regard to continued implementation is an appropriate assumption for analysis.

Finally, it should be noted that the empirical evidence that Chayes and Chayes (1995:239) use to support the managerial interpretation of the IMF is based on a faulty interpretation. They note that between 1954 and 1984, the Fund canceled fifty-six agreements and established a new arrangement within 30 days of the cancellation in all but nine of these cases. "Cancellation" is used when the Fund changes agreements or when the state does not desire further borrowing in the context of an existing program. It is not an indicator of program compliance.<sup>10</sup> While it is the case that the Fund does follow up agreements, it does not follow these up if a program is suspended until the original agreement expires. Thus, the case for a managerial interpretation of the IMF is tenuous. Though it is clear that the iterated process of negotiating, sanctioning, and renegotiating letters of intent does embody an attempt to persuade leaders of developing countries to implement austerity measures, to infer from this that politicians in developing countries are innocents that are overwhelmingly committed to neoliberal reforms is naive. As Koh (1997) notes, a fully specified managerial model incorporates when persuasion does and does not affect compliance. One needs to more fully integrate domestic political considerations in order to flesh out this model.

---

<sup>10</sup> As noted earlier, the extent of the problem with Fund compliance is much greater than this.

*Enforcement and Endogeneity*

The harshest criticism of Chayes and Chayes was offered by Downs, Rocke, and Barsoom (1996), who argue that the managerialist insight is confounded by endogeneity. If we accept that treaties are a product of strategic interaction between states, then the agreement that results from this interaction may be designed so that it imposes few costs on the signatories. Accords that make few demands on states may have high compliance purely because they require little sacrifice. Thus, inferences about compliance that do not control for the costliness of the agreement compared to the status quo are flawed. Enforcement, then, is essential for “deep cooperation,” a condition in which states are required to make sizeable departures from the status quo.<sup>11</sup> They counter Chayes and Chayes’ empirical study with an appraisal of the arms control regime, and in their view, the weakness of the arms control regime is attributable to an avoidance of deep cooperation. States avoid deep cuts in arsenals because they embody major departures from the status quo and create incentives to renege on the agreement later on. Because the scope of the enforcement problem is directly related to the amount of proposed cuts in the arsenals, this helps us to understand why progress on arms control has been so limited.

In contrast to the managerialists’ presumption of a general propensity to comply, Downs et al see mixed motives. States may face incentives to retain or reject provisions, and this is a necessary component of agreement design. Thus, for Downs et al,

---

<sup>11</sup> In contrast, Chayes and Chayes concede that the potential for cheating exists, but states do not take advantage of ambiguities in the agreement because they have a dominant strategy to comply. Thus, a distinction between these two arguments can be drawn. Chayes and Chayes operate with the implicit assumptions of cooperative game theory, while Downs et al operate in a non-cooperative game theoretic approach.

enforcement remains the essential backbone of international accords that require deep cooperation. In the absence of enforcement, the only pathway to deep cooperation lies in changing incentives so that the incentives to defect from costly agreements are minimized (397). Exactly how these incentives are to be changed, however, is an issue that Downs et al ignore.

Downs et al's approach is paralleled in a number of recent game theoretic approaches to cooperation (Fearon 1998; Leeds 1999). In these works, which deal with the development of bilateral agreements between states, a selection effect exists in that only enforceable agreements are signed. Just as Downs argues that international cooperation will remain shallow because actors will be unable to solve the problem of devising enforcement mechanisms, Fearon and Leeds suggest that actors do not form agreements that they do not think will be fulfilled. Thus, we can see good compliance internationally, but this reflects collaboration regimes in which there are little or no incentives for the actors to defect. Compliance without enforcement thus means that these agreements are much less theoretically interesting. The implications of these arguments for our work is profound, as these authors suggest that we cannot understand compliance without assessing the conditions under which agreements are selected.

Thinking substantively about the IMF's dealings with developing countries, however, challenges the claim that the bargaining and enforcement phases of cooperation are explicitly linked in this fashion. First, contrary to Downs' expectations, signing a Fund letter of intent does embody a departure from the status quo, in that politicians are moving from an economic equilibrium marked by balance of payments deficits to a more 'responsible' equilibrium that involves the use of Fund leverage and assistance to address

the balance of payments problem.<sup>12</sup> Politicians can thus face incentives to enter these agreements, precisely because they embody an attempt to change this status quo. More importantly, we can better see how Fund policies embody deep cooperation through the study of conditionality. The IMF's 'medicine' is a strong dose of fiscal and monetary austerity, which means cutting the budget and slowing the growth of the money supply. Implementing these measures complicates relations between leaders and their constituents. Thus, through the practice of conditionality, we see evidence that IMF stabilization agreements embody this 'deep cooperation.'

Second, not only is austerity deep cooperation, since it embodies a costly change in the status quo, but breaching it is not costless, since states can suffer losses from noncompliance. We know that once programs go "off-line," and aid is suspended, the Fund does not offer a new program until the old one runs out. This is important to note, since it tells us that states do suffer losses in violating their agreements. Compliance with IMF agreements may be a more fruitful testing ground for assessing the impact of enforcement than arms control. This is because arms control brings with it additional problems of monitoring that are absent in studying letters of intent.<sup>13</sup> Again, states know what they have to do to meet their conditions, and they are required to release statistics that reveal whether and to what extent they have met the Fund's performance criteria.

---

<sup>12</sup> Leeds' (1999) model explicitly suggests that the status quo is a payoff of zero, and this is where the selection effect comes in.

<sup>13</sup> Of course, some states have tried to deceive the Fund (Philippines, Ecuador, and more recently Ukraine and Russia). We have no way of knowing how much of a problem this poses.

Of course, to assume that these agreements somehow approximate perfectly enforceable contracts is a misnomer. Actors have domestic incentives to violate these agreements, because economic reforms entail policy measures that alienate constituents, but leaders can benefit from time to time from standing up to the IMF. Callaghy (1992) notes that Kaunda of Zambia became a staunch critic of the IMF when it became apparent that it would be very difficult to meet the Fund's conditions. He broke with the Fund publicly and began espousing a heterodox adjustment package that would be less keeping with the strictures of neoliberalism. Moreover, while enforcement of the letter of intent does exist, as the following chapters will point out, it is clearly imperfect. As recent high profile examples indicate, the Fund can be seen as treating states rather differently in accordance with their level of influence.

Taken together, these facts invite two critical questions. First, why is it that the Fund's enforcement regime is so weak? If the IMF "sets the tone" in these negotiations, then it should be able to design an agreement that is fully compliant. Second, why would lenders sign agreements if their ability to commit to the agreement was uncertain? To answer this question, we would have to accept that leaders make commitments as part of a risky strategy. Unfortunately, we cannot answer these questions with a "black box" model. The answer to each of these questions invites a more systematic examination of the motivations of politicians as well as that of the IMF. Downs et al do not supply this; rather they stress the importance of self-interest and mixed motives of what these are and how they change over time. By incorporating domestic politics, we can better assess the conditions under which compliance is more or less likely, even under deep cooperation.

Similarly, by examining enforcement incentives, we can better understand when and how international leverage can shape behavior.

Thus, these three systemic level accounts share a common deficiency—insufficient specification. This incompleteness stems from a neglect of domestic factors, which do much more than explain residual variance. One cannot understand the role of reputation, persuasion, and enforcement without understanding how states make tradeoffs domestically, are ‘persuaded’ by international norms, or choose to accept or adjust their calculation of the costs and benefits of continued compliance. In order to better understand the impact of domestic attributes and institutions, I turn to assess explanations on this level. This forms the subject of the next section.

### *Domestic Arguments*

Studies of the link between domestic politics and international outcomes have been a major growth area in the literature. These works have a great deal to say about the inside-out links between politics at home and commitments abroad. Different authors choose to focus on specific aspects of domestic institutions, which has given rise to a number of alternative approaches. I address these arguments, which address the role of partisanship, democratic advantage and veto players below. Many of these works are more refined approaches that attempt to provide answers to questions raised by Putnam’s (1988) question; when and how do domestic constraints matter?

### *Partisanship*

Many works attempt to explain policy choice and sustainability by referring to the partisan makeup of governments. Simmons (1994) notes that left governments were more likely to adopt policies that undermined the commitment to the gold standard. We might suspect it possible to make similar arguments about certain types of governments being more committed to Fund supported adjustment, but these sorts of arguments are less salient in developing countries for a number of reasons. First, one of the more interesting events of recent years was the ‘conversion’ to neoliberalism of populist leaders such as Menem in Argentina and Cardoso in Brazil. This waters down explicitly partisan arguments because these leaders adopted policies that one would think they would never adopt. The underlying reasons for this “Nixon to China” phenomenon are important and constitute a more fundamental weakness of partisan approaches. Constituent ties between parties and the electorate are generally much more tenuous in LDCs than in developed countries. Moreover, attempting an explanation through looking exclusively at actor preferences is difficult because the losers of reforms are overinstitutionalized relative to the winners (Haggard & Kaufman 1992).

A more powerful approach to partisanship focuses more on the degree of separation or polarization between political parties. Kaufman and Stallings (1991) note that populist policies were endemic in those Latin American systems that had high fractionalization and unstable cross-party alliances. Haggard and Kaufman (1995) suggest that fragmentation and polarization in party systems help explain variations in the initiation and consolidation of economic reform in new democracies. Achieving a consensus on reform becomes more difficult in an fragmented and polarized setting. In



these environments, collective decision making approximates a typical social choice problem, in that almost any solution can decisively defeat another.

It is not surprising, therefore, that poor policy outcomes result from a high degree of political polarization and fragmentation. Roubini and Sachs (1989) found that states with proportional electoral systems are prone to have large budget deficits, because the electoral system produces partisan deadlocks that make fiscal restraint more difficult. These original results were compromised by measurement problems (Edin and Ohlsson 1991), but more recent results support the original finding in the OECD nations (Kontopoulos and Perotti 1999). A similar result is linking fragmentation to poor fiscal outturns is reported for Latin American states (Inter-American Development Bank 1997).

How these independent variables are measured, however, differs across studies. Polarization is generally not measured, though it is often noted that polarization and fragmentation are often highly correlated (Sartori 1976:132-137). Each of the aforementioned studies adopts a different measure for fragmentation. Roubini and Sachs use dummy variables for the type of political system, the Inter-American Development Bank operationalizes fractionalization by district magnitude scores, and Kontopoulos and Perotti study the number of parties in the legislature.<sup>14</sup> Thus, while there is a consensus that fragmentation makes fiscal austerity more difficult, exactly how fragmentation is operationalized varies. The IADB study relies on the electoral rules themselves that generate the fragmentation, while Kontopoulos and Perotti rely on the outcome of the electoral rules. The field does not have a clear answer for which is more appropriate.

---

<sup>14</sup> District magnitude refers to the number of representatives that can be elected in a single district.

### *Regime Type*

Prior to the emergence of the democratic peace as a field of study, scholars suggested that the ability of states to keep commitments to implement reform varied according to regime type. The early work suggested that democracies would be less able to withstand the political pressures from the losers of the reforms, and it implied that reforms would progress further under authoritarian regimes (Skidmore 1977). Subsequent work (Haggard 1986; Remmer 1984; Geddes 1995) rejected this “authoritarian advantage” line of argument and instead suggested the success rates of reform across regime types was about equal.

More recent theoretical work suggests a need to look at the differences across regimes in a more refined fashion. “Democracy” is not a one-size-fits-all institution, and the use of regime type as an independent variable can mask significant institutional differences and obscure causal mechanisms. The presumption of a democratic (or an authoritarian) advantage invites a necessary question: what is it about democracy that affects the sustainability of reform? The early wisdom on the “authoritarian advantage” suggested that democracies were unable to make credible commitments to reform because politicians would be overtaken by demands for their rollback. The implication is that in the initial phases of reform, strong, insulated executives are essential for their success (Haggard and Kaufman 1992). New evidence challenges this hypothesis. In Eastern Europe, reforms have progressed furthest in those systems that allow for rapid turnover of executives (Hellman 1997). In these systems, the reforms were derailed by elites who gained early from the reforms and continued to extract rents. This line of argument

suggests that political contestation, rather than insulation, provides the ideal route for consolidating economic reforms.

In light of the weak results found by testing these explanations, it is not surprising that Bates and Krueger (1993) attempted to go beyond the regime type dichotomy by focusing their work on how electoral rules shape the preferences of politicians. This work is consistent with the literature on legislative organization and fiscal policy noted above: such an approach invites a more finely-grained theoretical account that allow us to more fully understand exactly how institutions matter.

### *Veto Players*

Another approach linking domestic institutions to sustainable commitments focuses on the existence and number of veto players. A veto player is defined as “an individual or collective actor whose agreement is required for a decision” (Tsebelis 1995). Tsebelis employs a spatial model of collective decision making to assess the conditions under which policies are stable, and derives three hypotheses. First, as the number of veto players goes up, policies become harder to change. Second, as the distance between veto player ideal points increases, policy stability increases. Third, policy stability is also directly related to increases in the internal cohesion of veto players that are collective actors.

This approach produces an interesting hypothesis. Tsebelis links the “bargaining” and “enforcement” phases of cooperation as states with a high number of veto players may find agreements harder to change, but harder to conclude in the first place. If the consent of a large number of veto players is necessary to make a policy change, then this

suggests that getting policy reform inaugurated will also be difficult. As throughout this chapter, we see yet another theoretical argument that attempts to link together the decision to honor agreements and the decision to make them.

An interesting twist on the veto players argument is offered by Heller, Keefer, and McCubbins (1998) who argue that reform is politically feasible if three conditions are met. First, reformers have to control the agenda. Second, reformers have to control all the veto gates (same term). Third, if reformers do not control all the veto gates, they must compensate anti-reform actors. This sequential approach helps unify institutions and actor strategies and offers a more fine-grained account of how politicians can implement a reform agenda.

Though the veto players/gates argument is tractable and promising, there are several problems with it, and more specifically with the Heller et al formulation. First, given that economic reform is inherently a distributional issue, is it necessarily the case that agenda control is an all or nothing proposition? Certainly technocrats or reformers might be able to take control of the agenda only following a crisis. However, the implication of this line of argument is seldom addressed. Many authors note the importance of economic crisis for allowing reformers to take control of the agenda, yet they do not discuss the fact that reformers may not be able to hold on to the agenda. The dislocations produced by the reform process can put anti-reform elements back on top.<sup>15</sup> The Heller et al model suggests that agenda control is an important facet of explaining reform, but what is needed is a more refined formulation that helps us better understand

---

<sup>15</sup> Turkey, Chile, Zambia, Ghana are cases noted in Bates and Krueger 1993. Somalia is noted as a similar outcome in Kahler 1993.

the circumstances under which reformers can take control of the agenda and when they may be likely to lose it.

Second, we may also question whether control of veto gates is a variable or a constant. As the cases above suggest, “control” of veto gates is something that may change over time. Again, compliance is a long term process of implementing the provisions of a deal negotiated internationally. Given this, it makes sense to suggest that a commitment to reform at the start of an adjustment program is not a guarantee of future commitment to reform. In light of the distributional effects of reform, we can certainly expect that commitment may wane over time, a syndrome that Nelson (1990) called “reform fatigue.”<sup>16</sup>

Thus, many contrasting explanations exist for why international commitments are kept. It is clear from the above that both international and domestic factors may take on importance. Some studies also hint at a logic of interconnectedness in that the decision to honor agreements is theoretically linked to the decision to enter them, or that consolidation and initiation of reforms are also theoretically linked. To better understand this interconnection, we have to answer the prior question of why commitments are made.

### **Why Make Commitments?**

The question of the conditions under which states make commitments to international institutions is a sprawling one. I will order this discussion in the following manner. First, I address how traditional IR theory explains the value of international cooperation and the role played by international institutions. Second, I move from

---

<sup>16</sup> Conway (1994) provides evidence of this “J-curve effect” of IMF programs.

systemic to domestic incentives by reviewing the newer theoretical literature on when states “delegate” internationally. Finally, I close this section with a review of the existing empirical studies of when and how states seek assistance from the IMF. These studies tell a great deal about the role of systemic versus domestic explanations for making commitments to international institutions. Existing theoretical traditions in IR address why international institutions are created, the forms that they take, and the functions that they serve. To this end, I address the contributions of Realist and Institutional theory to the debate over international institutions and their causal impact.<sup>17</sup>

### *Systemic Explanations*

#### *Realist Theory*

Realists are skeptical about the influence of international institutions and the possibilities of lasting cooperation. The realist worldview is summarized by E. H. Carr (1939:179), who once noted that international law “cannot be understood independently of the political foundations on which it rests and the political interests which it serves.” In practice, this means that realists deny causal power to international institutions: they cannot affect outcomes because compliance with them reflects short term calculations rather than a desire to be bound in perpetuity by the terms of an agreement. As Morgenthau notes (1985:299) “Governments...are always anxious to shake off the restraining influence that international law might have upon the promotion of their

---

<sup>17</sup> Readers will note that I do not distinguish between classical theorists and their “neo” progeny. This is done for reasons of space, and because I believe that these labels can obscure more than clarify. Where notable, differences within each tradition are underscored.

foreign policies, to use international law instead for the promotion of their national interests, and to evade legal obligations that might be harmful to them.” When commitments are inconvenient and states have the wherewithal, they can be violated at will. In other words, international institutions are epiphenomenal because they *reflect* power relationships rather than transcend them.

Realists do not deny that international cooperation might be of some utility (Waltz 1979:115-116). Rather, they are pessimistic about both the prospects and scope of international cooperation. One reason for this is the divide between low politics and high politics, and how this division affects the potential for cooperation. Realists regard issues of security and power to be the highest priority of state action. States are loath to make commitments in this sphere because of the potential for cheating and relative gains. As a result, the cooperation that emerges internationally will take place largely in those issue areas in which security concerns are not a problem. The existence of dilemmas of common interest (Stein 1982) produces incentives to cooperate, but these areas are largely issues of coordination in which no incentives to defect exist. Thus, the likelihood of international cooperation is inversely related to both the existence of incentives to defect and the extent to which defection is costly. This is a conclusion reached by game theoretic analysts as well (Downs, Rocke, and Barsoom 1996; Fearon 1998:279).<sup>16</sup> As noted earlier, though IMF agreements are not explicitly “high politics,” they certainly involve a significant change in the status quo, and commitments to Fund programs are costly as a result.

---

<sup>16</sup> This is not to say that all game theorists are realists. The use of non-cooperative game theory and the focus on cheating suggests a substantial degree of overlap.

Given that states face prisoners' dilemmas both in trade and security, why is it that cooperation is prevalent in one area and not in another? Lipson (1984) relies on the high politics/low politics divide to account for this distinction. He notes that the risks are much higher in security issues than in economic issues, which in turn means that states value the 'shadow of the future' differently. This line of argument is paralleled by Jervis (1978:172-173) and Mearsheimer (1994-1995) who notes that security is easier to attain when the costs of being exploited (the CD payoff in the Prisoners' Dilemma) is low.

A more sophisticated realist argument is offered by Krasner (1991), who argues that power matters in a more fundamental sense for cooperation. The existence of common interests is not enough to produce cooperation; states must simultaneously solve a distribution problem (Morrow 1994). For Krasner, international cooperation better resembles a Battle of the Sexes game than prisoners' dilemma. The cooperation "problem" is not merely one of avoiding a jointly sub-optimal outcome; after agreeing on a solution that avoids such an outcome, the two negotiating parties must decide who benefits the most from the agreement.<sup>17</sup> Thus, this second stage resembles a Battle of the Sexes, and the two players are selecting which of the two pure strategy equilibria will be chosen. Because the solution to the Battle of the Sexes is dictated by the player that moves first, Krasner argues that the relative bargaining power of actors dictates the form that international institutions take. In some areas, no institutionalization exists, because great powers have been able to secure their preferred outcome unilaterally. In others, great powers created institutions to secure their preferred outcomes, but as power

---

<sup>17</sup> Fearon (1998) devises a joint model in which states choose from a menu of possible agreements, and then play a prisoners' dilemma in the enforcement phase.



relations shifted among actors, distributional concerns became more prevalent, and the institution was embroiled in controversies over which equilibrium would be chosen.

This approach represents a different line of argument. Krasner's work suggests that the outcome that needs to be explained is not the presence or absence of agreements in a given area, but the form that interactions between states take, whether multilateral accords, bilateral accords, or unilateral actions.<sup>18</sup> Thus, the dependent variable in studies of cooperation is operationalized in these studies as a continuous rather than a dichotomous measure.

Scholars who focus on hegemony continue to stress the importance of power considerations in explaining why institutions are devised in the first place. Krasner's work, for example, addresses the benefits that hegemons can capture by creating international institutions.<sup>19</sup> Rather than focus from the standpoint of the country that is ensnared by international commitments, these scholars are more top-down, focusing on how creating international institutions can be an appropriate strategy for a hegemonic power.

For hegemonic powers, international institutions serve a number of roles in addition to public goods provision. First, they mask the exercise of overt coercion (Krasner 1985:62). Snidal (1985:587) and Gilpin (1981) note that hegemons can attempt

---

<sup>18</sup> Lake (1996, 1999) offers a similar line of argument. Both Lake and Krasner are in turn influenced by the literature on relational contracting (Williamson 1985).

<sup>19</sup> Schweller and Priess (1997) in their exhaustive review, divide realism's view of institutions along neorealist and classical realist lines. I find the distinction more a matter of emphasis than a division within the tradition traceable back to Morgenthau or Waltz.

to extract contributions from other actors in exchange for providing public goods.<sup>20</sup> Creating an institution can aid in the formation of ‘k-groups’ (Schelling 1978) that produce public goods.<sup>21</sup> Thus, through an institution, the hegemon can limit its use of coercive strategies. Second, creating a multilateral institution may be more efficient than bilateral deals. This is especially the case if creating such an institution allows for expertise to be invested within it. Creating an international institution can reduce the costs of oversight paid by the hegemon by passing it on to third parties. Allowing all the signatories to oversee each other’s performance reduces the costs of system management. Third, institutions may allow for the formation of transnational ideas that serve to bolster commitment and shape how actors perceive their strategic environments (Ikenberry and Kupchan 1990:291-292).<sup>22</sup>

Some recent work suggests that US influence is a key determinant of IMF operations (Killick 1995, Finch 1983; GAO 1999). Thacker’s (1999) empirical study of the affects of political influence on Fund programs is a major case in point. His paper establishes a link between a state’s degree of political affinity with the US and the probability that a state will receive a loan from the IMF. The implications for scholars

---

<sup>20</sup> Lake’s (1993) review of hegemonic stability theory divides the ‘coercive argument’ and the ‘benevolent argument’ into separate theories with different logical underpinnings. We have yet to come to grips with the conditions under which hegemons will solve the public goods problem coercively or benevolently. I return to this line of argument below.

<sup>21</sup> A k-group is the minimum size of a coalition that can come together to provide the collective good. For Schelling, the key aspect is the ratio of K to the number of group members N.

<sup>22</sup> Much has been made of the role of international institutions as ‘teachers of norms’ (Finnemore 1996), which represents an area of overlap between realism and constructivism.

concerned with compliance with these agreements are complex, but important. If it is the case that variables such as political affinity with the US have effects on compliance with agreements, then we have to jointly estimate both the decision to make and honor agreements in order to obtain reliable estimates of the parameters.

Taken in its entirety, Realist theory offers two vital lessons for us in this project. First, it suggests that the institutions that are created to solve public goods problems may not do so in a benign fashion. Second, and related, great powers have incentives to use these institutions to solve their own problems, and as a result the institution's degree of independence from these great powers is likely to be small at best.<sup>23</sup>

### *Institutional Theory*

In contrast to Realism's stress on the role of distributional issues, institutionalists (formerly known as neoliberal institutionalists)<sup>24</sup> focus on problems of market failure. These are problems in which mutually beneficial deals cannot be reached because of high transaction costs and imperfect information. International institutions are created to solve these problems and provide public goods. To be clear, they are generally created by hegemons, but hegemony is neither necessary nor sufficient to explain their persistence (Keohane 1984:49). Institutions alter transaction costs and allow for economies of scale in negotiation to be established. Moreover, they act to disseminate information and in so doing reduce uncertainty.

---

<sup>23</sup> Of course, we could think of the relationship between major powers and IO as a multiple principals problem, in which the IO can balance the competing interests of great powers against each other and in so doing hollow out a sphere of relative autonomy.

<sup>24</sup> Previously, this school was known as neoliberal (Keohane 1993).

There are several problems with this line of argument. First, it is admittedly functionalist, as the causes of regimes are explained in terms of their effects. This poses a problem because this explanation cannot account for the evolution and change of these institutions (Barnett and Finnemore 1999). If they solve problems, why would states not continue to employ them? For example, the willingness of major powers to use the IMF for policy coordination has changed over time (Pauly 1997; Putnam and Bayne 1987). Starting in the 1970s, a series of annual summit meetings were held by the G-7 outside the IMF's bailiwick. This poses a problem for a functionalist argument because it is clear that the problem of policy coordination did not change. Rather, the mode by which the major powers chose to organize their coordination changed. An argument that focuses exclusively on the problem that international institutions solve is ill-equipped to tell us what types of institutional arrangements are selected. In essence, institutional theory has to answer (even on its 'home turf' of economic issues) the question of why arrangements are chosen, and what form they take (Lipson 1991).

Relatedly, institutional theory, while originally created as an alternative to realism (Keohane 1984:84, 1993:292) is silent on issues of power and distribution. It suggests that hegemony is necessary to explain the creation of international institutions, but does not fully specify the conditions under which international institutions become actors in their own right. This is because the issue of delegation from the hegemon to the institution is unexplored by institutionalists. Certainly if the institution is not doing its job, we can imagine that a hegemon (or a collection of major powers) would revise the 'contract' and change the institution. A principal-agent model may be more appropriate here, as we can imagine that a hegemon would exercise "fire-alarm" oversight (following

McCubbins and Schwartz 1984) and intervene only rarely and in response to cataclysmic events. Why this would happen, though, remains unexplained within the realm of institutional theory. After all, the institution is created by a hegemon to solve problems, and after it is created, the hegemon is no longer necessary to oversee it.

Thus, the sources of institutional autonomy remain an important underresearched area (Powell 1994). Institutionalists, on one hand, seem to suggest that hegemons delegate authority to the institution in a once-and-for-all fashion. The realists offer a specific explanation, though it removes any autonomy from the institution by framing it as a creature of relative power of state actors. In this case, what international institutions do are largely determined by who is running the system.

This functionalist underpinnings of institutional theory suggest a further problem. If it is the case that international institutions are created to solve problems, we have bracketed issues of evaluation. Exactly how well or under what conditions these states solve problems are questions that become very difficult to answer in a functionalist perspective. As noted above exactly how or why an institution could fail to deliver on the promises that inspired its creation is a notable silence of institutional theorizing. The inability of this approach to address recent concerns surrounding the IMF's seeming politicization and 'mission creep' is a notable liability.

### *Domestic Sources of the Demand for International Cooperation*

In contrast to traditional systemic theory, which focuses on how international institutions solve coordination and distribution problems, newer works regard them as domestic-level problem solvers (Simmons 1998). That is, they provide resources to

politicians that allow them to address domestic issues and constituencies. For example, Goldstein (1996) notes that creating a binational panel under the US-Canada Free Trade Agreement allowed the President to circumvent the existing system that dealt with unfair trade cases. By empowering an international actor, the Free Trade Agreement replaced national judicial review and strengthened the commitment to free trade by circumventing those institutions that would be likely to rule against it. Thus, delegating internationally alters the domestic agenda and allows certain preferences to be “locked in.”

Moreover, international delegation may allow politicians to give benefits to constituents (Oatley and Nabors 1998). For example, Cowhey (1990) notes that the US has consistently pressed for an international telecommunications regime that strengthens domestic interests. Under Kennedy, the US advocated an international institution that mirrored the domestic telecommunications monopoly of AT&T. Under Reagan, with the move to deregulate, the administration became increasingly critical of Intelsat and advocated more private sector involvement. Thus, leaders design international arrangements so as to reinforce preferred outcomes domestically.

The problems prompting delegation may not be exclusively issues of gains to either politicians or their constituents. Chayes and Chayes (1995) note that one rationale for acceding to international environmental regimes such as the Montreal Protocol (which was intended to reduce ozone emissions) is that by doing this a state can solve problems of domestic incapacity. This institution established a Multilateral Fund to assist developing country compliance with its provisions. Thus, signatories that want to reduce ozone emissions receive assistance for doing so. On the other side of the negotiating table, industrialized countries gain “deniability” by creating this system, because funding

an international organization to make loans to countries to reduce ozone is certainly domestically preferable to unilateral action, which may be seen as taking money away from domestic priorities.<sup>25</sup>

Another additional benefit stemming from delegation is signalling. Acceding to an international institution is a maneuver designed to convey information about a state's policies in an environment of uncertainty. Dhonte (1997) develops this line of argument for IMF conditionality. Signing a letter of intent is intended to signal to private and state actors that a state is committed to adopting responsible policies of economic reform. Similar arguments have been developed for central bank independence (Maxfield 1997) and for states to open their capital accounts (Simmons 1999).

Delegation also allows for governments to secure scapegoats. It helps diffuse opposition because those concessions that might provoke domestic opposition can be framed as a necessary sacrifice at the negotiating table (Vaubel 1991). Negotiators can use their informational advantage to represent or frame concessions as necessary in order to complete the deal, and in so doing attempt to mollify aggrieved interests. This is a very common practice in developing country dealings with the IMF, though it does not always lead to successful implementation of the agreement. Sometimes the policies are initially proposed by governments and then framed at home as foreign imposed. Technocrats sometimes want more in the letter of intent than the Fund originally proposes, in the hopes of securing reform on a number of fronts. Scapegoating is

---

<sup>25</sup> This line of argument can help explain why the US did not create a new Marshall Plan for Eastern Europe after the Soviet Union collapsed. It was much more efficient to delegate the task of overseeing the economic transition to the Fund.

designed to convey information to domestic actors about the Fund's type (namely, that it will be a strict enforcer) and thereby reduce the potential for policy reversals.<sup>26</sup>

Thus, from the above we can better understand when delegation will be demanded. However, the presence of political gains by itself is not sufficient to account for the form that this delegation takes, since these concerns must be balanced against numerous other variables. Sometimes domestic gains can overlap with international imperatives, but at other times these can be at cross purposes. Especially in the area of economic policy reform, politicians face a daunting task of assessing the costs and benefits produced by reform when adopting their strategy of garnering support.

Having briefly surveyed the field on why it is that states sign agreements, the issue of how we adjudicate between these arguments becomes important. To shed a bit more light on this issue, we turn briefly to the existing literature on the selection of Fund agreements. I discuss a sample of relevant works, many of which are written by economists, in order to serve as a plausibility probe for the work that follows. If realist explanations hold empirical support, then we would expect that states that have political affinity with the G7 powers are more likely to receive agreements from the Fund. If, on the other hand, institutional explanations are supported, we would expect that political influence has no effect on the probability of a state receiving an agreement from the IMF. This is the subject of the next section.

---

<sup>26</sup> Others suggests that since scapegoating is most common in those governments that have a weak commitment to reform, it is more a symbol of a lack of credibility than a strategy to strengthen reform (Collier, Guillaumont, Guillaumont, and Gunning 1997). Kenya and Zambia have been adept at framing agreements as foreign imposed, and then breaking them precisely for this reason. (Economist Intelligence Unit, Callaghy 1990).



*Existing Studies of IMF Agreements*

The question of when and how states turn to the IMF has been a subject of a large literature in the field of economics, and increasingly in political science. A number of findings from previous studies are outlined in the table below.

<b>Table 2-1: Some Existing Studies of Selection Into IMF Agreements</b>		
<b>Study</b>	<b>Domain</b>	<b>Significant Variables</b>
Bird and Orme (1986)	31 LDCs 1976 & 1977	Current Account, Inflation, Income per Capita, Eurocurrency Credit, Imports, Reserves
McDonald (1986)	29 Countries 1972-1984	Inflation, Export Growth, Reserves, External Debt, Net Direct Investment, Depreciation Rate
Cornelius (1987)	11 Sub Saharan Africa countries, 1975-1977 and 1981-1983; 33 Non-oil LDCs 1975-1983	Debt Service, GDP per Capita, Imports, Reserves, External Borrowing
Joyce (1992)	45 Countries, 1980-1984	Government Expenditure, Domestic Credit Growth, Current Account, Reserves, GDP per capita
Edwards and Santaella (1993)	48 Devaluation Episodes in LDCs, 1948-1971	Relative GDP per Capita, Net Foreign Assets, Political Instability
Conway (1994)	74 LDCs 1976-1986	Previous Arrangement, Rate of Growth, Terms of Trade, Foreign Interest Rates, Current Account, Long Term Debt
Bird (1995)	40 LDCs 1980-1985	Inflation, GDP per Capita, Trade, Private Finance, Reserves
Santaella (1996)	104 arrangements in 74 countries 1973-1991	Program states have weaker balance of payments, external conditions, and fiscal and monetary policies compared to control group

Knight and Santaella (1997)	91 Non-oil LDCs 1973-1991	<i>Demand:</i> Reserves, Debt Service, GDP per Capita, Real Effective Exchange Rate, Previous Arrangement. <i>Supply:</i> Increasing Revenue, Decreasing Expenditure, Nominal Depreciations >5%
Przeworski and Vreeland (2000)	135 nations concluding 646 agreements from 1951 to 1990	<i>Entering:</i> Country: Reserves, budget deficit, investment, debt service IMF: Balance of payments, number under, dictatorship <i>Remaining:</i> Country: Reserves, investment, number of countries under IMF: Balance of payments
Thacker (1999)	87 LDCs from 1985 to 1994	Balance of payments, debt, interest on debt, reserves, and proximity and movement scores for key UN votes

Taken as a whole, what do these studies listed in Table 2-1 tell us? It seems that realist accounts find support, as do domestic level arguments. First, though many of the above studies omit tests for political affinity, the recent work by Thacker (1999) finds strong support for such an argument. Thus, we cannot overlook the role of political alignment with the US to explain selection into these agreements. Numerous authors have noted the potential for political influence to affect whether or not a state receives assistance from the IMF (Dell 1981; Finch 1988). Russia, for example, has been termed “too important to fail” and for this reason has been repeatedly awarded adjustment programs even when it is unlikely that they will be implemented. This implies a need to understand the role of geopolitical influence in the selection of Fund agreements as well as their enforcement.

The other lesson to be derived from Table 2-1 is that domestic politics is critical to understand the conditions under which states approach the Fund. The study by

Prezworski and Vreeland, for example, offers one finding directly relevant to our discussion. First, they find that agreements are more likely to be signed after elections rather than before them. This confirms the conventional wisdom about the importance of electoral cycles in international cooperation (Lohmann 1993) as well as the literature on economic policy reform, which suggests that stabilizations are more likely during electoral honeymoons (Haggard and Kaufman 1989).

Their second result is also interesting, as they find in their bivariate model that controlling for other economic variables, the Fund is more likely to sign agreements with dictatorships rather than democracies. The authors suggest this is a result of a preference by the Fund for negotiating with states that have lower audience costs. This finding has direct implications for not only the Fund's ability to "tip the balance" but also the debate over the role of regime type and commitment to reform (Kaufman 1985; Remmer 1986).

Domestic political variables enter directly into only one other study in Table 2-1; that of Edwards and Santaella (1993). In this instance, these variables are events data—the number of strikes, coups, government changes, etc. While this provides an interesting counter to arguments that Fund programs increase domestic discontent (Walton and Ragin 1990), this does little to help increase our understanding of how domestic *institutions* matter. It does point out, though, that studies of the link between Fund programs and domestic turmoil need to control for what happens both *before* an agreement is signed as well as the counterfactual for turmoil in the absence of a Fund program.<sup>27</sup>

---

<sup>27</sup> Future research will employ the techniques used in the empirical chapters to address this question.

Though few studies in Table 2-1 take politics seriously by employing independent variables that directly measure political phenomena, deriving the lesson that domestic politics is not relevant to the study of when and how states approach the Fund would be an incorrect inference. The reason why we suspect that domestic institutions are worth studying is that a number of the variables that drive states to adopt Fund programs are those that are controllable by policymakers: budget deficits, government expenditure and domestic credit creation and, to a lesser extent, inflation and the exchange rate. That these policy variables should correlate with the selection of Fund programs is not surprising given that they are a direct implication of the Financial Programming model that the IMF uses to design adjustment programs (Polak 1957, 1991). In other words, these policy variables are the source of the problem that drives the state to the Fund in the first place.

It should be noted that our discussion has come full circle. In order to understand why it is that commitments are kept, we need to understand why they are made, since we suspect that these two decisions are closely correlated. Moreover, we noted in the above section that domestic political institutions and actors, by creating the conditions responsible for balance of payments crises, shape the context under which delegation to the Fund occurs.

The implications for compliance are noteworthy. In order for us to answer the core question: how does domestic politics “matter” for scholars of compliance, we have to proceed carefully, and begin by assessing the role of domestic political variables at the initial stage of making commitments. Why is this the case? A strong view that domestic politics drives outcomes leads us to suggest that institutions affect whether or not

delegation to the Fund is necessary. Alesina and Drazen (1991:1183) suggest in their war of attrition model of reform that “countries with political institutions that make it relatively more likely for opposing groups to ‘veto’ stabilization programs not to their liking will stabilize sooner.” The implications of this argument are that domestic political institutions affect when and how stabilization occurs, meaning that they affect whether seeking the external support of an actor such as the IMF is necessary. We would expect that some states would be able to end the war of attrition and reform credibly, and some states will not be able to do so. Thus, by implication, Alesina and Drazen’s argument can be taken to mean that the decision to enter an agreement and a decision to comply with an agreement are closely correlated ones. Again, to generate reliable inferences in such a setting, we have to widen our theoretical scope—and attempt to understand not merely the factors that explain compliance, but also those that explain the necessity of an agreement with the IMF in the first place. The reason for this is that just as the states that enter into agreements with the Fund are economic outliers compared to the population at large, they may also be institutional outliers as well. Thus, we have evidence that a sample selection bias exists with respect to economic variables of interest, and our understanding of the politics of adjustment gives us reasons to suspect that it may exist with respect to domestic institutional variables of interest (i.e. on the political side) as well.

For the purposes of theorizing on the origins of compliance problems, the study of the politics of adjustment is fundamental. Understanding the factors that drive governments to adopt austerity measures is one thing, but understanding the genesis of these problems is quite another. The question raised by these findings is a simple one: why was it that these macroeconomic problems were not alleviated? In order to

understand why it is that domestic problems become intractable, we need to go beyond these studies and develop a more focused appraisal of the links between domestic institutions and adjustment outcomes.

Thus, in the pages that follow, I attempt to systematically demonstrate the importance of domestic politics to the study of IMF adjustment programs. As Gourevitch (1996) notes, if we accept that cooperation takes place through a two stage process of a convergence of policy objectives and a commitment to institutions that manage policy disputes, this implies that cooperation turns on fundamentally on domestic politics, since the political processes in countries must lead to both the policy convergence as well as behaviors that sustain the regime. Again, this argument recounts the themes noted above: to understand why commitments are kept, we have to understand why they are made.

It should be noted that studying commitment-making and commitment-honoring is important theoretically, but empirically as well. The consequences of failing to study both decisions jointly are that analyses of the factors that produce compliance may be skewed because of the danger of selection bias. This is because the rule by which the observations are selected is correlated with our dependent variable of interest (King 1989). States do not randomly go to the IMF, and we need to understand this process to avoid misspecifying program effects. In other words, we have no way of knowing what factors produce compliance in those states that do not sign agreements, and if the sample of states under agreements is systematically different from the population, our results are likely to be skewed. The same factors that determine compliance also determine whether or not a state selects a Fund program in the first place. To better assess the extent to

which selection effects may be a threat to inference, we need to employ appropriate techniques that allow us to accomplish this joint estimation.

### **Implications**

From the above discussion, we understand that any theoretical argument that attempts to explain variations in compliance with international agreements should develop an answer to two closely related questions. First, how can we understand the weaknesses in the IMF's enforcement regime? After all, in light of its bargaining leverage with the borrower, it should be able to design an agreement that would be compliant *ex ante*. Along the same lines, what incentives affect its decision to enforce agreements? Is it indeed the pawn of the US government that many would have us believe? (IFIAC 2000) Second, why is it that statesmen enter agreements that they may not be able to honor, and what implications follow for the study of domestic institutions? As political scientists, we would like to demonstrate the imperfections in IMF operations brought on by a systematic neglect of the politics of adjustment. But is it the case that institutions matter? If so, which ones? If they do, then how? Each of these issues is addressed in the subsequent chapter, which outlines my approach to the puzzle of IMF compliance.

### Chapter Three: Information, Inefficiency, and IMF Conditionality

“We don't need to form very sophisticated judgments about the political forces in (those) countries. We basically have to form a judgment on whether the government will do what it says it will do in an overall satisfactory way.”

Stanley Fischer, Former First Deputy Managing Director, 1998 <sup>28</sup>

In recent years, IMF conditionality has been under increasing attack, and calls for revising it, reforming it, or ending it are commonplace. This, of course, raises the question of why conditionality has failed. As noted earlier, the principal-agent argument suggests that the Fund gains from lending, so it prefers to respond to breaches of performance through consultation rather than enforcement. But the fact remains that the Fund does enforce the letter of intent by suspending programs; nearly 40% of the time. This makes the principal-agent line of argument harder to understand, since it cannot explain variation. It further invites us to think about the incentives of the Fund and of developing countries. Moreover, it suggests two closely-related questions that lie at the heart of the compliance problem. First, why do politicians agree to enter Fund programs, and more importantly, why enter them if their ability to honor them is uncertain? Second, why are Fund programs suspended so frequently? After all, if it ‘sets the tone’ in negotiations, shouldn't it be able to design an agreement that will be honored by the borrowing government? Answers to these questions should help us to better understand the specific ways in which conditionality has failed, and what can be done to address these deficiencies. The answers to these questions are the focus of the pages that follow.

---

<sup>28</sup> Epigraph from Press Briefing by Teleconference with Hong Kong SAR and Singapore, October 15, 1998.



Briefly, I suggest that the answers to both questions lie in the manner in which the Fund does business with its client states. I argue that the inefficiency in IMF conditionality comes from the information environment in which it operates. The Fund makes loans with little attention to the policy environment in borrowing countries. Since the process of implementing austerity requires the cooperation of domestic interests, program implementation is a more a political matter than an economic one. Though the Fund pays plenty of attention to the macroeconomics of adjustment, it cannot distinguish committed reformers from their less committed counterparts. Similarly, it makes decisions about whether or not to enforce the letter of intent given noncompliance under uncertainty about whether the borrower is committed to reform or not. In other words, the Fund does not really form the sorts of judgments noted in the epigraph.

Because the Fund operates in a low information environment, three important implications follow for compliance. First, the Fund's endorsement carries little weight with international lenders and investors. Despite the Fund's insistence that signing a program is a seal of approval, we see no evidence that inflows of loans or investments follow the announcement of a signed letter of intent. Second, the Fund fails to design its agreements around the domestic constraints that leaders face. As a result, it "tips the balance" only under certain domestic conditions. Third, it produces a mismatch between whether states breach the terms of the letter of intent and whether they are sanctioned. Thus, we see that even after controlling for a state's performance under the agreement, the Fund is more likely to sanction certain types of states, even though their performance was not different from states with other domestic attributes. Before we can draw out these hypotheses, more background on the source of the Fund's inefficiency is in order.

### **Conditionality and the “Market for Lemons”**

Arguing that the failure of conditionality stems from an information failure requires that we think more deeply about the problem that the Fund was created to solve. In the early days of the Fund, conditionality was intended to safeguard the Fund’s resources and prevent their abuse. This insurance was sought because the danger was that countries would implement domestic policies (such as fiscal or monetary expansion) that would make sustaining a fixed exchange rate difficult (DeVries 1987; Boughton 2002). Thus, performance criteria were placed on fiscal and monetary aggregates.

With the proliferation of programs to an increasing number of less developed countries, compliance with these programs has become a major issue—prompting the question of why the Fund cannot design fully compliant agreements *ex ante*. The answer to this question is the same as that of other market inefficiencies: uncertainty. Conditionality takes the form of a promise to undertake specific policies, but whether these policies will be implemented requires attention to the political environment in the country and whether a state can honor the pledges that it makes. The Fund does not make these assessments, and as a result, the deals that it makes are often prone to failure.

An analogy will suffice here. Akerlof (1970) explains the importance of information for efficient market transactions in his study of the used car market. Briefly, he argues that the uncertainty that buyers might have about the quality of the used car they are purchasing can lead the market to break down. Sellers have incentives to sell their used cars regardless of whether they are good or not. Because it is impossible to convince a potential buyer that the car is not a lemon, sellers cannot find an acceptable price for their good cars. The solution to prevent the market from breaking down is for

the more informed side—the seller, who knows whether the used case is a lemon or not—to act in ways that are informative to the buyer. Thus, sellers agree to pay for an independent mechanic to inspect the car before a purchase is made.

Models such as the market for lemons are surmounted in the real world because informed actors have incentives to reveal their private information.<sup>29</sup> For example, in Spence (1974), employers do not know whether the workers that they hire are highly motivated, but this information problem is surmounted by the fact that workers have incentives to invest in their education, because they believe that this will get them a higher wage. Thus, employers can distinguish motivations by assessing a worker's level of education. Informed actors thus have incentives to reveal their private information, and uninformed actors have incentives to both elicit this information and then use it to make decisions.

In our case, the Fund argues (Dhonte 1997) that conditionality is a similar screening mechanism that is used by governments to signal the credibility of their policies. By signing the letter of intent, a state provides information that it is committed to responsible macroeconomic policy. Of course, the alternative to an IMF program is a deteriorating balance of payments situation. These two situations, the market for IMF programs and a competitive market for used cars or workers, are very different, and important consequences result. Lacking a Fund program, the status quo is untenable. Thus, statesmen have incentives to enter Fund agreements whether they are committed to austerity programs or not, and even if the potential for sanctions from noncompliance

---

<sup>29</sup> In game-theoretic terminology, uncertainty is defined as a probability distribution over types.

exists. Conditionality, in other words, does not reveal additional information to the Fund about a state's ability to commit to a letter of intent.

To understand how this can be the case, consider the following game shown in Figure One below. This is a game of policy coordination between a Signatory (who would be a Minister of Finance) and an Implementer (who would be a legislature). The Signatory has to choose whether to sign the agreement or not, and then whether to comply with it (by adopting reforms) or not. The Implementer has to also choose whether to comply with the agreement or not.<sup>30</sup>

Payoffs are comprised of four parameters, all of which are between zero and one. Choosing not to enter an agreement means that actors receive the status quo payoff of  $-m$ . We can think about this as disutility from continued deterioration in the economy, or as the constraint placed on the economy by dwindling reserves.<sup>31</sup> Choosing to Comply means accepting both its benefits, denoted as  $b$ , as well as its costs, noted by  $c$ . Actors have incentives to free ride by choosing Don't Comply if the other actor chooses Comply, which means that they can obtain some of the benefits (noted as  $b/2$ ) without bearing the costs of reform.<sup>32</sup> In this case, the free rider reaps some of the benefits of reform, such as an improvement in economic growth, or a lessening of the reserve constraint. The costs associated with adopting these reform policies--namely alienating one's constituencies--are then passed on to the other player. Thus, we can think about the order

---

<sup>30</sup> One important feature of IMF agreements should be noted here. Since these agreements are not technically international treaties, they are not ratified by the Implementer.

<sup>31</sup> In Table 2-1, we noted that many studies find that states enter Fund programs when they face a shortage of reserves of foreign exchange or low growth.

<sup>32</sup> Again, this follows from thinking about reform as a public good.

of play in this manner. The Signatory may sign the agreement, but not comply with it, such as if the Fund and the Signatory differ over how much expenditures should be cut. This in turn forces the Implementer to adopt more stringent policies to keep the agreement on track.

In the game developed below, the Fund is not an actor in the strict sense. It is an aspect of the state of nature—and should be thought of as part of the strategic environment

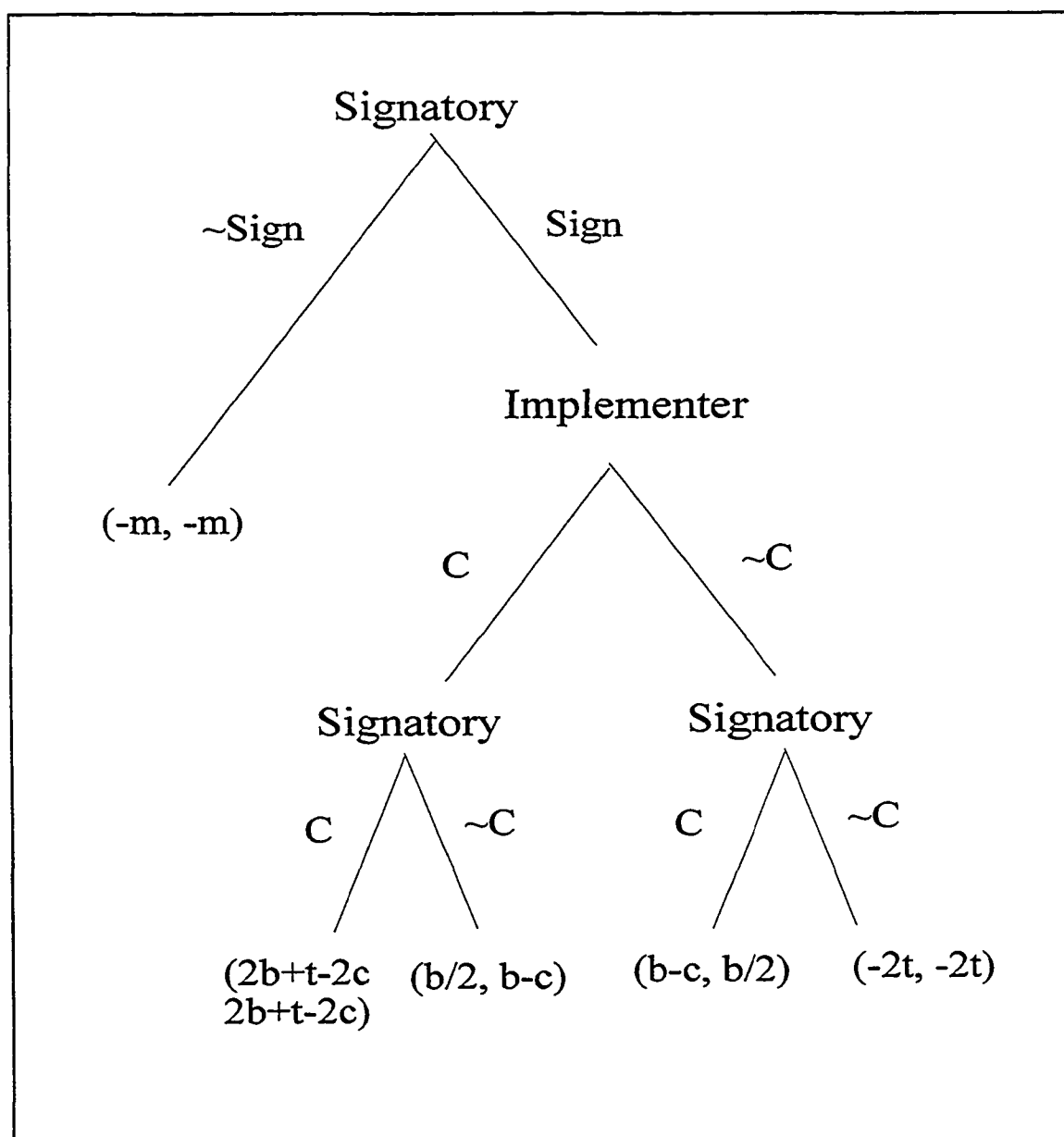


Figure 1

that politicians face.<sup>33</sup> The Fund affects the behavior of the domestic actors through its aid tranche. Thus, we add the variable  $t$  to the model, which reflects the utility actors receive from Fund assistance. Like  $b$  and  $c$ ,  $t$  is positive and greater than zero. The Fund's aid is conditional on the completion of reforms, and the Signatory and Implementer receive  $t$  only if both domestic actors comply. If neither actor complies, the Fund walks away from the bargaining table, imposing losses on both the Signatory and the Implementer. In building the model in this fashion we assume that the Fund sanctions all states that fail to fully implement the agreement. This is certainly not the way the Fund operates, but this is again a useful assumption to focus on the topic at hand.

If Dhonte's line of argument is correct, then we would expect to see the Signatory not enter the agreement unless both the Signatory and the Implementer comply with it. However, this is not the case. In this model, the Signatory gains from partial reform. To see this, we need only compare the first payoff at each of the nodes at the bottom of the tree with the value for not entering the agreement. If one actor chooses to be bound by the constraints of conditionality, then entering the agreement is the appropriate course of action regardless of what the other actor chooses.

To see this, assume that the Signatory is a committed reformer, and chooses to enter the agreement and comply with it independently of the Implementer. It receives a payoff of  $(2b + t - 2c)$  if the Implementer complies with the agreement, and  $(b - c)$  if the Implementer does not. So long as  $b > c - 2t$ , these outcomes are greater than zero and so

---

<sup>33</sup> This is a simplification meant to keep the model tractable. The key goal of the enterprise—to understand how domestic actors can coordinate given the Fund's oversight role—should be kept in mind.

produce greater utility than not entering the agreement.<sup>34</sup> Similarly, the Signatory can enter an agreement that it will not comply with, but only so long as the Implementer does. The only case in which the Signatory may not enter the agreement at all is if it and the Implementer choose Don't Comply. In this instance, the disutility from offending the Fund ( $-2t$ ) is similar to the disutility from not entering the agreement ( $-m$ ). If  $m < 2t$ , the Signatory will not enter the agreement.<sup>35</sup>

The implications of this line of argument are important. Both "committed" reformers (states with Signatories and Implementers that would choose compliance) and "less committed reformers" (states in which one of the actors chooses compliance and the other does not) face incentives to enter Fund agreements. Thus, the fact that a letter of intent is monitored and sanctions may be imposed does not 'screen' out less committed reformers. In fact, less committed reformers have every incentive to enter these agreements and use the leverage provided by conditionality to help solve their problems.

Many have argued that one reason states turn to the Fund is the leverage provided by conditionality. However, the implications of this argument for compliance with these agreements and for IMF operations more generally have not been developed. The population of Fund program states contains those states that need leverage and those that do not, as we have seen from above. But conditionality does not serve as a screening mechanism allowing only those states that are credibly committed to adjustment to enter.

---

<sup>34</sup> To see this, consider the bottom node for the Signatory on the right hand side. For the Signatory to choose Comply here, it has to be the case that  $b > c - 2t$ . If this inequality is met, and  $t$  is less than  $m/3$ , then the one on the left hand side will be met as well.

<sup>35</sup> In Chapter Five, I present evidence that suggests that this inequality will not be met.

Economists term such problems, in which the buyer faces uncertainty about the quality of the goods that are sold, as problems of adverse selection (Wilson 1987).

Since conditionality does not serve as a screening mechanism, the Fund is saddled with agreements that are not fully compliant. In the model in Figure 1, both Signatories with Implementers that choose Comply and those that choose Don't Comply enter the agreement.<sup>36</sup> In the real world, adverse selection (as in the market for lemons) is overcome by institutional arrangements that elicit information. Conditionality does not accomplish this, and this raises the larger question of why such an arrangement remains an equilibrium outcome. To understand this, we have to turn to the Fund's incentives to acquire information about the types of state it confronts. The problem of low information exists here as well.

We have argued that IMF conditionality approximates a market for lemons because both committed reformers and marginal reformers have incentives to seek agreements. In order to understand why the Fund would allow such a suboptimal arrangement to persist, we need to think about the Fund's incentives to redesign conditionality so as to better screen out nonreformers. Here, the parallel to a competitive market does not hold, and this has important consequences. In contrast to our market for used cars, where a buyer can opt out, the Fund is constrained in the sense that its very job is to make loans. The fifth point of the Fund's Articles of Agreement in outlining the Fund's mission, stresses this tension well:

To give confidence to members by making the general resources of the Fund temporarily available to them under adequate safeguards, thus providing them

---

<sup>36</sup> Moreover, under some conditions, even Signatories that choose Don't Comply and Implementers that choose Don't Comply still enter the agreement.



with opportunity to correct maladjustments in their balance of payments without resorting to measures destructive of national or international prosperity.

Exactly what these safeguards are varies across programs with the content of the conditions imposed. In some sense, though this tension between lending to states where reforms are not guaranteed and ensuring the full implementation of a letter of intent has been at the heart of the IMF's mission. After all, the Fund was created to insulate governments from the effects of economic integration (Pauly 1997:115).

The Fund's 1979 Guidelines on Conditionality, which were a codification of the objectives of conditionality, also reflect this tension between lending and securing reform. The fourth point notes that adjustment programs will be devised with attention to "domestic social and political objectives, the economic priorities, and the circumstances of members, including the causes of their balance of payments problems." The seventh point notes that the Managing Director will recommend approval of a program when he feels "the program is consistent with the Fund's provisions and policies and that it will be carried out." Thus, on one hand the Fund is cognizant of a need to pay attention to differences across borrowers, on the other it feels a need to safeguard its resources by lending selectively.

To date, the Fund has paid more attention to the issue of lending than it has ensuring program implementation. Each letter of intent carries with it a report summarizing it—concluding with a staff recommendation. While these discuss the appropriateness of the policy instruments, they do not assess the probability that a given letter as written will be implemented. They generally suggest a need for the "authorities" to be sustained and determined for the program to succeed, or laud their courage, but little else. At the same time, it is clear that compliance with Fund conditionality is low,

suggesting that the incentives that the Fund has in place for leaders to honor their promises are clearly insufficient for the task. This is in part why the argument is perennially made for greater selectivity in lending (Killick 1998; IFIAC 2000).<sup>37</sup>

I note below four pieces of evidence consistent with the low information argument, and go on to suggest the consequences that have resulted from the Fund lending with relative uncertainty about a state's policy environment and the probability that the state will honor the loan ex ante. I will argue that developing a solid understanding of the politics of adjustment provides a way for the Fund to operate more efficiently, but also demonstrate why low information conditionality is an equilibrium.

### **Preliminary Evidence**

Four pieces of evidence support the claim that the failure of conditionality stems from low information lending. In various ways, they point to a common inefficiency in the present system of conditionality. First, despite the Fund's mantra that program implementation is a matter of political will in the borrowing countries, compliance with Fund programs has been and remains a concern. Second, we note that repeated use of Fund programs has become an important issue, though the roots of this failure--why demand for Fund programs has become persistent--have not been explored. Third, we note the Fund's recent review of conditionality suggests a need to take politics seriously, but we also note the limits of the Fund's approach. Finally, we focus on the role that prior actions play in Fund programs, and we note that these have not been used to their full

---

<sup>37</sup> Of course, this raises the issue of what should the Fund be selective about.

potential. These four threads suggest that information failures lie at the heart of the failure of conditionality.

Turning to the issue of Fund program compliance, while the scope of the problem has been with us for years, it remains as important as ever. A brief summary of some of the relevant studies detailing the scope of the problem is shown in the table below. Each of the studies below was either concluded by Fund staff or by external scholars reviewing Fund documents.

<b>Table 3-1: Selected Studies of Fund Program Compliance</b>		
Authors	Domain	Findings
Reichmann and Stillson 1978	79 Standby programs from 1963-1972	"Principal purpose" of program achieved in 76% of cases
Edwards 1989	34 Standby and EFF programs approved in 1983	Percentage of programs meeting performance criteria varied from 30.9% (budget deficit) to 59.1% (net domestic credit to government)
Schadler 1995	59 Standby and EFF programs from 1989-1991	Fund suspended assistance in 35 cases
Mecagni 1999	45 SAF and ESAF programs	Only 10 programs did not suffer interruptions
Mussa and Savastano 1999	All Fund arrangements 1973-1997	Percentage of programs with fully disbursed loans: 34.8%*
* Includes programs with end dates after 1997. Average from 1973-1992: 40.5%		

Taken together, these studies, especially the last two, suggest that Fund program noncompliance is a profound problem that shows little signs of improvement. The data I

have gathered for this project on Fund programs from 1979 to 1995 supports this inference. From the data gathered in this project, the compliance rate for programs over the 1979-1995 time period is illustrated in the figure below. The line represents the percentage of noncompliant country-year observations as a fraction of country-years under. Two disclaimers should be made about the figure below. First, because of left-censoring, the first data point for 1979 is biased downward.<sup>38</sup> Second, the data for 1996 and 1997 are provisional and reflect incomplete data. Excluding 1979 and 1996-1997, the average program suspension rate by program year is 30.4%. The peak of 53% occurs in 1990, corresponding with the first programs for Eastern Europe.



**Figure 2**

---

<sup>38</sup> In other words, I did not count programs started before 1979. This is a common approach in time series models.

These studies suggest that the problem of Fund compliance is both considerable and persistent. Understanding its sources, however, is another issue. The only study published by the Fund that assesses the sources of program interruptions suggests that political factors account for more than 'unexplained variance.' Mecagni's (1999) paper for the review of ESAF programs<sup>39</sup> finds that only 10 of 45 SAF and ESAF programs ran their full term of four years without an interruption of more than six months in the multiyear arrangement. Of the 51 program interruptions he studies, some 2/3 of them (N = 33) are explicitly due to deviations in policy commitments. Thus, in these cases, the governments do not implement the policies they pledge at the outset of the program. In the majority of these cases, the deviation was in fiscal policy, but exogenous factors also played a role. These are also detailed in the table below.

---

<sup>39</sup> These are concessional programs that are targeted toward very low income countries predominantly in Sub Saharan Africa. These programs are not a focus of the research in this project.

<b>Table 3-2: Source of ESAF Program Interruptions (From Mecagni 1999)</b>		
<b>Primary Causes</b>		
	Future Policy Disagreements	8
	Political Disruptions	10
	Deviations from Prior Commitments	33
N		51
<b>Exogenous Sources of the 33 Deviations</b>		
	Political Events	13
	External Shocks	11
	Natural Disasters	6
	Other	3

Listed as another primary cause were political disruptions, which took the form of riots, civil war, and changes of government. Some of these changes were not exogenous to the adjustment program, and yet again this raises questions about how programs are designed.<sup>40</sup> Turning to the policy deviations, political factors enter in a second-order sense here, since political events are closely connected to the government's breach of promise.

The two main conclusions of this study are worth quoting at length, since they tell us a great deal about the environment in which the Fund operates. First, the case for more safeguards is clear, as "the incidence of interruptions in IMF arrangements might be reduced by seeking greater assurances than in the past regarding the authorities ability to carry out policy commitments before processed with IMF support" (Mecagni 1999:238). Similarly, the author also makes the case for greater selectivity in lending, and lacking

---

<sup>40</sup> The notion that Fund programs cause civil discontent and riots is not new (Walton and Ragin 1990, Bienen and Gersovitz 1985)

this, “interruptions are likely to remain a feature of the ESAF experience, as the IMF continues to assist members at the margins of commitment and in the midst of difficult political transitions.” (Mecagni 1999:240) Thus, the author suggests that the problem of program interruption can be ameliorated by acquiring better information and incorporating that into Fund lending decisions through better assurances that governments will honor their word.

A more damaging piece of evidence about the nature of the problem that the Fund faces can be taken from the data that I have gathered for this project. A skeptic might suggest that the existence of a high rate of program noncompliance may be due to random shocks such as natural disasters. Evidence that program suspensions are systematic across time suggests a deeper problem consistent with an information failure argument.

Fortunately, we can test this proposition.

If it is the case that Fund program suspensions follow a time trend, then this suggests we can find evidence for autocorrelation. Beck, Katz, and Tucker (1998) provide for such a technique, which entails modeling the hazard rate, which is the probability of a program being suspended as a function of time. The hazard rate is simulated through a series of cubic spline segments. Testing the joint significance of these segments then suggests duration dependence. In our case, this means that the probability of a state’s program being suspended in a given year is in part a function of whether it was suspended in the previous year. Throughout this project, joint F tests find that we can consistently reject the null hypothesis that duration dependence does not exist. In other words, it is not merely the case that Fund programs fail, it is that programs that are suspended tend to

remain so. Again, this suggests a need to look for deep causes of this inefficiency, as these suspensions have systematic components.

### *Program Recidivism*

A related problem that suggests that Fund operations are inefficient lies in the repeated use of Fund programs. As originally devised, Fund programs were intended to be precautionary (Guitain 1995; DeVries 1986). Implicit in the term “stand-by,” this was intended to ensure that the Fund’s resources would remain available to all borrowers. However, repeated use has become a recent concern, as states have needed to obtain consecutive Fund programs. Between 1973 and 1997, Pakistan and Panama have each been under 13 programs of various forms. Kenya, Senegal, Philippines, Haiti, Jamaica, and Uruguay have been under 12 programs. Mauritania, Togo, Costa Rica and Guyana have been under 10 (Mussa and Savastano 1999:14).<sup>41</sup> Bird, Hussain, and Joyce (2000) find that frequent users typically have lower reserve levels, larger external current account deficits and higher debt service ratios than infrequent users. Thus, weaker macroeconomic fundamentals may translate into weaker implementation, and in turn a higher demand for new programs. If repeated use and repeated program noncompliance go hand in hand, this raises the question of why dysfunctional agreements are still concluded, as well as why the Fund does not learn from previous program failures and

---

<sup>41</sup> Some find this program recidivism objectionable, and suggest that the presence of continued Fund oversight fosters “aid addiction” (Bandow and Vasquez 1994) or undermines national sovereignty (IFIAC 2000). In either event, understanding why it persists remains important.



develop more effective programs. Though the issue of repeat use has also been with us for years, the Fund is just starting to develop research on this question.<sup>42</sup>

### *Conditionality and Program Ownership*

The notion that more attention should be paid to the role of political variables in ensuring commitment to Fund-backed programs was also made apparent in a review of the External Consultations brought about in the Fund's recent review of conditionality. Through a series of seminars in Berlin, Tokyo and London, and through posting its review for public comment, it received a great deal of public feedback.<sup>43</sup> Some of the lessons implied by this feedback suggest a need "to get better assurance that policy conditions are consistent with political realities in the borrowing country" and a further need to alter the conditionality guidelines "to give weight to political economy considerations" (IMF 2001:17). Again, as in the Mecagni study, the notion that conditionality should make reflective of political realities in the borrowing countries is stressed. In other words, the Fund needs to condition its lending programs using better information about the political contexts in which austerity is being attempted.

This admission, however, is nothing new for the Fund. The idea that politics affects a state's ability to commit to Fund programs has been always present in the Fund's thinking. In 1959, Managing Director Per Jacobsson noted that "programs can only succeed if there is the will to succeed in the countries themselves" (James 1996:109). In

---

<sup>42</sup> The Fund's Independent Evaluation Office is just starting this research as of February 2002.

<sup>43</sup> The complete file of public comments (of which my commentary was a part) is a 200-plus page document.

the Fund's own account of its approach to adjustment, it notes that "adherence to government controls and the political resistance to reforms vary widely across countries" (IMF 1987:47). An IMF staff review of agreements signed in the 1978-1980 period, for example, concluded that 'political constraints' and /or 'weak administrative systems' accounted for 60% of the breaches of credit ceilings." (Remmer 1986:6) Jacobsson's point is certainly taken, yet it raises the larger issue of how an institution with primarily economic expertise can assess this "political will" ex ante. Thus, simultaneous with the Fund's realization that political variables affect a state's degree of commitment to conditionality has been a parallel unwillingness to develop the capacity to assess this "political will" and incorporate that into the design of its programs.

The inability of the Fund to take politics seriously was also noted in the External Review of Surveillance, which noted that the IMF staff "appear in general to be reluctant to give advice...that takes into account the political and institutional constraints within which policymakers need to operate" (1999:95). Given this reluctance, the fact that the Fund uses terms like "political will" that it does not define is not surprising.

The Fund's recent invocation of "borrower ownership" reflects its implicit understanding of political factors well. The term was originally developed by the World Bank, and it refers to the idea that an adjustment program is developed and managed by a client state as opposed to being imposed by a donor. The initial empirical study (Johnson and Wasty 1993) measured ownership as a composite of four variables: the extent to which the program was initiated by the state leadership; the degree of intellectual conviction of policymakers; the extent of which the state leadership supported the program; and the extent to which policymakers sought to build a consensus. The authors

find that this ownership index is a strong determinant of program success. More recent work finds that World Bank aid is more effective in countries with high ownership, suggesting that more selective lending will be likely to generate better results (Dollar and Svensson 1997).

Despite the Fund's Executive Board agreeing that ownership is an essential component of successful conditionality and that it should be fostered (Public Information Notice (PIN) No. 01/125 December 14, 2001), there is no real theory behind borrower ownership and how it works. More importantly for this project, the causal antecedents of ownership—i.e., its institutional roots—remain elusive.<sup>44</sup> We do not know its sources, which means that it can be rather difficult to foster.<sup>45</sup> It is clear that ownership can, however, be inhibited. If the donor adopts a strategy of lending that is unrelated to a state's performance under the prior agreement, then it can act to undercut program ownership rather than aid it (Killick 1998; Collier 1997). Thus, while the Fund is recognizing the importance of ownership in theory, it has yet to issue detailed guidelines as to how ownership matters on an operational level, such as how the Fund distinguishes between degrees of ownership, and what then is done about this in practice for the design of Fund programs.<sup>46</sup> In other words, the Fund has not developed the informational wherewithal to make ownership substantively matter.

---

<sup>44</sup> An interview with a high ranking Fund staff member brought the following answer to the question of how one knows a program is owned: "I don't know."

<sup>45</sup> Khan and Sharma (2001) stress the importance of streamlining conditionality as a means to foster ownership. Lending selectively, or developing letters of intent that recognize ownership problems, is a different matter entirely.

<sup>46</sup> Of course, our discussion of the links between selection and compliance suggest that low ownership is arguably part and parcel of the reason a state is seeking Fund conditionality in the first place.

### *Prior Actions and Screening*

In the literature on contracting, the mere existence of uncertainty does not prohibit agreements from being concluded; rather it requires the development of specific mechanisms to make contracting possible. Williamson (1985) poses the question of when bilateral trading arrangements can be effective between rival firms, given that they have incentives to cheat each other. The key lies in the development of *ex ante* mechanisms through the creation of “hostages;” specific sunk assets that are transaction-specific and have no prior uses. If firms can exact hostages from each other, they can guard against opportunism because the benefits derived from cheating are no longer sufficient to warrant it.

In our context, the IMF traditionally requires a state to signal its willingness to implement an adjustment package through the use of precommitments (Kahler 1992:114-116). These are sometimes currency devaluations or passage of an austerity-compliant budget, though they may entail some institutional reforms.<sup>47</sup> These are intended to be implemented prior to Executive Board approval of the letter of intent. Unfortunately, these precommitments—known in Fund jargon as prior actions—are seldom effective as a hostage. They have not been used uniformly in Fund programs, and it is sometimes impossible to tell in Fund letters of intent exactly which actions were prior actions that

---

<sup>47</sup> As a precommitment, the Fund insisted that the Philippines create a more independent central bank (Economist Intelligence Unit 1993:11). More recently, the new arrangement with Russia is predicated on the Russian Parliament's passage of several bills to reform the bankruptcy laws, strengthen tax collection and overhaul the banking system. (Sanger 1999:A12).

were necessary as a precondition to Executive Board approval of the program.<sup>48</sup> The Policy Issues component of the 2001 Conditionality Review makes this plain, as incidents of misreporting prior actions have prompted that they be explicitly referred to in decisions (page 17). Because they are not uniformly employed across programs, it is perhaps no surprise that no real correlation exists between the number of prior actions and successful program implementation (IMF 2002:footnote 12). Again, we see evidence that the Fund does not devise screening contracts that separate committed reformers from shirking ones. This is consonant with the other pieces of evidence above, which suggests that the Fund operates in a low information equilibrium about the types of borrowers that it faces. The consequences that result from this low information equilibrium are the subject of the next section.

### **Implications**

In the previous section, I suggested that the problems with conditionality stem from information. The Fund has not developed a serious in-house understanding of what makes its programs succeed or fail, and it has not developed systematic tools intended to separate credible reformers that are ideal recipients of its programs from states that operate, in Mecagni's words, "on the margins of commitment."

Three empirical implications follow from this line of argument. First, we expect that markets will not respect the Fund informational endorsement. One of the traditional

---

<sup>48</sup> In my archival research, I could seldom ascertain in reading letters of intent and the attached documents exactly what the prior actions were. At the same time, while many states suggest that they have taken numerous actions before the Fund mission arrives, it was impossible to tell whether these were autonomous actions or IMF requests.

institutional claims about international institutions is that they serve as information providers. However, our argument suggests IMF conditionality does not separate good reformers from bad ones. Private markets know this, and we do not expect the announcement of a letter of intent to produce ‘catalytic’ inflows of new loans and investments.

Second, the Fund ‘tips the balance’ for reform only very rarely. Because it devises letters of intent under uncertainty about whether the borrower is committed to reforms or not, it cannot design these contracts to account for the different domestic conditions that borrowers face. The Fund tips the balance only where domestic and international incentives are in harmony—which is when politicians in the borrowing state are unconcerned with the political consequences of Fund-supported austerity. Thus, only politicians in nondemocracies that face domestic constraints are likely to seek Fund agreements to tip the balance at home.

Third, basing the failure of conditionality on a failure of information means that mismatches exist between crime and punishment. Thus, we find little evidence that democracies with highly fractionalized legislatures exhibit poorer performance under Fund programs, but controlling for their performance, these states are more likely to be sanctioned by the IMF. One interpretation of this is that the Fund operates with a prior belief that these states are poor adjusters, and as a result it devises conditionality systematically tougher in these states. Thus, even though these states are not “criminals” in the sense that they are more likely to breach austerity, they are more likely to be punished. Unfortunately, because the Fund does not acquire the information that would

allow it to design conditionality around a state's domestic constraints, this pattern persists over time.

If my argument is correct that information is the source of the conditionality problem, then this suggests that conventional arguments for Fund behavior will find weak support. Thus, we see only limited evidence that realist variables have an effect on Fund behavior. This is consistent with the notion that major powers practice, at best, 'fire alarm oversight' (McCubbins and Schwartz 1984) over the IMF.

A disclaimer is in order here. By evaluating a large sample of states, we are looking at aggregate patterns. While some evidence exists that the Fund staff learns from individual agreements, and attempts to solve the problems that bedeviled previous programs, our evidence does not suggest that the Fund behaves as a learning organization in the aggregate. This suggests that gathering better information about its programs and incorporating these lessons into program design is a viable route for the Fund to design more successful and more credible adjustment programs. However, a word of caution is in order. Arguing that a "low information equilibrium" exists for the Fund suggests that future efforts to acquire this information is likely to fail. In other words, in the true game-theoretic sense, parties have no incentives to change their strategies.

In order to bring some added clarity to this project, I address each of the four claims made above in separate sections. The chapter closes with a restatement of the hypotheses that will be tested in the empirical chapters that follow.

### **Catalytic Finance: What's a "Seal of Approval" Worth?**

One justification that the IMF employs is that its programs serve as a signal of borrower credibility (Dhonte 1997). By signing a letter of intent and agreeing to implement it, the borrowing state sends a message to the outside world that it is about to adopt responsible economic policies. This claim has been perennially espoused as a rationale for conditionality and for making commitments more generally (Maxfield 1997). Unfortunately, evidence in support of this 'catalytic' effect has been scant.

The claim that Fund programs catalyze international finance is an important one for several reasons. First, this justification helps to further our understanding of how and why states delegate to international institutions: not only do they help to signal credibility, but they also provide resources to help resolve pressing domestic problems. Evaluating the extent to which Fund programs produce these catalytic flows thus tells us a great deal about the influence that international institutions have over markets; in other words, whether their endorsement is seen as credible.

I argue that we can study the catalytic effects of IMF programs as added evidence to support our information failure argument. The reason why there is little evidence in support of the catalytic effect is that private actors know that the Fund cannot screen out committed reformers from their less committed counterparts. Thus, we see evidence of FDI and portfolio outflows following the announcement of a Fund program, even when a state's economic fundamentals present good investment opportunities. A state's level of compliance under the program has no effects on flows, suggesting that a state's degree of adherence to the letter of intent does little to allow investors to update their beliefs.



The broader relevance of these results suggest that the Fund is not a “force multiplier,” and the fact that its signal carries no positive information to international markets limits its effectiveness. Because program announcements produce investment flight, this helps us to understand why negotiations between the Fund and LDCs are often so protracted, as well as why reformers in these countries can lose control of the agenda.

### **Domestic Institutions and Selection: Tipping the Balance?**

The conventional wisdom is that international institutions provide politicians with the means to “tip the balance” by allowing pro-reform coalitions to be formed that would not exist in the absence of an IMF. In this manner, Fund assistance is used by weak governments to build a consensus. How common is this? Many that make this claim neglect the importance of international level incentives to make it work. Briefly, if the Fund suffers from information failures, then it will not devise contracts so as to address the differential levels of domestic constraints that leaders face. The Fund thus does tip the balance, but only for nondemocracies. Because leaders in established democracies are more sensitive to the potential costs of austerity, building a pro-reform coalition is more difficult than in a nondemocracy. More lenient bargaining from the Fund (that is, making conditionality less onerous for these states) would aid this process, but because the Fund cannot distinguish between states by their political makeup, this international level bargain does not take place.

To understand why states turn to the Fund, we have to understand why they adopt reforms. Distributional consequences have long been seen as the reason why governments fail to adopt appropriate policies (Rodrik 1996). The problem of reform is more

complex, though, because a desire to minimize distributional costs can prevent even optimal policies from being implemented. Alesina and Drazen (1991:1183) make such an argument to explain delays in stabilization, and note the following implication:

“Countries with political institutions that make it relatively more difficult for opposing groups to ‘veto’ stabilization programs not to their liking will stabilize sooner.” If this line of argument is correct, then this suggests that we can create testable hypotheses linking domestic institutions to the decision to seek assistance from the IMF.

Governments, after all, have to build coalitions to implement reform, and the institutional context, by shaping the relevance of distributional concerns to individual politicians, affects the ability of states to implement reform.

As noted in Chapter 2, we know that specific institutional mixes combine to produce comparatively poorer policy outcomes. States with a high number of parties in the legislature are states in which the process of adjustment is more difficult, because the bargaining problem in forming a pro-reform coalition increases with the number of actors (Haggard and Kaufman 1995; Haggard and Stallings 1991). PR electoral systems have been found to produce the same sorts of problems (Roubini and Sachs 1989; Edin and Ohlsson 1991; Kontopoulos and Perotti 1999; Inter-American Development Bank 1997). Similarly, a large literature focuses on the effects of regime type on reform. In established democracies, accountability drives up the price of reform provision, and the possibility of electoral removal induces caution on the part of policymakers. For this reason, the effects of the collective action problem that reform engenders are likely to be stronger in democracies, where politicians have to answer to their constituents explicitly

at the voting booth for their conduct.<sup>49</sup> If the link between domestic institutions and delegation has merit, then we would expect that politicians in countries with these contexts to be less likely to form pro-reform coalitions at home, and that the demand for IMF programs would be considerably higher in these countries.

What this argument omits, however, are the international level incentives. Forming an agreement requires both that one be demanded and that one be supplied. To make the information failure argument work, we have to suggest that systematic differences exist across polities that are unrecognized by the Fund, and then assess the consequences that result. Otherwise, the Fund seems to behave as a sophisticated negotiator, recognizing that austerity varies in its acceptability across domestic contexts. Testing a tipping the balance argument requires that we specify exactly what the institutions on the ground are and whether and to what extent they can tipped.

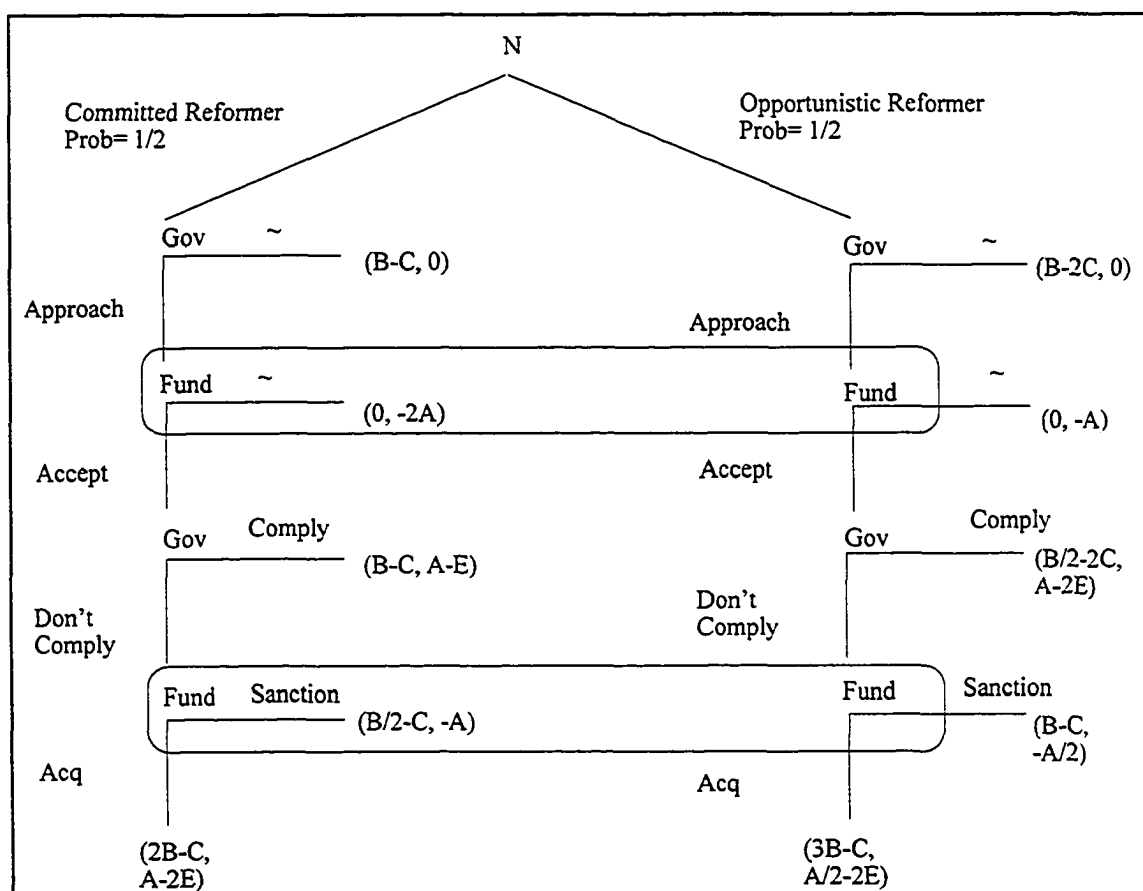
If this argument is correct, then the implications are as follows. Holding all other factors constant, democratic political leaders face higher costs for entering Fund programs than nondemocracies. The Fund's inability to recognize these greater domestic constraints *ex ante* means that even when we control for the economic factors that drive states to the Fund, only nondemocracies are more likely to enter Fund programs as the degree of legislative fractionalization goes up. The Fund's failure to design contracts around domestic constraints means that it tips the balance only in domestic contexts where the costs of imposing austerity are relatively lower.

---

<sup>49</sup> We can think about this in either a distributional or a competence sense. I discuss this distinction in Chapter Six.

### Enforcement Costs: Under What Conditions Does the Fund Sanction?

In our thinking about these two earlier questions, the Fund was relegated to the background as 'nature.' To ultimately understand whether a more informed Fund is one that sees higher levels of compliance, we need to invert the strategic problem and make the Fund the actor that makes choices. In the interests of simplifying the model, we treat the government that negotiates with the IMF as a billiard ball. Such a model is presented in Figure 3 below.



**Figure 3**

This game involves a government and the IMF. Both players choose jointly whether to enter an agreement as well as the level of performance under it. The Fund does not know

whether it faces a Committed Reformer or an Opportunistic Reformer, and has to make its decision under uncertainty. The Fund has a prior belief about what it faces, which we set to  $1/2$ . Thus, its *ex ante* assessment is that it faces both types of reformers with equal probability.

The game begins with a move by Nature, which determines whether the government is a Committed or Opportunistic Reformer. Following this, both the state and the Fund decide whether to enter an agreement. The government chooses then whether or not to comply with the letter of intent, and the Fund chooses to sanction it or not.

The Fund's task is to approve and enforce violations of its agreements. It faces a tradeoff between lending and reforms, and it makes decisions under uncertainty. Payoffs are comprised of the value of the agreement (noted as  $A$ ) and the costs of enforcing it (noted as  $E$ ). Thus, the Fund derives some utility from the act of lending, though this is offset by the costs of oversight and monitoring to ensure some level of policy reform. The payoffs depend in part on the state's type. If it does not lend, it forfeits the value of the agreement, but the Fund's opportunity losses are greater if it fails to lend to a Committed Reformer. If the Fund sanctions a state, the enforcement costs become zero, since it is no longer monitoring the state, and the Fund loses the value of the loan, which is greater for Committed Reformers than Opportunists. Similarly, it has greater enforcement costs for dealing with Opportunists, and acquiescing to an Opportunist's noncompliance lowers the value of the loan. Thus, this model incorporates the notion that the Fund faces a tradeoff between lending and reform.<sup>50</sup>

---

<sup>50</sup> Readers will note that this is a more realistic assumption than those underpinning "money pushing" arguments.

Payoffs for the state are also comprised of two parameters. States entering agreements receive both the benefits, noted as B and the costs, noted as C. As noted above, all reformers are not equal, which in turn affects the payoffs. If the state is Opportunistic, it is reform averse, and would prefer to be sanctioned following noncompliance rather than comply with the agreement.

We defer a full discussion of this game to a subsequent chapter, but the testable implications are as follows. First, we expect to see more noncompliant behavior in Fund programs with high enforcement costs. Second, we expect that as enforcement costs increase, the willingness of the Fund to punish goes down. This implies a test of the determinants of performance under Fund agreements, as well as understanding under what conditions the Fund sanctions states.

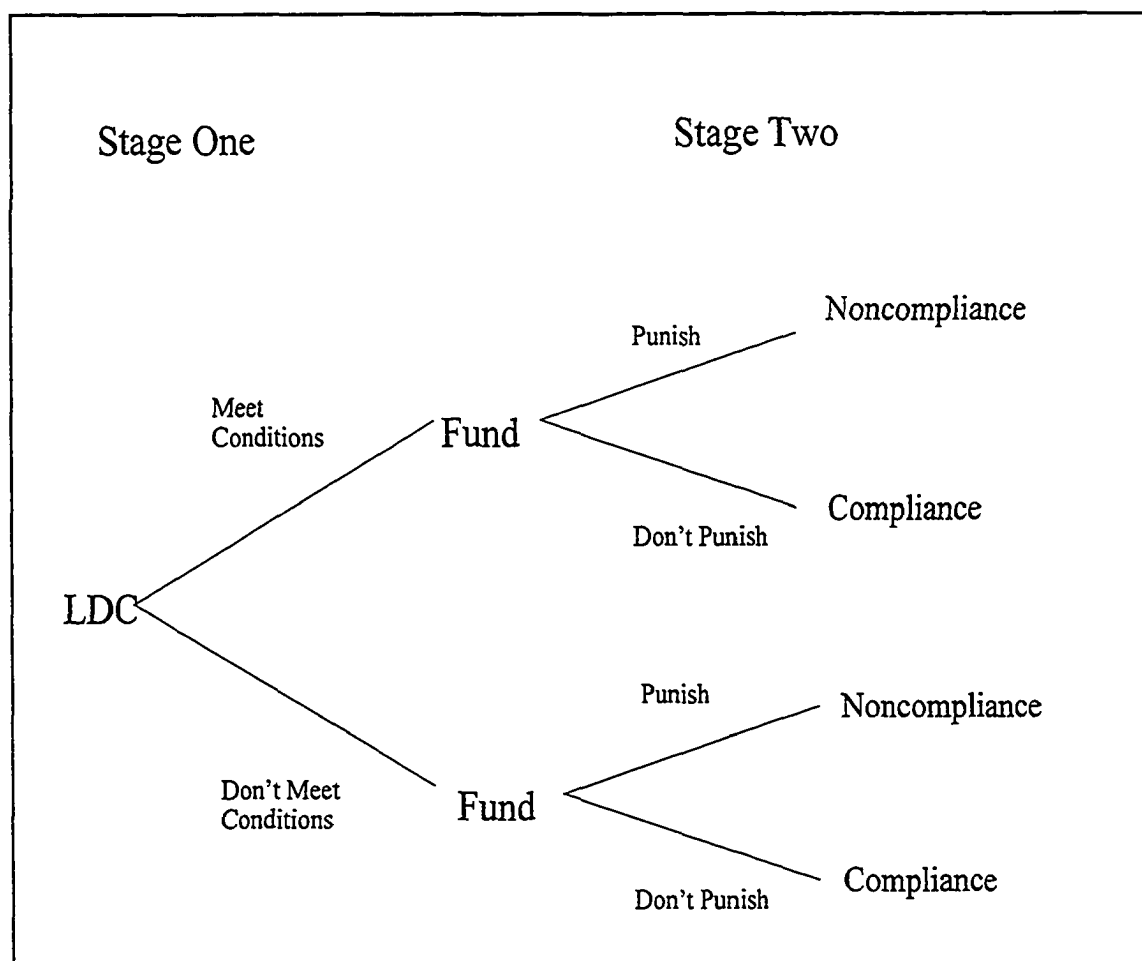
To answer the larger question about whether a more informed Fund sees a higher rate of compliance requires that we consider what the equilibria of this game are under full information. In other words, if we assume that the Fund has the ability to distinguish Committed Reformers from Opportunistic Reformers ex ante, is it the case that it does not lend to Opportunists, and is it the case that Committed Reformers have no incentives to cheat? Again, deferring the solution of this model until later, we find that this modeling enterprise helps us to understand why the failings of conditionality are in fact equilibria. Acquiring information does not allow the Fund to design contracts that ‘screen out’ reform-minimizing governments. In fact, acquiring full information and making choices according to that information has the effect of altering the portfolio of Fund loans. Committed Reformers no longer enter Fund agreements, and the portfolio of loans will be held exclusively by Opportunists that will still need to be sanctioned by the Fund. Thus,

complete information is not a panacea, and it could have the effect of worsening the problem by deterring those clients for whom Fund programs are in fact intended. Thus, not only is it the case that the Fund faces a tension between lending and enforcing, which limits its attempts to elicit a borrower's private information, but even if it had this information, this will not solve the compliance problem, because the Fund has incentives to lend to states that will flaunt conditionality, even if it knows that these states will not comply. Thus, not only does this modeling exercise help us to derive testable hypotheses, but comparing the limited information outcomes to the full information outcomes helps us to understand why it can be said that a low information equilibrium exists.

### **Compliance as Strategic Interaction**

Understanding compliance requires both a conceptualization of the problem as well as an appropriate operationalization. Compliance is generally defined as behavior that is expected as a result of adherence to a rule. As Simmons (1998) notes, studying compliance can prove difficult, because strategic interaction is implicit in the notion of 'rule following.' The process of compliance with Fund agreements unfolds in the following manner. A state is subject to performance reviews by the IMF during the course of its adjustment program. Because the program is designed to instill fiscal and monetary austerity, a state is obligated to keep specified economic variables within certain limits. The performance criteria, which are outlined in the letter of intent, are common knowledge, and they are initially developed through macroeconomic models. A

state is rewarded for achieving its performance criteria through receipt of aid tranches.<sup>51</sup> If a state's economic performance breaches the terms of the letter of intent by an excessive amount, the program can be suspended. Thus, we can think about the causal process that produces compliance and noncompliance as following a decision tree as shown in Figure 4.



**Figure 4**

In this figure, the outcomes that we observe (either compliance or noncompliance) are the result of a joint decision on the part of a borrowing state and an overseeing IMF.

---

<sup>51</sup> A tranche is a portion of the total arrangement.



Compliance results if a state is not punished whether it met the conditions or not.

Noncompliance exists if a state is punished for either meeting its conditions or not.

Readers will note that this differs from the earlier definition of compliance, and may find this objectionable. However, it is clearly the case in real world instances that states are not punished when they do not meet their conditions. This shift allows us to draw a broader theoretical lesson about the efficiency of the Fund's enforcement regime.

Thinking about compliance as a process of strategic interaction brings with it two important implications. First, it departs substantially from the legal tradition of compliance by acknowledging that the decision to comply or not is a choice. This has important consequences, for it allows us to accept the possibility that states may cheat on their commitments.<sup>52</sup> Thinking about compliance as strategic interaction also means that we need to carefully consider the alternative. In Figure 4 above, compliance results both because states meet their commitments and are not punished (at the top node), and because states do not meet their commitments, but are not punished (at the bottom node). Our explanations, therefore, need to focus on the behavior of both the LDC and the IMF. In order to guard against faulty inference, we need to match "crime" and "punishment." If we want to make a claim about the effectiveness of a given enforcement regime, we have to assess its efficiency. In other words, we have to make sure that those states that are being punished are actually the ones that did not meet their conditions. Thus, in this project we gather information on a state's performance under the agreement as well as whether or not sanctioning takes place. Thus, our approach to thinking about compliance

---

<sup>52</sup> Writings in the 'managerial school' (Chayes and Chayes 1995) tend to regard noncompliance as a 'no fault' problem. I previously discussed the problems with this line of argument in Chapter 2.

necessitates both a focus on alternative explanations as well as a sustained attempt to match nonperformance and noncompliance. Though this makes the research design complex, this provides us a means to understand the sources of inefficiencies in conditionality.

If the claim that conditionality is prone to information failure holds, then we would expect that the Fund will not be able to assess crime and punishment appropriately when we control for the effects of domestic institutions. States may have institutional mixes that result in poorer performance under Fund programs, but are not more likely sanctioned. At the same time, there may be evidence of average performance, but a higher likelihood of sanctioning, even after controlling for performance. If our information failure argument holds, we would expect that democracies with a high number of legislative parties exhibit poorer performance under these programs, but they are still more likely sanctioned by the Fund, even after we control for this performance. This line of argument, if supported, makes the case for IMF reform that much stronger, since not only is the Fund not being fair, but it is also systematically setting back the cause of reform in those countries it is intending to help. Again, the reasons for this inefficiency stem from the Fund operating in an uninformed strategic setting.

## **Conclusion**

The notion that conditionality fails is nothing new, and in its wake numerous proposals have addressed how the Fund should be engaged in lending to support balance of payments and whether this lending should be conditional. I have argued above that the failure of conditionality is an informational failure: the Fund lends indiscriminately to

states with balance of payments problems, and as a result it should not be surprising that its programs fail. I have outlined four pieces of evidence that suggests that the informational problem indeed exists, focusing on the nature of the compliance problem, the problem of program recidivism, the Fund's own evaluations in the recent review of conditionality, and the limited use of prior actions. Moreover, I have used this informational argument to advance four hypotheses that will be tested in the pages that follow. These appear in the table below.

<b>Table 3-3: Testable Implications of the Information Failure Argument</b>
<p><b>Hypothesis One:</b> The Fund does not send useful information to lenders about a state's borrowing environment, and thus markets do not respond positively to a letter of intent with loans and investment. They also do not respond either positively or negatively to whether the program was successfully implemented. (Chapter 5)</p>
<p><b>Hypothesis Two:</b> The Fund "tips the balance" for leaders with domestic constraints only if a state is not an established democracy. (Chapter 6)</p>
<p><b>Hypothesis Three:</b> IMF decisions to sanction states are driven more by the enforcement costs of individual borrowers than they are by the borrower's degree of political influence with the US. (Chapter 7)</p>
<p><b>Hypothesis Four:</b> Systematic mismatches exist between performance and sanctioning in IMF programs. Democracies with a high number of legislative parties do not exhibit poorer performance compared to other states, yet are more likely to be sanctioned. (Chapter 8)</p>

Each of these hypotheses is empirically tested in Chapters Five through Eight. We now turn to discussing the research design that underpins these empirical tests.

## Chapter Four: Research Design

This chapter details the procedures used for the empirical tests in this project. Below, I justify the use of a large N research design and detail variables, case selection, estimation techniques, and coding criteria and reliability issues. Appendix One reviews the econometrics of sample selection.

The case for a large N research design is based on both opportunity and necessity. First, in contrast to many existing studies of cooperation, we have the potential for using a large N sample because the Fund signs many agreements with many countries. This helps ensure that our findings are not a function of the cases that we have selected, since the cases are drawn from a large sample of states and agreements over time. Second, using a large N sample is appropriate in light of our focus on the data generating process. If we want to study why commitments are made as well as honored, we can only approach this using a technique that allows us to estimate the extent to which these two stages are interlinked.

### Independent Variables

The independent variables for this project were numerous. I begin with the economic measures, then the political ones, and finally the US influence and enforcement costs measures.

The economic independent variables, unless otherwise indicated, came from the World Bank's World Development Indicators CD-ROM.

*Debt Service:* Debt service ratio/GDP

*Reserves:* Gross international reserves measured in months of imports

*Growth:* Annual percentage growth rate of GDP at market prices based on constant local currency.

*Fixed Exchange Rates:* Dummy for fixed exchange rate regime. Source is issues of IMF's Annual Report on Exchange Arrangements and Exchange Restrictions.

*Capital Controls:* Dummy for presence of controls on capital account, from IMF's Annual Report on Exchange Arrangements and Exchange Restrictions.

*Net Domestic Credit Growth:* Net domestic credit is the sum of net credit to the nonfinancial public sector, credit to the private sector, and other accounts. Data are in current local currency. Growth rate calculated relative to previous year.

*GNP Per Capita:* GNP per capita is gross national product divided by midyear population, measured in constant 1995 US dollars.

*Trade:* The sum of exports and imports measured as a percentage of GNP.

*Budget Deficit:* measured as a percentage of GDP.

*Inflation, GDP deflator:* Inflation as measured by the annual growth rate of the GDP implicit deflator.

*Gross Domestic Investment (% of GDP):* Gross domestic investment consists of outlays on additions to the fixed assets of the economy plus net changes in the level of inventories.

*Interest Rate Spread (Lending Rate minus LIBOR):* Interest rate spread is the interest rate charged by banks on loans to prime customers minus the interest rate paid by commercial or similar banks for demand, time, or savings deposits.

*Terms of Trade Adjustment:* The terms of trade effect equals capacity to import less exports of goods and services in constant prices.

*Total Debt Service (% of GNP):* Total debt service is the sum of principal repayments and interest actually paid in foreign currency, goods, or services on long-term debt, interest paid on short-term debt, and repayments (repurchases and charges) to the IMF.

*Exports of goods and services (% of GDP):* Exports of goods and services represent the value of all goods and other market services provided to the world.

*Extended Fund program:* Dummy variable for whether the Fund program in question is a drawing from the Extended Fund Facility. Source is IMF Annual Reports.

*Democracy:* Dummy variable from Polity III (Jagers and Gurr 1996). The democracy score is a composite of four measures: executive recruitment openness, executive recruitment competitiveness, competitiveness of political participation, constraints on chief executive. These measures are summed into a eleven-point scale. Following standard practice in quantitative IR scholarship, an established democracy is any country with a score of seven or higher.

*Fractionalization:* From the Rae (1967) study, this is the probability that any two legislators picked randomly will be from different parties. Measured on a zero-one scale. Data here are from the Database of Political Indicators (Beck et al 2001).

*PR:* Dummy variable for whether the electoral rules are based proportional representation. Data here are from the Database of Political Indicators (Beck et al 2001).

*Similarity:* S measure of similarity (Signorino and Ritter 1999) in voting records between the US and states in the General Assembly. Source is from Gartzke, Jo, and Tucker (1999).

*US Official Development Assistance:* Net official development assistance from the US. Source is from the OECD-OCD Geographical Distribution of Financial Flows to Developing Countries, 1988-1994.

*Fund Quota:* Represents state's total borrowing privileges in the Fund. Measured in millions of SDRs. Source: IMF Annual Report, 1998

### **Dependent Variables**

The dependent variables in this project are also numerous. We can lump these into three groups: those dealing with selection, those dealing with outcomes, and those dealing with sanctioning.

*Agreement Selection:* Zero-one indicator noting if a country is under an IMF stand by or extended fund facility (EFF) arrangement between 1979 and 1995. This information is found in the annexes of the Fund's Annual Reports. For some estimations, we focus only on the first year of the relevant Fund program.

Turning to outcomes, we focus on performance under the adjustment program by looking at the annual growth rate of net domestic credit and the budget deficit. Both are defined above as independent variables and are common Fund performance criteria.

There are three other outcome measures.

*Foreign Direct Investment, Net Inflows:* Foreign direct investment (net) shows the net change in foreign investment in the reporting country. This measure is scaled over GDP and measured in US dollars. Source is the World Bank's Global Development Finance CD-ROM.



*Official Net Resource Flows:* Official net resource flows are the sum of official net flows on long-term debt to official creditors (excluding IMF) plus official grants (excluding technical cooperation). Source is the World Bank's Global Development Finance CD-ROM.

*Portfolio Flows:* Portfolio equity flows are the sum of country funds, depository receipts (American or global), and direct purchases of shares by foreign investors. Source is the World Bank's Global Development Finance CD-ROM.

The final dependent variable is *program suspension*. In 138 of the 347 Fund programs studied, states were not eligible for all of the drawings either because they missed performance criteria and were unable to obtain a waiver from the Fund or they failed a quarterly review. Codings for the dependent variable came from the Schadler report and quarterly country reports of the Economist Intelligence Unit. Information about program compliance for programs prior to 1988 was obtained through a careful analysis of reports from the IMF archives, including letters of intent and program reviews.

My operationalization of noncompliance (defined in terms of whether or not a state lost eligibility for future drawings) differs from the extant literature.<sup>53</sup> Other studies of Fund compliance (Bienen and Gersovitz 1985; Bartilow 1997; Chayes and Chayes 1995) use evidence on program cancellations, which are public statements by governments to terminate agreements as indicators of compliance problems. The distinction between cancellations and the loss of eligibility is a critical one. Cancellations are an imperfect measure of compliance with Fund programs for three reasons. First, the

---

<sup>53</sup> In Figure 4, we noted that noncompliance also occurs if states meet their conditions, but are not punished. I assess the efficiency of the Fund's enforcement regime in Chapters Seven and Eight.

arrangements are cancelled by the member state for reasons unrelated to compliance. For example, in 1993 Argentina cancelled its EFF because it no longer needed the funds. This cannot be taken as evidence of the Fund sanctioning Argentina for noncompliance, since it has the right to draw resources as it sees fit.<sup>54</sup> Other cancellations, such as that of Lithuania's stand-by in 1993, represent rollovers into different types of arrangements, in which the old program is cancelled to make room for the new one. This would be directly analogous to renegotiating a mortgage and tearing up the first before signing the second. Neither case can be taken as evidence of a compliance problem.

Not only is the cancellation measure conceptually flawed, it severely underestimates the scope of the problem. In a subsample of agreements between 1988 and 1995, only twenty-two agreements were officially cancelled. The extent of the compliance problem is much greater than this. States lose eligibility in a given year, and then in many cases are not able to agree with the Fund on measures to restore the program. Schadler (1995b:28) notes that of the 45 arrangements studied between 1988 and 1992, countries were eligible for all purchases in only 17 of them. Focusing purely on cancellations, which is more a legal rather than procedural matter, underestimates the scope of the problem.

Other scholars employ proxy measures based on the percentage of tranche withdrawn (Drabkin 1993, Conway 1994, and Killick 1995). Thus, the assumption is that if a percentage of the arrangement is undrawn, then this represents a compliance problem. In Killick's 1995 book, a compliance problem is said to exist if 20% or more of the

---

<sup>54</sup> While there may be strategic misrepresentation here, in that states cancel because they fear that they will be sanctioned, as noted below, the number of cases that could be affected is small.

agreement is undrawn. This measure is also not without problems. First, the threshold for noncompliance is actually too high. For example, consider the following cases in Table 4-1 below, which made no drawings against their adjustment programs. This table compares the Killick coding against the reported history in the IMF conditionality report (Schadler 1995), which provides a nice reliability test. Per the Killick criterion, all of these programs are noncompliant ones. In reality, this is not always the case. Some states were eligible for all the drawings, yet took none. In other states, these states were not eligible for any drawings. The problem is that some of these programs may well be precautionary, thus it would not make sense to assume that there is a compliance problem in these instances. Thus, Killick's measure can conflate precautionary programs with good compliance with failed programs.

<b>Table 4-1: Contrasts between Existing Measures of IMF Compliance</b>							
State	Year	Amount Agreed	Amount Undrawn	Percent Undrawn	Killick coding	Schadler coding	Agreement?
Tunisia	1988	138.2	138.2	100.00	Fail	Full eligibility	No
Costa Rica	1989	42	42	100.00	Fail	Lost eligibility	Yes
Nigeria	1989	475	475	100.00	Fail	Full eligibility	No
El Salvador	1990	35.6	35.6	100.00	Fail	Lost eligibility	Yes
Papua New Guinea	1990	26.36	26.36	100.00	Fail	Full eligibility	No
Nigeria	1991	319	319	100.00	Fail	Lost eligibility	Yes

Amount Agreed and Amount Undrawn are in millions of SDRs.  
All information from IMF Annual Reports

Another problem with this operationalization concerns the mechanics of research design. Killick's measure is an explicitly cross-sectional one, which can make inference very difficult. For example, consider a standard performance criterion, the growth of net domestic credit in the monetary system. During the program, this variable may increase, which reflects an inability to keep the growth of the money supply in line, up to a point where the Fund suspends the program. Following this, the state may attempt to get back in the Fund's graces, but it is unable to restart the program. A cross-sectional measure of this variable over the duration of the program might not show any change, since it would solely evaluate the change between the starting and ending levels. For this reason, we would expect that an indicator based on a country-year unit of analysis will be more likely to generate reliable parameter estimates. This necessitates the use of an annual measure of program compliance.

### **Case Selection**

This study is based on a panel of 126 states studied from 1979-1995. 106 states entered a total of 347 Fund stand by and EFF programs during this timeperiod. The other 20 states did not enter a Fund program of any type during this timeperiod. The list of cases in each group is detailed in the tables on the subsequent pages.

Afghanistan	Colombia	Israel	Singapore
Angola	Cape Verde	Lebanon	Suriname
Bahrain	Djibouti	Malta	Syrian Arab Republic
Bahamas	Fiji	Malaysia	Vanuatu
Botswana	Indonesia	Paraguay	Republic of Yemen

State	Program Start Year
Albania	1992
Algeria	1989, 1991, 1994, 1995
Argentina	1983, 1984, 1987, 1989, 1991, 1992, 1995
Armenia	1995
Azerbaijan	1995
Bangladesh	1979, 1980, 1983, 1985
Barbados	1982, 1992
Belarus	1995
Belize	1984
Bolivia	1980, 1986
Brazil	1983, 1988, 1992
Bulgaria	1991, 1992, 1994
Burma	1981
Burundi	1986
Cameroon	1988, 1991, 1994, 1995
Central African Republic	1981, 1983, 1984, 1985, 1987, 1994
Chad	1994
Chile	1984, 1985, 1989
China	1981, 1986
Congo	1979, 1981, 1990, 1994
Costa Rica	1980, 1981, 1982, 1985, 1987, 1989, 1991, 1993, 1995
Cote D'Ivoire	1981, 1984, 1985, 1986, 1988, 1989, 1991
Croatia	1994
Cyprus	1980

Czech Republic	1993
Czechoslovakia	1991, 1992
Dominican Republic	1983, 1985, 1991, 1993
Dominica	1981, 1984
Ecuador	1983, 1985, 1986, 1988, 1989, 1991, 1994
Egypt	1987, 1991, 1993
El Salvador	1980, 1982, 1990, 1992, 1993, 1995
Equatorial Guinea	1980, 1985
Estonia	1992, 1993, 1995
Ethiopia	1981
Gabon	1980, 1986, 1989, 1991, 1994, 1995
Gambia	1979, 1982, 1984, 1986
Georgia	1995
Ghana	1979, 1983, 1984, 1986, 1987
Grenada	1981, 1983
Guatemala	1981, 1983, 1988, 1992
Guinea	1982, 1986, 1987
Guyana	1979, 1980, 1990
Haiti	1982, 1983, 1989, 1995
Honduras	1979, 1982, 1990
Hungary	1982, 1984, 1988, 1990, 1991, 1993
India	1981, 1991
Jamaica	1979, 1981, 1984, 1985, 1987, 1988, 1990, 1991, 1992
Jordan	1989, 1992, 1994
Kazakhstan	1994
Kenya	1979, 1980, 1982, 1983, 1985, 1988
Korea	1980, 1981, 1983, 1985
Kyrgyz Republic	1993
Laos	1980
Latvia	1992, 1993, 1995
Lesotho	1994, 1995
Liberia	1979, 1980, 1981, 1982, 1983, 1984
Lithuania	1992, 1993, 1994
Macedonia	1995
Madagascar	1980, 1981, 1982, 1984, 1985, 1986, 1988
Malawi	1979, 1980, 1982, 1983, 1988, 1994
Mali	1982, 1983, 1985, 1988
Mauritania	1980, 1981, 1985, 1986, 1987
Mauritius	1979, 1980, 1981, 1983, 1985

Mexico	1983, 1986, 1989, 1995
Moldova	1993, 1995
Mongolia	1991
Morocco	1981, 1982, 1983, 1985, 1986, 1988, 1990, 1992
Nepal	1985
Nicaragua	1979, 1991
Niger	1983, 1984, 1985, 1986, 1994
Nigeria	1987, 1989, 1991
Pakistan	1980, 1981, 1988, 1993, 1994, 1995
Panama	1979, 1980, 1982, 1983, 1985, 1992, 1995
Papua New Guinea	1990, 1991, 1995
Peru	1979, 1982, 1984, 1993
Philippines	1979, 1980, 1983, 1984, 1986, 1989, 1991, 1994
Poland	1990, 1991, 1993, 1994
Portugal	1983
Romania	1981, 1991, 1992, 1994
Russia	1992, 1995
Rwanda	1979
Senegal	1979, 1980, 1981, 1982, 1983, 1985, 1986, 1987, 1994
Sierra Leone	1979, 1981, 1984, 1986
Slovak Republic	1994
Solomon Islands	1981, 1983
Somalia	1980, 1981, 1982, 1985, 1987
South Africa	1982
Sri Lanka	1979, 1983
Sudan	1979, 1982, 1983, 1984
Tanzania	1980, 1986
Thailand	1981, 1982, 1985
Togo	1979, 1981, 1983, 1984, 1985, 1986, 1988
Trinidad	1989, 1990
Tunisia	1986, 1988
Turkey	1979, 1980, 1983, 1984, 1994
Uganda	1980, 1981, 1982, 1983
Ukraine	1995
Uruguay	1979, 1980, 1981, 1983, 1985, 1990, 1992
Uzbekistan	1995
Venezuela	1989
Vietnam	1993
Western Samoa	1979, 1983, 1984

Yugoslavia	1979, 1980, 1981, 1984, 1985, 1988, 1990
Zaire	1979, 1981, 1983, 1985, 1986, 1987, 1989
Zambia	1981, 1983, 1984, 1986
Zimbabwe	1981, 1983, 1992

It should be noted that while this does not comprise the entire panoply of Fund programs, this is not a threat to inference. Other Fund facilities exist, such as the Extended Structural Adjustment Fund facility (now renamed Poverty Reduction and Growth Facility) but these are not included in our analysis for several reasons. First, these programs are concessional, and are geared toward countries with strict criteria (in the form of low incomes). Thus, it is harder to gauge Fund incentives here, and it could be that the Fund has a strong desire to make conditionality lax in these states to justify the flow of aid to them. Moreover, because these programs have explicitly structural components, establishing exactly what conditionality is (and devising variables that would help capture this in a cross-national context) is a more difficult task *ex ante*. Finally, because these states are very low income and have weak administrative capacity, including these states also invites problems owing to missing data. Focusing on Stand Bys and EFF programs allows us to better confront the Fund on its home turf.

### **Estimation Techniques**

This project uses statistical techniques designed to control for the potential confound of sample selection bias. That is, we attempt to estimate outcomes given that a state may or may not be under a given arrangement. Of critical importance here is the coefficient  $\rho$  ( $\rho$ ), which captures the correlations in the error terms in the two appendices. Its sign and significance tells us whether selection bias is a problem, and it



which direction it functions. It may be positive, meaning that unobserved variables in the selection equation are more likely to produce outcomes, or it may be negative, meaning that unobserved variables are less likely to produce outcomes.

Statistical tests in this project employ both Heckman's sample selection techniques and treatment regressions. A brief distinction between the two is worth illustrating through an example.<sup>55</sup> If we wanted to estimate the effects of union status on wages, we can make two different types of arguments. First, we could suggest that the sole difference between union workers and nonunion workers is an upward shift in wages, holding all other independent variables constant. This type of a model allows us to tease out a separate coefficient for union status in the outcome model. The sign and significance tells us what the effect of union status is, conditional on all the other independent variables. The costs of this model (termed a treatment regression) are that we have to assume that the other independent variables are identical between the groups, or that education has the same effect on wages regardless of one's union status.

The Heckman model frees us from this constraint. By design, the values of the independent variables differ across samples, and the effect of union membership is not teased out as a separate dummy variable, but in the slopes of the coefficients and in the error term. Our tests in Chapter 5 use the treatment model, while the Heckman is used in all the other chapters. Our results do not differ between the two techniques. Rather the difference lies in the choice of how the data are actually generated, and our willingness to assume that the two samples are homogenous across the independent variables.

---

<sup>55</sup> This is taken from Millimet 2001.

We employ a set of additional tools to address additional threats to inference. Throughout the project, we control for heteroskedasticity through the use of robust standard errors, and control for temporal issues using a two fold strategy. In analyses with continuous dependent variables, we employ a one term lag of the dependent variable. In analyses with dichotomous dependent variables, we add a set of cubic splines to the estimation, following Beck, Katz, and Tucker (1998). These splines approximate the autocorrelation in the dependent variable. Finally, where relevant, we address the potential for regional effects with a set of dummy variables for Latin America, Sub-Saharan Africa, and Eastern Europe and the Former Soviet Union. While ideally we would want to model each country individually with separate dummy variables, this costs us 125 degrees of freedom.

### **Coding Criteria**

If the main dependent variable is whether or not a program was suspended by the IMF, how do we know a program suspension if we see it? I built these data from a variety of sources: Schadler's 1995 report, which discussed a range of Stand By and EFF programs concluded between 1988 and 1991, the quarterly reports of the Economist Intelligence Unit, and extensive use of the IMF's archives. Our goal was to ascertain whether the program was suspended, and in the case of the EIU reports, this is mentioned explicitly in the context of a state's relationship with the Fund. In the archival records, information on program reviews, Article IV consultations, and request for new programs often included information on the success or failure of the previous program. Notably,

they did little to diagnose the sources of program failure, aside from blithely noting that conditions were not met.

### Reliability Checks

One final concern in this project is the veracity of the codings. Since the dependent variable came from three sources (Schadler 1995, Fund archives and Economist Intelligence Unit quarterly reports) we need to ensure that all the sources match. Of course, our real concern is with the Economist Intelligence Unit reports: do they accurately indicate when and how programs broke down?

To test the reliability here, I gathered a set of 14 Fund programs that overlapped with the EIU reports I studied. These are listed below.

Chile 1989	Hungary 1988	Honduras 1990
Cote d'Ivoire 1988	India 1991	Malawi 1988
Dominican Republic 1991	Jamaica 1988	Pakistan 1988
El Salvador 1990	Jamaica 1989	Yugoslavia 1988
Guyana 1990	Jamaica 1991	

Detailed information on the programs appears below. Briefly, the EIU and Fund archival evidence point to the same result in 10 of the above programs for which there was evidence on each. This produces an agreement level of 100%, and the probability that this result is due to chance is .0003. Thus, we have strong confidence in our ability to use multiple sources here.

Interestingly enough, where discrepancies existed, they fell between the Fund sources. For example, in El Salvador's 1990 program, the program was declared

ineligible when in fact no purchases had been made under the agreement. Follow up examination of similar parallel cases suggests that the risk that the codings are faulty is quite low. Thus, based on the initial test above, we can have strong confidence that using the EIU reports was a wise strategy to employ.

**1. Chile 1989:**

*Fund Source:*

C/Chile/1760 October 12, 1989 Staff Report for the 1989 Article 4 Consultation and Request for Stand By Agreement (EBS/89/193): Proposed agreement is in first credit tranche.

*EIU Source:*

1<sup>st</sup> Quarter 1990:9 "Program is not binding as no disbursements are tied to performance."

Result: Intersource agreement–program not suspended.

**2. Cote d'Ivoire 1988**

*Fund Source:*

C/Ivory Coast/1760 November 2, 1989 Staff Report for 1989 Article 4 and Request for Stand By Agreement (EBS 89/212 ): Government only made initial purchase of SDR7 million as it missed end December 1987 performance criteria. Furthermore, the first review scheduled for June 1988 could not be completed because understandings on the needed corrective measures could not be reached. Inoperative.

*EIU Source:*

Annual Country Profile: 35. Government failed to meet deadline for resuming interest payments to commercial creditors, as well as London and Paris Clubs. Fund suspended the standby as a result.

Result: Intersource agreement–program suspended.

### **3. Dominican Republic 1991**

*Fund Source:*

C/Dominican Republic/1760 June 17, 1993 Request for Stand by Arrangement and Use of Fund Resources under the Compensatory and Contingency Financing Facility: 1-2.

Government purchased the full amount under the arrangement, and “observed all performance criteria established under the program.”

*EIU Source:*

1<sup>st</sup> Quarter 1993:21 Government does not intend to seek an additional standby following the expiration of the 1991 program, “none of which has been drawn.”

Result: Intersource Agreement. Program not suspended.

### **4. El Salvador 1990**

*Fund Source:*

C/El Salvador/1760 Staff Report for 1991 Article 4 Consultation and Request for Stand By Arrangement (EBS 91/197): 1. Arrangement expired on 8/26/1991 “observed all performance criteria under the arrangement through December 1990, many with margins.”

*Fund Source:*

Schadler 1995: 2. Lost eligibility around February 1991.

Result: Intersource Disagreement. Article 4 report says no purchases made under the agreement, thus it is hard to see this is noncompliance.

## 5. Guyana 1990

*Fund Source:*

C/Guyana/1760 October 1, 1991 Staff Report for the 1991 Article 4 Consultation and Review under Stand by Arrangement, and Request for Second Year Arrangement under ESAF: 2. "Guyana has made all purchases available under the SBA and obtained all disbursements under the ESAF to date"

*Fund Source:*

Schadler 1995: 2. Program did not lose eligibility.

Result: Intersource Agreement. Program not suspended.

## 6. Honduras 1990

*Fund Source:*

C/Honduras/1760 EBS 91/81 May 24, 1991 Review and Modification of Stand by

Arrangement:1. Nonobservance of performance criteria at end-December test:

Limit on central government expenditure exceeded by .8% GDP, "mainly as a result of higher transfers to the state electricity company...to cover external debt servicing"

Net domestic financing of public sector was 1% GDP higher than programmed, "largely because of a shortfall in central government revenue"

Net Domestic Assets of Central Bank missed by 2.5% above program ceiling, because of above and “unanticipated growth in central bank rediscounts”

Net international reserves missed by US \$12 million, including the accumulation of new external payments arrears of US \$5 million to multilaterals.

Supplement 1 to this report recommends waiver of above breaches and completion of review.

*Fund Source:*

Schadler 1995: 2. Program did not lose eligibility.

Result: Intersource agreement. Program not suspended.

## **7. Hungary 1988**

*Fund Source:*

C/Hungary/1760 February 22, 1990 Request for Stand By Arrangement: 56. First four purchases made, fifth was not owing to breaches of end-March 1989 performance criteria.

Values for credit to government and net domestic assets were exceeded, and serious data misreporting problem emerges involving understating domestic debt of government and external debt. Government agrees to repurchase noncomplying purchases as precondition to 1990 Stand by (May 15, 1989 Request for Extension of SBA: 1, 51, 3).

*EIU Source:*

4<sup>th</sup> Quarter 1989: 12. Final tranche not released because Fund and government could not agree on targets for the current account.

Result: Intersource agreement. Program suspended.

## 8. India 1991

### *Fund Source:*

C/India/1760 EBS 92/175 Nov 6 1992 Staff Report for 1992 Article 4 and Second Review under SBA:3. Program “broadly on track” with end-June ceilings on overall central government borrowing and short-term debt exceeded by small margins, but all performance criteria for September appear to have been met.

### *EIU Source:*

3<sup>rd</sup> Quarter 1993:8. Program completed, though Fund tolerated “slippage in fiscal targets and a gradual approach to reform in recognition of the country’s political constraints.”

Result: Intersource Agreement. Program not suspended.

## 9. Jamaica 1988

### *Fund Source:*

C/Jamaica/1760 March 6, 1990 Request for SBA:1. No purchases made under the arrangement after July 1989. 3: “As of end-September 1989 the performance criteria relating to the financing requirements of the public sector and the Central government were observed with large margins, but the ceiling on the net domestic assets of the Bank of Jamaica was exceeded by J\$631 million and the target for the net international reserves was missed by an equivalent amount....The ceilings on the public sector’s foreign borrowing also were exceeded...Arrears accumulated further in the following two months, and it became clear that the 1989/90 economic program was beyond repair.”

18: “Difficulties in the public finances caused by Hurricane Gilbert in Sept 1988 were compounded by spending pressures in the period leading up to the general elections of



February 1989, with adverse consequences for the international reserves, including the emergence of external arrears.”

*EIU Source:*

3<sup>rd</sup> Quarter 1989: “Jamaica’s drawdowns under the US \$118 million Stand By were suspended in March.”

Result: Intersource agreement. Program suspended.

## **10. Jamaica 1990**

*Fund source:*

C/Jamaica/1760 June 14, 1991 Request for SBA:3 “Problems emerged in the implementation of the program in the initial months of the arrangement” as performance criteria for net international reserves and net domestic assets of BoJ missed for end march and end June 1990.

*EIU Source:*

4<sup>th</sup> Quarter 1990:18. Fund program “suspended in March after Jamaica failed to meet certain performance criteria.”

Result: Intersource Agreement. Program suspended.

## **11. Jamaica 1991**

*Fund Source:*

C/Jamaica/1760 Request for Waiver 9/8/92:1. Jamaica observed all performance criteria for end march 92 and end June 92 save one—ceiling on net domestic assets of Bank of Jamaica for which they have requested a waiver—the breach reflected the effect of an

increase in legal reserve requirements which raised base money. Staff recommends board approval of a waiver allowing Jamaica to make last purchase.

*EIU Source:*

4<sup>th</sup> Quarter: 19. Final tranche of stand by disbursed. The one performance criteria that the government did not meet—net domestic assets—was waived on technical grounds.

Result: Intersource agreement. Program not suspended.

## **12. Malawi 1988**

*Fund Source:*

C/Malawi/1760: June 17, 1988 First Review under SBA and Request for Arrangements under ESAF: 1,3. Government made only one purchase, though the performance criteria for end-march 1988 were observed, “the authorities have indicated their intention to make no further purchases under the stand by arrangement...following Exec Board approval of their request for loans under the ESAF”

*EIU Source:*

2<sup>nd</sup> Quarter 1989:31. FY 1989 revenue performance was “much better than projected” and expenditure control was “more successful.”

4<sup>th</sup> Quarter 1989:7. Government receives “excellent report” from IMF.

Result: Intersource agreement. Program not suspended.

## **13. Pakistan 1988**

*Fund Source:*

C/Pakistan/1760 June 1, 1990 Request for Extension of SBA. Of the 273.15 million SDR available under the agreement, purchase 4 (26 million SDR) was not made owing to nonobservance of performance criteria on net bank credit for budgetary financing and bank credit for commodity operations. Last purchase (53 million SDR) contingent on observance of march 1990 performance criteria and completion of second review by the executive board.

*Fund Source:* Schadler 1995:2. Program lost eligibility at end 1989.

Result: Intersource Agreement. Program suspended.

#### 14. Yugoslavia 1988

*Fund Source:*

C/Yugoslavia/1760 March 2, 1990 Request for Stand By Arrangement:3. 1988 program failed to control inflation, which accelerated from 167 percent in the 12 months ending December 1987 to 251 percent in 1988. "Failure to stick to the nominal ceilings on credit and wages soon veered the program off track and only the first two disbursements were made. Efforts to revive the program in the context of a scheduled review were unsuccessful."

*EIU Source:*

4<sup>th</sup> Quarter 1988:14. "inflation has exceeded the original target by a huge margin"

1<sup>st</sup> Quarter 1989:4. Failure to control inflation "represents a setback for the reformers."

Result: Intersource agreement. Program suspended.

## **Appendix One: An Overview of Sample Selection Bias**

The goal of statistical inference is to reason from the sample of data that we have to the entire population of cases. Sample selection bias exists when we are unable to draw these inferences. Many outcomes, such as vote choice or dispute escalation, are imperfectly observable in the real world. We do not have a full set of observations on these variables, because their realization is contingent on a prior event. For example, the sample of individuals that choose to vote for a given party is comprised only of those individuals that have chosen to vote. Similarly, a sample of disputes is comprised of interactions between states that reached a high enough level of conflict to be coded as a dispute.

Why does this pose a problem? Thinking about whether selection bias is a danger requires a thought experiment. One has to ask a basic question: What is the process that generates the data that I am interested in? If we think that the sample is somehow systematically different from the population, this makes inferring about the properties of the population much more difficult. In a manner of speaking, the sample is comprised of unusual observations (Achen 1986:76). The voting example makes this plain: we know voters differ systematically from nonvoters on a number of criteria—education being one of them. Thus, one should be aware that analysis on a sample of voters may mean that this sample is, relative to the population, over educated (Dubin and Rivers 1989).

Whether this difference between the sample and the population is a threat to drawing valid inferences depends on our dependent variable of interest. If we think that the data generating process is correlated with the dependent variable, then sample

selection bias may exist (King 1989). In the above examples, we have *theoretical* reasons for thinking this is a problem. This is an important factor to note. Our reasoning about the choice of method here is driven by our thinking about how the data we are interested in are generated, as well as existing theory. We suspect that the same sorts of variables that affect whether states enter disputes (regime type, alliances, power, interdependence) affect whether these disputes escalate to war. Moreover, we also suspect that the same sorts of variables that affect both the decision to turnout (education, race, party ID, feelings toward the candidates) as well as the vote choice. If we fail to address this problem, our estimates of the parameters may be compromised, because we are basically selecting our cases on values of the independent variable. Think about the problem this way. If more educated individuals are more likely to turn out to the polls, our estimate of the impact of education on the vote choice will be run on a sample of outliers—all highly educated individuals. Because the degree of variation in the sample on this variable is reduced, our estimate of the causal effect of this parameter will be considerably attenuated.

We can also think about this more formally. Consider the following pair of equations:

$$Y_1 = X\beta_1 + u_1$$

$$Y_2 = X\beta_2 + u_2$$

Assume that we are interested in understanding  $Y_1$ . In this case,  $Y_1$  refers to either vote choice or the outbreak of war. However, we know that in the real world, we do not have a full set of observations on  $Y_1$ . We observe  $Y_1$ , our variable of substantive interest, only

if  $Y_2 \geq 0$ . Again, this corresponds to our intuition above, where  $Y_2$  is either whether or not a state enters a dispute or whether or not a person goes to the polls.

Our theoretical intuition leads us to suspect that these two equations are connected. Given this, what can be done? One solution is to do nothing. Some work in the field of health economics does exactly this, employing what are termed two part models (Manning, Duan, and Rogers 1987) which represent separate regressions of a set of independent variables on  $Y_2$  and  $Y_1$ . From a theoretical standpoint, this seems unrealistic. After all, we have reasons to suspect that similar variables affect both  $Y_2$  and  $Y_1$ .

Moreover, assuming that these two equations are independent produces inferential problems. Recall that information on  $Y_1$  is only available for some observations (again, we do not know who nonvoters would have chosen). The regression model that we run, then, is

$$Y_1 = X\beta_1 + E(u_1 | \text{the sample is selected}).$$

Let's think about the final term of the above equation. This represents our expectation regarding the disturbances or errors in the model. Because a process determines whether the sample is selected, our expectation for  $u_1$  is conditional on this process. From the above equation for  $Y_2$ , we can rewrite this final term as  $E(u_1 | Y_2 \geq 0)$ , and this inequality is the case when  $X\beta_2 + u_2 \geq 0$ . Thus,

$$Y_1 = X\beta_1 + E(u_1 | u_2 \geq -X\beta_2).$$

This tells us that the regression equation for our variable of interest  $Y_1$  depends on our knowledge about the process by which the data are generated. If we think back to the assumptions that underpin the classical regression model, this is where the problem rears

its ugly head. Recall that we need to have assumptions about the error term in order for least-squares regression to produce unbiased estimates. Two assumptions that are relevant here. First, the error terms should not be correlated with one another (this is commonly called autocorrelation). Second, there should not be a correlation between the errors and the independent variables. Thus, if  $u_1$  is correlated with either  $u_2$  or  $X\beta_2$ , then the regression will be biased and inconsistent. Establishing that these biases are not present is made more difficult by the fact that unobserved variables may influence both selection and outcomes, which also has the effect of inducing a correlation between  $u_1$  and  $u_2$  (Achen 1986). If our world were comprised of random samples, we would be able to rule out the possibility of correlated error terms between these two equations and generate reliable parameter estimates. The world described by the two part models would be reality.

The extent to which selection bias exists is ultimately an empirical question. It requires us to think fundamentally about the data generating process. In this study, we can only understand the link between institutions and compliance if we first understand the selection process. Ignoring the first stage makes inferences about the second stage much more difficult. Heckman (1979) developed a procedure that turns the sample selection issue into an omitted variable problem. What it does is allow us to estimate the model for  $Y_1$  given an understanding of how observations are selected into the sample (the  $X\beta_2$ ) as well as an estimate of the extent to which  $u_1$  and  $u_2$  are correlated. This information is then used to generate another independent variable that assesses the probability that each observation is in fact observed. Including the sample selection variable allows one to generate reliable unbiased estimates of the parameters of interest.

## Chapter Five: Credibility and Catalytic Finance

“Apart from its financial value, this accord is a passport allowing Romania to return to the international markets.”

-Romanian Prime Minister Radu Vasile,  
August 9, 1999

One justification that the IMF perennially employs for its programs is that they serve as a signal of borrower credibility (Dhonte 1997). By signing a letter of intent and agreeing to implement it, the borrowing state sends a message to the outside world that it is about to adopt responsible economic policies. This claim has been perennially espoused as a rationale for making commitments more generally (Maxfield 1997; Simmons 1999). Unfortunately for the IMF, evidence in support of this so called ‘catalytic’ effect of its programs has been scant.

The claim that Fund programs catalyze international finance is an important one for several reasons. First, this justification helps to further our understanding of how and why states delegate to international institutions: not only do they provide resources to help resolve pressing domestic problems, but they also help states to signal credibility. In theoretical terms, the notion that international institutions solve information problems is one of the cornerstones of institutional theory (Keohane 1984). Of course, in order to signal credibility, the Fund itself must be able to do what private markets cannot, and develop the means to screen out committed reformers from their opportunistic counterparts. Evaluating the extent to which Fund programs produce these catalytic flows thus tells us a great deal whether its endorsement is seen as credible by outside observers.

Moreover, assessing the value of the Fund’s informational endorsement is important to understand how more successful Fund programs can be designed. As



Schadler (1996) notes, securing external financing to support the adjustment program is part of the Fund's strategy for addressing macroeconomic imbalances. Fund programs are designed with specific assumptions about how economic variables will behave months in advance. If an adjustment program operates with the assumption that additional external loans or foreign investment will come in as a result of the program, and these fail to materialize, then it makes meeting the other benchmarks of the program more difficult. As a recent review of Fund conditionality noted, in a number of countries, external flows failed to materialize as projected (Schadler 1996:15). A failure to secure additional external financing (above pre-program levels) can pose problems if the current account deficit is not reduced to a sustainable level, thus necessitating further adjustment.

Existing studies of the catalytic effect find little evidence in support of it. Previous studies on this question (Killick 1995; Conway 1994; Hajivassiliou 1991; Bird and Rowlands 1997, 1999; Rodrik 1995; Adji et al 1997) find no evidence to suggest that signing a Fund program affects aid, investment, and flows of new loans. The existence of weak findings for the catalytic effect serves to add to the chorus of criticisms of Fund programs (Killick 1995; Bird 1995; Edwards 1989) and strengthen the claims of those who charge that the Fund is ineffective.

Unfortunately, two problems of inference and research design call these findings into question. First, the Fund's endorsement is only sought under certain conditions, and these conditions also can be informative to borrowers and lenders. Program involvement—the decision to sign a Fund letter of intent—is undertaken for very specific reasons. Existing studies of the conditions under which Fund agreements are chosen suggest that an array of economic variables account for the decision to enter into a Fund

supported reform program. Because states seek Fund assistance only under certain conditions, this has important consequences for the study of catalytic finance. It suggests that the economic crisis that produces the decision to obtain assistance from the Fund may have independent effects on whether investors and lenders choose to lend to a country that signs a letter of intent. As a consequence, our results can be confounded because we have to separate the effects of the balance of payments crisis from the effects of the Fund program on the dependent variable. Put more bluntly, one needs to control for the effects of self-selection in order to generate reliable estimates of the parameters of interest. This approach is not merely methodological muscle-flexing, since it allows us to tease out the effects of the Fund's endorsement on investor behavior from the effects of the economic crisis on the behavior of investors. In this manner, it allows us to assess exactly what 'lessons' are being learned by outsiders once they see a state enter a program.

A second problem with the existing studies is that they assume that the program is fully implemented. This, unfortunately, is rarely the case. In making this assumption, these works statistically overestimate the degree to which a state is actually "under" an IMF program (Kahler 1992:94-96). This measurement error attenuates the magnitude of the regression coefficients, making it appear like the program produces weak economic effects (King, Keohane, and Verba 1994:156,167-168).

Theoretically, we can also think about this issue from the standpoint of potential investors. They can make *ex ante* assessments about whether a Fund program can successfully be implemented, and on the basis of these assessments, they may or may not elect to invest. After all, their returns are likely to also be affected by whether the agreed-

to program is in fact carried out. For both reasons, we need to understand the effects of program implementation on flows of capital and investment.

This chapter is designed to address these lacunae. I pose two questions. First, after controlling for selection, do Fund programs produce catalytic finance? Second, does the degree of previous compliance that a state has affect its likelihood of receiving additional finance? I motivate these issues using a Bayesian decision theoretic model, which suggests that Fund programs do not entice increased flows of portfolio investment and FDI. These findings suggest that the claim that the Fund has an informational advantage over markets has little merit. External observers are not impressed by the Fund's endorsement because they know that the Fund's endorsement only comes when economies are in trouble. Thus, the presence of a Fund program confirms economic crisis more than it endorses a credible solution.

Similarly, the programs states enter may not be fully honored. As a result, potential investors have to assess the ex ante probability that the adjustment plan actually will be carried out. Thus, the decision maker forms a belief about not only the probability that her investment will have positive returns, but also about the probability that the program will be honored. Using the same Bayesian model, I find that good program implementation only allows an investor to distinguish good investments from bad ones under very rare conditions. Knowledge about program compliance allows an investor to learn new information that would serve as an endorsement, and thus allow catalytic finance, only if she believes that compliance is virtually guaranteed at the outset.

Thus, these findings raise a number of important questions. First, they pose a challenge to the institutionalist notion that international institutions serve as information

providers. If the Fund's seal of approval has value, then we would expect that its decision to lend to states would allow potential investors to update the probability of positive returns and allocate their assets accordingly. However, the value that the Fund has is to confirm that those states that enter its programs are indeed in economic crisis. Second, these results supports the larger argument in this project. The reason that the Fund's endorsement has little value is that the Fund cannot distinguish committed reformers from virtuous ones. As a result, investors do not gather new information from the announcement of a signed program. Thus, because the Fund does not separate types, it does not convey new information. Thus, the prior beliefs of investors are confirmed: states seek Fund assistance when they face economic problems, thus they expect downturns and do not commit added resources. Third, these findings bear implications for the conditions under which Fund agreements are approved. While politicians believe that a Fund program provides an opportunity for improved financial market access, this opportunity does not arise. It is in some sense little wonder that negotiations between developing countries and the Fund are divisive, since a politician is faced with the certain costs of austerity and little external benefits in return.

### **Background**

Before we can proceed to the heart of the matter, more setup of the problem is warranted. First, the IMF has a panoply of lending programs. The ones that I focus on in this project are termed Stand-By arrangements and Extended Fund Facility

arrangements.<sup>56</sup> These programs are designed to address disequilibria in a state's balance of payments. Thus, it is not surprising that studies of the conditions under which these arrangements are concluded focus on a consistent number of factors: low reserves, high levels of debt, high current account deficits, and often high inflation, budget deficits and rapid growth of the money supply (Knight and Santaella 1997; Conway 1994; Joyce 1992; McDonald 1986).

The conventional justification for Fund conditionality is that states lack credibility, and need to make a commitment to an external agent to bolster their resolve. From the standpoint of a potential lender, this is not hard to understand. Reform produces costs as well as benefits, and because implementing it often involves alienating the constituencies that leaders depend on for support, committing to reform can prove difficult. Foreign observers know this: leaders can promise all the reform they want, but a lender's return is directly related to a leader's ability to implement reform. For this reason, foreign actors may be loath to commit investments or loans *ex ante*.

Fund programs often entail the introduction of fiscal and monetary austerity. This is because the Fund frames balance of payments problems as emerging largely from problems of domestic profligacy.<sup>57</sup> In this account, politicians undertake inflationary activities, and given the constraints of a fixed exchange rate, they create pressure on the currency. Thus, the Fund program aims to alleviate the balance of payments constraint by

---

<sup>56</sup> Stand-by agreements are generally 12-18 month arrangements, while EFFs are for a longer duration (24 months and up) and for a larger amount.

<sup>57</sup> Of course, the East Asian crisis was more a banking crisis or a private sector problem than a public sector problem. While increasingly important, the source of the balance of payments problem is not a key issue for this paper, since the domain of cases ends in 1995.

providing reserves, and in exchange the state is supposed to introduce policies designed to reduce the current account deficit. These take the form of fiscal and monetary restraint as well as devaluation to improve the exchange rate. Additionally, so-called 'structural reforms,' such as privatization and the removal of export price supports and subsidies, are often mandated to strengthen the competitiveness of the economy.

Thus, the claim that the Fund espouses is that conditionality serves as a signal of 'borrower credibility' in that it allows external agents to distinguish between 'committed' and 'uncommitted' reformers and allocate their assets accordingly. In other words, observers see that a country signs a letter of intent, and this informs them that a country is adopting credible policies (Rodrik 1995; Dhonte 1997). As a result, one would expect an increasing in flows of capital and direct foreign investment following the decision to sign a Fund letter of intent.

There are three problems with this line of argument. First, successful reform is never guaranteed. In fact, implementing reform can sometimes prove politically costly. Implementing the measures required in Fund letters of intent can often upset those constituents upon which politicians depend for support. For this reason, reform is not a simple matter of developing a game plan and then implementing it. As numerous authors have noted, economic reform is politically problematic, and politicians have incentives to renege on their commitments (Nelson 1984; Haggard 1986; Haggard and Kaufman 1992; Bates and Krueger 1993). It is therefore not surprising that IMF programs are prone to break down.

The existence of uncertainty about whether reform can be sustained can serve to deter additional loans (Rodrik 1989, 1991). After all, potential lenders and investors are

concerned about rates of return, and if they think that reform backtracking will reduce their returns, they will not invest in these markets. Because Fund programs are often signed to ‘tip the scales’ and use international leverage to bolster domestic reform, it is not always a given that the program will be successfully implemented. Indeed, our discussion of the incentives that politicians have to enter Fund programs suggests that both states that can make credible reform commitments and states that cannot have incentives to enter Fund programs. This makes the Fund’s endorsement a rather noisy signal, since external observers still do not know whether a state is committed to reforms or not.

Finally, the market has other reasons to be skeptical of the Fund’s endorsement that are related to the manner in which the Fund conducts its business. Allegations of political influence underpinning Fund operations are commonplace, and these claims do have a measure of empirical support (Thacker 1999). At the same time, the Fund is vulnerable to public choice arguments suggesting that it benefits from lending and has incentives to push loans. Both these claims have the effect of weakening the notion that the Fund’s endorsement in the form of an approved letter of intent carries new information about a state’s degree of commitment to macroeconomic adjustment.

### **Past Studies**

The effect of Fund programs on capital and FDI flows has been the subject of a great deal of research. Below, I discuss the contributions of five studies to this research question. What constitutes “catalysis” often varies with the specific study, though it is

generally agreed that the subject to be evaluated comprises both capital as well as FDI and is both public and private in nature.

Each of these studies differs in terms of the temporal domain, yet one thing is clear from these existing studies: there is very little evidence that suggests that IMF programs have catalytic effects. In fact, in more than one study below, IMF programs have the opposite effect: rather than act to attract foreign capital and investment, they deter it.



Table 5-1: Previous Studies on the Catalytic Effect		
Author	Specifics	Findings
Conway 1994	Sample of 74 LDCs from 1976-1986	No evidence of a credentialing effect for growth, current account, inflation, investment/GDP
Hajivassiliou 1991	Sample of 79 LDCs from 1970-1982	Fund programs are negatively related to new debt/exports and level of debt service in arrears
Bird and Rowlands 1997	Sample of 90 LDCs from 1974-1989	Fund programs are negatively related to net commitments over exports, and positively related in the case of official lending.
Bird and Rowlands 1999	Sample of 115 low and middle income countries from 1971-1995	<i>Low income country sample:</i> EFF/ESAF positively related to FDI and official source, negatively related to private debt. SBA positively related to official source. Incompletion negatively related to official source.  <i>Middle income country sample:</i> EFF positively related to portfolio flows, negatively related to private debt. Incompletion positively related to official source.
Adji et al 1997	Sample of 23 LDCs from 1970-1981	EFF/CFF programs have no effect on nominal/real FDI.
Rodrik 1995	Sample of countries from 1970-1993	No evidence that IMF lending affects private transfers/GDP.
Unless otherwise indicated, only relevant findings are reported.		

Exactly why this is the case is not that hard to understand. Potential lenders and investors either may understand the effects of Fund programs, and may discern that implementing austerity may, over the short run, reduce their returns rather than increase them. For this reason, they do not commit additional resources after a letter of intent is signed.

Alternatively, they may also regard the commitment to Fund-backed reform as incredible,

and for this reason they refuse to make additional commitments. Each of these lines of argument bring with them important inferential implications that many of the above studies ignore.

The inferential problem with these studies is that they essentially regard the decision to select into Fund programs as a random one. Thus, they essentially operate as if developing countries parceled themselves into a control group without Fund programs and a treatment group in which these states signed Fund programs. In reality, we know that this is far from the case. These “naive results” based on an assumption of random selection are the mere difference between the mean flows of selected versus nonselected programs, and are shown below.

<b>Table 5-2: “Naive” Model of Program Selection and Flows</b>			
	Official	FDI	Portfolio
Selected programs (mean)	.066871	.0104779	.0012313
Nonselected programs (mean)	.0675273	.0219178	.0010968
Difference	-.00066	-.0114399	.0001345
Number of Observations: 1130 (Nonselected for Official and FDI) 1090 (Nonselected for Portfolio) 687 (Selected)			

If we accepted these findings, we would conclude that Fund programs deter official flows and FDI and attract portfolio flows. Is this really the case? We know that states seek help from the Fund when they face balance of payments crises. This poses inferential problems for our study, because we cannot easily form a counterfactual. Assuming that states randomly select into Fund programs causes a problem because we have to make the

argument that if the state had not entered the adjustment program, it would have behaved the same as the other states that did not have adjustment programs. In practice, this means that the flows to states in economic crisis that do not undertake Fund programs are the same as those in states that are not suffering from economic crisis. This is counter to our understanding of how lenders think. As a result, if we are comparing non-Fund states without crises to Fund states with crises, then this means that we have to disaggregate the effects of the crisis that brought the country to the IMF from the effects of the program, and then assess their independent effects on catalytic finance.

An example from Greene (1997:981) should help make this clearer. Suppose that we want to understand the effect that a college education has on one's earnings, and we run the following regression:

$$\text{Earnings} = \beta x + \delta \text{College} + \epsilon$$

The  $\beta x$  term is a shorthand for a vector of independent variables that predict earnings. We use a dummy variable for whether someone went to college or not. The key question that we have to answer is this: does  $\delta$ , our estimate for the effect of college attendance, accurately capture the causal effect we seek to estimate? Our answer depends on the counterfactual: what are we assuming about the typical individual that goes to college? If we assume that they would possibly (owing to their above average gumption) have high earnings even if they did not go to college, then our OLS regression will actually overestimate the effect of college on earnings.

Why is this the case? One of the classic assumptions underpinning the standard regression model is that the independent variables are not correlated with the error term. In this example, we suspect that this assumption is violated, since there may be

unobserved factors that determine whether or not a person goes to college that have an independent effect on earnings. If we run a regression, our coefficient  $\delta$  will capture both the effect of education on earnings and the effect of unobserved variables on earnings. Eliminating these confounding effects is one reason to use a random sample.

In our case, the problem is the opposite from that of the above. The existing studies on catalytic finance noted above use the following model:

$$\text{Flows} = \beta x + \delta \text{IMF} + \epsilon$$

and a similar problem exists in these studies, since we know that the same variables that affect financial flows, such as high levels of debt service, also affect whether or not a state goes to the Fund. This compromises one of the assumptions that we make in regression, which is that the factors that are not in the model do not systematically affect the dependent variable.<sup>58</sup> Since we would suspect that the sample of Fund client states will be less likely to attract catalytic finance even if they didn't go to the Fund (owing to their weak macroeconomic fundamentals), a regression model based on the above form will underestimate the effects of the Fund. Given this, it is not surprising why the results of previous studies have been so meager. Thus, to generate reliable parameter estimates, we have to take sample selection seriously. The empirical tests below employ the techniques developed to handle non-random selection to better assess the effects of Fund programs (Goldstein and Montiel 1986; Ul-Haque and Khan 1999; Bordo and Schwartz 1999; Bagci and Perraudin 1997). This allows us to separate the effects of the state's economic fundamentals from the effects of the letter of intent. Before we can begin to

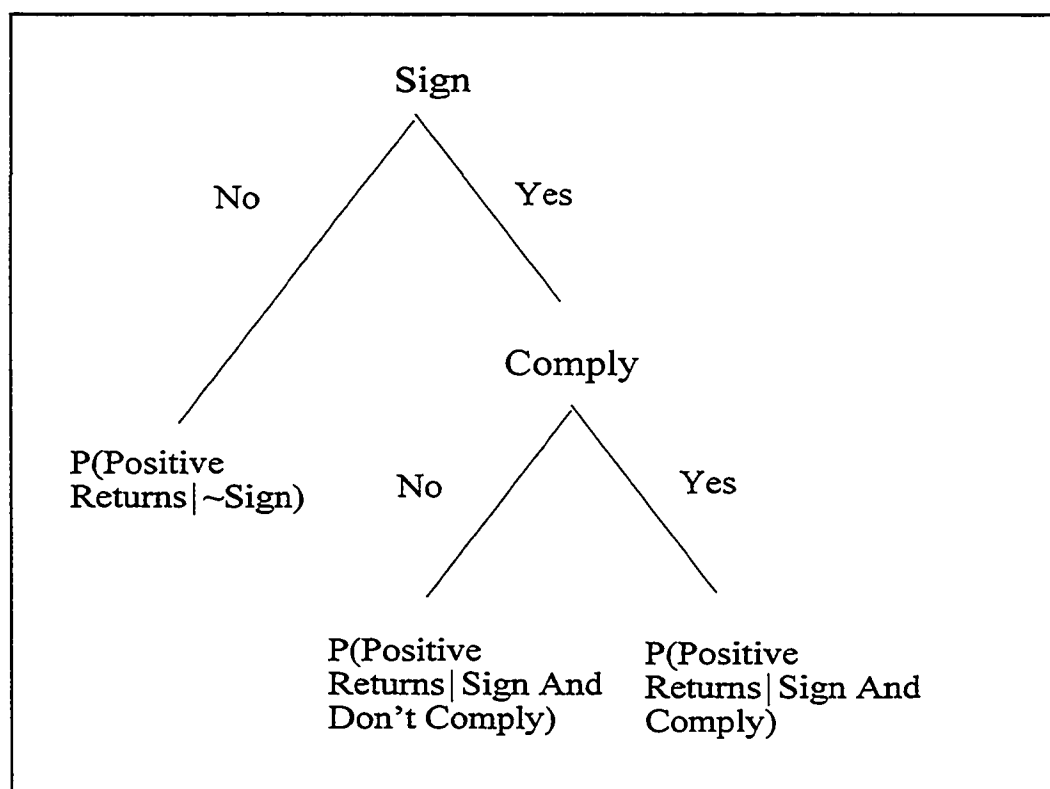
---

<sup>58</sup> In other words, we assume that  $E(u_i | X) = 0$ . The problem is that unobserved factors induce a correlation between the error term and the independent variable (Berk 1983; Achen 1986).

discuss the findings however, we need a model to justify the argument as to why the Fund does not catalyze. This is the subject of the next section.

### A Model of Investor Belief Formation

In order to understand how international markets may react to the signing of a



**Figure 5**

letter of intent, as well as to compliance with it, we develop a simply Bayesian decision theoretic model. Here an investor has to decide to allocate assets to a country under uncertainty. The investor wants to allocate resources to states that will produce positive returns, but she does not know precisely (or, with certainty) whether a certain state actually will produce these returns. She does, however, observe the presence or absence

of an IMF program, as well as a degree of compliance with it.<sup>59</sup> These new pieces of information allow her to update the probability that her investment will produce positive returns, and allow her to make a decision. This decision tree is shown in Figure 5.

It should be noted that the ‘payoffs’ at the terminal nodes are probabilities that are updated given new information (the updating is denoted with a vertical line, which means “given.”) Given the formula for Bayes’ Rule, we can calculate an updated probability using simulated data. To do this, however, we need to make a pair of assumptions. Let us consider the investor’s choice if a state does not choose a Fund program. Seeking a policy environment that generates positive returns, her updated probability in light of this new information is the following:<sup>60</sup>

$$\text{Prob (Positive Return | State Does Not Sign)} = \frac{\mathbf{P(\text{Not Sign | Positive Return})} \cdot \mathbf{P(\text{Positive Return})}}{[\mathbf{P(\text{Not Sign | Positive Return})} \cdot \mathbf{P(\text{Positive Return})} + \mathbf{P(\text{Not Sign | Negative Return})} \cdot \mathbf{P(\text{Negative Return})}]}$$

To solve this, we have to assess what the probability is that states don’t sign agreements with the Fund given positive investor returns (the bold faced terms above). To answer this question, we have to revisit what we know about what determines whether states enter Fund programs. Since the evidence is clear that they enter when they face an unsustainable balance of payments, and these problems are also correlated with low growth and low investment, then it makes sense to conclude that states will sign agreements when investor returns are low, since both these decisions are correlated with the state of the economy. Thus, the probability of states signing given low investor returns

---

<sup>59</sup> For purposes of simplification, we assume that the Fund can assess compliance and noncompliance appropriately.

<sup>60</sup> This follows directly from Bayes’ Rule:  $P(a|b) = p(b|a)p(a) / [p(b|a)p(a) + p(b|\sim a)p(\sim a)]$

is high. Similarly, they won't sign IMF agreements when investors are doing well, since this implies that the economy is in good shape. Thus, we assume that the probability of states signing Fund agreements given good returns is low.

Based on these assumptions, we can 'solve' for an investor's updated beliefs about the probability of positive returns given whether or not they observe a Fund program. These probabilities are shown in the table below.

<b>Table 5-3: Updated Beliefs, Agreement Decision Phase</b>		
Prior probability of positive returns	Updated probability of positive returns given signed Fund program	Updated probability of positive returns given state does not sign
.1	0.027	0.308
.25	0.077	0.571
.5	0.200	0.800
.75	0.429	0.923
.9	0.692	0.973

*Assumption:*  
 $p(\text{Don't Sign Program} | \text{Positive Returns}) = .8$

This table suggests that the Fund does convey information to prospective investors, but the information that it conveys suggests that the state is not an ideal target for investment. We note that for every single initial assessment, the updated probability of positive returns is lower if a state signs a Fund program (or that the middle column is consistently less than the left most one).<sup>61</sup> It should be noted that these results are unaffected by how

---

<sup>61</sup> This result is not merely an artifice that stems from the specific values given at the outset. Subsequent robustness checks based on a draw of 100 random numbers suggests similar results hold so long as  $p(\text{Don't Sign Program} | \text{Positive Returns})$  is greater than  $p(\text{Don't Sign Program} | \text{Negative Returns})$ .

we assess the assumed probabilities above. So long as the decision to enter a program is associated with negative returns, investors that see a state enter a Fund program lower their ex ante assessment of whether this state is a profitable investment environment, and do not invest accordingly.

Thus, this model helps us to understand why the previous findings regarding the catalytic effect of Fund programs has been so meager. Foreign investors see the presence of a Fund program as an indicator of economic turmoil rather than as a signal of credibility. The problem with past studies, however, has been that they have essentially ignored the fact that states enter Fund programs because they face economic hardships. In other words, because Fund programs are nonrandomly selected, we need to better assess this selection effect to better generate empirical tests that will allow us to assess this model.

The next stage of the model involves assessing the effects of compliance on the investors decision to enter or not. Here, the investor possesses a prior belief about whether the program will be implemented as well as a prior belief about whether her returns will be positive. The investor sees whether or not a program is signed, and whether or not the agreement is honored, and she uses both pieces of information to update the probability of positive returns. On the basis of these results, a decision whether or not to invest in the state is made. These updated probabilities, which follow directly from Bayes' Rule, are computed as follows:

$$\text{Prob( Positive Returns | Sign, Comply)} = \frac{[P(\text{Positive Returns}) \text{Prob}(\text{Sign | Positive Returns}) P(\text{Comply})]}{[P(\text{Positive Returns}) \text{Prob}(\text{Sign | Positive Returns}) P(\text{Comply}) + P(\text{Negative Returns}) P(\text{Don't Comply}) P(\text{Sign | Negative Returns})]}$$



$$\text{Prob(Positive Returns | Sign, Not Comply)} = \frac{[P(\text{Positive Returns}) P(\text{Sign} | \text{Positive Returns}) P(\text{Don't Comply})]}{[P(\text{Positive Returns}) P(\text{Sign} | \text{Positive Returns}) P(\text{Don't Comply}) + P(\text{Negative Returns}) P(\text{Comply}) P(\text{Sign} | \text{Negative Returns})]}$$

Using the same procedure as above, we can use a range of values to generate these updated probabilities. These appear in the table below. The second column here is the prior probability of positive returns, which appeared on Table 5-3 as the first column.

Prior probability of compliance	Prior probability of positive returns	Updated probability given state complies	Updated probability given state does not comply
.5	.1	0.027	0.027
	.25	0.077	0.077
	.5	0.200	0.200
	.75	0.429	0.429
	.9	0.692	0.692
.75	.1	0.077	0.009
	.25	0.200	0.027
	.5	0.429	0.077
	.75	0.692	0.200
	.9	0.871	0.429
<i>Assumption:</i> p(Sign program   Negative Returns) = .8			

These results suggest several findings. First, if investors are completely uncertain whether an agreement will be complied with, so that they assess compliance and noncompliance as equally likely, they learn nothing from the outcome. That is, whether or not a state complies does not affect the investor's updated belief. However, we note that both the probabilities in the two updated columns are less than the prior probability of positive

returns. The investor, as above, reduces her estimate of the probability of positive returns given complete uncertainty over compliance.

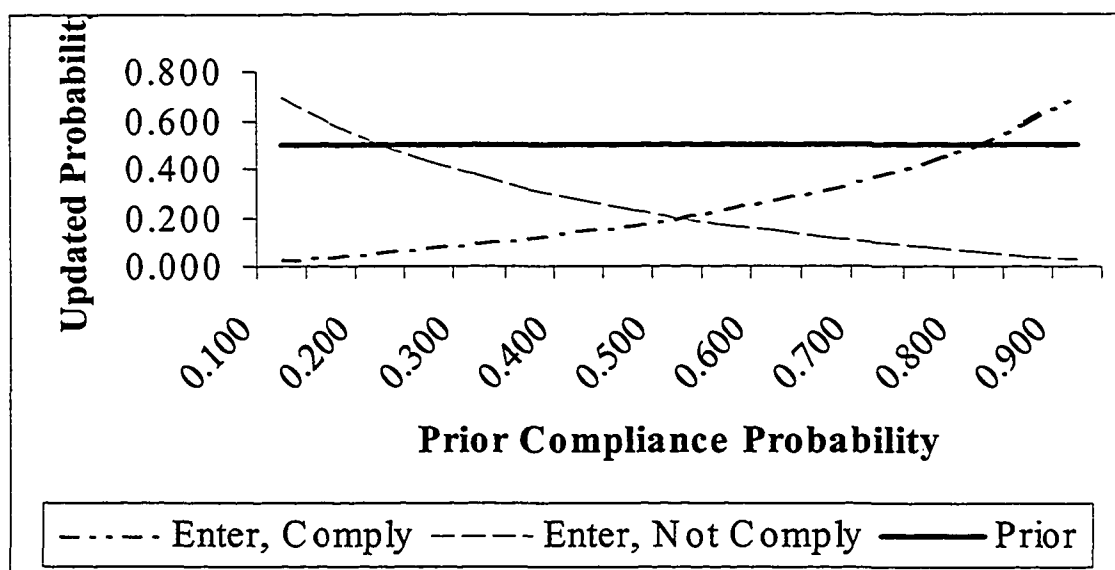
If we assume the probability of compliance is higher (75%), we see the same result: the investor's updated probability is less than her prior belief. While it is true that the updated probability is much less in the case of noncompliance, it is true across the board that the investor is less likely to commit resources to a state regardless of whether they honor the letter of intent. Thus, the belief that the Fund only grants states agreements if they are in financial trouble (and that this implies investors will suffer negative returns) is not overcome by a state's performance under these agreements.

This is an important result. Rodrik (1989) suggests that states can attempt to overcome investor uncertainty by "overshooting" and implementing policies such as excessive currency devaluations.<sup>62</sup> If Rodrik's argument were correct we would expect that catalytic flows can be induced if states honor their commitments to the Fund. Our argument here suggests that this is unlikely, since investors update the probability of returns based on whether agreements are signed as well as whether they are honored. Investors acquire information from this first stage as well as from the second.

We can demonstrate that catalysis is rare through the use of the chart below. This is a graphical representation of Table 5-4 using simulated data, and here we plot the prior compliance probability against the updated probability of returns assuming that the prior belief is .5. This approximates Rodrik's argument, in that lenders are uncertain over whether a given reform can be implemented.

---

<sup>62</sup> We could also think about these results as contrasting with a costly signalling argument (Fearon 1994).



**Figure 6**

Here, an investor is only likely to commit resources only if her uncertainty can be reduced. In this Figure, this only happens if the updated belief is greater than the prior belief of .5. This only takes place for a prior compliance probability of greater than .8. Thus, it is not that states that “overdo” it are able to attract investors, but states that are almost certain to implement the commitments that they sign that can attract new investments. Thus, if this argument is correct, we will see no evidence for a catalytic effect even after controlling for the degree of program implementation in a large sample of states.

### Research Design

In order to shed some light on this issue, I created a dataset of 126 states that negotiated Stand-by or Extended Agreements between 1979 and 1995.<sup>63</sup> In order to proceed in this project, we have to do two things. First, we have to better understand the

---

<sup>63</sup> 20 states in this panel had no Fund programs at all during this time period.

factors that cause states to select Fund agreements and understand how to control for the effects of selection. Second, we need to develop a model of loan and investment flows.

### *Understanding Selection*

In order to assess if selection matters, we have to first develop a set of independent variables that predict to it. Fortunately, the existing literature already suggests a number of relevant variables, which we use for the first step in the process. Using the existing literature as a starting point, I used the following variables to predict whether a state will be under a Fund program: debt service ratio, GDP growth, and reserves measured in months of imports. These variables reflect the conventional wisdom, and are supported by a number of studies (Knight and Santaella 1997; Conway 1994; Przeworski and Vreeland 2000). All variables are lagged one year to avoid simultaneity. In addition, to address the potential for autocorrelation, I included a series of temporal splines per Beck, Katz, and Tucker (1997). Our results appear in the table below.

<b>Table 5-5: Baseline Selection Model</b>	
Debt Service $t-1$	.014918*** (.00400)
Reserves $t-1$	-.06699** (.02181)
Growth $t-1$	-.027395* (.01188)
Budget Deficit $t-1$	.00445 (.0091)
Growth of Net Domestic Credit $t-1$	.00418 (.00366)
Constant	.89350*** (.12422)
Cubic spline $\chi^2$ test: 473.16 ( $p > 0.0000$ ) N = 850 observations (86 countries) Percent Correctly Predicted: 84.82%	

The conventional wisdom, which suggests that debt service, reserves, and growth lead politicians to enter Fund programs, is supported here. The  $\chi^2$  test on the cubic spline segments were highly significant, telling us that we certainly have autocorrelation in this model. Including these segments controls for it (Beck, Katz, and Tucker 1997).

Our conjecture is that these variables serve as a proxy for the overall robustness of the economy. As a result, they should affect the expected returns that potential lenders and investors receive, and therefore affect the level of flows that occur after the state signs a letter of intent. How, then, do we control for selection? To go back to the earnings example from Greene, all that we need to do is use the information that we obtain from this model to estimate our equation of interest.

Thus, in order to ascertain the effect of selection on outcomes, I need to first develop an instrument to capture the potential selection effect. This is traditionally

known as the hazard rate or lambda, and represents the probability of being under a Fund agreement over time. We form the hazard rate from the predicted values from this model. Thus, the hazard rate is a variable that captures the effects of all of the independent variables on selection. When used as an additional variable in our estimation of the equation of interest, in this case, aid and investment flows, this “controls” for the effects of selection.<sup>64</sup>

Finally, we need to also develop independent variables that predict investment and capital flows. Here we rely on a model developed in Bird and Rowlands (1997, 1999) that includes the following independent variables: per capita GNP, GDP growth, inflation, investment/GDP, exports/GDP, debt service, and international lending rates.<sup>65</sup> As for the dependent variables, we focus on three: net Foreign Direct Investment, portfolio investment, and net official flows. All variables are measured over GNP.

Thus, the table below shows the models for flows with the selection instrument lambda included as an independent variable on the right hand side.

---

<sup>64</sup> It is for this reason that this process was originally referred to as ‘two-step’ estimation following Heckman (1979).

<sup>65</sup> All relevant information regarding the variables is found in the research design (Chapter Four).

<b>Table 5-6: Effects of Selection and Fund Programs on Flows</b>			
	FDI/GNP	Portfolio Flows/GNP	Official Flows/GNP
Dependent Variable $t-1$	.59899*** (.12798)	.5324*** (.1153)	.8183*** (.0702)
GNP per capita $t-1$	5.93e-07 (4.43e-07)	1.25e-07 (1.52e-07)	-3.94e-06** (1.15e-06)
GDP growth $t-1$	.0001196 (.000146)	-.00006* (.00003)	-.0006953 (.000645)
Inflation $t-1$	-6.31e-07 (4.63e-07)	-1.06e-07 (1.18e-07)	.0000149 (.000011)
Exports $t-1$	.000191*** (.0000595)	.000031 (.000031)	-.0000844 (.000133)
Investment $t-1$	.0000968 (.0000788)	-.0000216 (.0000354)	-.0002469 (.000156)
Lending-LIBOR $t-1$	4.53e-08 (2.66e-06)	1.96e-08 (2.35e-08)	-6.36e-07 (8.51e-07)
Debt Service $t-1$	.0001243* (.000057)	.00001 (.000029)	-.000197 (.000188)
Fund Program	<b>-.00638**</b> <b>(.0021)</b>	<b>-.001442</b> <b>(.00235)</b>	<b>-.009943</b> <b>(.006842)</b>
Lambda	<b>.002535</b> <b>(.001785)</b>	<b>.00144</b> <b>(.00259)</b>	<b>.000176</b> <b>(.00423)</b>
Constant	-.002657 (.00327)	.000593 (.000102)	.03632** (.0115)
Model x2 (all three models): 0.0000 Number of observations: 574 for Official and FDI, 561 for Portfolio flows Robust standard errors are in parentheses. *, **, *** represent levels of significance at the .05 level, .01 level, and .001 levels, respectively.			

The parameters in the last rows of the table in bold face merit our attention. Overall, the models are highly significant, but the estimates for lambda are not significant for any type

of flow. This suggests that the weaker economic fundamentals of Fund program states have no independent affect on flows. In other words, while Fund program states and non-Fund program states may differ in terms of their fundamentals, these differences have no independent effect on flows.

Of course, our regression also included a dummy for the presence of a Fund program, which is critical to establish evidence of a catalytic effect. In the above table, Fund programs have a negative but insignificant effect on official and portfolio flows, and a negative and significant effect on FDI flows. To better get a sense of the magnitude of these program effects, we can use the regression equation from the FDI model to generate estimates of FDI flows given that the state was and was not under a Fund program. Subtracting these two estimates gives us the selection corrected estimate of program effects, which is equal to the value of the coefficient on the Fund program dummy. This is shown in the table below.

<b>Table 5-7: Selection-Corrected Estimates of Program Effects on FDI</b>				
	Mean	S.D.	Minimum	Maximum
Program States	.00581	.0838586	-.065903	2.634583
Nonprogram States	.01219	.0838586	-.059523	2.640963
Difference	<b>-.00638</b>			
Number of Observations: 1002 Estimates are scaled over GNP.				

Thus, we see that states not under Fund programs have an estimated annual FDI inflow of .01219 percent of GNP, while states under Fund programs have an estimated annual FDI inflow of .00581 percent of GNP. While this difference does not seem like much in



practice, when we measure the size of this flow for states in our sample, we find that this difference is approximately 205.9 million US dollars annually.

The evidence above suggests that Fund programs do not catalyze international lending, but it should be noted that this is not an explicit test of our model. Because we are essentially pooling all the program years, this lumps together the first year of programs and all others. The model developed here tells us little about how flows respond to programs over time. The most appropriate test of the catalytic effect, therefore, requires that we focus on the first year of Fund programs. This test appears in the table below.

<b>Table 5-8: Effects of Selection and Fund Programs on Flows (First Program Years)</b>			
	FDI/GNP	Portfolio Flows/GNP	Official Flows/GNP
Dependent Variable $t-1$	.60712*** (.12843)	.5952*** (.1456)	.8215*** (.0703)
GNP per capita $t-1$	5.32e-07 (4.32e-07)	1.13e-07 (1.51e-07)	-3.96e-06*** (1.15e-06)
GDP growth $t-1$	.0001 (.000147)	.000032 (.000036)	-.000698 (.000662)
Inflation $t-1$	-4.00e-07 (5.23e-07)	1.86e-07 (2.31e-07)	.0000151 (.000011)
Exports $t-1$	.000188** (.000059)	.000043 (.000036)	-.0000885 (.000134)
Investment $t-1$	.0001122 (.00008)	-.000035 (.000038)	-.0002345 (.000155)
Lending-LIBOR $t-1$	3.54e-08 (2.44e-08)	2.42e-08 (1.45e-08)	-6.17e-07 (8.31e-07)
Debt Service $t-1$	.000115* (.000057)	.000031 (.000028)	-.0002298 (.00019)
Fund Program	<b>-.01034**</b> (.00262)	<b>-.0071668*</b> (.003099)	<b>-.013725</b> (.01094)
Lambda	.004292*** (.001125)	.00144* (.00259)	.00741 (.00558)
Constant	-.002657 (.00327)	.001038 (.000769)	.0352** (.0113)
Model x2 (all three models): 0.0000 Number of observations: 574 for Official, FDI 561 for Portfolio flows Robust standard errors are in parentheses. *, **, *** represent levels of significance at the .05 level, .01 level, and .001 levels, respectively.			

These results merit some discussion. The Fund program dummy is negative and significant for flows of FDI and Portfolio investment. The decision to enter a Fund

program produces lesser inflows for both these measures. We note that the effect of Fund programs on FDI is stronger in the first program year than in later years, as compared to Table 5-6. What is also important is the control variable. Our estimate for Lambda, which captures the effects of economic variables driving states to the Fund, is positive and significant for both FDI and Portfolio investment. Thus, this suggests that these states have weak economic fundamentals that actually induce new flows. One way to think of this is that the effects of devaluations are being captured, which can entice new flows as investors seek to take advantage of bargains. Even after we control for this effect, though, Fund programs deter inflows. The coefficients for the Fund program dummy are greater than the effects of the economic fundamentals captured by lambda, so the message they send is swamped by the announcement of the program.

These results also pose a puzzle in that official lending, which here includes bilateral and multilateral loans, also do not respond positively to the announcement of a Fund program. Though the value of the IMF loan is not included in this total, we would expect that positive evidence for catalysis would be most likely here. Exactly why official flows don't coordinate on the Fund's signal remains a topic for future inquiry, but the fact is that countries that need assistance are not receiving it, and this holds regardless of whether how we measure the duration of the program. One possibility is that this reflects geopolitical biases, but this is a topic best left for further research.

Asset specificity may account for the differences between Table 5-6 and Table 5-8. FDI is less likely to enter a state in the first year of a Fund program, and it remains consistently less likely to enter a state during the duration of its program. Portfolio investment, on the other hand, is less likely to enter a state only in the first year of the

program. Because FDI is a investment of fixed assets, this suggests that investors will be more prudent in their allocation decisions, and thus tend to stay away from Fund program states because their economic climates are uncertain. Portfolio investment on the other hand is a more flexible asset, and while in the first year investors may be deterred, the states comparatively better fundamentals may prove enticing in later years. Since these investments are not sunk costs, the losses that can be experienced from investing in a bad climate are much less.

Wrapping up this section, we note that using appropriate estimation techniques across types of flows and with different temporal measures that the case for a Fund seal of approval is not supported. A possible objection to these results is that the model has been misspecified, and that politicians in developing countries can still signal their type not by entering the program, but by honoring it. Whether this is the case is the subject of the next section.

### *Compliance and Catalysis*

Our Bayesian model suggested that there was very little possibility that successful program implementation could overcome the inference that lenders gather from seeing the announcement of an IMF program. Is this really the case? The one previous study that addressed this question, that of Bird and Rowlands (1999), found mixed results. A history of uncompleted programs was found to reduce official source lending in low income countries, but it increased official source lending in middle income countries. They measured nonimplementation by the number of agreements in the past four years of which more than 20% of the total arrangement was left undrawn. The problem with this

measure is that it conflates the existence of a compliance problem with the number of arrangements that a country has in a given year. For example, if a state has four agreements in four years and one fails, this is coded the same as a state that has one agreement in four years which fails. One might think that this second state would be better 'managed' than the first. Thus, it is not quite clear that the measure they use accurately captures the concept.

I elected to operationalize this variable more simply and avoid the confound of tying compliance with the number of programs in a given amount of time. I gathered information on whether a program was suspended at any point for noncompliance for programs between 1979 and 1995. In 138 of the 347 programs signed between 1979 and 1995, states were not eligible for all of the drawings either because they missed performance criteria and were unable to obtain a waiver from the Fund or they failed a quarterly review (Schadler 1995a:2 and interviews).<sup>66</sup>

If it is the case that catalytic finance is linked to a state's past history of implementing programs, then we would expect that whether the most recent Fund program failed or succeeded would have significant effects on new flows of lending and investment. However, we must proceed very carefully in this estimation. After all, while the decision to enter a Fund agreement is a nonrandom event, whether a Fund program is suspended is also a nonrandom event. Thus, I need to understand the factors that allow states to make and keep their commitments. Using the hazard rate from this model, we can then plug it into the same equation that we employed earlier to assess the impact of compliance on flows. Thus, rather than a simple probit of program selection as

---

<sup>66</sup> I was unable to find information on an additional 22 programs.

above, I employ a joint probit model of selection and compliance. Using a different hazard rate makes sense since some states leave Fund programs because of compliance problems, and some leave because the 'medicine' seems to have worked. Thus, changing the hazard helps us to better distinguish successful implementers from failed ones. Since we know what Fund performance criteria entail, we can build an appropriate model.

Below I use a probit model with sample selection to assess the factors that produce compliance, given the prior decision to enter an agreement (Van de Ven and Van Pragg, 1981). The selection model is the same as that in Table 5-3 and the model of compliance includes variables that are often the basis of Fund performance criteria: the growth of the money supply and the budget deficit, and the level of reserves (Beveridge and Kelly 1980; IMF 1987; Guitain 1995).<sup>67</sup> We also include a dummy variable for whether the agreement is in an EFF program. As a control variable, we also include a state's level of trade openness (defined as exports plus imports as a percentage of GNP). These results are shown in the table below.

---

<sup>67</sup> These variables are not lagged, because we assume a specific chain of events. Contemporaneous increases in the money supply and budget deficit, or decreases in reserves, suggest that the Fund program is not being implemented, and can lead to program suspension (Leone 1991, Guitain 1994, Cottarelli 1993, Gylfason 1987).

<b>Table 5-9: Heckman Probit Model of Selection and Sanctioning</b>			
Selection Model		Sanctioning Model	
Debt <sub>t-1</sub>	.01384*** (.00325)	Change in Net Domestic Credit	.0902* (.0374)
Growth <sub>t-1</sub>	-.02948*** (.0081)	Budget Deficit/GDP	-.02862* (.0148)
Reserves <sub>t-1</sub>	-.05290*** (.0156)	Log of Reserves	-.1205 (.0696)
Change in Net Domestic Credit <sub>t-1</sub>	.00481 (.00513)	Openness <sub>t-1</sub>	.000073 (.00237)
Budget Deficit/GDP <sub>t-1</sub>	.00917 (.00683)	EFF Program Dummy	-.0429 (.1603)
Constant	.83125*** (.1102)	Constant	-.1597 (.2056)
Rho	-.51074		
Rho $\chi^2$	0.0026		
Model $\chi^2$	0.0000		
Number of observations: 812 $\chi^2$ results for cubic spline segments not shown. *, **, *** represent levels of significance at the .05 level, .01 level, and .001 levels, respectively.			

This model confirms our understanding of surveillance in Fund adjustment programs.

These findings suggest that Fund programs break down because states are unable to implement policies of fiscal and monetary restraint, for as the budget deficit worsens, and the growth in the money supply increases, the program is more likely to be suspended by the Fund.<sup>68</sup>

---

<sup>68</sup> Lagged measures of aid flows were not related to whether the program was sanctioned by the Fund.

Using this system of equations, we can now move to stage two. We start by forming the hazard rate, which is the joint probability of selection and noncompliance. Then using this as an instrument, we add it to the previous model of flows. As noted above, because we are add a compliance variable to the equation, which is only observed if there is an agreement in the first place, this reduces our number of observations substantially. These results use the same estimation procedure as in Table 5-4.



<b>Table 5-10: Impact of Program Suspension on Flows</b>			
	FDI/GNP	Portfolio/GNP	Official Flows/GNP
Dependent Variable $t-1$	.31024 (.2247)	.73798*** (.2249)	.56203*** (.06224)
GNP per capita $t-1$	8.09e-07 (5.15e-07)	2.20e-08 (1.07e-07)	5.94e-06*** (1.06e-06)
Growth $t-1$	.0002326 (.000223)	8.37e-06 (.000378)	-.000255 (.000371)
Inflation $t-1$	-7.20e-07 (1.88e-06)	-9.00e-08 (1.26e-07)	.0000412*** (8.68e-06)
Investment $t-1$	.000153 (.000139)	-8.59e-06 (.0000156)	-.00031 (.00020)
Exports $t-1$	.000141* (.000067)	-.0000186 (.0000304)	.000153 (.000164)
Lending-LIBOR $t-1$	5.52e-06 (4.46e-06)	-.000054 (.000013)	-.0000359*** (.0000129)
Debt Service Ratio $t-1$	.000163* (.000073)	2.14e-06 (2.35e-06)	-.00022 (.000165)
Program Suspension	<b>-.000953</b> <b>(.00251)</b>	<b>-.0003183</b> <b>(.0004287)</b>	<b>-.000397</b> <b>(.00405)</b>
Lambda	<b>.0000923</b> <b>(.00472)</b>	<b>.0004424</b> <b>(.000649)</b>	<b>.00754</b> <b>(.00848)</b>
Constant	-.007832 (.00769)	.0009 (.00126)	.04643*** (.01302)
N=248 Panel-corrected standard errors appear in parentheses. Model $\chi^2$ : 0.0000 (for all three) Coefficients identified with *, **, *** and **** are significant at .05, .01, and .001 levels respectively.			

Several non-results merit our attention here. First, we note that the hazard variable is not significant in any of our models. This tells us that differences in economic fundamentals between compliant and noncompliant states have no independent effect on loan flows. At

the same time, we also note that there is no independent effect on any flow from the Fund suspending an arrangement. Subsequent robustness checks found little reason to question these results. Developing different hazard rates using more simplified models, adding exchange rate regime and capital control variables, and looking at lagged sanctioning produced similar non-findings regarding Fund sanctions and unobserved variables.

How then, can we make sense of these results? Earlier I noted that the literature on costly signalling suggests a rationale for why compliance matters. I contrasted this argument with one stemming from a Bayesian decision theoretic model, which suggests that potential investors gather more information from whether a state enters a program than whether it honors it. The information that they gather from observing a concluded IMF agreement is that the state that signs the letter of intent is a risky investment. Knowing that compliance with these agreements is a probabilistic endeavor, learning about whether a state honors its commitments only rarely allows a outside observer to reduce uncertainty and then make an investment decision.

Thus, the model suggests that external observers lower their updated belief about whether a state was an appropriate investment climate after seeing a Fund program. Our evidence supported this claim, as states under Fund programs deter FDI inflows throughout the duration of the program. In the first year of an IMF program, flows of portfolio and FDI investment fall off substantially, even though those states possess economic fundamentals that would attract inflows. Fund conditionality seems to convey independent information to outside observers, and this information acts as a deterrent.

What message appears in the Fund's signal? There are two messages here. First, the Fund makes these endorsements lacking the ability to genuinely address the problem

of adverse selection. Because both credible reformers and less credible ones have incentives to enter Fund programs, this means that the Fund's endorsement does not convey a clear signal about a state's ability to successfully implement reform. The announcement of a letter of intent does not allow external observers to separate good investment climates from bad ones. Second, and related, because states seek Fund programs when they face low growth and low investment, investors connect the presence of a Fund program with weak returns, and for this reason they do not enter. The signal that conditionality sends is that states under Fund programs are relatively more risky investments.

### **Implications**

These findings bring with them broader lessons for the study of international institutions and for the analysis of the effects of Fund programs more generally. First, in terms of the IMF, the evidence here points to an 'incredibility effect' in that despite the Fund's repeated invocations to the contrary, little evidence exists to support the so called 'catalytic' effect of IMF programs. Our analysis builds on the previous works in this area and corrects for inferential problems. Thus, though international institutions are created to solve problems of market failure—and in this case the 'market failure' is real, since rational individuals won't allocate assets to countries that are macroeconomic basket cases, in this case, we see little evidence the Fund's endorsement is an effective one.

In terms of the broader debate over the IMF, it is clear that these findings cast doubt on its level of influence over global markets. A common critique of the Fund is that its programs open LDC economies to penetration by foreign corporations. If this

argument is true, then we would have seen evidence that flows of investment respond positively to the announcement of letters of intent. Especially in the case of FDI, we see no evidence for this argument. Thus, the claim that the Fund is a ‘force multiplier,’ which both the Fund and its critics make, has little empirical basis.

At the same time, in terms of the larger debate over the Fund, this analysis suggests that better Fund selectivity can cause its signal to have value in private markets. Our Bayesian model suggests that if investors are virtually certain that a Fund program can be implemented, then they can use the fact of good implementation to update positively their prior beliefs and then allocate resources to these states. What this requires, though, is that the Fund develop much better means to identify credible reformers *ex ante*.

Our findings have important implications for the conditions under which agreements are selected. Because Fund programs deter inflows of portfolio investment and FDI, it is almost no wonder that negotiations between the Fund and developing countries are often protracted, or that “reform fatigue” (Nelson 1990) can set in. This analysis confirms the findings of those who suggest that reform offers certain costs and uncertain benefits (Bates and Krueger 1993; Haggard and Kaufman 1992).

Turning to the literature on the analysis of Fund programs, this project has two important implications. First, analysis of this sample shows evidence of a selection bias comparing Fund program and non-Fund states in only two of the nine models studied. At the outset, I noted that because states select when to enter Fund programs, and that because the reasons why they enter Fund programs may have independent effects on catalytic flows, that selection bias may be a threat to inference. The extent to which a selection effect is operating in the data is a function of exactly how we specify the

argument. In most of the cases here, it was not a threat to inference, and it had the more effects over the short-term rather than over the long term. This suggests that future progress in evaluating the effects of Fund programs will come from more precisely identifying under what conditions variables (both observed and unobserved) affect outcomes.

Similarly, we see a need to blend theory and empirical testing in our analysis of the effects of Fund program compliance on flows. Ul-Haque and Khan (1999) argue that many studies of Fund program effects are biased because the degree of implementation is often an important omitted variable. This begs a prior question though of exactly what those effects are. In our model, we suggested that the initial signal provided by a state signing a Fund program was more influential in the investor's decision than information about program compliance.

Finally, this paper reflects the larger argument that we advance in this project. The Fund makes its decisions regarding lending and enforcement in a low-information equilibrium, and this lack of information has serious consequences for its effectiveness. Because the Fund does not use conditionality to separate committed reformers from less committed ones, markets do not respond positively to the announcement of a Fund program. Developing the means to better understand the forces impelling states to select its programs provides a means for the Fund to be more informative internationally, and thus more influential. However, the Fund's own uncertainty has remained in place for years (thus the notion of an equilibrium) and this stems from its basic tension between lending and policy reform. Future progress here is possible, but this requires the IMF to

make hard decisions about unanswered questions that lay at the heart of its original design.

## Chapter Six: Domestic Institutions and IMF Agreements

One of the core issues in the study of international cooperation concerns the conditions under which states turn to international institutions. We know what it is that international institutions do, but the question of the conditions under which states use them remains unanswered. In the case of the IMF, the study of the conditions under which states turn to it has largely been conducted by economists. Their findings suggest that states request assistance from the Fund when they face balance of payments crises (McDonald 1986; Joyce 1992; Conway 1994; Bird 1995; Knight and Santaella 1997).

Political scientists, however, should not find this answer satisfactory, since some of the variables we care about, namely institutional variables used to explain why governments do what they do, are omitted from the analysis. The problem, however, is not merely that "our" variables are omitted ones—it is that we suspect the findings that result from this omission are not very meaningful as a result. After all, if a state adopts loose monetary and fiscal policies, and these policies lead the state to request assistance from the IMF, the ultimate sources of these policies lay in the decisions of policymakers. If we want to better understand the conditions under which governments turn to the Fund, we have to take politics seriously.

Studying the effects of political institutions on the 'demand' for IMF agreements provides a means for us to answer a number of broader questions. The idea that the Fund serves to "tip the balance" is perennially made (Fischer 1997; James 1999) but is rarely tested. Presently, the Fund operates as if the decision to enter its agreements has no institutional antecedents. This omission in empirical studies is particularly glaring, since

the answer to the question of how domestic institutions shape the demand for Fund programs tells us a great deal about the IMF's efficiency. That is, does it genuinely act to tip the balance? If the answer to the question is yes, then this suggests that information-based critiques of Fund conditionality, such as the one advanced in this project, have little merit. On the other hand, if when we control for relevant economic factors we see institutional differences across states in whether their programs are approved by the Fund, this tells us that the Fund may better tip the balance by acquiring better information about the political constraints that its borrowers face. In other words, the Fund's lack of knowledge limits its ability to design credible adjustment programs.

Recent critiques of the Fund suggest that it needs to become more selective to be more effective (Killick 1995, 1998). It is rare, however, for authors making selectivity arguments to actually suggest criteria that the Fund should use to make its decisions. Focusing on the politics of entering IMF agreements helps to answer this question. If we see that specific institutional mixes produce a higher demand for Fund programs, this has implications for how conditionality should be designed, and what the Fund should ultimately be selective about.

The importance of domestic institutions enters the picture at yet another level. If we are also concerned with understanding compliance with the agreement, then a knowledge of the contents of the proverbial "black box" of the state is also essential. There have been, to date, few research efforts on the part of the international financial institutions to understand the role that political institutions play in structuring the incentives of politicians and affecting the robustness of commitments. Some notable progress has come from the World Bank (Johnson and Wasty 1993; World Bank 1995,



1997). The Fund, by contrast, has been notably silent; best exemplified by their repeated invocations of undefined terms such as “ownership” and “political will.” This project aims to fill this gap, but in order to better understand compliance, we must first understand the politics of forming agreements with the IMF. Thus, this research question is important in its own right, but also because of its broader ramifications.

In this chapter, I develop an argument for why and how domestic politics affects the decision to select agreements with the IMF. The demand for Fund assistance grows out of domestic collective action problems. An inability to implement reform at home causes politicians to look for an external guarantor. IMF conditionality is intended to help solve this problem. I develop a model that sheds some insight on the determinants of this decision. The model suggests that the decision to enter agreements is determined in part by a politician’s assessment of the negotiating costs. These costs have both an international and domestic component. The international component of the negotiating costs is determined by the extent of links between the borrower and the US. The domestic component is determined by regime type—whether a state is an established democracy or not. Established democracies are expected to feel the political costs of austerity more directly than nondemocracies, and this effect reduces the incentives to enter a Fund agreement to solve collective action problems. Thus, we see evidence that an increasing degree of legislative fractionalization creates incentives for states to enter Fund programs, but only if the state is not an established democracy. These results hold in the presence of numerous controls, and they suggest more broadly that the Fund does not uniformly design agreements to ‘tip the balance’ in light of the domestic constraints that borrowers face. Thus, the Fund’s inattention to the politics of adjustment produces inefficiency in its

lending operations, as multiparty democracies—states for whom Fund conditionality would be appropriate—are not more likely to enter its agreements. On the hand, multiparty nondemocracies, states with lower costs of Fund-backed adjustment, are more likely to enter Fund programs. I conclude with a discussion of the results and implications for further research. I start first by addressing the larger issues raised by this question.

### **The Problem and its Importance**

The link between domestic politics and international outcomes is the subject of an expansive literature. While the broader social science issue raised by these works is "how institutions matter," there are different means to answer this question, and these bring differing consequences for how we think about institutional effects. We can think about how to ascertain the effects of institutions both in short term and long term contexts. Do institutions affect the *decisions* that politicians make, or rather do they affect the *demand* for making these decisions over time? Certainly the strongest possible case for institutional effects would seek to uncover their effects in both senses.<sup>69</sup> If institutions shape choice, then we would expect short run effects. If they are "sticky," this implies long run effects (i.e. effects over time) as well. The pages that follow attempt to answer both of these issues.

A natural starting point to understand the politics of IMF agreements is the existing economic research which suggests the variables that lead to the adoption of Fund programs are those associated with balance of payments crises: high debt, low growth,

---

<sup>69</sup> Most previous studies of selection into Fund agreements miss this, and conflate what determines the duration of the program spell with what determines whether states enter the agreement in the first place (Knight and Santaella 1997).

and low levels of reserves. While this work is important, it does little to tell us why it is that these balance of payments problems emerge, much less why it is that these problems are so difficult to resolve. In order to understand these deeper dynamics, we have to turn to the literature on the politics of economic policy reform, which will serve as a springboard for building hypotheses on the links between domestic institutions and international agreements. The pages that follow develop this argument sequentially, by focusing on the demand for Fund arrangements and the supply of Fund arrangements. Each is integrated into a game-theoretic model, which is used to derive hypotheses that are tested in the pages that follow.

### **Why Demand Fund Agreements?**

Conventional wisdom suggests that reforms are adopted in large measure in response to crises. The qualification is an important one, as many note that economic crisis only is a necessary condition for reform, not a sufficient one (Williamson 1994). Economic reform is a political choice: state leaders *decide* to adopt austerity measures and change the value of the currency in order to address the state's macroeconomic ailments. A strict formulation of equating crisis with reform implies that all politicians respond simultaneously and with the same mix of policies to an economic shock; a line of argument that has been soundly rejected by the political economy literature from Katzenstein (1978) to the present. One reason governments do not choose to adopt policies at the same time is because reform creates winners and losers. The desire to avoid these distributional consequences prevents governments from adopting liberalizing policies (Rodrik 1998).

The problem of reform is more complex than the mere avoidance of costs. A desire to minimize distributional costs can prevent even optimal policies from being implemented. Alesina and Drazen (1991) make such an argument to explain delays in stabilization. In hyperinflation countries, political actors know that it is necessary to end the inflation for the sake of all parties concerned. Yet despite this realization, they take no action, in the hopes that their political opponents will take the initiative and in so doing bear the political costs. In their model, the probability that stabilization will be implemented is inversely related to the degree of polarization in society. As societies are more divided (or as the degree of spatial distance between political actors increases), the relative burden of stabilization will be shared unequally between groups, which means that groups defer stabilization in the hopes that their adversaries will concede. Thus, as stabilization is delayed, the situation must worsen in order to make bearing the costs of more palatable. Thus the decision to implement reform takes on a war of attrition.

One of the lessons of Alesina and Drazen's work is that the problem of reform is essentially one of supplying a public good. Though all may benefit in the long run from adopting reform, in the short-term it requires actors to make decisions that are suboptimal. Thus, by framing reform as a public good, we can then focus on how institutions shape whether and to what extent it is provided. This approach is promising as it tells us a great deal about the possibility of initiating reform. Moreover, institutional settings that allow for politicians to adopt and sustain reform are those in which the Fund's external leverage is less necessary. These states are less likely to need the Fund's conditionality, and are thus less likely to enter agreements with it.

To better think about institutional effects, consider the following game (from Drazen 2000).

<b>Table 6-1: A Public Goods Game</b>		
	<i>Cooperate</i>	<i>Defect</i>
<i>Cooperate</i>	1,1	1,2
<i>Defect</i>	2,1	0,0

In this model, both *Cooperate, Defect* and *Defect, Cooperate* are Nash equilibria.<sup>70</sup> In both cases, the good is underprovided, as each actor prefers to rely on the efforts of the other. What Olson (1965) added to the collective action literature was a focus on group size; large groups fail to provide the public good, while small groups can. Schelling (1978) developed an important amendment to Olson's argument, by framing public goods provision as a continuous level variable rather than a dichotomous choice. Public goods can therefore be "lumpy" in that the cooperation of a minimum level of contributors is necessary to provide it (Ward and Taylor 1982). Schelling considers a situation of  $N$  identical individuals, where subgroups can form (comprised of ' $K$ ' members) that can provide the public good. For Schelling, the crucial factor for goods provision is not the overall size of the group, but rather the ratio of this critical subgroup size  $K$  to  $N$ .

More recent applications of this argument focus on the differential size of actors. Snidal (1985) argues that coalitions of states can provide public goods internationally, even following hegemonic decline. Similarly, in legislatures, not all politicians are

---

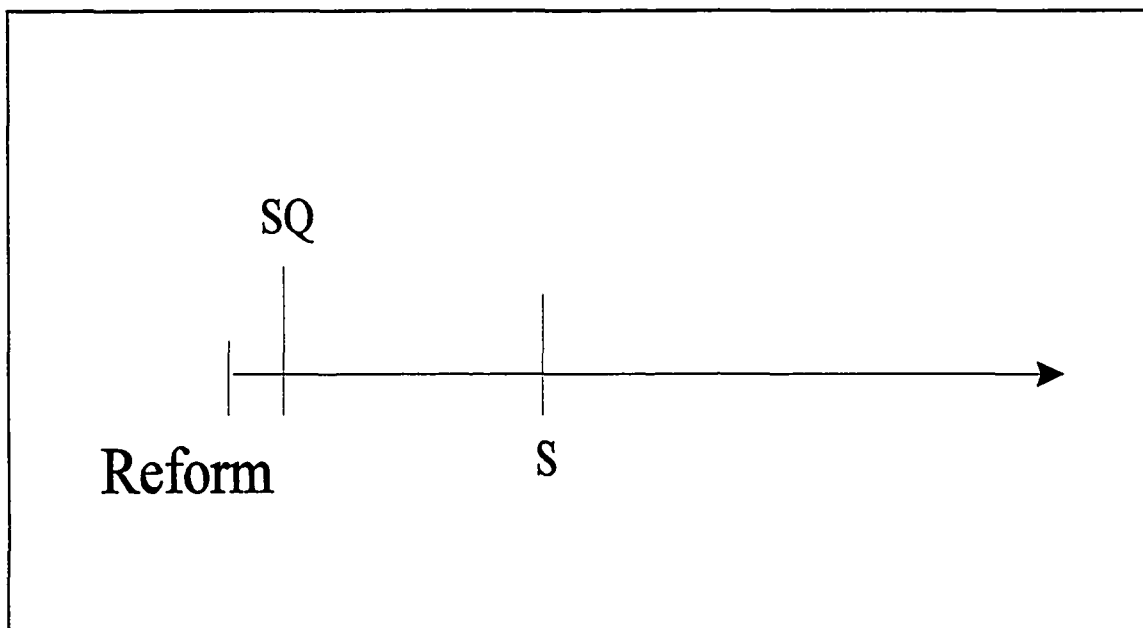
<sup>70</sup> Equilibria are the best responses of each player to the strategies adopted by the other player.

necessarily equal. They organize into political parties of varying size and capability. Just as coalitions of large states may combine to produce public goods (Lake 1988; Snidal 1985) coalitions of political parties may combine to pass a reform package. However, the ability to form a K group coalition is critically dependent on the relative size of the actors. It is easier to bring two large parties together in a coalition than a dozen small ones, since the transaction costs increase dramatically as the number of members of a K-group increases.

This argument suggests that a legislature's degree of fractionalization affects the likelihood that a pro-reform K-group can be established.<sup>71</sup> Pro-reform K-groups are easily formed with a few number of parties, but as the degree of fractionalization increases, bargaining costs increase and coalitions are not formed. States with highly fractionalized legislatures are exactly those more likely to seek external support to generate reform, and thus more likely to enter Fund agreements.

---

<sup>71</sup> Fractionalization refers to the probability that any two legislators picked randomly will be from different parties.



**Figure 7**

We can think about this using a simple spatial model shown above. The line represents the level of reform in a country preferred by each individual actor. As the degree of fractionalization in the legislature increases, the probability that its ideal point is at or to the left of the status quo (labeled SQ) increases. Leaders whose ideal points are to the right of SQ (such as at point S) have a corresponding desire to seek international assistance to provide added benefits to leverage reform. As the distance between the legislature and leaders ideal points increases, the demand to use the Fund correspondingly increases as well.

### **Why Supply IMF Agreements?**

The above result, however, is only part of the story. While it may be the case that domestic level institutional incapacities leads to an increased demand for international-

level assistance, this does not tell us whether such an agreement will be accepted. In order to understand outcomes, we also have to think about the role of the IMF. The notion that the Fund ‘tips the balance’ and provides resources allowing governments to overcome anti-reform elements is a common one. However, this argument can be made in different forms, which has different implications for how we think about the Fund’s efficiency.

We can distinguish between a ‘naive’ and a ‘sophisticated’ form of the tipping argument. In the first, the Fund merely responds to country requests for letters of intent and grants them without consideration for the domestic setting. Thus, in this argument, domestic constraints matter, as countries with pro-reform executives that face anti reform legislatures are those that enter and receive agreements. However, we can also suggest a richer argument, in which the Fund negotiates with foresight and endogenously designs conditionality so as to secure the support of both pro-reform executives and legislatures that are less committed to reform. This argument implies that an informed Fund negotiates proactively with respect to a borrower’s domestic constraints.<sup>72</sup> In other words, the Fund makes conditionality intentionally weaker in these countries not only to reduce the domestic costs on the leader to secure his approval, but also to lower the risk of involuntary defection later on. In this formulation, the leader successfully uses his domestic constraints to extract concessions from the Fund. Moreover, the Fund, observing these constraints, lessens the burden of conditionality to secure the acquiescence of the anti-reform legislature.

---

<sup>72</sup> In the Putnam (1988) sense, the Fund offers agreement within the level one winset in the naive form, and within the winsets of both the negotiator and the ratifier in the sophisticated form.



The distinction between how we frame the tipping argument is subtle but important, because it carries with it implications for the Fund's preferences and how it uses information. In the naive tipping argument, the Fund is uninformed about domestic opposition. In the sophisticated form, the Fund devises policy so as to enhance the likelihood of the legislature approving it. Of course, the argument that has the most empirical support brings with it important implications for how we think about the Fund's level of influence over the implementation of the agreement that follows. The answer to this question thus tells us a great deal about the Fund's ability to secure compliance. The distinction between these two arguments is shown in the table below.

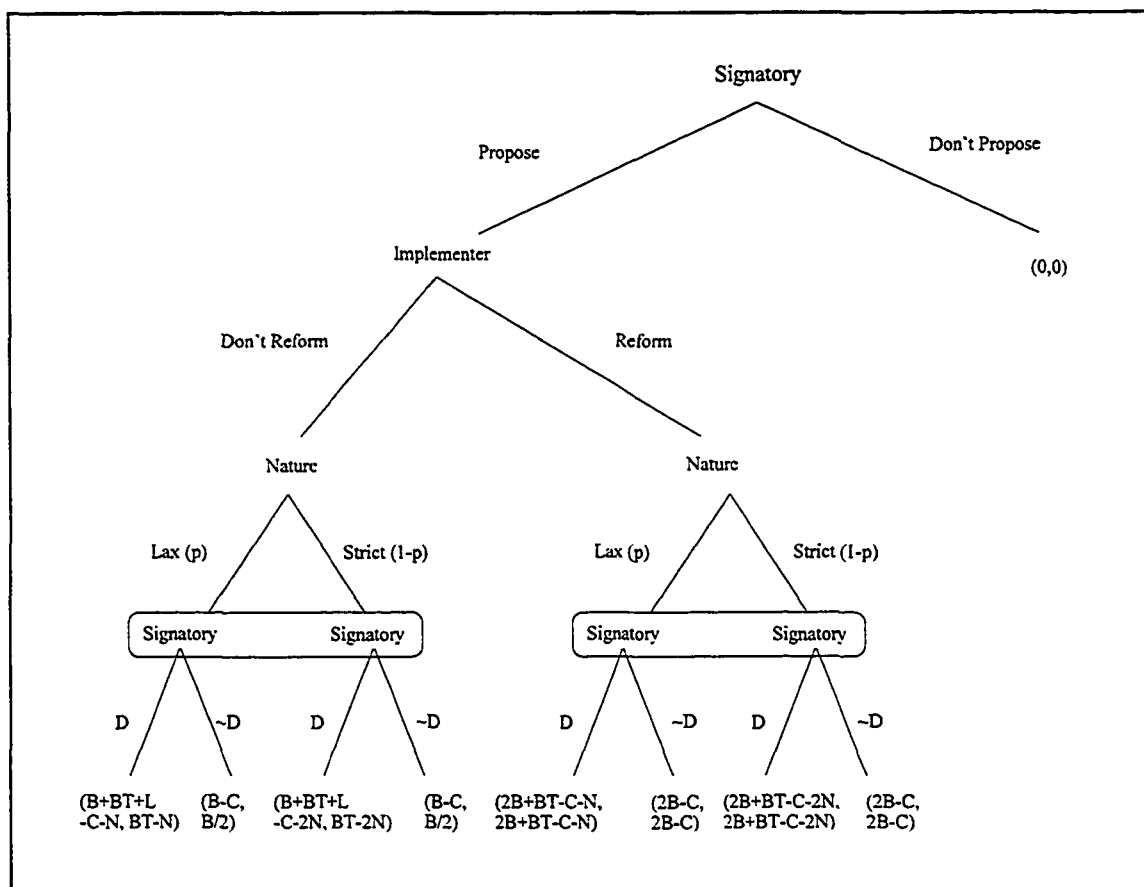
<b>Table 6-2: Potential Forms of Tipping Argument</b>			
Perspective	Argument	Fund use of information	Implications for Fund influence
"Naive" Tipping	Domestic constraints affect probability of entering Fund program	Limited	Moderate
"Sophisticated" Tipping	Fund contracts differentially and shapes conditionality to states with differing domestic constraints	Substantial	Substantial

If the argument that we have developed throughout this project has merit, we will see little evidence to support the sophisticated tipping argument. Again, this claim implies more about the informational capacities of the IMF than evidence would have us suggest. We therefore expect that those institutional factors that make countries more likely to demand Fund agreements will not produce an increasing supply of them by the IMF.

One way to think about whether an IMF agreement will be chosen is to model the decision process game theoretically. We build on the above public goods model in

several ways. First, we employ a set of variables to represent payoffs, which will be explained below. Second, we add the decision to seek an agreement from the Fund as an explicit move in the game. Finally, we represent the Fund as 'Nature.' In doing this, we do not give the Fund an explicit move, but rather we cast it a constraint. In this model, the Fund can be either lax or tough, which has consequences for the decision to enter agreements with it as well as the previous decisions to reform or not. This approach helps us to clarify the conditions under which domestic and international settings combine to produce a Fund agreement.

The game in the above figure is played by two domestic actors; a Signatory, who proposes reform, and an Implementer, who adopts it. The Signatory's move is to propose reform or not, and then the Implementer has to agree to adopt the reforms or not. When we describe 'reform' as a move in this game, we mean that the actors choose to adopt policy changes to solve the balance of payments problem (such as cutting the budget or changing the value of the currency). Following the Implementer's choice, the Signatory makes the decision to enter a Fund agreement or not (noted as D and  $\sim$ D). Thus, reform at home and entering a Fund agreement can be policy substitutes, as the Signatory may use Fund conditionality to push reforms through over an intransigent Implementer.



**Figure 8**

The decision to enter a Fund agreement is under made under uncertainty. While the Signatory has perfect recall over what has happened in the game earlier, but it does not know for certain whether the Fund will be lax or strict with it. We assume that the Signatory only forms this belief in the process of negotiation.<sup>73</sup> Moreover, since Fund agreements are not ratified, the Implementer's information does not matter for the outcome.

Actors that choose reform accept both its benefits (labeled B) and its costs (labeled C). Actors that free ride by choosing not to reform receive a partial benefit (noted

---

<sup>73</sup> Again, this is a simplifying assumption, since in suggesting that only the negotiating process matters we do not focus on the prior history of dealing with the Fund, if any.

as  $B/2$ ) and do not pay the cost term  $C$ . The Fund comes in as an actor that gives both benefits through the aid tranche (noted as  $BT$ ) and leverage through conditionality (noted as  $L$ ). Yet the Fund's assistance comes neither freely nor unconditionally. It imposes negotiating costs (noted as  $N$ ) on both the Signatory and the Implementer that increase if the Fund is strict. Thus, actors pay  $N$  when the Fund is weak, and  $2N$  when the Fund is tough. Moreover, if the Implementer chooses Reform, the Fund does not offer provide the leverage term  $L$ . Failure to propose by the Signatory means that both the Signatory and the Implementer receive a payoff of zero.<sup>74</sup> All the variables ( $B$ ,  $C$ ,  $BT$ ,  $L$ , and  $N$ ) are positive and greater than zero.

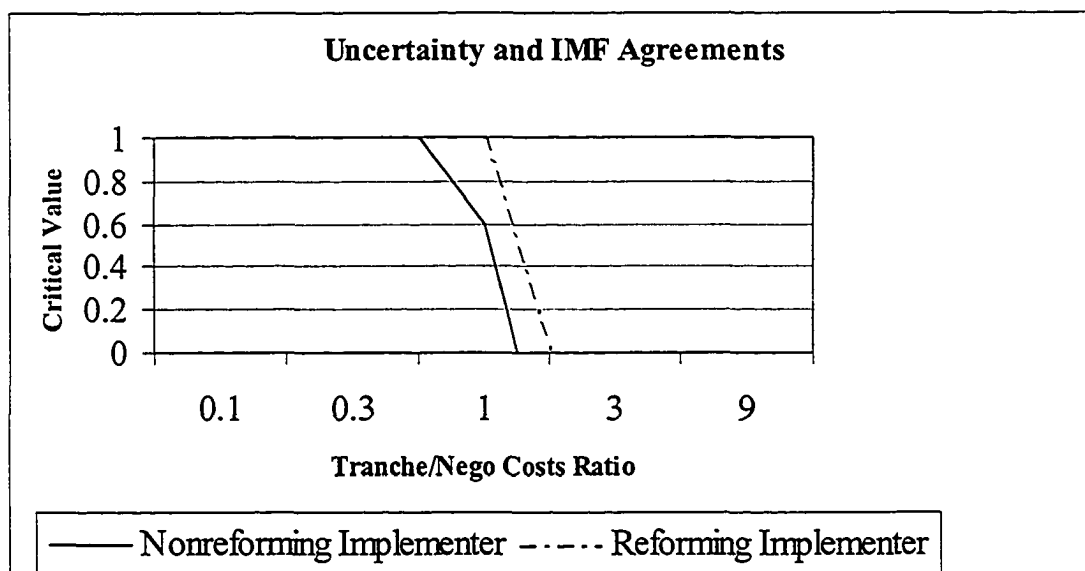
In this model, what determines the decision of the Signatory to delegate? The Signatory does not know whether the Fund is lax or tough, but it does have a probability distribution over these possibilities, noted as  $p$ , which represents its belief. To understand how uncertainty affects the decision to delegate, we can solve for  $p$  on both the left and right sides. On the right side (if the Implementer chooses Reform), the Signatory chooses to delegate if  $p > (2N-BT)/N$ , and on the left side (if the Implementer chooses Don't Reform) the Signatory delegates if  $p > (2N-BT-L)/N$ .<sup>75</sup> We can compare these functions to show how changes in the ratio of the tranche benefits to negotiating costs affect the probability of entering the agreement for different situations. This is shown in Figure 9 below.

---

<sup>74</sup> We could change this such that actors face increasing costs for inaction, along the lines of the model in Chapter 3, though this does not change the results.

<sup>75</sup> These inequalities were obtained by solving for  $p$  on both sides.

Focusing on the right most line (the dashed line) first, which represents the Signatory's decision if it faces a Reforming Implementer, we observe that as the ratio of



**Figure 9**

the value of the tranche to the negotiating costs increases, agreements become more likely. Agreements cannot form unless this ratio is greater than one, for no belief about the Fund's type can support it. Thus, as the Signatory places more value on the benefits of the tranche, and lessens his assessment of the negotiating costs, the probability of an agreement increases. However, if the Implementer chooses Don't Reform (the solid line or left most line), then the Signatory can use the leverage from delegating to the Fund. This has the effect of reducing its threshold for delegating. Thus, the Signatory is more

likely to choose to delegate if the Implementer chooses Don't Reform.<sup>76</sup> Of course, this is a common justification for conditionality.

If this helps to characterize the factors that drive the Signatory's decision, then what about that of the Implementer? The Implementer's decision depends on what the Signatory does. If the Signatory chooses to Delegate, the Implementer's best response is to play Reform so long as the benefits of reform are greater than 1/2 its costs. If the Signatory chooses Not Delegate, the best response for the Implementer is to choose reform if its benefits are greater than 2/3 of its costs. The difference between these two inequalities is one of the conventional justifications for conditionality. By providing resources, the Fund can help to bolster reform by reducing the costs of committing for the Implementer.

### **From Models to Hypotheses**

The above model is only a starting point. While it can tell us a great deal about how choices shape outcomes, developing testable hypotheses requires that we consider the institutional contexts that shape the values for the variables that we saw at work. To develop hypotheses, we need to think about what affects the values of L, BT, and N. Following from our above discussion of reform as a public good, we would expect that L would be high if the legislature is highly fractionalized, since this means that establishing a pro-reform coalition would be very difficult. If the argument we developed above holds,

---

<sup>76</sup> Another way to think about the effects of these variables is to take the partial derivatives. Holding the other variables constant, as the benefits of the Fund's aid tranche increase, or as the Signatory's value for the leverage term increases, the probability of an agreement increases. On the other hand, an increase in the negotiating costs lowers the probability of an agreement.

then statesmen facing legislatures with a high level of fractionalization are those that are more likely to seek Fund assistance. We would expect that BT would be high for high levels of debt and low levels of reserves, since the Fund's aid tranche is intended to alleviate balance of payments disequilibria.

The N term, which captures our estimate of negotiating costs, is comprised of both international and domestic factors. First, we would expect that states with substantial influence with the US have lower negotiating costs, since they can use their great power patron to obtain more beneficial terms from the Fund. Second, we would expect that negotiating costs are shaped by regime type, and are higher in democracies than in nondemocracies for two reasons. The term refers to the costs of negotiating a Fund-backed program, which is a specific solution to the state's economic crisis that entails the imposition of austerity measures. Politicians in established democracies have to be sensitive to the effects of the program on their constituents, and thus have to balance the need to solve the collective action problem of reform provision with the solution proposed by the IMF. Thus, Fund leverage comes at a higher price in democratic states because austerity means that politicians feel the effects of the program from their constituents. Thus, as before when we discussed Alesina and Drazen, politicians in democratic regimes are more sensitive (through electoral accountability) to the distributional consequences of Fund programs. For this reason, their negotiating costs are greater than in regimes where such accountability is limited.

Similarly, we can also think about why the negotiating costs are higher by thinking from the perspective of the median voter (Alesina 1994). Voters lack full information about whether an incumbent is a good or bad politician, but can make

estimates of their competence. Given an economic crisis, bringing in the IMF becomes a clear admission of policy failure by the government. Politicians in these systems would be loath to send such a message, and thus prefer to muddle through with reform at home rather than use Fund conditionality.<sup>77</sup> Thus, in a ‘rational retrospective’ sense, we would also expect that democracies would encounter higher negotiating costs in dealing with the IMF.

Regardless of whether we think that distributional concerns or competence concerns are more important, the collective action argument that we want to test is a contingent one. Only in nondemocracies, where politicians are less concerned with the effects of the Fund program on their constituents (or less concerned with the median voter’s evaluation of their competence), do we expect to see a connection between increasing fractionalization and the probability that a state enters a Fund agreement. These hypotheses are detailed in the table below.

<b>Table 6-3: Testable Hypotheses from the Model</b>
1) Probability of agreement directly related to degree of legislative fractionalization, but only in nondemocracies.
2) Probability of agreement directly related to scope of economic crisis.
3) Probability of agreement directly related to degree of state influence with the US.

Following the logic of the argument developed above, we expect our institutional argument to have both short-term and long-term implications. Nondemocracies with fractionalized legislatures are more likely to enter Fund agreements in the event of a

---

<sup>77</sup> Others suggest that authoritarian leaders are reform-adverse because austerity may incite protests that lead to their downfall. If this is correct, then we will see very different results in the data.



crisis. Moreover, since institutions are sticky over the long run, we would also expect that these states are more likely to be under Fund agreements, since the demand for external discipline is correspondingly higher. Thus, a proper test of our argument necessitates looking at the effects of institutions both in the short term and the long term.

We gain added value from specifying these hypotheses in this fashion. If it is the case that the collective action argument holds only under certain conditions, then this suggests an informational inefficiency in IMF operations. Because multiparty democracies are neither more likely to enter or be under Fund agreements, because they face higher costs for entering Fund programs, this suggests that the Fund is not optimally designing conditionality to aid all marginal reformers. In other words, some states that need Fund leverage are not receiving it, because the costs associated with conditionality are too high. Finding support for this conditional argument tells us that future Fund operations can be more effective by better understanding the political constraints that borrowers are under and designing more credible adjustment programs. Thus, the empirical tests of this hypothesis tell us a great deal about the effects of domestic institutions, but also about the effectiveness of the IMF as an institution intended to aid reformers. Below we discuss the research design, and then move on to assess the findings. The paper closes with a set of implications for future research.

### **Research Design**

In order to investigate these hypotheses, I have assembled a large dataset detailing the interaction of countries with the IMF. This dataset contains information on 367 high conditionality programs signed by 106 states between 1979 and 1995. These agreements

are Stand-By and Extended Fund Facility accords, which range between 12-18 months (Stand-By) and 24-36 months (Extended). Both programs are designed to address problems of balance of payments support, and in both cases conditionality is designed to solve the problem through fiscal and monetary restraint.

Operationalizations proceed as follows. The reader should refer back to Chapter 4 for more details here. We rely on two difference measures of the dependent variable (IMF program). To test short term institutional effects, our variable takes on a value of one for the first year of the program and zero otherwise. The 'long run' test of institutional effects dummy takes on a value of one for all program years. The use of both tests allows us to better discern what determines forming an agreement at which times.

The independent variables are measured as follows. The economic variables are all standard in studies that attempt to explain the decision to enter Fund agreements: debt service, reserves, and growth. Measures of legislative fractionalization were obtained through the Database of Political Institutions (Beck et al 2000). Given that many studies have suggested a link between great power influence and the decision to enter a Fund program (Thacker 1999), we also include an additional variable to test for this alternative explanation. Thus, I included a measure of preference similarity with the US through using the Signorino and Ritter (1999) S measure of voting records in the UN General Assembly. Similarly, I added a measure of a state's annual development assistance from the United States, since the foreign aid literature suggests that the US gives foreign aid for strategic reasons (Schraeder, Hook, and Taylor 1997). We expect that states with foreign policy positions close to the US are more likely to be offered loans. Lastly, we add a pair of policy variables (lagged budget deficit and lagged growth rate of net

domestic credit) intending to serve as a robustness check on our results. As a final robustness check, we include a state's quota in the IMF, to ensure that US influence is shaping Fund decisions and not specific attributes of the borrowing state.

One last note on the domestic politics hypothesis is in order. Because we want to test a contingent argument, namely that fractionalization "matters" only if the state is not an established democracy, this necessitates an interactive hypothesis.<sup>78</sup> Thus, in the regression tables below, we include a dummy variable, taken from Polity III, for whether the country is an established democracy, as well as an interactive term.

A conceptual objection should be addressed here. The notion that we want to look at legislatures in nondemocratic regimes may strike some as controversial. On its face, we might expect that these institutions would be pure rubber stamps that have no real policy influence. We may deal with this objection in two ways. First, the existing case study literature on the politics of economic reform suggests that legislatures do matter for reform even in nondemocratic contexts.<sup>79</sup> Second, we can look at the data. Those countries that have legislatures tend to be semi-democratic, so the potential danger here is minimal.<sup>80</sup>

---

<sup>78</sup> The alternative would be to run the test on restricted samples. The results throughout this chapter for this procedure are unchanged, but the advantage to a full model is two fold. First, we gain from using more observations, which increases our confidence in the estimates. Second, in using one sample we avoid problems of comparison across samples.

<sup>79</sup> Holt and Roe 1993 discuss Egypt, Bates and Collier 1993 make reference to Nigeria, and Lal and Maxfield 1993 mention authoritarian Brazil. I return to this issue below.

<sup>80</sup> Examples of nondemocracies with legislatures include Albania, Georgia and Armenia (after 1994), Central African Republic, the Congo, and Guatemala. States such as USSR are coded as missing for fractionalization.

As for techniques, our tests require a probit estimation with appropriate corrections for a cross-sectional time-series data panel. This specification captures the argument in Figure 9. In a probit model, we do not observe the latent variable, which in this case is a state's desire to enter Fund programs but we do observe an outcome, which is whether the letter of intent is approved. Figure 9 suggests that states with non-reforming legislatures have a greater 'desire to participate' and are thus above the probit threshold. Thus, following Beck, Katz, and Tucker (1997), our estimation includes robust standard errors to control for heteroskedasticity and a set of cubic splines to control for autocorrelation.

Our first model is the short term test, where the dependent variable is the first program year. These results appear in the table below.

Table 6-4: Sources of IMF Programs, First Program Years		
	Model One	Model Two
Debt <sub>t-1</sub>	.00816* (.00374)	.01305*** (.00379)
Reserves <sub>t-1</sub>	-.1587*** (.0336)	-.1656*** (.0428)
Growth <sub>t-1</sub>	-.02278* (.0101)	-.02772* (.01337)
Fractionalization	.3697* (.1976)	.3662 [.10] (.2241)
Democracy	.4223 (.2573)	.4129 (.4115)
Fractionalization * Democ	-.4786 (.4254)	-.6077 (.6268)
Similarity <sub>t-1</sub>	.6214** (.2279)	.4388 [.10] (.2438)
US Development Assistance <sub>t-1</sub>	-.8756 (3.102)	-6.744 [.10] (3.965)
Fund Quota <sub>t-1</sub>	-.000057 (.000043)	-.000079 (.000062)
Budget Deficit <sub>t-1</sub>		.01085 (.1219)
Growth of Domestic Credit <sub>t-1</sub>		.01338* (.00667)
Constant	-.3555 (.1945)	-.45515* (.2346)
N	802 (92 states)	590 (72 states)
% Correctly Classified	75.69%	77.12%
$\chi^2$ for Cubic Spline Segments	0.0023	0.0007
Model $\chi^2$ : 0.0000 (for both)		* p < .05
Robust standard errors in parentheses.		** p < .01
		*** p < .001

What does this table tell us? First, these findings support the existing economic studies that suggest that high debt and low reserves and low growth are the factors that drive states to the Fund. Second, we see mixed results on the proposition that great power influence may affect Fund lending, as states with policy positions close to the US are more likely to enter Fund programs, but states that receive increasing levels of US development assistance are less likely to enter Fund programs. Third, Model Two suggests that LDC policy choices matter, as states with a high growth rate of net domestic credit seem more likely to receive IMF agreements. Fourth, we find strong support for the argument advanced above, namely that legislative fractionalization is an additional factor that impels politicians to choose IMF programs. It should be noted however, because we are interactively testing democracy and fractionalization, that the coefficient should be interpreted for fractionalization is actually the effects of fractionalization if the state is not an established democracy.

We see that this coefficient is positive and significant, although less so in Model Two. This represents a more demanding test, because we are attempting to assess the effects of institutions whilst simultaneously controlling for the policy outcomes generated by those institutions. We need now to ensure that it is indeed the case that fractionalization is positively correlated to the probability of forming agreements only in nondemocracies. To do this, we need to assess the effects of fractionalization on agreement formation in democratic regimes. This is a joint test of two of the coefficients in the above table, and these results appear below.

<b>Table 6-5: Interactive Tests, First Program Years</b>	
Model One	
Fractionalization + Democracy * Fractionalization	-.10898 (.3989)
Model Two	
Fractionalization + Democracy * Fractionalization	-.24149 (.61064)

These non-results suggest that increases in legislative fractionalization in democracies do not increase the probability that a state receives an agreement from the Fund. Indeed, these coefficients are negative, suggesting a deterrent effect exists, though it is not significant. Thus, in established democracies, the presence of multiparty systems does not lead politicians to enter Fund programs to tip the balance.

A sensible question raised by these results is how much these factors matter. In order to facilitate some comparisons, we can generate predicted probabilities and then see how changes in the values of the independent variables affect the probability of forming an agreement. To get a better sense of these substantive changes, we employ a standard mathematical transformation of the fractionalization score by turning it into a measure of the “number of effective parties” (Laakso and Taagepera 1979). This has a much more meaningful interpretation than a raw fractionalization score, and it should be noted that this variable was also significant in these regressions, though not listed in the table above. These predicted probabilities, which are derived from Model Two above, are shown in the table below.

<b>Table 6-6: Predicted Probability of Fund Agreement, First Program Years</b>	
Baseline probability of entering Fund program	17.94%
Scenario	Change in Probability
Increase UN similarity from Mean to Maximum Value	Increases by 13.48% ( $p < .05$ )
<i>In Democracies:</i> Increase effective number of parties from Mean to Maximum Value	Decreases by 11.67%
<i>In Non-Democracies:</i> Increase effective number of parties from Mean to Maximum Value	Increases by 32.39% ( $p < .05$ )

The mean effective number of parties was 2.46 in this sample, and the maximum was 14.87. More substantively, this means comparing the probabilities that Dominican Republic and Morocco enter Fund programs (for nondemocracies) and comparing Costa Rica and Ecuador (for democracies). Again, we note that the predicted probability estimates are only significant for nondemocracies, paralleling our findings in the above table.<sup>81</sup>

In order to better understand some comparisons across countries, the table below uses the same predicted probabilities as above. Here our focus is on the use of real world examples. For each level of fractionalization, we provide an example taken from the dataset of a country classified as a democracy or a nondemocracy, along with the corresponding probability that it will enter a Fund agreement. For these predicted probabilities, the values of all other variables are held at their means. This allows us to isolate the effects of domestic political institutions.

---

<sup>81</sup> Again, reestimating these models using a sample of all democracies or all nondemocracies does not change the results.



Table 6-7: Regime Type, Legislative Fractionalization, and Agreement Selection		
Frac	Democracy	Non-Democracy
.15	Trinidad (1987-1991) Pr = 25.2 %	Sri Lanka (1986-1989) Pr = 15.5 %
.25	Gambia (1988-1992) Pr = 24.1 %	Malaysia (1982-1986) Pr = 16.3 %
.4	Jamaica (1990-1993) Pr = 22.4 %	Egypt (1988-1990) Pr = 17.7 %
.5	Honduras (1990-1995) Pr = 21.5 %	Paraguay (1984-1989) Pr = 18.6 %
.75	Poland (1994-1995) Pr = 19.4 %	Thailand (1984-1986) Pr = 21.2 %
.8	Chile (1994-1995) Pr = 19.0 %	Romania (1993-1995) Pr = 21.8 %
.95	Ecuador (1982-1984) Pr = 18.1 %	Morocco (1978-1984) Pr = 23.5 %

Note: Frac refers to the degree of legislative fractionalization.

As before, we see that increases in the degree of legislative fractionalization in democracies (moving from Trinidad to Ecuador) reduce the probability that a state enters an agreement. On the other hand, changing this same measure in nondemocratic regimes (contrasting Sri Lanka and Morocco) increases the probability that a state enters a Fund program. As before, the probability estimates for democratic regimes are not significant. Thus, states with increasing domestic constraints (and a higher concomitant demand for Fund programs) are not seeing their needs met with an increasing supply of Fund programs.

A brief review of some of the histories of these countries will help make the larger point clearer. In Ecuador, (classified as a democracy with a very fractionalized

legislature), the decision to enter a Fund program in 1983 was taken amidst a deteriorating economy and a political crisis between a president and a divided legislature. An earlier package of reforms proposed by President Hurtado in 1982 was met with protests and strikes. In the legislature, the opposition parties accused the government of mismanagement. Soon after, the government backpedaled on price increases for gasoline and also raised the minimum wage. As the economy continued to deteriorate, it requested a loan from the IMF in early 1983, which it secured later in the year. One of the key reasons for the turn to the Fund was the realization by Hurtado that he lacked the political support needed to implement the necessary policy changes at home (Grindle and Thoumi 1993: 138-140, 172). The presence of a signed Fund agreement did little to mollify his critics though, as the congressional reaction and public protests did not relent.

In Egypt (classified as a nondemocracy with a low degree of legislative fractionalization), a decade of financing budgetary imbalances through monetary expansion led to a weakening of competitiveness and a growing debt burden, prompting a need for economic adjustment. In the last months of 1986, the government adopted a broad package of reforms aimed at producing a fiscal adjustment. However, these reforms were not fully successful, as fiscal reforms stalled in parliament (Holt and Roe 1993). President Mubarak responded by replacing the Prime Minister and named an economist as his successor, with the mandate to address the economic crisis (Associated Press, November 9, 1986). As the level of foreign debt became increasingly unsustainable, the government turned to the IMF in the Spring of 1987, reaching an agreement in April of that year.

Contrasting these two cases helps to underpin our argument. While in both states, legislative intransigence was a barrier to the successful implementation of reform measures, we see no evidence that the political constraints to bringing in the IMF existed in Egypt as opposed to Ecuador. Indeed, the president used his constitutional authority to reorganize the government so as to appoint a dedicated reformer. This option was not available to Hurtado in Ecuador; despite appointing his own team, his policies were constrained by a divided legislature.

We can see this contrast further by comparing Ecuador with Romania. Both states have a high degree of legislative fractionalization, though they differ by regime type. Romania's early political transition produced a scattering of political parties in the legislature in a regime that was semi-democratic. Even the ruling party was a coalition of a dozen small groups (Economist 1993). In this setting proposing economic reforms was a dangerous enterprise, as the government of Prime Minister Vacaroiu faced four no-confidence votes between 1992 and 1994 (Economist Intelligence Unit 1994). A governmental reorganization replaced the chairman of the Council for Coordination, Strategy, and Economic Reform in September 1993, and a set of fiscal reforms that addressed the IMF's concerns, as well as the parliament's endorsement of the letter of intent in December, produced a new standby in December 1993. As in Egypt, the presence of legislative opposition need not pose a danger to whether reforms can be concluded.

These mini-case studies raise a consistent question. Why is it that the Fund does not recognize the political constraints that leaders face *ex ante*, and in so doing make it easier for states such as Ecuador to enter Fund programs? One possibility is that the Fund

does not suspect these states can honor their pledges, and thus makes conditionality comparatively more “expensive.” I return to this argument in Chapter Eight.

Regardless of the foundations of this claim, it is clear that conditionality is being systematically under-delivered to states that can certainly benefit from the leverage provided by an IMF agreement. This inefficiency is informational in nature. Lacking the ability to appropriately ascertain a borrower’s degree of commitment to adjustment, the Fund has no capacity to make these fine grained decisions regarding how its programs are perceived by domestic actors.

As a doublecheck on these results, I re-estimated these models adding three dummy variables representing whether the state was in Eastern Europe and the Former Soviet Union, Latin America, and Sub-Saharan Africa. The coefficients for these dummies were not significant, and the substantive results regarding fractionalization and similarity were unchanged by including these variables.<sup>82</sup> Even controlling for unobserved regional heterogeneity, increases in fractionalization in nondemocracies still result in a higher likelihood of entering Fund agreements.

Taken as a whole, these results support the argument developed thus far. Even after controlling for great power influence, policy choices, and potential regional effects, nondemocracies are more likely to enter Fund agreements as their degree of legislative fractionalization increases. States with a demand for leverage seek Fund agreements, and this grows out of an inability to build a pro-reform coalition in the legislature. In democracies, however, the game is different, as are the results. Politicians in democracies

---

<sup>82</sup> The use of regional dummies in this context controls for the new states of Eastern Europe and the Former Soviet Union, which tend to have highly fractionalized legislatures.

are less willing to enter these agreements, because the effects of austerity on their constituents is significant, which in turn affects their survival. Furthermore, states with close ties to the United States are more likely to receive IMF loans. As argued above, this is because politicians believe that the Fund will impose low negotiating costs on them. As the predicted probabilities suggest, both great power influence and domestic constraints 'matter,' and we find support for a domestic explanation even after controlling for variables from other levels of analysis.

We know that institutions have effects over the long run, and if it is genuinely the case that political institutions shape the demand for IMF assistance, states with fractionalized legislatures are more likely to be under Fund agreements. Testing this argument involves merely changing the dependent variable. Here the dependent variable is not the first program year, but the duration of program spell. In other words, every year a state is under a program is a "one." Turning to the long term test, we see the following results.

Table 6-8: Sources of IMF Programs, All Program Years		
	Model One	Model Two
Debt <sub>t-1</sub>	.01002** (.00404)	.01393** (.005)
Reserves <sub>t-1</sub>	-.1125*** (.0307)	-.1332*** (.0339)
Growth <sub>t-1</sub>	-.03786** (.0123)	-.0325* (.01687)
Fractionalization	.39537* (.1814)	.3893* (.1586)
Democracy	.61293 (.3852)	.513 (.4345)
Fractionalization * Democ	-.8376 (.5882)	-.8533 (.6508)
Similarity <sub>t-1</sub>	-.0913 (.2763)	.3115 (.3517)
US Development Assistance <sub>t-1</sub>	5.339 [.10] (2.851)	11.422 [.10] (6.128)
Fund Quota <sub>t-1</sub>		.0000719 (.0000554)
Budget Deficit <sub>t-1</sub>		.01443 (.0099)
Growth of Domestic Credit <sub>t-1</sub>		.00536 (.00577)
Constant	.7196*** (.1666)	.95517*** (.1955)
N	802 (92 states)	590 (72 states)
% Correctly Classified	81.80%	84.07%
$\chi^2$ for Cubic Spline Segments	0.0000	0.0000
Model $\chi^2$ : 0.0000 (for all three)		* p < .05
Robust standard errors in parentheses.		** p < .01*** p < .001

These results are very similar to the results in the test of first program years. Debt, reserves, and growth all seem to still be correlates of being under Fund programs, which is not surprising. We still see some effects of great power influence. This suggests that states that receive US development assistance are under Fund programs longer than US adversaries. It should be noted that in contrast to the first model, a different measure of political influence is significant. Of course, over the long run, US foreign aid might be a better predictor of who the US favors rather than whether states adopt similar positions in the UN General Assembly.

Our policy variables are not significant now, and this bears some discussion. Changes in a state's budget deficit or changes in its growth rate of net domestic credit do not seem to affect the duration of a state's "stay" under Fund programs. Some works have concluded from this table that the Fund doesn't really care about these policy variables. However, as we previously noted, this is a faulty inference.

We see support for the argument that fractionalization's effects are contingent on regime type here. Again, as fractionalization increases in nondemocracies, these states are under programs longer. To test the effects of fractionalization in democracies, we need the joint test as above. This appears in the table below.

<b>Table 6-9: Interactive Tests, All Program Years</b>	
Model One	
Fractionalization + Democracy * Fractionalization	-.4422 (.5736)
Model Two	
Fractionalization + Democracy * Fractionalization	-.46404 (.63967)

Our results are the same as in the previous interactive tests. Democracies with increasingly fractionalized legislatures are not more likely under Fund programs, while nondemocracies with increasingly fractionalized legislatures are. The difference in these outcomes reflects a long-term demand for external discipline brought on by the difficulty of building reform coalitions at home that is offset by a greater sensitivity in democracies to the effects of Fund austerity. The result remains the same—the Fund tips the balance only in those states where domestic and international incentives are in harmony.

We can gain a more intuitive sense of these results by examining how changes in our independent variables of interest affect the predicted probability of being under a Fund agreement. This is shown in the table below.



<b>Table 6-10: Predicted Probability Under IMF Agreement (All Program Years)</b>	
Baseline probability of being under Fund program	81.27%
Scenario	Change in Probability
Increase US development assistance from Mean to Maximum Value	Increases by 17.43% ( $p < .05$ )
<i>In Democracies:</i> Increase effective number of parties from Mean to Maximum Value	Decreases by .24%
<i>In Non-Democracies:</i> Increase effective number of parties from Mean to Maximum Value	Increases by 12.75% ( $p < .05$ )

As before, this means comparing the probabilities that Dominican Republic and Morocco are under Fund programs (for nondemocracies) and comparing Costa Rica and Ecuador (for democracies). We can generate a table parallel to Table 6-7, which provides a set of examples of countries with the probability that they are under an agreement. This is detailed below.

<b>Table 6-11: Regime Type, Legislative Fractionalization, and Agreement Persistence</b>		
<b>Frac</b>	<b>Democracy</b>	<b>Non-Democracy</b>
.15	Trinidad (1987-1991) Pr = 85.1 %	Sri Lanka (1986-1989) Pr = 77.4 %
.25	Gambia (1988-1992) Pr = 84.3 %	Malaysia (1982-1986) Pr = 78.5 %
.4	Jamaica (1990-1993) Pr = 82.7 %	Egypt (1988-1990) Pr = 80.0 %
.5	Honduras (1990-1995) Pr = 81.5 %	Paraguay (1984-1989) Pr = 81.0 %
.75	Poland (1994-1995) Pr = 78.0 %	Thailand (1984-1986) Pr = 83.2 %
.8	Chile (1994-1995) Pr = 77.2 %	Romania (1993-1995) Pr = 83.6 %
.95	Ecuador (1982-1984) Pr = 74.6 %	Morocco (1978-1984) Pr = 84.7 %

Note: Frac refers to the degree of legislative fractionalization.

Of course, this raises the important issue of why certain states are under Fund programs for less time, which raises the issues of their ability to comply with the agreement. This issues will be the focus of the subsequent chapters.

### **Extensions**

A skeptical reader may lodge several counterclaims against the argument I have developed and tested here. In this section, I will address two of these; one dealing with sensitivity and one dealing with model misspecification. One argument could be that the results stem basically from the cutoff used to denote established democracy. Though the notion that a Polity democracy score greater than six is an industry standard, does

changing it alter the results? The answer here is a negative. Changing the threshold at which we call a democracy established in either direction does not substantially alter the results.

The second countercharge is that I have problems of variable omission stemming from the fact that I overlook a popular argument on policy choice which focuses on the role of veto players. Tsebelis (1995) argues that political systems with a high number of veto players are those in which policy change is difficult. By extension, Vreeland (2001) the probability of delegating to the Fund is highest in systems with a large number of veto players.<sup>83</sup> Thus, to the existing regressions I added a measure of the number of veto players taken from the Database of Political Institutions.<sup>84</sup> Our results regarding fractionalization are unchanged, and the coefficient on the number of veto players measure is not significant. Indeed, running the regressions with only the veto players measure also generates a null finding.

Exactly why this is the case is not hard to understand. We have argued in this paper that the collective action problem of reform only leads to entering Fund programs under certain conditions. It is not the mere presence of domestic constraints, but the realization that Fund austerity is politically inexpensive that creates the decision to enter the Fund agreement. Thus, it is not surprising that a measure that overlooks these more

---

<sup>83</sup> Though our samples differ, I have been unable to replicate Vreeland's results.

<sup>84</sup> This variable is the sum of the following: the executive branch counts as one veto player, each legislative chamber counts as a veto player, multiple parties that are legal and compete in executive elections count as another, and one is added if parties in the legislature appear closer to the opposition than the executive party.

subtle institutional effects is not positively correlated with the decision to enter a Fund agreement.

Thus, even after controls with other measures we find continued support for our argument. Because the Fund only conditionally ‘tips the balance’ this tells us that information is an impediment to more efficient operations by the Fund. If the Fund had better information about the domestic constraints that its borrowers face, it could design programs systematically so as to account for the greater degree of reluctance to enter Fund programs for democracies with a high degree of legislative fractionalization. Thus, conditionality would be systematically less onerous in democratic states, especially those with high levels of legislative fractionalization. Again, this inefficiency stems from low information—the Fund does not appreciate how institutional differences in domestic constraints shape the demand for its agreements.

This point is strengthened when we compare the positions of states when they enter Fund programs in their first year. A simple comparison across polity types of reserves, debt service, current account ratio and budget deficits revealed no systematic difference between multiparty democracies and nondemocracies. These states do not appear different in terms of their initial conditions when they enter the program. Thus, the Fund seems to have little basis for not designing conditionality so as to increase the probability that multiparty democracies enter its programs.

### **Implications for Future Research**

To recap, we have argued that the Fund tips the balance only under certain conditions; while reform is a collective action problem, domestic institutions shape how

this problem will be resolved. We found that the probability of reaching a Fund agreement increases with the degree of legislative fractionalization, but only in nondemocracies. This suggests inefficiency, as the Fund does not provide sufficient benefits to multiparty democracies to entice them to use conditionality. Following our larger argument about the failure of conditionality, we suggested that this inefficiency is informational in nature. Because the Fund operates with little information about the political constraints of its client states, and does not offer side payments to reduce the costs of Fund-backed austerity, multiparty democracies are less likely to sign letters of intent.

This argument contrasts with what we know about the effects of legislatures on international cooperation. The general consensus is that democratic legislatures are a good thing for cooperation, as they reveal clearer information (Fearon 1994, Schultz 1999) and make commitments more credible (Martin 2000). Why are these results different? While these arguments have relevance for a crisis bargaining context, here these signals are not heard by the Fund. In democratic states, entering a Fund program is a genuinely difficult choice. Politicians are forced to trade off the no-program status quo for a scenario in which the effects of the Fund program on their constituents can affect their political survival. Thus, they face a tradeoff between a need to use Fund leverage and a concern for the added costs associated with that leverage. Because the Fund does not lower the burden of conditionality so as to make it profitable for politicians to use the Fund, those states that need Fund leverage to tip the balance do not receive it. It is not surprising that they are less likely to enter Fund programs and less likely to remain under them. If the Fund could better design agreements to match up with the political

constraints that borrowers face, multiparty democracies would be more likely to utilize it. Thus, while many suggest an informational rationale for why legislatures matter, these works point to a weakness of IMF programs, as the Fund does not use this added information to design workable letters of intent.

This argument has important implications for how we think about the presence of adverse selection in Fund programs. The evidence in this paper suggests that conditionality actually does screen out states, but not in the expected fashion. The Fund's claim that conditionality is a signal of credibility is based on the notion that only committed reformers use Fund programs. The sanctioning power of the IMF induces deterrence. In our analysis, states choose not to enter Fund programs due to the higher ex ante costs of austerity on their constituents rather than because of the potential for Fund sanctions. Thus, not only is it not the case that only virtuous states enter Fund programs, but the Fund underdelivers conditionality to states that could use the leverage. We saw no evidence that multiparty democracies enter Fund programs on poorer terms, which again raises concerns about the efficiency of IMF operations given that it concludes agreements under uncertainty about the borrowing state.

This work also has clear implications for the study of compliance with these arrangements. A testable implication from this paper is that compliance problems with the Fund will be most prevalent in democracies with high levels of legislative fractionalization. In these states, the higher costs of conditionality means that constituents will lobby for policy reversals, and as a result these states are more likely to be sanctioned by the Fund. Such a claim will be tested more fully in a subsequent chapter.

Turning to the broader ramifications of these findings, they represent a need to move beyond levels of analysis in evaluating the sources of policy choice and policy stability. For too long, academic debate has turned on which level one should focus on, as if IR scholars all played roulette and placed their chips on one color or another. In this project, we see that while the decision to turn to the Fund is one that is made under varying degrees of uncertainty, it fundamentally grows out of a domestic collective action problem. We noted from the predicted probabilities that while political influence from the US increased the likelihood of states receiving Fund assistance, it was also the case that the decision to enter agreements was driven by domestic institutions as well as economic conditions. The study of cooperation demands that we assess the effects of both international and domestic incentives rather than view each in isolation from the other.

More broadly, one of the lessons gained from this study is that we have to understand the role of selection effects to assess the robustness of cooperation. In more recent works in international security, this notion comes up repeatedly—particularly in studies of alliance reliability (Leeds 1999) as well as dispute escalation (Reed 2000): the sample of states that we are interested in—whether they are states in alliances or engaged in disputes or states under IMF programs—is not randomly drawn from the population of states as a whole. In order to make inferences about outcomes, we have to develop a theoretically informed understanding of the selection process. Put more forcefully in this context, if we want to draw inferences about why states honor their commitments, we need to understand how and why they are made in the first place. Since states go to the Fund when they need leverage, understanding compliance requires that how domestic

institutions affect a state's degree of commitment to austerity. By uncovering institutional determinants of Fund agreements, this paper suggests that a selection effect may exist in the analysis of compliance with Fund programs.

Finally, these findings are certainly relevant for our consideration of the Fund's operations. In principle, the Fund is "politically blind" in that it only considers whether a state has a balance of payments problem before proposing a letter of intent. It is not surprising that critics of Fund operations have called attention to its inability to distinguish borrowers by type *ex ante*. We have seen evidence that this low information equilibrium produces inefficiency, since certain states that have a demand for Fund leverage do not enter the program because the costs of adjustment are too high. Certainly if the Fund wants to focus more substantively on "good governance" which is now an emerging theme in its lending practices, then it should act to tip the balance in a more sophisticated fashion. Progress can only come on this front by taking politics substantively out of the error term. Focusing on institutions and their effects in their entirety offers one pathway for ultimately devising more credible adjustment programs.



## Chapter Seven: Understanding IMF Sanctioning Practices

IMF agreements break down frequently. Schadler et al (1995) note that in a sample of 59 adjustment programs, Fund assistance was suspended in 35 cases. These states failed to meet the terms of their letters of intent and were declared ineligible for further drawings by the IMF. Thus, they were punished by the Fund for not holding up their end of the conditionality bargain. How can we explain the variation in these outcomes? Under what conditions does the IMF punish states for noncompliance? Does the Fund treat all states equally, or is it prone to bias, and what are the sources of this bias?

In the pages that follow I develop a game-theoretic model of the enforcement process, and test a series of explanations against a sample of adjustment programs signed between 1979 and 1995. I begin with the assumption that the Fund makes and enforces agreements under uncertainty about whether the borrowing state is in fact committed to reform, and derive testable hypotheses from a model. I find that variations in sanctioning are explained with reference to both borrower behavior in implementing austerity as well as structural factors such as the degree of borrower influence. I find mixed evidence supporting a Realist argument focusing on the role of the US. These findings thus support institutional accounts to a certain extent, since the Fund does behave according to its own stated rules. However, I also find evidence that suggests that the Fund's degree of influence differs across borrowers according to the costs of enforcing the agreement. The model suggests a further dilemma for institutional theory, as it is clear that the Fund makes decisions under uncertainty about the borrower's ability to honor its agreements.

The equilibria from this model under full information help us to understand why low information conditionality is an equilibrium.

### **Broader Questions**

The issues raised by this project lie at the heart of contemporary debates between Realists and Institutionalists: how effective are international institutions? While theory tells us what types of functions international institutions perform (Keohane 1984; Axelrod and Keohane 1985), it is more difficult to assess how effective international institutions are in achieving their goals.

We can approach this effectiveness question by evaluating whether the Fund behaves impartially. Some argue that the Fund is prone to politicization and influence. Of course, the creation of the IMF following World War Two was a US-backed enterprise, as was the development of conditionality (Dell 1981; Kahler 1990:95). Observers note that one reason for the Fund's mixed record of influence is great power meddling (Finch 1989; Haggard and Kaufman 1989), and empirical studies have found a link between a state's degree of political affinity with the US and whether it receives a loan (Thacker 1999; Killick 1995:118-199). If the Fund enforces agreements capriciously, or treats some states advantageously, then this leads us to question its effectiveness.<sup>85</sup>

Not surprisingly, the Fund's public documents outline a norm of equal treatment. The March 1979 guidelines on conditionality mandates that the Managing Director will

---

<sup>85</sup> Arguments that stress the moral hazard implications of IMF lending (Kapur 1998; Feldstein 1998; Hale 1998) take a similar tack in suggesting that Fund programs have unintended consequences.

“ensure adequate coordination in the application of policies relating to the use of the Fund’s general resources with a view to maintaining the nondiscriminatory treatment of members.” (Decision No. 6056 (79/38), Point 8). Moreover, the model that the Fund uses to design adjustment programs bases the design of the letter of intent solely on a state’s economic variables. On paper, the Fund is politically blind.<sup>86</sup> In the pages below, I employ a game theoretic approach to address these arguments and help bring domestic and international factors together.

Unfortunately, few existing studies take advantage of the large sample of Fund agreements to assess the factors shaping Fund decisions through the use of quantitative techniques. Killick (1995) and Mosley (1992) both employ difference of means T-tests, which, as a univariate technique, does little to build or test theory. Drabkin (1993) and Santaella’s dissertations (1992) both employ a small sample of agreements negotiated in the early eighties. These studies suggest that variations in compliance are best explained by variations in borrower attributes and the occurrence of exogenous shocks. We should take the findings of these studies skeptically, however, because they employ estimation techniques that are ill-suited for the size of the sample in the study.<sup>87</sup>

---

<sup>86</sup> Eckhaus (1986) and Gold (1979) suggest that the Executive Board’s reliance on consensus rather than voting provides a mechanism for great power influence.

<sup>87</sup> Drabkin, for example, runs a simultaneous equation probit model using 14 independent variables on a sample size of 54. Santaella’s probit model uses a sample of 51 programs, and his empirical tests include a minimum of eight independent variables. Long (1997) recommends that one have ten observations for every independent variable when using maximum likelihood techniques.

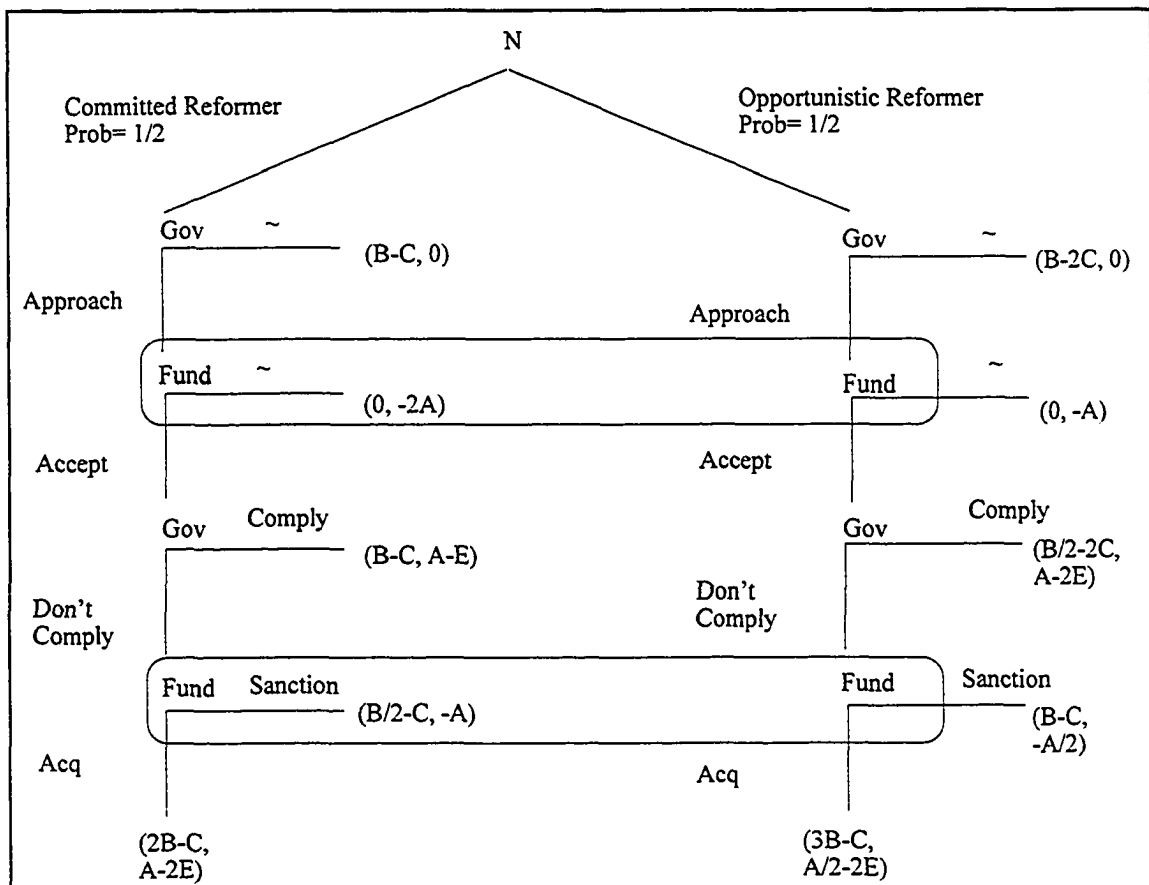
Thus, as a means to better understand the leverage that the IMF has and evaluate the efficiency of Fund assistance, the pages that follow ascertain the conditions under which the Fund sanctions states for noncompliance.

### *Argument and Implications*

Throughout this project, we have argued that one of the fundamental problems that the IMF faces is a low information equilibrium. That is, the Fund does not know when it signs an agreement whether it will be honored by a borrower. The claim that the IMF contracts under uncertainty raises two vitally important concerns. First, given this uncertainty, what drives Fund behavior regarding lending and enforcement? Second, exactly why is this perverse outcome an equilibrium? Game theory provides the means for us to address both these questions, which are the focus of the pages that follow.

In Chapter Three, we developed the argument that the Fund makes decisions to lend and enforce its agreements under uncertainty. Since Fund lending is aimed at reducing balance of payments constraints, governments that do not approach the Fund either take no action to solve the problem, which can be costly, or implement austerity without the Fund. Given this, governments should attempt Fund agreements whether they are committed reformers or not. Conditionality thus takes on the appearance of adverse selection, and governments of both “types” seek Fund assistance.

Thus, we start with the assumption that the Fund contracts under uncertainty, and then derive testable hypotheses. It is an assumption that we relax later to better understand the presence of this low information equilibrium. Our model appears in Figure 10 below.



**Figure 10**

This game involves two actors, a government and the IMF (noted as Fund). Both players choose jointly whether to enter an agreement as well as the level of performance under it. The Fund does not know whether it faces a Committed Reformer or an Opportunistic Reformer, and has to make its decision under uncertainty.<sup>88</sup> The Fund has a prior belief about what it faces, which we set to  $1/2$ . Readers will note that this streamlines the process substantially, since I essentially treat the borrower as a billiard ball. While some may find this assumption distasteful, this modeling choice accurately captures how the

<sup>88</sup> We assume that the government knows its own type ex ante. This simplifies the analysis considerably.

Fund regards its borrowing clients.<sup>89</sup> Thus, its ex ante assessment is that it faces both types of reformers with equal probability, which captures our assumption that the Fund contracts under uncertainty.

The game begins with a move by Nature, which determines whether the government is a Committed or Opportunistic Reformer. Following this, both the state and the Fund decide whether to enter an agreement. The government chooses then whether or not to comply with the letter of intent, and the Fund chooses to sanction it or not. Committed Reformers prefer to comply, though honoring the agreement is costly. Opportunistic Reformers are averse to honoring their pledges.

In this model, the Fund makes trade-offs between lending and reforms under uncertainty. The Fund pays an enforcement cost  $E$  if a state is under an agreement, and agreements have value  $A$ . Its payoffs are also shaped by the type of state it confronts. If it does not respond to country requests with a loan, it forfeits the value of the agreement,  $A$ . The losses are greater, however, if it fails to lend to a Committed Reformer. The Fund does not merely push loans without considering the borrower's type. It has greater enforcement costs for dealing with Opportunists, and acquiescing to an Opportunist's noncompliance lowers the value of the loan.

Similarly, states entering agreements receive both the benefits, noted as  $B$  and the costs, noted as  $C$ . As noted above, all reformers are not equal, which in turn affects the payoffs. If the state is Opportunistic, it places a higher weight on the costliness of reforms, and it would prefer to be sanctioned following noncompliance rather than

---

<sup>89</sup> Recall in the previous chapter we found no evidence that the Fund behaved strategically so as to tip the balance in a sophisticated fashion across the population of borrowing states. We relax this assumption in the following chapter.

comply with the agreement. Again, we build the model starting with the premise that the Fund faces tradeoffs between lending and reform.

We should be aware of the model's limits. This is a one-stage game, and we do not address how actor behavior changes over time with consecutive agreements. One way to approach this simply would be to allow the Fund's ex ante belief to change. This would have the effect of increasing the probability that the Fund is facing a specific type. Second, there is no domestic politics within the government. That is, we do not articulate what makes a reformer Committed or Opportunistic. That having been said, the acid test of a model lies in its ability to make testable hypotheses.

The game has two equilibria, which correspond to differing evaluations of the enforcement cost term.<sup>90</sup> If the enforcement costs are high, the Fund acquiesces, and both Committed and Opportunistic Reformers Don't Comply. If the enforcement costs are low, the Fund plays a mixed strategy of Sanctioning and Acquiescing. As a result, Opportunistic Reformers never comply, and Committed Reformers also mix strategies between compliance and noncompliance.

Thus, there are two testable implications of this model. First, we expect to see more noncompliant behavior in Fund programs with high enforcement costs. Second, we expect that as enforcement costs increase, the willingness of the Fund to punish goes down. This implies a test of the determinants of performance under Fund agreements, as well as understanding under what conditions the Fund sanctions states.

In order to run these tests, we have to address the potential inferential problem of selection bias. If the data that we observe emerge as the result of another underlying

---

<sup>90</sup> The results are discussed in detail in the Appendices.

process, then studies that neglect the initial process may have biased coefficients (Heckman 1979; Achen 1986).<sup>91</sup> Below, I address this problem by jointly modeling selection as well as enforcement, which helps us to better capture the real-world data generating process. Thus, below I estimate the model developed here using Heckman estimation. Moreover, I include a series of cubic spline segments designed to capture the duration dependence in the selection of Fund agreements (Beck, Katz, and Tucker 1998).

The notion that agreement formation and agreement enforcement are linked is a common one in the area of international cooperation (Leeds 1999; Fearon 1998; Downs and Rocke 1996). Some suggest that states do not sign agreements that they cannot honor (Chayes and Chayes 1993, 1995). In this model, we see a different sort of result: statesmen of all types have incentives to enter agreements, the Fund has incentives to accept agreements, and then it decides to sanction or not based on a state's attributes and its choices. In this model, both Committed and Opportunistic Reformers pool on entering agreements regardless of whether they honor them. To be clear, I am not arguing that this holds for all international agreements, and in fact, one can see evidence that statesmen are selective under certain conditions. Indeed, this was the empirical result developed in Chapter Six. Regardless of the modeling result we derive, employing a statistical model that captures the strategic interaction is appropriate (Signorino 1999; Smith 1998). In this case, we gain two added lessons from using a more complex statistical model. First, we develop a better inferential control, which is especially important for economic variables

---

<sup>91</sup> Readers should refer to the Appendix in Chapter Four for a review of these issues.



and US influence-based explanations.<sup>92</sup> Second, we can use this model to test existing alternative explanations.

## **Empirical Tests**

### *Determinants of Agreement Selection*

Modeling the decision to choose an IMF agreement is simple, as this is the subject of numerous studies (Thacker 1999, Przeworski and Vreeland 2000, Knight and Santaella 1997, Conway 1994). These studies converge on a number of important variables: high debt service, low GDP growth, and low reserves. This reflects the conventional wisdom that states seek Fund assistance when they face balance of payments disequilibria. These variables serve as a proxy for the value of the agreement (or the “A” component of the Fund’s utility.)

To these measures, we add three variables designed to capture state influence and major power proxies. These are designed to capture the enforcement cost term (or the “E” component of the Fund’s utility). First, we include a measure of the annual development assistance given by the US to a state in a given year, scaled over GNP. Second, we add a measure of foreign policy similarity derived from the degree of overlap in positions taken in the UN General Assembly. Finally, we add the state’s quota in the Fund in millions of

---

<sup>92</sup> While I have argued that domestic political leaders “pool” on entering agreements, they enter these agreements when they face economic crises. As a result, we still need to model the sample selection issue. Relative to the population, states that sign Fund programs are outliers in many categories of economic performance, and they are certainly more likely to be US allies. This was covered in Chapter Two.

SDRs.<sup>93</sup> Each of these measures are lagged a year. Putting these two different types of measures in helps us to assess what comprises the Fund's enforcement costs: are they a function of borrower influence with the Fund or are they a function of great power influence?

We measure performance under the program in two different ways. Since the goal of conditionality is to underpin policies of fiscal and monetary restraint, we focus on two outcomes: the growth of net domestic credit, and the budget deficit. These variables are chosen to exemplify performance under the program since they are often performance criteria; that is to say that they are often the yardsticks by which compliance is measured (Killick 1984; Polak 1991; IMF 1987).

#### *Determinants of Performance Under Fund Agreements*

While testing the effects of these variables on the selection of agreements serves as a nice diagnostic for potential selection problems, a test of our hypotheses requires that we turn to the study of performance under Fund agreements. Below, I test for the effects of our variables of interest on the annual growth rate of net domestic credit and the budget deficit.<sup>94</sup> The goal is to assess what factors, if any, affect a state's performance under the Fund program. The outcome variables assume importance because they are often used by the Fund to design adjustment programs and monitor compliance with them. Both net domestic credit and the budget deficit are common performance criteria

---

<sup>93</sup> The Special Drawing Right (SDR) is the currency used by the IMF. One SDR is presently equivalent to about \$1.40.

<sup>94</sup> Control variables—growth and GNP Per capita—appear in similar models in Cheibub 1998 and Edwards and Tabellini 1991.

in letters of intent (Guitain 1995; IMF 1987; Beveridge and Kelly 1980). These results are detailed below.

<b>Table 7-1: Fund Program Selection and Performance</b>		
<b>Performance Model</b>		
<b>Dependent Variable</b>	<b>Net Domestic Credit</b>	<b>Budget Deficit</b>
Dependent Variable $t-1$	.09421*** (.02434)	.65233*** (.1123)
Per Capita GNP $t-1$	.000334*** (.0001)	.0002199 (.10) (.000137)
Growth $t-1$	-.02246* (.01)	-.01095 (.0311)
Trade $t-1$	-.00714*** (.0022)	-.02227 (.01503)
US Official Development Assistance $t-1$	5.5639 (4.837)	13.428 (24.532)
Similarity $t-1$	-.9733* (.4230)	-.58762 (.9393)
Fund Quota $t-1$	.000622* (.00027)	-.00046 (.10) (.00028)
EFF Program Dummy	-.57183* (.2621)	-.9762 (.10) (.5766)
Constant	-.09513 (.2176)	-.5480 (1.295)
<b>Selection Model</b>		
Debt $t-1$	.01545*** (.00317)	.01568*** (.00324)
Reserves $t-1$	-.08142*** (.01988)	-.0763*** (.02)
Growth $t-1$	-.02895*** (.0087)	-.02599** (.0088)
Budget Deficit $t-1$	.00549 (.0068)	.000263 (.0077)
Net Domestic Credit Growth $t-1$	.006192 (.0061)	-.001258 (.0063)

US Official Development Assistance $t-1$	6.1815 (.10) (3.8293)	6.1777 (3.906)
Similarity $t-1$	.46504* (.1989)	.44655* (.2019)
Fund Quota $t-1$	.0001003* (.000052)	.0001135* (.000051)
Constant	.91769*** (.1236)	.8261*** (.1290)
Rho	-.08092	.22177
Rho Chi Sq	0.1400	0.0087
Model Chi Sq	0.0000	0.0000
Number of Observations: Net Domestic Credit: 762 Budget Deficit : 743		

Turning to the selection models first (in the bottom half of the table), we note that debt, reserves, and growth are correlated with the decision to turn to the Fund. Policy variables (lagged budget deficit and domestic credit growth) are not correlated with the decision to enter a program in either of the models. We see some support for our realist variables, which confirms the findings of other studies. As states adopt policy positions closer to the US, they are more likely to be under a Fund program. Even after controlling for these other measures, increases in a state's quota in the Fund also increase the probability that it receives a loan from the Fund. Thus, larger states are more likely to receive Fund assistance.

The results for the performance stage appear in the top half of the table. Our realist variables provided mixed results. We would expect that great power influence would mean that US allies are consistently more able to challenge the terms of Fund conditionality. This claim is not supported in these data. Our tests of a realist argument

were either positive, but not significant, or negative and significant, suggesting that increasing US influence produces stronger commitment to the tenets of conditionality. This effect could be because we are also capturing ideological effects in these measures; states with similar voting portfolios to the US may also share a commitment to strict money or better fiscal management. Similarly, the EFF dummy was negative and significant across the board, suggesting that states under these programs have better monetary performance, but slightly poorer fiscal performance.

Our other measure of enforcement costs, which was captured in the quota measure, was significant in both models, as states with large quotas tended to have higher domestic credit growth and higher budget deficits. This joint Heckman model controls for the effects of the program itself—thus, our results here suggest that *ceteris paribus*, large quota states are more likely to “misbehave” from the Fund’s standpoint. This was one of the intuitions suggested by our game theoretic model.

Because the results point in different directions, with realist measures predicting greater adherence to conditionality, the findings suggest that the quota measure makes more intuitive sense as a measure of enforcement costs. Nevertheless, we continue to include these indicators, as joint Wald tests suggest that we can reject the hypothesis that both the realist variables are in fact zero.

Lastly, the effects of the program are captured in the rho coefficient, which measures unobserved factors that affect both selection and performance. Rho was negative and not significant for net domestic credit growth, but positive and significant for budget deficits. Interpreting the sign and its effect on this variable require some

care—but it suggests that Fund program states are more likely to run lower deficits. This is exactly the deterrent effect that conditionality was intended to achieve.

Another way to think about rho is that is a diagnostic test for the potential for moral hazard.<sup>95</sup> Here we are focusing on the behavior of governments rather than politicians rather than the behavior of investors. Thus, if Fund programs suffered from moral hazard, we would expect that rho would be positive and significant for credit growth, and negative and significant for the budget deficit. The findings here suggest that moral hazard is not a concern across the board, which in turn raises questions about the extent to which voluntary defection is a problem for Fund programs. We revisit this issue in the next chapter.

As a robustness check on these results, I reestimated the above three models with regional dummies for Eastern Europe and the Former Soviet Union, Latin America, and Sub-Saharan Africa. This did not alter the findings. Similarly, I included additional controls to each model, such as lagged debt to the fiscal model and lagged inflation to the monetary model, which did not alter the results. Open economy controls such as dummies for fixed exchange rate and capital controls also did not alter the results.

While performance under the adjustment program is one thing, broader outcomes such as whether the program is suspended or not are more crucial. The existence of potential biases in performance under Fund programs need not constitute inefficiency if these biases are corrected in the enforcement of the agreement. In other words, the

---

<sup>95</sup> Moral hazard exists where economic agents maximize their utility to the detriment of others (Kotowitz 1989). In this case, the canonical form of the argument suggests that politicians under Fund agreements pool on cheating. Because the Fund will lend to them regardless, they have no incentive to honor their commitments.

evidence here suggests that states with large Fund quotas are less likely to actually comply with the Fund's conditions. However, this is not necessarily a problem if the Fund sanctions these states more often. Thus, to better assess the efficiency of the enforcement regime, we turn from studying the determinants of crime to studying the determinants of punishment.

### *Determinants of Fund Program Suspensions*

The dependent variable in this section is whether or not a program was suspended by the IMF. In 138 of the 347 Fund programs studied, states were not eligible for all of the drawings either because they missed performance criteria and were unable to obtain a waiver from the Fund or they failed a quarterly review. Codings for the dependent variable came from the Schadler report and quarterly country reports of the Economist Intelligence Unit. Information about program compliance for programs prior to 1988 was obtained through a careful analysis of reports from the IMF archives, including letters of intent and program reviews.

The model below is a Heckman probit estimation, which allows us to test whether a state was under a program and whether that program was suspended by the Fund. The model that we use to estimate sanctions includes our political influence variables (lagged development assistance, lagged quota, and lagged similarity) as well as a variables for net domestic credit growth, the budget deficit, and the logged level of reserves, which are common performance criteria across letters of intent (Guitain 1995). I also added a dummy variable for high conditionality EFF programs to discern any appreciable differences across program types.



One objection needs to be addressed at the outset. Some have argued that the problem with Fund conditionality has been a growth in what has been termed “structural conditionality” which refers to policies aimed at improving productive capacity or resource efficiency rather than managing aggregate demand (Goldstein 2000:4). These policies include banking regulation, corporate governance, trade liberalization, pension policies, environmental policies, among others. Many recent observers suggest that this growth of conditionality has compromised the effectiveness of Fund programs (IFLAC 2000; Council on Foreign Relations 1999; Bird 1999).<sup>96</sup> Of course, for our argument, this raises an important question: does the growth of structural conditionality mean that our regression model is not properly specified? I would argue that the danger on this front is small, and the response to this charge is three-fold. First, had there been a growth of structural conditionality that led to effects on program compliance, I would have observed this directly in my qualitative data gathering, which I did not. Archival evidence, as did the EIU reports, consistently stated that the breached conditions that prompted program suspensions were fiscal and monetary in nature and not structural.<sup>97</sup> Second, for structural conditionality to be a problem for my claim, they would have to be performance criteria. That is, they would have to be fulfilled as a condition of the receipt of further tranches. It is highly unlikely that the Fund would hang the success or failure of an entire agreement on one of these issues, especially if a state had good macroeconomic performance under

---

<sup>96</sup> One of the most visible examples was a provision in Indonesia’s letter of intent in 1997 requiring the dismantling of the clove and plywood monopoly (Ahmed, Lane, Schulze-Ghattas 2001).

<sup>97</sup> Mecagni 1999 makes a similar argument in his study of ESAF program suspensions.

the program.<sup>98</sup> Finally, even if we concede the first two points, the inferential danger to this sample posed by structural conditionality here is minimal. Goldstein (2000:34) finds that Stand Bys and EFFs averaged 1.7 structural performance criteria for arrangements started between 1993 and 1999. So, even if we could develop a composite of structural reform across programs, it would not likely add much to the model, since the number of criteria here is so small. Our results appear in the table below.

---

<sup>98</sup> Indeed, Indonesia's October 1997 letter of intent says nothing about cloves and plywood in the performance criteria, though it does appear in later policy memoranda. This raises the question of who proposed this policy change.

<b>Table 7-2: Unified Model of Agreement Selection and Sanctioning</b>			
Selection Model		Sanctioning Model	
Debt <sub>t-1</sub>	.01455*** (.00319)	Net Domestic Credit Growth	.13086** (.0491)
Reserves <sub>t-1</sub>	-.06808*** (.0204)	Budget Deficit	-.03529* (.01604)
Growth <sub>t-1</sub>	-.03119*** (.0085)	Log Reserves	-.10728 (.07812)
Budget Deficit <sub>t-1</sub>	7.4518 (.10) (4.033)	Trade <sub>t-1</sub>	-.00502 (.10) (.002873)
Net Domestic Credit Growth <sub>t-1</sub>	.0001241* (.000053)	Fund Quota <sub>t-1</sub>	-.0004112*** (.000125)
US Official Development Assistance <sub>t-1</sub>	.40259* (.2069)	Similarity <sub>t-1</sub>	-.16265 (.39014)
Similarity <sub>t-1</sub>	.01346 (.10) (.0077)	US Official Development Assistance <sub>t-1</sub>	-7.5647 (.10) (4.443)
Fund Quota <sub>t-1</sub>	.004668 (.00647)	EFF program dummy	-.00328 (.17557)
Constant	.8429*** (.1339)	Constant	.5689* (.2612)
Rho	-.5111		
Rho $\chi^2$	.0046		
Model $\chi^2$	.0000		
Number of observations: 723			
$\chi^2$ results for cubic spline segments not shown.			
Outcome Stage Percent Correctly Predicted: 70.95%			

Again, these results parallel those of previous tables. In the selection stage (the left half of the table), economic variables such as debt, reserves and growth drive the decision to enter Fund agreements. In terms of the policy variables, states that are improving their budget deficits are more likely under Fund programs, though this effect is weak. The

political influence variables and the quota measure are both positively correlated with being under a Fund program.

In the right half of the table, we estimate what determines the Fund's decision to suspend programs. The policy choices of the borrower are important here, as those states that are unable to reduce their budget deficits or control the growth rate of domestic credit are those that are likely to be sanctioned. Our results for the realist argument weaken substantially in this estimation, as increases in US development assistance reduce the probability that a state is sanctioned, though this is significant only at a .10 level.<sup>99</sup> Finally, we see support for the enforcement costs argument, as states with large IMF quotas are less likely to be sanctioned.

The above table also points out that our corrections for duration dependence and sample selection were appropriate ones. First, the chi square test for the cubic splines was 390.54, which gives us a clear indication of autocorrelation in the sample. Moreover, our suspicion that selection bias was a danger is supported, as we note that the estimate for the rho coefficient is both negative and significant. This tells us that selection effects are indeed at work in this model. Moreover, the sign on the covariance term is negative, suggesting that unobserved variables that determine whether states enter Fund programs reduce the probability that these states are sanctioned by the Fund. One way to interpret this is that it captures the Fund's own self-deterrence from punishing states. This reflects the perennial tension that we noted earlier between the Fund's incentives to lend to aid states and its incentives to enforce policy reform by suspending programs.

---

<sup>99</sup> Wald tests suggest that we cannot reject the null hypothesis that the coefficients on the realist variables in the sanctioning stage are in fact zero. This poses a serious challenge to the claim that US influence matters at this stage of Fund operations.

As a robustness test on these results, I reestimated this model using regional dummies for Latin America, Subsaharan Africa, and Eastern Europe and the Former Soviet Union in both equations, and the results were unchanged. To assess the effect of open economy factors, I also added dummies for whether the state had a fixed exchange rate regime and capital controls. Including these would also ensure that our link between fiscal and monetary policy and Fund sanctions was supported through the logic of the “unholy trinity.”<sup>100</sup> In both the selection and sanctioning models, neither variable was significant, nor were the results changed. Finally, I added a measure of changes in terms of trade to the sanctioning equation, and replaced the net domestic credit measure with the annual percentage growth in money and quasi money. These changes did not affect the sanctioning results substantially.

While statistical significance is important, the substantive effect of these variables is also worth noting. Below are the predicted probabilities of Fund sanctions given certain levels of our independent variables of interest. All other independent variables are set at their means.

---

<sup>100</sup> Economic policy is constrained by the effects of capital mobility and flexible exchange rates. Moreover, including these variables helps vitiate concerns over omitted variables based on structural reforms.

<b>Table 7-3: Predicted Probabilities of Fund Sanctions</b>	
Baseline Sanctioning Probability	42.38%
<i>Scenario</i>	<i>Change in Probability</i>
Increase Quota to Maximum, keep Net Domestic Credit Growth/Budget Deficit and US Development Assistance at Mean	Decreases by 41.08% (Falls to 1.30%)*
Increase Net Domestic Credit Growth/Budget Deficit to Maximum, keep Quota and US Development Assistance at Mean	Increases by 57.49% (Rises to 99.88%)*
Increase US Development Assistance to Maximum, keep all other variables at Mean	Decreases by 36.14% (Falls to 6.24%)
Increase Quota to Maximum, Reduce US Development Assistance to Minimum, keep other variables at mean	Decreases by 40.7% (Falls to 1.69%)*
Increase US Development Assistance to Maximum, Reduce Quota to Minimum, keep other variables at mean	Decreases by 33.82% (Falls to 8.56%)
* Indicates a statistically significant change in probability	

It is clear from these above probabilities that the policy choices of LDCs matter in assessing their compliance with the Fund. Those states unable to practice policies of fiscal and monetary restraint are more likely to see their programs suspended. However, this result is conditioned on the size of the economy; controlling for performance, large quota states are also less likely to be sanctioned. While our realist measures seem to affect the probability of sanctioning, we must take these estimates with a grain of salt. Because the link between US aid and Fund sanctions is statistically weak, the estimates of predicted probabilities are also not significant.

These results suggest a puzzle for future research. While our findings on the sources of program selection supported a realist explanation, we found very weak evidence that a state's degree of affinity with the US affected the Fund's decision whether

or not to sanction it. At the same time, we found no consistent evidence that US allies behave opportunistically while under Fund programs. Our similarity measure was negative and significantly related to the growth rate of net domestic credit, and negative (though not significant) for budget deficits implying that US allies are better implementers of Fund conditionality. At the same time, our US development aid variable was positively related to credit growth and budget deficits, though not significant.

One possible explanation for this puzzle would be that this outcome represents efficient delegation by the US. While the US can shape Fund decisions with regard to securing loans for its allies, shaping the Fund's oversight regime is a more difficult issue. Obtaining better terms for its allies is more costly for the US, since it invites opposition both within the Executive Board and with the Fund Staff.<sup>101</sup> Because US allies know that their patron cannot secure greater benefits, they in turn do not challenge the Fund. Thus, while not denying that international institutions are the agents of the great powers that create them, we note that a straightforward realist explanation of Fund sanctioning decisions has limited empirical support—especially for sanctioning behavior, which would be a key piece of evidence vindicating a realist account of IMF behavior.

### **Implications**

The evidence presented in this paper suggests that compliance with Fund programs is a multifaceted problem, as both borrower attributes and policy choices emerge as important explanations for the Fund's sanctioning practices. These findings

---

<sup>101</sup> Of course, if this is the case, then we would expect similar results by looking at Britain and France's proxy states as well. This is a subject for future research.

suggest that Fund sanctions are not merely an issue of political influence or leverage, nor are they exclusively a matter of the borrower state faithfully adhering to the terms of the letter of intent.

In terms of the theoretical traditions in IR scholarship, we find some support for institutionalist explanations for the behavior of international organizations. In Keohane's (1984) formulation, international institutions are impartial providers of public goods. The findings in the sanctioning model do suggest that the Fund behaves according to the conditionality guidelines; countries with excessive credit growth or excessive growth in the budget deficit are those that are not implementing austerity measures. Thus, it is not surprising that these states are more likely to be sanctioned. Had we found no results linking sanctioning to a state's degree of implementation of the adjustment program, this would have raised questions about the Fund's actual commitment to its stated standards.

However, we note that the Fund's enforcement power is not politically neutral. We found that states with large quotas exhibit poorer performance on both monetary and fiscal measures, and are less likely to be sanctioned. One interpretation of this result is that the Fund recognizes that larger states are more influential in the world economy, and as a result are self-deterred from sanctioning them because of the consequences to international markets. Our findings on trade lend some support to this argument, since we also found that the Fund was less likely to suspend programs in more open economies, and openness also produced better monetary performance under the program. Even after controlling for openness, however, larger programs seem more likely to breach the letter of intent and are less likely to be sanctioned. This is where the Fund's tension between lending and enforcement is made most visibly apparent, and this is where the



consequences are the most important. We noted from the earlier analysis of performance that states with large quotas were less likely to restrain the growth of domestic credit and the budget deficit while under agreements. Thus, for large Fund clients, less punishment produces more crime. Such an interpretation may help us to understand the Fund's relationship with Indonesia in recent years.

Realist hypotheses were supported in these data, but only at specific stages.

While it was clear that the selection of agreements was affected by a state's ties with the US, these connections did not allow for poorer performance under the program, nor did it affect the willingness of the Fund to sanction states. Thus, in supporting the findings of others in this area (Thacker 1999; Killick 1995), we note that focusing solely on borrower relations with the US can only explain so much. Again, I noted above that this result may be an efficient outcome for the US, since it can influence Fund behavior and preserve its autonomy in the area of program monitoring and enforcement.

Evidence from this chapter also suggests the danger of moral hazard from governments breaching Fund programs is small. If it were the case that Fund programs produced moral hazard with performance, then we might expect the rho coefficient to be positive with respect to credit growth and negative with respect to budget deficits. This would tell us that unobserved factors such as private information about the probability of noncompliance increase the probability of states breaching the agreement. For both outcome variables, the findings suggest that unobserved factors act to constrain actors from breaching the agreement. This suggests that Fund conditionality is in fact more stringent than most believe.

These findings have mixed implications for the evolving debate over IMF reform. On one level, we find that the Fund is susceptible to great power influence, but in the aggregate its enforcement decisions are unaffected by this influence. Thus, the Fund is not “captured” by the US Treasury Department, which is a key claim in the Meltzer Commission Report (IFLAC 1999). A greater danger, however, may lie in the Fund’s differential treatment of borrowers. Our finding that large quota states are less likely to be sanctioned suggests that the Fund is “buying” relatively little reform in these countries. Thus, proposed reforms to simplify conditionality are unlikely to improve policy outcomes in these countries.

Finally, we see in this model that international and domestic factors interact in very specific ways. One of the implications of this model is that international incentives—namely the belief that the Fund will not sanction noncompliance—can often trump the domestic commitment to reform. Even in the low enforcement costs equilibrium, Committed Reformers play a mixed strategy of sometimes complying with the Fund and sometimes defying it. The existence of these mixed strategy equilibria pose a challenge to the Fund’s attempts to solve its uncertainty problem with developing countries, especially for new Fund clients. In essence, it suggests that attempting to identify successful reformers *ex ante* is likely to prove difficult, since leaders have incentives to be opportunistic, and play against their type by picking fights with the Fund. Even the more faithful implementers of IMF programs in recent years, such as Turkey and Argentina, have had clashes with the Fund over the content of conditionality and their degree of program implementation.

The most interesting finding from the model is one that is unaddressed in the above discussion. One might argue that the solution to Fund conditionality lies in better information. That is, if (following from Figure 10) the Fund knew if it was facing a committed reformer or an opportunist *ex ante*, then it could design fully compliant agreements. Unfortunately, reassessing the model given full information produces a unique result: full information does not “solve” the problem of program compliance. Under full information, if the Fund sanctions noncompliance, committed reformers do not enter the program; only opportunistic ones do.<sup>102</sup> These states still enter the program, breach the agreement and are sanctioned by the IMF.

The policy implications of this full information “non-result” are important. This helps us to understand why the Fund’s “low information equilibrium” exists. Developing the means to ascertain a borrower’s type *ex ante* does not produce better results for the Fund, and in fact can cause its loan portfolio to be comprised entirely of reform-minimizing states. In this model, because the Fund cannot distinguish between types at the outset, committed reformers face incentives to enter agreements and not honor them.

What this model also paradoxically suggests is that the greatest policy disagreements will come not from the Fund relating to reform minimizers, but from committed reformers. This second type plays a mixed strategy of complying and not complying, and it is here where the need for frequent Fund missions to renegotiate programs will be highest. This is also not surprising, given that in many developing countries technocrats have incomplete control of the reform agenda and external shocks can cause technocrats to lose influence (Kahler 1992; Bates and Krueger 1993).

---

<sup>102</sup> This argument is detailed with the game solution in Appendix Two.

Thus, we are presented with a dilemma. If it is the case that even committed reformers have incentives to breach their agreements, what can be done to improve program compliance? One strategy lies in going beyond the notion of “types” to discuss more substantively about how domestic institutions affect the ability to honor commitments. After all, simplifying state types to committed or opportunist elides a subtle but important difference between voluntary and involuntary defection. Cooperation may break down because leaders that sign agreements cheat on them, or they may break down because those same leaders are unable to secure the cooperation of other actors necessary to implement the agreement. As we saw earlier, politicians have incentives to turn to the Fund to solve domestic problems, and the sample of Fund program states may be prone to involuntary defection by design, since the pursuit of resources to enable reform is a reason to enter the program in the first place. To build the model in this chapter required that we regard the IMF as treating the state as a billiard ball. Of course, this is a contestable assumption. In the previous chapter, we noted differences in the probability of states entering Fund programs based on their domestic institutions. Certainly we have reasons to suspect that these institutions also affect the ability of state to honor their commitments.

The evidence gathered so far in this project suggests that both voluntary and involuntary defection are problems for the Fund to address. First, while governments under Fund programs do not universally cheat, it is clear that larger programs produce comparatively more crime and less punishment. For example, the decision to suspend the Indonesian program in 1998 came only after a high number of tranche delays (Lane et al

1999: 4-5). While the Fund did not want to suffer such a visible failure, the behavior of the government left it little choice.

Involuntary defection problems also exist and may be more manageable. We found already in this project that an increasing number of legislative parties produces a greater demand for IMF assistance. Multiparty democracies, however, do not use the Fund to tip the balance, and we argued earlier that this is because the Fund does not recognize the higher *ex ante* costs of austerity in these countries. By implication, we suggested that multiparty democracies may be prone to a higher incidence of compliance problems because building a pro-reform coalition is much more difficult.

To resolve involuntary defection problems, however, requires information. As the two level literature makes plain, developing a better understanding of what a ratifier will or will not accept offers the means to build better agreements (Iida 1996, Milner 1997). How though, will this information be provided? The analysis here suggests that conditionality is a low information equilibrium. The empirical study in the next chapter builds on this one by incorporating domestic political considerations into assessing their effects on performance and sanctioning under Fund programs. Thus, this allows us to raise questions about the extent to which the Fund's enforcement regime is efficient in matching crime and punishment given that domestic institutions affect the probability that the state will honor its commitments. Using what we know about institutions and their effects, we will be in a better position to assess the conditions under which the Fund aids domestic interests and when it undercuts them.

### Appendix One: Solving the Game.

To solve the game, we start from the end and work to the beginning. The Fund's choices are under uncertainty, as noted by the information set. Thus, it does not know whether a state is run by Committed or Opportunistic Reformers. It acquiesces if

$$(1/2) (A-2E) + (1/2) (A/2-2E) > (1/2) (-A) + (1/2) (-A/2), \text{ or if } A > 4/3 E.$$

This result makes sense; the Fund keeps states under agreements if it gains more from having them under than from enforcing the agreement.

How do governments respond? To understand this, we look for the indifference probabilities. For Opportunistic Reformers, the State never complies if the Fund acquiesces. This is because there is no point at which an Opportunistic Reformer is indifferent between complying and not complying, given Acquiescence by the Fund.

Committed Reformers, on the other hand, are indifferent if

$$0 = t (2B-C) + (1-t) (B-C), \text{ or if } t = (C-B)/B.$$

If a Committed Reformer thinks the Fund will Acquiesce with a probability greater than  $t$ , it does not comply. On the other hand, if the Fund plays Acquiesce with a probability less than  $t$ , it complies. Thus, in this model Committed Reformers can take advantage of the Fund.

What does this mean for the Fund? Working back up the game tree, we now need to understand what inferences the Fund gathers, and assess its updated belief about the state's type on the basis of its behavior in the game thus far. The Fund's belief about whether or not a state complies is derived from its prior belief and Bayes's Rule as follows.

$$P(\text{State plays Don't Comply}) = \frac{p(\text{State is Committed}) p(\text{Don't Comply} | \text{Committed})}{[p(\text{Committed}) p(\text{Don't Comply} | \text{Committed}) + p(\text{Opportunist}) p(\text{Don't Comply} | \text{Opportunist})]}$$

From the above, we know that the probability a state is committed is  $1/2$ , and the probability a state does not comply if it is an opportunist is 1. Thus, we have to solve for the  $p(\text{Don't Comply} | \text{Committed})$ , a term we will call  $Z$ . This is simply substitution and algebra. We find that

$$Z = 2E/(3 - 4E)$$

We can think of  $Z$  as the probability of a Committed Reformer not complying that makes the Fund indifferent between Sanctioning or Acquiescing. This mixed strategy can only be supported if the initial belief about the Reformer's type ( $1/2$ ) is less than  $Z$ . Solving this inequality

$$1/2 < 2E/(3 - 4E) \text{ produces an } E > 3/8.$$

If  $E > 3/8$ , the Fund expects noncompliance and acquiesces. Under what conditions, then, should it enter an agreement? It accepts the agreement if its utility for doing so exceeds its utility for not accepting the agreement, which occurs if

$$1/2 (-2A) + 1/2 (-A) < 1/2 (A-2E) + 1/2 (A/2-2E), \text{ or if } A > 8/9.$$

On the other hand, if  $E < 3/8$ , the Fund plays a mixed strategy in which it acquiesces with probability  $Q$  and sanctions with probability  $(1-Q)$ , and it accepts the agreement if

$$1/2 (-2A) + 1/2 (-A) < 1/2 (A-2E) + 1/2 (Q (A-2E) + (1-Q) (A-E)), \text{ or if}$$

$$A > (E(3 + Q)) / 5$$

In practice, this means there are two equilibria. Given that the Fund's initial belief that it faces a Committed Reformer is  $1/2$ ,

**Equilibrium One:** If  $E > 3/8$  and  $A > 8/9$ , the Fund plays (Accept, Acquiesce) and the State plays (Enter, Don't Comply | Committed and Don't Comply | Opportunist).

**Equilibrium Two:** If  $E < 3/8$  and  $A > (E(3 + Q)) / 5$ , the Fund plays (Accept,  $(2b-c/b)$  Sanction,  $(1-2b-c/b)$  Acquiesce, and the State plays Don't Comply | Opportunist,  $(2E/(3 - 4E))$  Don't Comply | Committed,  $(1 - (2E/(3 - 4E)))$  Comply | Committed.



## Appendix Two: Equilibria under Complete Information

Just as in the above we solved the game for incomplete information, we can solve it for perfect information as well. We start by considering two scenarios: one in which the Fund only faces committed states, and one in which it only faces opportunists.

### Committed States: Full Information Case

In the first case, the Fund acquiesces at the last node only if  $A > E$ . This creates two possibilities, which we address in turn below.

*Case A: Assume  $A > E$ , and that the Fund acquiesces to Noncompliance by Committed Reformers.*

Does the Government comply? Never. To see this, we note that given acquiescence by the Fund, that a committed reformer would comply if  $B - C > 2B - C$ . For this to be the case,  $-B$  would have to be greater than zero, which cannot occur since we defined all parameters as positive and nonzero. Thus, Committed Reformers do not comply, and the Fund acquiesces.

Does the Fund accept? Given the previous choices on the game tree, we note that the Fund accepts agreements if  $A - 2E > -2A$ . For this condition to hold, it has to be the case that  $A > 2/3E$ .

Does the Government approach the Fund? Always. Its decision is to enter the agreement if  $2B - C > B - C$ . Since this holds for all values of  $B$  greater than zero, it always enters the agreement.

*Case B: Assume  $A < E$ , and that the Fund sanctions Noncompliance from Committed Reformers.*

Does the Government comply? Always. In this case, the government complies if  $B - C > B/2 - C$ . For this to be the case,  $B/2 > 0$ .

Does the Fund accept? Given the previous choices on the game tree, we note that the Fund accepts agreements if  $A - E > -2A$ . For this condition to hold, it has to be the case that  $A > 1/3E$ .

Does the Government approach the Fund? This is where the problem crops up. Its decision is to enter the agreement if  $B - C > B - C$ . In this case, Committed Reformers are indifferent between reforming with the Fund and without it.

### **Opportunistic States: Full Information Case**

What if the Fund faces an opportunist under full information? Here we note that it acquiesces at the last node only if  $A > 2E$ . As before, this produces two different scenarios.

*Case C: Assume  $A > 2E$ , and that the Fund acquiesces to Noncompliance from Opportunistic Reformers.*

Does the Government comply? Never. In this case, the government complies if  $B/2 - 2C > 3B - C$ . For this to be the case,  $B > -2/3C$ , which cannot happen, so the Government does not comply, and the Fund acquiesces.

Does the Fund accept? Given the previous choices on the game tree, we note that the Fund accepts agreements if  $A/2 - 2E > -A$ . For this condition to hold, it has to be the case that  $A > 4/3E$ .

Does the Government approach the Fund? Its decision is to enter the agreement if  $3B - C > B - 2C$ , which holds if  $2B > -C$ . Thus, Opportunistic Reformers always approach the Fund.

*Case D: Assume  $A < 2E$ , and that the Fund sanctions Noncompliance from Opportunistic Reformers.*

Does the Government comply? Never. In this case, the government complies if  $B/2 - 2C > B - C$ , which never happens. Thus, even under full information, Opportunistic Reformers are sanctioned by the Fund.

Does the Fund accept? Always. Given the previous choices on the game tree, we note that the Fund accepts agreements if  $-A/2 > -A$ . This is always the case by definition.

Does the Government approach the Fund? Always. Its decision is to enter the agreement if  $B - C > B - 2C$ . Thus, Opportunistic Reformers always sign agreements, which the Fund always accepts, and the Opportunists never comply and the Fund sanctions them.

What are the implications? Solving the information problem is only one of the dilemmas that the Fund needs to remedy to become more effective. We note that Opportunists still have incentives to enter agreements, and these states will still be sanctioned by the Fund. In the case of Committed Reformers, if the Fund is too tough, it can actually lose business. If the Fund sanctions noncompliance, Committed Reformers

are indifferent between entering the agreement or not. Only if the Fund acquiesces does it entice Committed Reformers to enter.

We can think of this result as a “Gresham’s Law of Conditionality.” If the Fund plays tough and has full information about a state’s type, the portfolio of Fund loans will be comprised purely of Opportunists, since Committed Reformers need not enter the agreement. Note as well that even Opportunists will still be sanctioned by the Fund. Thus, complete information is not a panacea.

What this suggests more broadly is that preferences and information interact, and that if the Fund behaves as the loan pushing animal described by Public Choice theory, then it has important incentives to remain uninformed about a reformer’s type. Limited information, then, seems to be an equilibrium outcome.

## Chapter Eight: Domestic Institutions and IMF Program Compliance

“You have observed that programs work only if governments want them to work.”  
–Michel Camdessus

“In a phrase, the problem is that economists most of the time treat the policymaker as a machine that can be programmed. Once the right policy has been singled out...it is up to the policymaker-machine to implement it.”  
–Edwards and Tabellini (1991:S16)

Throughout this project, we have focused on the importance of information and incentives in IMF operations.<sup>103</sup> I argued that the Fund works in a low information equilibrium in which developing countries face incentives to enter programs whether they can honor them or not. At the same time, because the Fund faces a tension between lending and ensuring reform, it has little incentive to develop more selective lending programs that distinguish committed reformers from their opportunistic counterparts.

If the Fund operates under this low information equilibrium, three consequences follow which were the subject of previous chapters. First, the Fund does not convey information to international markets and as a result the hypothesized ‘catalytic effect’ of Fund programs is weak. Second, the Fund does not design conditionality with respect to a leader’s domestic constraints. As a result, it is inefficient as the Fund does not “tip the balance” for democracies with a high number of legislative parties. Third, building on a model of the Fund’s oversight and enforcement decisions, our results suggested that enforcement costs were a stronger determinant of outcomes than US influence. The perfect information version of this model also suggested that even if the Fund knew that it

---

<sup>103</sup> The epigraph is from the Concluding Remarks at the Closing Joint Session of the Annual Meetings, Washington, D.C., October 8, 1998.

confronted a state that was going to breach its commitments, the Fund would still lend and the agreement would break down. More information is not enough to prevent agreements from being breached, and the Fund sanctioning these states, without more sustained attention to the lending-reform tradeoff.

In this chapter, we continue to evaluate the role of domestic institutions in explaining commitment to austerity. Our approach is the same as in the previous chapter, and the goal here is to ascertain whether Fund behavior in linking performance and sanctioning is efficient. The last chapter suggested that subject to enforcement costs, the IMF's enforcement regime seems efficient in the sense that it suspends programs in those states that breach austerity. Here I pose two questions: do domestic institutions matter, and when we consider their effects, is Fund enforcement efficient? I build on the argument in Chapter Six and look at the effects of regime type and legislative organization. The evidence suggests that the Fund's enforcement regime is inefficient. We see no evidence that multiparty democracies demonstrate poorer performance under Fund agreements, yet even after we control for performance, the Fund is more likely to sanction these states. Thus, for multiparty democracies, the Fund consistently commits Type I errors of punishment without crime. In these states, involuntary defection is not the problem. Rather, the Fund's inefficiency in sanctioning is the problem, and this stems from its inability to identify committed reformers *ex ante*.

These findings suggest not only that better information about the domestic constraints of borrowers will help the Fund make appropriate decisions. It also implies that the Fund operates with a prior belief that multiparty democracies are poor reformers, which is why it is more likely to sanction them. This belief however, does not have a

strong empirical basis. Not only does the Fund need to become more informed about the politics of adjustment, but it needs to better design conditionality across states to become more efficient.

### **Existing Answers to the Puzzle:**

Fortunately, the existing literature provides many answers to account for variations in compliance over time. I focus first on explanations articulated by economists, and then turn to the political science literature. Noting the limits of existing work helps pave the way for my own account, which I develop below.

Killick (1995) notes that one of the primary causes of noncompliance with Fund programs is the occurrence of exogenous shocks. Essentially, the argument is based on rigidity: once the Fund and the developing country agree to a letter of intent, then it holds countries accountable for the performance criteria regardless of the course of events. Thus, if export price shocks or natural disasters occur, the Fund does not “change the bar” for country compliance in light of changes in the underlying plan’s assumptions. Not surprisingly, this line of argument carries more weight among Fund critics, since it is based on a view of the Fund as inflexible. Unfortunately, neither of the premises underpinning it are true. Flexibility is a common feature of Fund agreements. Letters of intent have provisions in them for adjustment and alteration in response to new events, and it is just not true to say that the Fund makes no allowance for important shocks that have effects on the performance criteria. For example, banking crises in Chile in 1983 and the Philippines in 1980 meant that they had to expand credit to address the shortfall of capital inflows as well as liquidate failed banks. The Fund waived the breaches of

performance criteria for Chile, and adjusted them upwards in the Philippine case (IMF Central Files, C/Chile/1760, March 9, 1984 Staff Report for the 1983 Article 4 Consultation and Consultation under Stand-By Arrangement; IMF Central Files, C/Philippines/1760, July 30, 1981 Stand-By Arrangement - Review and Modification). Similarly, natural disasters have been occasions for Fund staff to revise the program assumptions and performance criteria, as in the case of Jamaica after Hurricane Gilbert, which resulted in the 1988 standby being extended and augmented (IMF Central Files, C/Jamaica/1760, March 6, 1990 Request for Stand-By Arrangement:2). Similarly, the Fund waived performance criteria breaches that came from a series of cyclones and floods that hit Mauritius in 1979-1980 (IMF Central Files, EBM 80/78, May 9, 1980; IMF Central Files, C/Mauritius/1760, August 22, 1980 Request for Stand-By Arrangement). To be fair, in this instance, the natural disasters did eventually produce a series of policy disagreements between the government and the Fund that resulted in the agreement breaking down later that year, but a new Stand-By was approved soon after.

While not only is it the case that the Fund exhibits flexibility, it is also the case that not all exogenous shocks contribute to the breakdown of agreements. For example, Niger's performance under its 1983 standby was exemplary, and all the performance criteria were observed "notwithstanding the reemergence of drought conditions and the adverse effects of the closure of the border with Nigeria" (IMF Central Files, C/Niger/1760, October 21, 1985 Request for Stand-By Arrangement:4). Thus, not only do we have to make heroic assumptions about the Fund's preferences to justify this sort of argument, but we also need a better operationalization of what actually constitutes a shock and how one identifies one *ex ante*. Moreover, to give this argument justice



requires a more specified research design than Killick's, since he selects his cases on the dependent variable by gathering a sample of failed programs and then looking for generalizable lessons.

Three recent arguments have been developed by political scientists to account for variations in compliance with international agreements. One such argument stems from what has been called "two level games" (Putnam 1988) which involve deal-making between leaders (his Level One) and deal commitment between ratifying bodies (his Level Two). Thus, the challenge for leaders is to find a zone of contracts that will be acceptable to both parties, since cooperation can fail because of voluntary defection, in which deals are scuttled by the actors that negotiated them, or because of involuntary defection, in which actors fail to ratify the agreement.

Of course, the two level game research program was a cottage industry for years, spawning an edited book and scores of articles. While the metaphor remains profound, as it has deeply shaped everything that followed it, the approach is of limited applicability to the study of compliance.<sup>104</sup> Iida (1993:409ff, emphasis added) notes that the two governments that obtain ratification of the agreement "have incentives to defect from the agreement in implementing it," a problem that is not addressed in his formulation. Kahler (1993) suggests that notion of negotiation and ratification does not fit how IMF

---

<sup>104</sup> More broadly, the two-level research program has slowed because it lacks theoretical content. Exactly what determines winset sizes was never fully articulated. More recent formalizations of two level games (Iida 1996, Mo 1995) shifted the focus to how institutions shape the information that actors hold about the preferences of Level Two, and the conditions under which agreements are ratified. Of course, shifting the focus to information revelation can only get us so far, since even if the negotiators have full information about a Level Two winset, this by itself does not guarantee cooperation, since the winsets may not intersect (Milner 1997).

agreements unfold. This is because 'ratification' comes after sustained adherence to the terms of the letter of intent.<sup>105</sup>

Another problem was the focus exclusively on involuntary defection. Voluntary defection was never well understood within the two level framework, since leaders presumably had an interest in making the agreement in the first place. Of course, issues of information and monitoring that had been the crux of other parts of the cooperation literature faded in importance, meaning that if voluntary defection ever occurred, a two level approach would not be able to explain it without resort to ad-hocery. Thus, while the two level metaphor had a profound impact on the field, it raised more questions than it was capable of answering.

More recent work on international compliance adopts what has been termed a managerial approach (Chayes and Chayes 1993, 1995). These works argue that compliance can exist in the absence of enforcement because states essentially want to honor the agreements that they sign. Noncompliance problems, when they exist, occur because of contractual incompleteness and domestic capacity problems. While this approach shared with two level games a central focus on the issue of contract design, it represented a significant step backward in how it framed the role of domestic politics. The state was changed from multiple actors back to a billiard ball, and the focus on capacity raised further questions about the conditions under which agreements are selected. To the point, why would a statesman sign an agreement that could not be carried out because of capacity problems?

---

<sup>105</sup> IMF letters of intent are not treated as treaties in international law.

Recent critiques of the managerial approach challenge the implicit presumption that international agreements are self-enforcing and suggest that enforcement is essential for agreements that require substantial departures from the status quo (Downs, Rocke, and Barsoom 1996). Thus, they adopt what could be termed a ‘sophisticated’ enforcement perspective. The central claim is that the managerialist insight that compliance occurs without enforcement may be correct, but substantively meaningless. In the agreements that result, compliance is never a problem, because they impose no costs on the contracting states. Thus, compliance with the set of international agreements the managerialists are concerned about is the rule rather than the exception because there are no incentives to deviate from them.

While the sophisticated enforcement perspective helps us to better see the links between selection of agreements and the ability to commit under them, there are limits to how this argument can help us understand compliance with IMF agreements. First, it is certainly the case that noncompliance is costly, but this does not imply that politicians avoid the Fund. After all, the Status Quo is a worsening of the balance of payments problem. Our formal analysis suggested that the Fund faces a low information problem precisely because conditionality alone does not screen out reform-minimizing borrowers. Moreover, our findings in Chapter Six suggest that domestic factors also determine whether or not states enter Fund agreements, and under some conditions, states with institutional mixes that make policy making difficult are those that enter Fund programs. Thus, politicians face multiple and cross-cutting incentives regarding entering Fund programs.

At the same time, while the collective action problem of economic reform can necessitate a greater demand for Fund assistance, statesmen know that the Fund is at best an imperfect enforcer. Statesmen can exploit the Fund and sign agreements that they will not honor, because they do not think that they will be punished by the Fund. Thus, understanding compliance with IMF programs requires that we understand both how the Fund's incentives affect its oversight of states, as well as how domestic institutions affect the commitment to implementing austerity measures. The table below briefly summarizes this section.

<b>Table 8-1: Contending Approaches to the Study of Compliance</b>		
<b>Approach</b>	<b>Variations in Compliance Explained By</b>	<b>Main Analytical Problem</b>
Exogenous Shocks	Occurrence of natural disasters that force states to adopt policies opposed by the Fund	Operationalization and endogeneity
Two-Level Games	Inability to reach deals between levels 1 and 2 (obtain ratification)	Focusing exclusively on level 1 and 2 assumes away voluntary defection and places the focus on informational advantage
Managerialists	Incomplete contracts/capacity problems	Assumes away both voluntary and involuntary defection problems
"Sophisticated" Enforcement	Nature of enforcement regime	Overlooks potential for adverse selection and enforcement uncertainty, brackets domestic politics

Since we are testing both whether states meet their commitments as well as whether they are sanctioned, this means that we can distinguish between these arguments. I address exactly how this is done below.

## **Domestic Institutions and the Efficiency of IMF Enforcement**

In the last chapter, we found that the Fund sanctioned those states that could not implement policies of fiscal and monetary austerity, and that this was tempered by the costs of enforcing the agreement, which were greater for large Fund clients. Thus, subject to this constraint, we noted that the Fund did seem to actually punish states that breached the terms of the letter of intent. Its enforcement regime therefore seems efficient. The core question in this chapter is whether these results hold when we consider the role of domestic institutions.

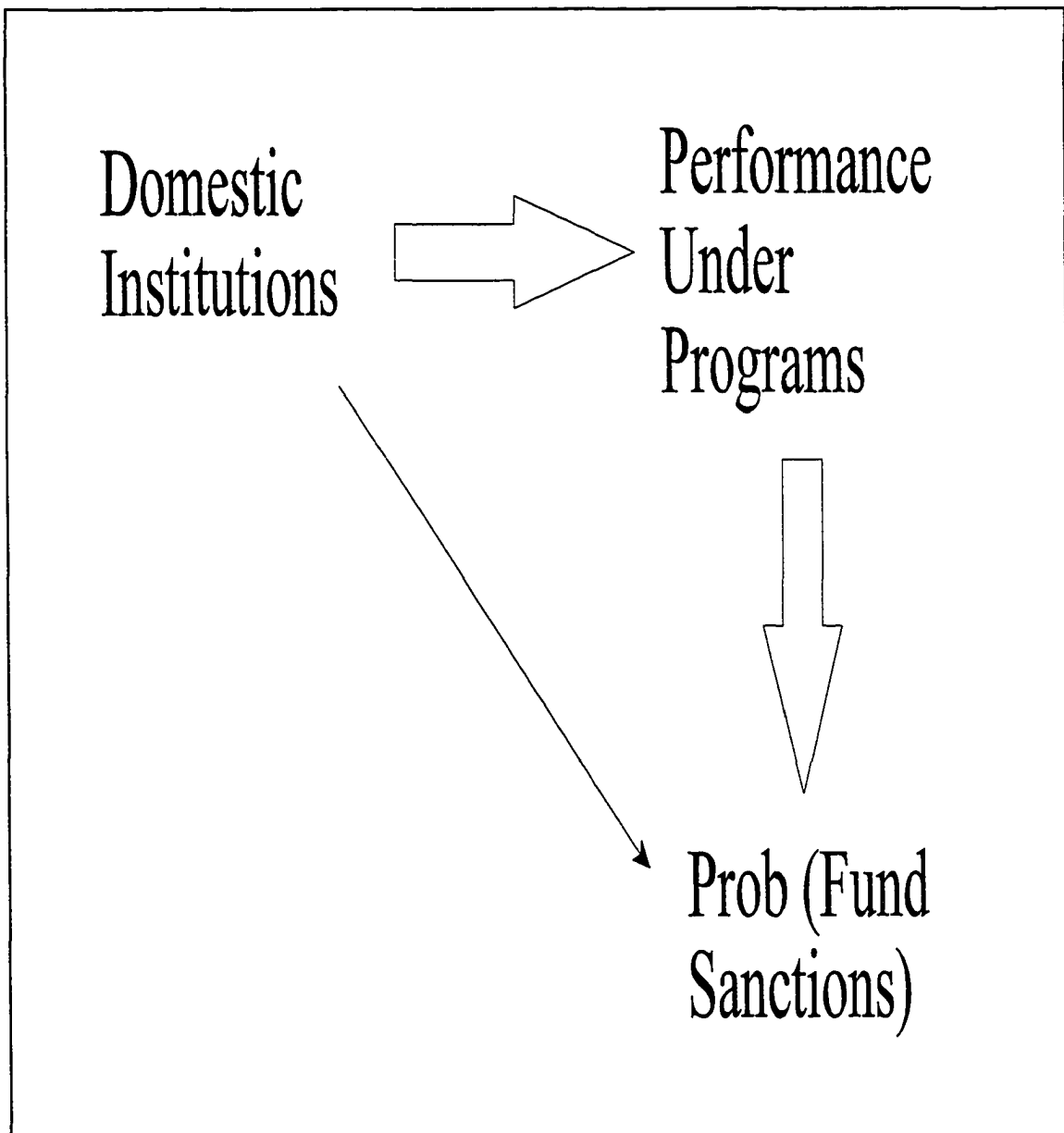
Thus, there are two questions that need to be answered. First, when we suggest that domestic politics “matters,” how does it matter? Earlier, we developed an argument suggesting that reform is a public good, and starting from this basis we developed a set of hypothesis linking regime type and legislative organization to the ability of politicians to provide this public good. This argument also has important implications for performance which I address below.

The second issue is how the Fund makes choices regarding enforcement. As in the previous chapter, we will study the effects of domestic institutions on both the level of state performance under the agreement and whether or not the agreement is suspended. We expect that domestic institutions affect the state’s level of performance under the agreement. Given this, we expect that states with these institutions may be more or less likely to be sanctioned.

The toughest test for the effects of institutions, however, requires that we assess their effects on sanctioning even after controlling for performance. This allows us to genuinely assess how the Fund evaluates the performance of the states under its

programs. We can think about the research design for this chapter in the scheme depicted in Figure 11 below. Here the straight line represents an “easy” test, and the blocked arrows represent a more difficult test of the effects of domestic institutions. The empirical tests below take this more demanding route to inference. This schematic symbolizes the research design developed in the pages below.

The answers to this question tell us a great deal about the efficiency of the IMF’s



**Figure 11**

enforcement regime. After all, if domestic institutions adversely affect performance, and these outcomes produce a higher rate of sanctioning, it is difficult to charge the Fund with being onerous in enforcing conditionality. However, this is not the only possible option. As we noted in the previous chapter, large quota states produce a higher incidence of crime and a lower incidence of punishment. These other possibilities are detailed in the table below.

<b>Table 8-2: Choice and Efficiency in IMF Enforcement</b>		
<i>Fund Observes</i>	<i>Fund Chooses</i>	
	Enforce	Don't Enforce
Poor Performance	Fund enforcement regime is efficient	Inefficient: Type II Error
Average or Better Performance	Inefficient: Type I Error	Fund enforcement regime is efficient

In the last chapter, we saw some evidence of Type II errors through the enforcement costs argument. In this chapter, the evidence suggests that Type I errors emerge when we consider the effects of institutions on Fund decisions.<sup>106</sup> This mismatch suggests that the Fund operates in a low information setting. Of course, finding specific effects of domestic institutions suggests different paths to reforming conditionality, by breaking the low information equilibrium and developing better knowledge about institutional effects and incorporating that knowledge into IMF operations.

---

<sup>106</sup> Statistically, a Type I error takes place when we reject a true null hypothesis. A Type II error occurs when we do not reject a false null hypothesis.

### **Which Institutions Matter**

In an earlier chapter, I developed an argument linking two types of institutions—regime type and legislative organization—to the decision to enter Fund agreements. I started by framing reform as a public good, and argued that increasingly levels of legislative fractionalization hamper reform for two reasons. First, coalition building is more difficult, for as the number of parties in the legislature increase, the transaction costs of building a pro-reform coalition also increase. Second, legislators have weaker incentives to provide reform as the degree of legislative fractionalization increases. In multiparty systems, legislators have weaker incentives to provide public goods to remain in office, and greater incentives to provide private goods to constituents rather than the median voter (Myerson 1993).

At the same time, we also addressed the effects of regime type, and I argued that in democracies, the effects of Fund austerity will be clearly passed from constituents to politicians through lobbying and the electoral process. We saw in Chapter Six that this factor mitigated against the need to use the Fund for leverage, and that multiparty democracies were not more likely to enter Fund agreements. Because signing a Fund agreement is a clear signal to voters of the incompetence of the present administration, elected leaders are loath to send this signal to the public.<sup>107</sup>

Thus, if this argument has merit, we would expect that performance under IMF agreements would vary systematically according to the table below:

---

<sup>107</sup> New administrations tend not to face this problem, with the result that reforms tend to be implemented in the postelectoral ‘honeymoon’ (Haggard and Kaufman 1992).



	Low Fractionalization	High Fractionalization
Democracy	Moderate Performance	Poor Performance
Non-Democracy	Good Performance	Moderate Performance

To better understand the effects of domestic institutions on performance and sanctioning, this necessitates a test of the determinants of performance under Fund agreements as well as assess the determinants of Fund sanctions.

As in the previous chapter, we now assess the effect of democracy and fractionalization on performance under Fund programs. Our dependent variables are the same as in Chapter Seven; net domestic credit growth and the budget deficit. Both these variables are used by the IMF as performance criteria to judge adherence to conditionality.

Two objections should also be addressed at this point. Some institutions that clearly have effects are not addressed here. The effects of central bank independence are omitted in the model below. While not disputing that the central bank's degree of independence or specific attributes of the central bank's charter clearly shape monetary policy--and arguably fiscal policy as well (Cukierman 1992; Cukierman, Webb, and Neyapti 1992; Fry 1995), the availability of data regarding central bank independence in developing countries is meager at best. Including measures such as turnover of the central bank governor reduces our number of observations to well under 200, which places doubts about the robustness of these results.<sup>108</sup>

---

<sup>108</sup> In future projects, I will attempt to circumvent this problem by using multiple imputation, which is a simulation based approach to missing data (King et al 2001).

## Research Design

If we are to assess the effects of domestic institutions in their entirety, as well as understand the process that produces compliance and noncompliance, this necessitates focusing on the links between institutions, crime, and punishment. Our discussion of the effects of institutions suggests that they have important effects on the ‘demand’ for the Fund as well as compliance with the agreement that results. Testing these claims requires us to employ estimation techniques that allow us to link these sequential choices explicitly. Thus, the analyses below employ the techniques developed by Heckman (1979) to address the potential for sample selection biases.

Thus, the regressions parallel those in Chapter Seven, but with variables for democracy, fractionalization, and the interactive term at both the selection and outcome stages.<sup>109</sup> As a further robustness check, the regressions include additional control variables: lagged inflation in the monetary model, and lagged total debt in the fiscal model.

---

<sup>109</sup> To reiterate, we gain added value from not running a split sample, and the results are consistent whether we use interactive tests or only select cases on democracies and nondemocracies.

<b>Table 8-4: Domestic Institutions, Program Selection and Performance</b>		
<b>Performance Model</b>		
<b>Dependent Variable</b>	<b>Net Domestic Credit</b>	<b>Budget Deficit</b>
Dependent Variable $t-1$	-.19131 (.02434)	.6678*** (.1015)
Per Capita GNP $t-1$	.000286** (.00013)	.000061 (.00013)
Growth $t-1$	-.02841 (.01658)	-.05267 (.04348)
Trade $t-1$	-.004311 (.00363)	-.04599* (.02412)
US Official Development Assistance $t-1$	-29.2435* (15.03)	-30.081 (24.956)
Similarity $t-1$	-1.103* (.4504)	-.11974 (1.2033)
Fund Quota $t-1$	.000572* (.000259)	-.00061 (.10) (.00033)
EFF Program Dummy	-.76588* (.3163)	-1.108 (.10) (.6468)
Democracy	-.73143 (.6049)	4.0498* (1.9538)
Fractionalization	-.96877*** (.2965)	2.0653* (.9013)
Democracy * Fractionalization	1.9677 (1.087)	-6.0677* (2.734)
Inflation $t-1$	.00703** (.00264)	
Debt $t-1$		.01613 (.00994)
Constant	.217455 (.24642)	-.71846 (1.845)
<b>Selection Model</b>		

Debt <sub>t-1</sub>	.017423*** (.00389)	.014138*** (.00379)
Reserves <sub>t-1</sub>	-.12797*** (.02868)	-.12156*** (.02865)
Growth <sub>t-1</sub>	-.018323 (.10) (.0112)	-.02591* (.0104)
Budget Deficit <sub>t-1</sub>	.02179* (.00908)	.00383 (.00878)
Net Domestic Credit Growth <sub>t-1</sub>	.01* (.00497)	-.00132 (.00582)
US Official Development Assistance <sub>t-1</sub>	9.857* (4.868)	10.001* (4.249)
Similarity <sub>t-1</sub>	.08471 (.25592)	.37393* (.10) (.2238)
Fund Quota <sub>t-1</sub>	.0000598 (.000062)	.000082 (.000057)
Democracy	.62522* (.2745)	.55113* (.2916)
Fractionalization	.47289* (.19645)	.42693* (.1888)
Democracy * Fractionalization	-1.0989* (.45912)	-1.023* (.4784)
Constant	.65432*** (.18145)	.81744*** (.1704)
Rho	-.03348	.2816
Rho Chi Sq	0.6429	0.0021
Model Chi Sq	0.0000	0.0000
Number of Observations: Net Domestic Credit: 555 Budget Deficit: 576		

A number of findings emerge from the above table. First, in terms of support for realist accounts, we see contradictory evidence. As in previous chapters, it is clear that those states that are large recipients of US foreign aid are more likely to receive loans from the

IMF. However, when we turn to the effects of US influence on performance under these agreements, we see mixed results. On the monetary side, US clients actually perform better under Fund programs, as noted by the fact that US clients have lower rates of credit growth. On the fiscal side, however, there is some evidence that Fund clients run higher deficits, though this is not significant for either measure of US influence.<sup>110</sup>

The enforcement costs variable is also appropriately signed, suggesting that states with large Fund quotas are prone to higher credit growth rates and greater budget deficits. This reflects the intuition developed in Chapter Seven, where large program states are less likely to be punished and more likely to breach the letter of intent as a result.

Specification tests on both the realist and enforcement costs arguments suggest that each variable should remain in the model. Joint Wald tests for each variable suggest that the probability that each coefficient is actually zero can be rejected at a .05 level for the quota and the two US influence measures.

The result on the effects of IMF conditionality (noted in the rho term here) differ between the various policy types. We note that rho is negative and insignificant in the case of monetary policy, but positive and significant in the case of fiscal policy. This suggests that the influence of the Fund is to provide pressure to lower the program state's budget deficit. In some sense, these findings should not be surprising, since this is the explicit goal of IMF conditionality.

Turning to the effects of domestic institutions, our results are very counterintuitive. At the selection stage, we saw the same result as in Chapter Six:

---

<sup>110</sup> This can be befuddling, but the variable is measured as a surplus. So a negative coefficient means that the surplus decreases, going more negative, meaning a larger deficit.

increasing the degree of fractionalization in nondemocracies increases the probability that a state will enter an IMF program. At the performance stage, increasing the level of legislative fractionalization in nondemocracies also produces better performance under Fund programs. Higher fractionalization in nondemocracies produces both lower growth of domestic credit and an improved fiscal position (lower budget deficits).

Our hypothesis about democratic regimes, however requires an interactive test of the hypothesis. These results are shown below.

<b>Table 8-5: Interactive Tests: Effects of Domestic Institutions on Performance</b>	
<i>Net Domestic Credit Growth</i>	
Fractionalization + Frac * Dem	.99896 (.9556)
<i>Budget Deficit</i>	
Fractionalization + Frac * Dem	-4.002 (2.574) $p > .120$

Here, it seems that increasing the level of fractionalization in democracies has varying effects on economic performance under the program.<sup>111</sup> On the monetary side, we see that these states do not have noticeably poorer performance, but on the fiscal side, we see slight evidence of institutional effects, as the interactive test is significant at a .12 level. Thus, there is some weak evidence that increases in fractionalization lead to a deteriorating fiscal position. These results were consistent across specifications with the additions of open economy control variables (dummies for fixed exchange rates and capital controls) and regional dummies (for Latin America, Eastern Europe and the

---

<sup>111</sup> The same results are obtained with a split sample of only democracies.

Former Soviet Union, and Sub-Saharan Africa). Moreover, replacing our fractionalization measure with a measure of the number of effective parties (following Laakso and Taagepera 1979) or with a dummy for an electoral system based on proportional representation does not change the results. This robustness check is important, since it tells us that the effects that emerge in the model are a direct result of the makeup of the legislature and are not an artifact of how parties are measured.

Using the above results, we can plot the predicted levels of fiscal and monetary performance across types of institutions.<sup>112</sup> These figures appear below. Again, it should

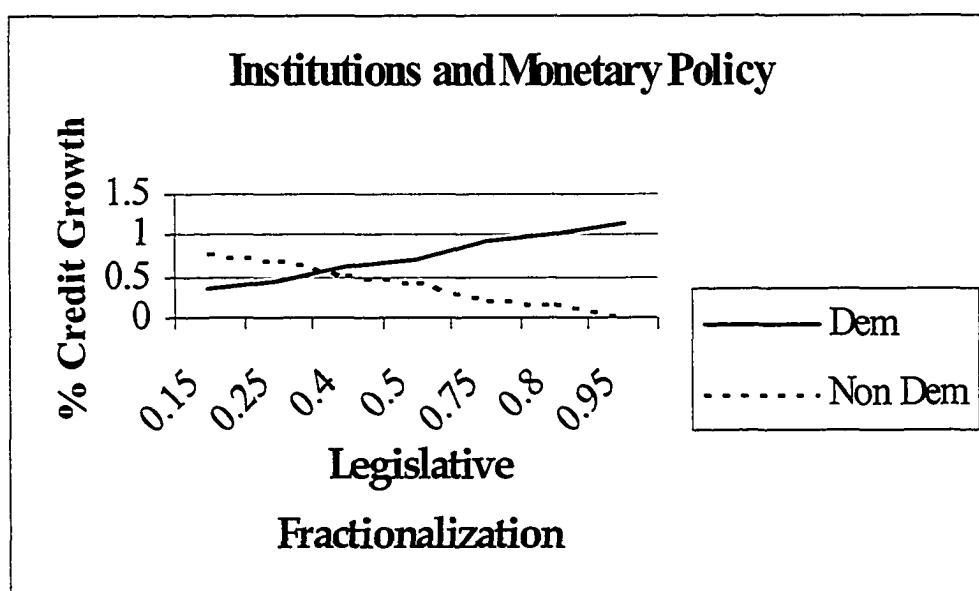


Figure 12

be stressed that these are only predicted values, and because there are no confidence intervals here one can be easily misled. To reiterate, fractionalization only had significant effects in nondemocracies.

<sup>112</sup> Readers should refer to Table 6-7 for representative country examples at each level of fractionalization.

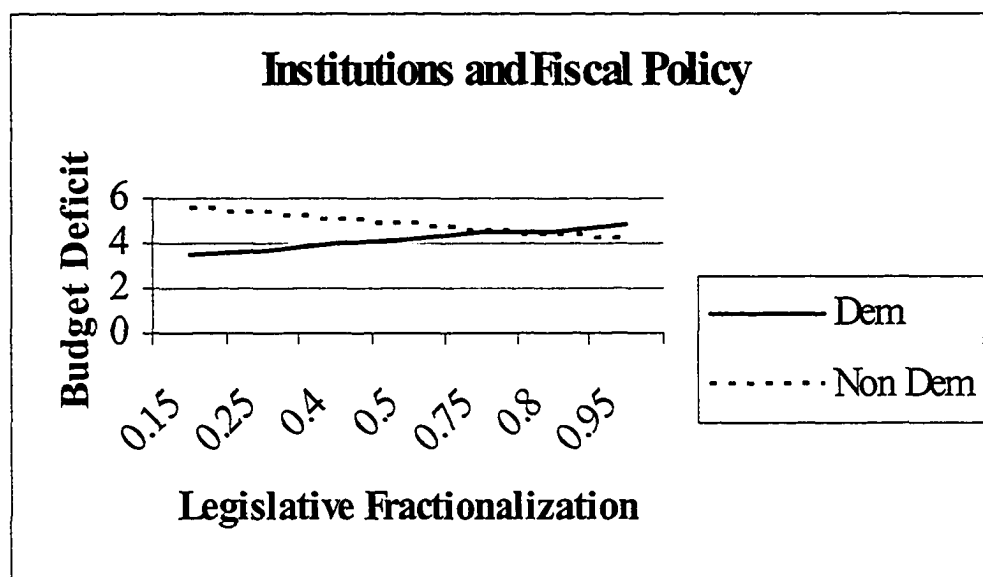


Figure 13

Thus, to make clear the distinction between our expectations and our findings, we reprint Table 8-3 from above. This time, our findings appear in italics below the expectations.

Table 8-6: Institutions and Performance Under Fund Programs		
	Low Fractionalization	High Fractionalization
Democracy	Moderate Performance <i>Moderate</i>	Poor Performance <i>Poor*</i>
Non-Democracy	Good Performance <i>Moderate</i>	Moderate Performance <i>Good</i>

\* Represents results that are weakly confirmed.

Why do we obtain these results? These findings are in contrast with much of the extant literature (Grilli, Masciandaro and Tabellini 1991, Kontopolous and Perotti 1999, Roubini and Sachs 1989) Thus, we need to better understand the mysterious nature of these results for both democracies and non-democracies. One quick answer—that



institutions don't matter--can be dispensed with immediately. Were this the case, we would not see evidence of the effects of fractionalization in nondemocracies.<sup>113</sup>

Thus, there are two different puzzles to understand. First, why is it that the public goods model only obtains weak support in democracies? Second, why is it that the opposite results obtain in nondemocracies? The answer to this first question is that the Fund program itself induces fiscal restraint. As a result, politicians in democracies have to balance their own personal demands for private goods with the constraints placed on them by the IMF. This acts to force 'convergence' in fiscal policy outcomes across democratic states; irrespective of the state's degree of fractionalization, the presence of conditionality mitigates against legislator's pursuing private goods to the detriment of reform.

We saw these results in the fiscal policy section of Table 8-4. Here the estimate for rho is positive and significant, suggesting that Fund programs produce lower deficits than non-Fund programs. Reexamining this claim in a split sample of democracies produces a similar estimate for rho. What this suggests about the IMF's degree of leverage is an issue I address in the following section.

In nondemocracies, the evidence suggests an agenda setting explanation. One of the findings from Chapter Six is that the Fund tips the balance only in nondemocracies with increasing degrees of legislative fractionalization. Thus, the same sets of states that are more likely to choose Fund agreements seem to perform better under them. In Chapter Six I argued that the distinction here turns on the effects of

---

<sup>113</sup> Alternatively, joint Wald tests suggest that we can reject the hypothesis that all the institutional coefficients are in fact zero ( $p > .025$  for fiscal policy;  $p > .0005$  for monetary policy).

austerity on constituents. Because conditionality comes at a cheaper price to nondemocracies, leaders in these states are more likely to turn to the IMF to solve problems at home.

At the same time, though, leaders in these countries are more able to use conditionality to propose comparatively more stringent austerity measures, and they are more likely to be adopted by legislatures. In regimes that are not established democracies, a politician's accountability to the distributional demands of groups is limited because the ballot is not a viable means of replacement. In the context of an IMF agreement, therefore, politicians in these states are better able to implement austerity because their reform packages are less likely to be overturned by legislators seeking pork for their constituents. In other words, the constraints imposed by a legislature in a nondemocracy are not very constraining.

These findings echo that of an earlier literature on "authoritarian advantage." Here, however, the results do not suggest that democracies are noticeably poorer reformers. Rather, they are not statistically different from the population of states under Fund programs. Leaders in nondemocracies—especially those that face a fractionalized legislature—appear more able to implement Fund-backed austerity measures.<sup>114</sup> Of course, implementing a Fund program is one thing, but whether or not such implementation is rewarded by the Fund is another matter entirely. To better understand this, we have to now turn our attention to the IMF's decision to suspend the agreement

---

<sup>114</sup> Numerous case study counterexamples exist. For example, in Kahler (1993), the Somalia EFF program of 1983 collapsed because the government would not develop a foreign exchange auction. Turning the distribution of foreign exchange over to a market mechanism would deprive political leaders of a tool of patronage.

based on performance. These results do offer one relevant insight for the next phase of the inquiry: we see no strong evidence of involuntary defection here. Evidence of involuntary defection would be reflected in a link between fractionalization and poorer policy outcomes. As we have seen, this link is both weak (as in not statistically strong) and contingent (as in only in the case of fiscal policy in democracies).

Understanding the links between various independent variables and the incidence of “crime” tells us little about how they affect the incidence of “punishment.” To answer this question, we need to assess how each affect the probability that the Fund suspends the agreement. Thus, our analyses follow the same form as in Chapter Seven, but with the addition of our institutional variables. It’s important to clarify what these findings can tell us. Since we are already controlling for performance under the agreement, we expect that what determines whether or not states are sanctioned will be measures of fiscal and monetary austerity. If even after controlling for variations in performance we still see institutional effects, this is important, as it suggests that whether or not a state is sanctioned by the IMF is driven by differences in treatment rather than by differences in policy outcomes. In other words, if we see evidence that certain types of regimes are more likely to be sanctioned than others, we can link this with our findings on selection and performance to assess the efficiency of the Fund’s operations. Thus, if we see that institutions affect performance adversely, and that these states are more likely to be sanctioned, then it is hard to argue that the Fund’s low information equilibrium poses a problem. However, if we see adverse institutional effects on performance but a lower likelihood of sanctioning, or good institutional effects on performance and a higher probability of sanctioning, then our case for informational inefficiency is much stronger.

As in Chapter Seven, the analyses below employ a Heckman probit estimator, since the dependent variable for the outcome stage is no longer policy performance, but rather whether a program is sanctioned by the Fund. As a further check on the results, we include two control variables for the presence of fixed exchange rates and the presence of capital controls, since both these are believed to discipline policy makers and ensure better economic performance. These results appear below in Table 8-7.

<b>Table 8-7: Domestic Institutions, Selection, and Program Suspension</b>			
<b>Selection Model</b>		<b>Sanctioning Model</b>	
Debt <sub>t-1</sub>	.01280*** (.00374)	Net Domestic Credit	.11846** (.04381)
Reserves <sub>t-1</sub>	-.11191*** (.02816)	Budget Deficit	-.02790 (.10) (.10267)
Growth <sub>t-1</sub>	-.03249*** (.00982)	Log Reserves	-.10728 (.10) (.01592)
Budget Deficit <sub>t-1</sub>	.02004* (.0087)	Trade <sub>t-1</sub>	-.00943* (.00429)
Net Domestic Credit Growth <sub>t-1</sub>	.005566 (.00581)	US Development Assistance <sub>t-1</sub>	-4.4079 (4.39)
US Development Assistance <sub>t-1</sub>	12.465** (4.8799)	UN Similarity <sub>t-1</sub>	.074652 (.43314)
UN Similarity <sub>t-1</sub>	.31055 (.22805)	Quota in Fund <sub>t-1</sub>	-.000455*** (.0000859)
Quota in Fund <sub>t-1</sub>	.0000936 (.00006)	EFF program dummy	.18726 (.2006)
Democracy	.59085* (.28723)	Democracy	-.95973* (.49747)
Fractionalization	.50445** (.18865)	Fractionalization	-.277846 (.31649)
Democracy * Fractionalization	-1.1229* (.46848)	Democracy * Fractionalization	1.9221* (.7848)
Constant	.81981*** (.1692)	Fixed Exchange Rates	-.09629 (.18759)
		Capital Controls	.31889 (.26559)
Rho	-.4928	Constant	.61258 (.43354)
Rho $\chi^2$	.0266		
Model $\chi^2$	.0000		
Coefficients for Cubic Spline Segments Not Shown.			
Number of Observations: 559			
Percent Correctly Predicted: 72.69%			

The selection results parallel those reported earlier. First, we see continued support for realist accounts, since the measure for US development assistance is strongly correlated with whether a state is under a Fund agreement. Similarly, nondemocracies with high degrees of legislative fractionalization were more likely under Fund agreements.

Focusing on the outcome model, we note that states that have high growth rates of domestic credit, deteriorating budget deficits, and low reserves are those that are sanctioned by the Fund. This is not at all surprising, since these variables lie at the heart of IMF conditionality. However, to jump from this to argue that the Fund is politically blind would be fallacious. Though our realist measures were not significant in this stage, we see support for our enforcement costs argument, as the Fund is less likely to sanction those states with large quotas. Though Fund sanctions are determined in part by performance under the program, a state's degree of influence has a powerful moderating effect on the decision to sanction it.

So, as noted above, a state's performance under adjustment programs affects the probability of the Fund sanctioning it, but so does a state's level of influence within the Fund. Even after controlling for these variables, we note that political institutions have systematic effects here. Turning to the interactive tests, these results appear below.

<b>Table 8-8: Interactive Tests: Effects of Fractionalization in Democracies</b>	
<i>Sanctioning Stage</i>	
Fractionalization + Frac * Dem	1.6443* (.7539)
<i>Selection Stage</i>	
Fractionalization + Frac * Dem	-.61847 (.44931)

Thus, these interactive tests confirm our earlier findings for the selection stage: as fractionalization increases in democracies, these states are not more likely to be under Fund programs. At the sanctioning stage, however, as fractionalization increases in democracies, these states are more likely to be sanctioned by the IMF.<sup>115</sup> It should be noted that we have controlled in the above for alternative explanations: variations in both fiscal and monetary performance, openness, great power influence, and enforcement costs. Taken with our earlier results, it seems that democracies with high levels of fractionalization do not exhibit poorer performance than other states, but are consistently more likely to have their programs suspended by the Fund. Similarly, nondemocracies with high levels of fractionalization exhibit better performance under Fund agreements, but are not less likely to be sanctioned.<sup>116</sup>

We can generate some substantive results by holding variables at their means and generating predicted probabilities. These appear in the table below.

<b>Table 8-9: Predicted Probability of Fund Sanctioning</b>	
Baseline Probability	32.43%
Scenario	Change in Probability
Increase Fund Quota from Mean to Maximum Value	Decreases 31.76% ( $p < .05$ )
Change Legislative Fractionalization from .1 to .9 in Democracies (or from one to ten "effective" parties)	Increases 39.89% ( $p < .05$ )

---

<sup>115</sup> We obtain the same results on a sample of only democracies.

<sup>116</sup> In the pages below, I do not focus on this result (regarding nondemocracies), but it can be explained with the same logic as the previous result (regarding democracies)

Again, these results suggest both that a state's degree of influence with the Fund is important, but also that a state's institutional attributes have important effects on whether or not the Fund suspends its agreement.

We can also think about the relationship between democracy, fractionalization and the probability of Fund sanctions in the following figure. The numerical estimates in this table are generated from the statistical model presented in Table 8-7, and these probabilities are derived from setting the performance of states under Fund programs at their mean values. Thus, given a mean level of credit growth, reserves and budget deficits, how do changes in fractionalization affect the probability of Fund sanctions? This is shown in the figure above. The real world equivalents along the X axis in Figure 14 are Trinidad, Jamaica, Fiji, Philippines, Papua New Guinea, and Ecuador (from left to right). Thus, *ceteris paribus*, Ecuador is more likely sanctioned by the Fund than the Philippines or Trinidad. Again, these predicted probabilities suggest that the Fund treats states differently even when they have the same levels of economic performance. These differences in treatment embody inefficiency because they stem from the state's domestic institutions, and they are not correlated with the effects of these institutions on Fund performance criteria.

As noted above, these results reflect the claim that the Fund operates in a low information equilibrium, and that it does not seem to sanction those states that are actually violating the terms of their programs. In the previous test of the effects of institutions on performance, we saw no strong evidence that democracies with a high degree of legislative fractionalization exhibited poorer performance on either fiscal or monetary measures. However, even when we control for their performance under the



program, it seems that these states are more likely to be sanctioned by the Fund. Ecuador, for example, thus seems to suffer from punishment without crime.

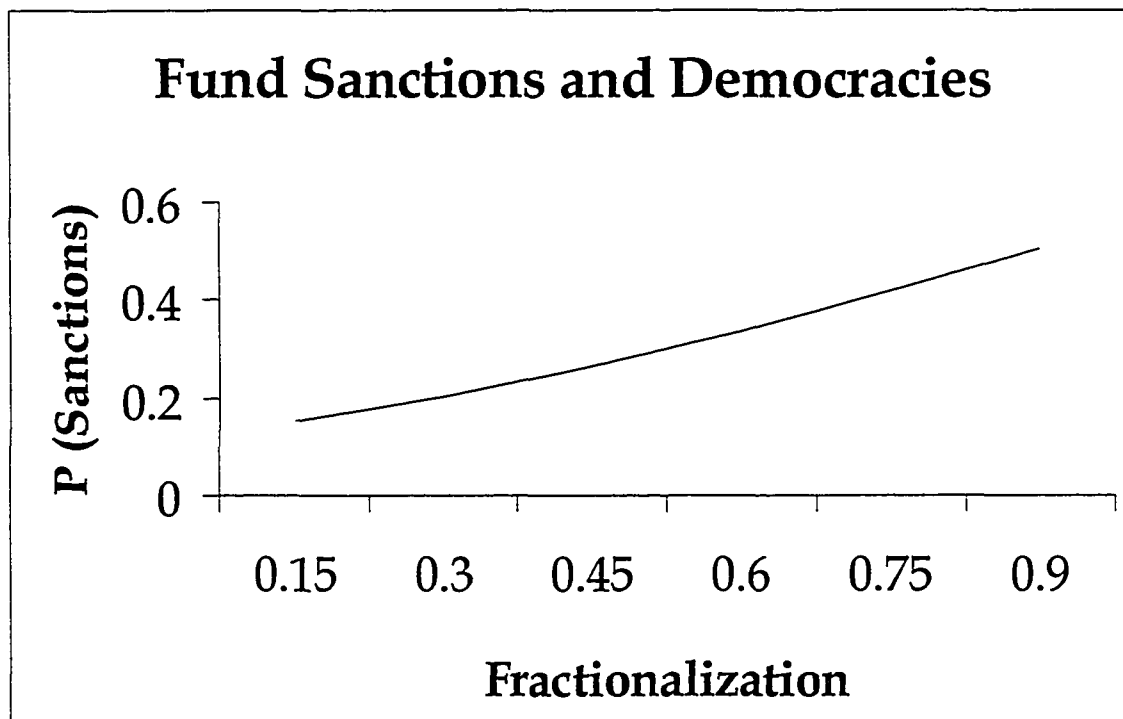


Figure 14

As robustness checks on these results, I performed a series of tests. First, to address cross sectional heterogeneity, I reestimated the above model with regional dummy variables for Eastern Europe and the Former Soviet Union and Latin America. Of course, there is only so much that we can do on this front. Since the dataset is cross-sectionally dominant, we cannot simply create 106 dummy variables for each state without using up all our degrees of freedom. Thus, we have to operate at higher levels of abstraction. Our tests with the regional dummies left the democracy and fractionalization findings unchanged. Alternative specifications of the error process relaxing the assumption that the errors are independent across cases also did not alter our results.

Similarly, omitted variable bias is a potential danger. To check this problem, I reestimated the model in Table 8-7 with a series of added variables to the outcome equation. I added a measure of lagged terms of trade to assess the effects of potential exogenous export or import shocks. I also added the state's lagged spread over LIBOR, which measures a state's degree of access to international borrowing.<sup>117</sup> In both cases, the institutional findings are unchanged. Finally, I added a set of variables to test the effects of debt on Fund program enforcement. Both dummy variables for programs that were classified as severely indebted in 1997 and lagged debt service did not alter our results.<sup>118</sup>

As a final robustness check, we can assess if our results hold with slightly different institutional specifications.<sup>119</sup> As in Chapter Six, I reestimated the model in Table 8-7 with a measure of the number of veto players in the political system which adds the effects of bicameralism and preference polarization between the executive and the legislature. Our original findings were supported, and the coefficient on the veto players variable was negative and not significant. Finally, I reestimated the model in Table 8-7 with a dummy for proportional representation electoral system. Our results were the same as with fractionalization: the Fund is more likely to sanction democracies with PR electoral systems. The p value on the joint interactive test was .023. Again, establishing the exact same statistical result with different measures increases our confidence in our

---

<sup>117</sup> LIBOR is an acronym for London Interbank Offer Rate.

<sup>118</sup> Recall that the model in Table 8-7 already controlled for capital controls and exchange rate regime.

<sup>119</sup> A brief summary of the regressions with alternative institutional specifications can be found in the Appendix.

results, as the problem with conditionality stems from how it treats states differently rather than other factors.

### **Conditionality as Low Information Equilibrium**

This project has focused on the role of information in IMF operations. I argued earlier that the Fund faces a perennial tension between lending and enforcement, which limits the Fund's ability to gather information about the types of states that it faces. As a result, in this chapter its enforcement regime operates uninformed about the effects of domestic institutions. The inefficiency in IMF operations stems from a mismatch between crime and punishment. Certain types of domestic institutions show no deleterious effects on performance under Fund agreements, yet states with these institutions are more likely to be sanctioned by the Fund.

Thus, two questions are paramount. First, why is the failure of conditionality an informational one? In the language of the game developed in Chapter Seven, the Fund seems to regard democracies with high levels of fractionalization as Opportunistic Reformers—states that are unlikely to honor the promises embodied in the letter of intent. This is consistent with thinking about the Fund's relationships with governments as stemming from uncertainty. The fact that the Fund does not know whether a state will honor its promises does not imply that it cannot form a probability estimate, or a prior belief.

The prior belief that democracies with fractionalized legislatures are less likely to honor their promises is not at all irrational. One can imagine that failed bargains between pro-reform executives and anti-reform legislatures is a source of program breakdown. The

decision to bring the IMF in is certainly politically costly in democracies, and we saw in Chapter Six that these states are less likely to sign a letter of intent when they face balance of payments problems.

However, it is a misnomer to assume that involuntary defection is the real source of the compliance problem that the IMF faces. In other words, this prior belief, while not irrational, is clearly inefficient. Our statistical evidence found no link that democracies with a high number of parties had visibly poorer performance on either fiscal or monetary policy. Figure 14 suggests that after controlling for economic performance, democracies are more likely to be sanctioned as their degree of fractionalization increases.

Qualitative evidence supports our argument, as a brief review of a number of cases of Fund programs makes clear. The cases below are democracies with high degrees of legislative fractionalization. Below, I detail the number of programs entered and the number of program suspensions. In the rightmost column, I dig more deeply into the source of the program failures and assess whether policy disagreements between the executive and the legislature scuttled any programs initiated between 1992 and 1995. Thus, from the statistical analysis, these are “problem cases.” If the claim that involuntary defection has merit, then these cases are most likely to fail, and the problem is likely to stem from a policy disagreement between the Executive and the Legislature.

<b>Table 8-10: Democracy, Legislative Fractionalization, and Program Compliance</b>			
	Number of programs	Number of breakdowns	Were there policy disagreements between Executive and Legislature?
Ecuador Mean Fractionalization: .860	7	4	Yes: Congress rejects 1995 budget and opposes tax increases
Hungary (1991-) Mean Fractionalization: .710	2	2	No: Problem in 1994 program was policy disagreements between Fund and Finance Ministry
Pakistan (1988-) Mean Fractionalization: .730	4	3	Unclear: 1994 program dominated by rule by executive order, but subsequent program failures stem from govt borrowing from banking system
Panama (1990-) Mean Fractionalization: .746	2	1	Yes: Legislature blocks passage of 1993 budget
Papua New Guinea Mean Fractionalization: .878	3	1	No: Source of program failure in 1995 was govt dispute with World Bank over rate of logging and authority over granting of logging licenses
Poland (1990-) Mean Fractionalization: .766	4	2	No: Legislature easily passes 1993, 1994 budgets
Uruguay (1985-) Mean Fractionalization: .687	4	2	Yes: Bickering over wage restraint dominated 1992-1993 program leading to a fiscal crisis
Qualitative Evidence from programs signed between 1992 and 1995. Source: Economist Intelligence Unit Quarterly Country Reports.			

In three of these cases, divisions between a pro-reform executive and an anti-reform legislature doomed the program. In three other cases, the fact that the legislature was polarized did not pose problems for the Fund program, and in fact, two of these three states (Papua New Guinea and Hungary) had programs that failed because of “voluntary” defection, or the government’s intentional choice to breach the letter of intent. Again, involuntary defection is not the sole source of the problem.

Our statistical evidence suggests the Fund seems to regard multiparty democracies as less likely to credibly implement reform, and thus feels it has to push harder by suspending programs more frequently. The problem of course is that we see little evidence suggesting that legislative intransigence borne out of an inability to build pro-reform coalitions is the source of agreement failure. Both the statistical evidence presented earlier and the qualitative evidence above suggest that democracies with high levels of legislative fractionalization need not pose a danger to successful reform. These results also controlled for international level factors that may inspire cheating by opportunistic executives.<sup>120</sup> Again, the results suggest the Fund’s ‘prior belief’ that these states are poor reformers seems to have little basis in fact. Thus, the problem that the Fund faces is certainly not programs being undone by involuntary defection.

Earlier results suggest a similar line of argument. Previously in this chapter we found that Fund conditionality leads to policy convergence in democratic states; the effects of fractionalization in democracies on fiscal policy are muted, and this is also a result of IMF influence. Thus, not only is the Fund more likely to sanction democracies

---

<sup>120</sup> Similarly, we can rule out a ‘rational anticipation’ account of defection, in which the leader knows that a policy is not going to pass, and thence scuttles the agreement. Evidence in the performance models would have corroborated this.

with high fractionalization given similar levels of program implementation, but it also seems to pressure these states more in the area of fiscal policy.<sup>121</sup> Again, this reflects a belief that these types of states are unlikely to honor their pledges; a belief that is certainly sensible, but not supported by evidence.

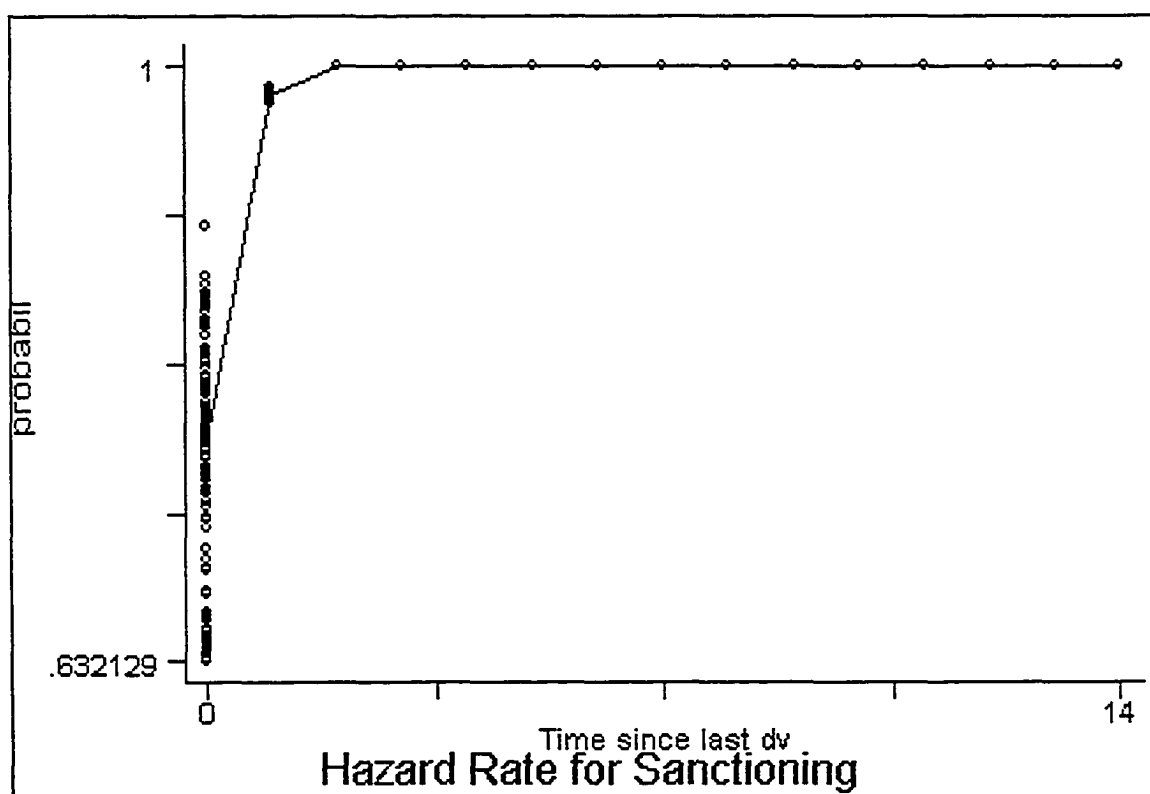
This raises a very important question. If the Fund tends to regard these states as reform minimizers, even though the evidence does not support this claim, why does it not update this belief? There are several reasons here. First, the Fund is not designed as a learning organization. The World Bank, by contrast, has an extensive Operations Evaluation Department that publishes annual information about program success and failure and tries to distill lessons from its experiences. The IMF has only created a similar institution in the past year, and with a much narrower mandate. Thus, any “learning” that goes on is more micro-level with individual program staff members assigned to specific countries than with the Fund’s loan portfolio as a whole.

This is not to say that the Fund staff does not recognize some of the sources of compliance problems. For example, reforms in the area of increasing central bank independence are intended to solve this problem. Similarly, a 1995 Stand By for Costa Rica mandated legislative approval of detailed budget breakdowns and a prohibition of financing current expenditure from sources other than current revenue. These clauses were written into the letter of intent to stem the incidence of preelection spending sprees. (Quarterly Country Report, Economist Intelligence Unit, Costa Rica, 4<sup>th</sup> Quarter 1995:17).

---

<sup>121</sup> Split sample results corroborated this finding, as the rho coefficient was only positive and significant for democracies.

While there is ‘micro-level learning’ going on at the Fund, there is little ‘macro-level learning’ about the sources of program failure. Our evidence suggests an inefficiency in IMF operations in that it sanctions democracies with a high number of parties though there is little evidence that these states have poorer performance on either the fiscal and monetary areas. This inefficiency seems regrettably stable over time, as it is clear that a suspended program remains suspended. We can see this from examining the discrete hazard rate, which represents the probability of program failure over time. This is shown in the figure below.



**Figure 15**

This figure represents the probability of sanctioning in a year given that an agreement was suspended in the previous year. The hazard rate in Figure 15 increases over time, suggesting that programs that are suspended have a higher probability of remaining



suspended in subsequent years. Evidence from the hazard rate also confirms our intuition that the failures of conditionality stem from low information. If the Fund systematically acquired better information about what determines program success or failure, and used that information in the design of its programs, we would expect this hazard rate for that state to decrease over time, and not increase. This change in the hazard rate would imply better contracting as the Fund updated its information and designed more implementable adjustment programs.

We developed an argument suggesting why a low information equilibrium exists in the previous chapter. Because the Fund faces a tradeoff between lending (doing its job) and reform, it does not screen out marginal reformers. Because acquiring more information about the effects of political institutions on the states ability to adjust (what we termed its ‘type’) does not alter this tradeoff, the Fund is in equilibrium still saddled with noncompliant agreements. Learning more by itself does not alter the results of the game unless the payoffs are changed, or unless the Fund uses that information to limit or constrain its lending to those states in which reform is more fragile. For the Fund to do this however, poses fundamental challenges to its mission. Thus, continued Type I errors and mismatches between crime and punishment are a likely consequence in the foreseeable future. I return to this theme in the following chapter.

## **Conclusions**

The results presented clarify both a theoretical debate and an important policy debate. First, our findings suggest that the debate over which variables from which levels of analysis best explain compliance with international agreements is not constructive to

future scholarship. In this project, we have seen support for realist, domestic, and public choice accounts at different stages of the process. While it is clear that a state's influence with the US affects the duration of its stay under Fund programs, having the US as an ally does not appear to permit poor performance under the program, nor does it reduce the incidence of Fund sanctions. Similarly, while we saw no effects for the Fund quota variable in terms of selecting agreements, we saw marked effects for at the performance and sanctioning stages. The use of these advanced statistical techniques helps us to better understand how and at what phases specific sorts of explanations matter.

Of course, the domestic findings were the main focus of this chapter. We noted that reform requires solving a collective action problem, and that institutions affect the extent to whether this problem can be solved. We found only weak evidence that electoral rules and legislative organization affect a state's ability both to implement the policies of fiscal and monetary restraint required by the Fund. But, given a mean level of performance, democracies with a high degree of legislative fractionalization are more likely to have their programs suspended by the Fund. This implies that the Fund feels a need to push harder in similar contexts. These domestic factors had effects on our dependent variables even when variables from systemic explanations were included. Again, to focus exclusively on building theory at one level and disregarding variables from other levels is likely to lead to fallacious inferences and missed steps.

These findings have clear relevance for the study of compliance with international agreements. In the previous chapter, we explored the basis for voluntary defection, and found evidence that borrowing states with large quotas were less likely to implement conditions and less likely to be sanctioned. In this chapter, we found little evidence

supporting the potential for involuntary defection. Using our collective action argument as well as the existing literature, we found no evidence that democracies face a more difficult task of implementing austerity when they face a large number of legislative parties. However, our work supports the low information argument since the Fund seems to regard these states as less capable of honoring their commitments.

Finally, this work also challenges the work of recent formal theorists that suggest that states only enter those agreements that they will be certain to honor. In Chapter Six, we found no evidence that democracies with a high degree of legislative fractionalization were more likely to enter Fund programs. In this chapter, the evidence suggests these same states are more likely to be sanctioned. It is clear that the higher sanctioning probability does not deter politicians in these countries for entering the agreement in the first place. If it had, we would have seen that an increasing number of parties in democracies deters politicians from entering the agreement.<sup>122</sup> Again, this reflects an earlier claim that the links between entering and honoring agreements are more subtle than many have argued.

The policy implications of this work are notable, since this work links performance and sanctioning under Fund programs. Even after we control for international factors, democracies with fragmented legislatures are not more likely to be under Fund programs, nor are they more likely to experience high rates of credit growth or budget deficits while under Fund programs. Yet despite this, they are more likely to be sanctioned by the Fund for noncompliance. Thus, the implication of these results is that

---

<sup>122</sup> It should be stressed that the conditional coefficients were not significant in Chapter Six.

the Fund's inattention to the political face of adjustment has to date served to create inefficiency. By operating with little information about the constraints that leaders face, the Fund seems to assume that involuntary defection is the source of compliance problems in these states. However, our results make clear that for democracies, the Fund consistently commits Type I errors by sanctioning without crime. It has certainly served to set back the cause of reform in these countries, as politicians gamble on entering Fund agreements only to be punished by it. Since recent work at the Fund has found a link between compliance under Fund programs and growth (Mercer-Blackman and Unigovskaya 2000), one relatively costless way for the IMF to have a greater impact in developing countries lies in taking politics seriously in enforcing conditionality more systematically.

These results also represent a challenge to the existing manner in which the Fund regards politics. The IMF's new buzzword is "borrower ownership," which is regarded as the key to ensuring that programs are successfully implemented. Our results suggest that a focus on an unoperationalized term such as ownership is likely to be detrimental over the long run. The results for the selection phase suggest that what deters states from entering Fund programs is the fear of the effects of austerity on the median voter. While nondemocracies with a high number of parties are likely to turn to the Fund to resolve their balance of payments problems, we would not consider these states to be more responsible adjusters by any means. Indeed the Fund does not seem to reward these states, since while they exhibit better performance in implementing both fiscal and monetary

austerity, they are not less likely to have their programs suspended.<sup>123</sup> Democracies are prone to the same sort of problem. While not more likely to turn to the Fund in the event of an economic crisis, they also are not more likely to exhibit an inability to implement austerity. Yet those democracies that do enter Fund programs are not rewarded for their efforts, as their programs are more likely suspended by the Fund. Thus, the Fund does not seem to be rewarding those states whose programs are more ‘owned’ regardless of how we think about how to define this term.

The presence of this systematic inefficiency in the enforcement of conditionality stems from a low information equilibrium. The Fund does not know whether the terms of its contracts can be met when it negotiates them, and revising its operations so as to use this information raises deep questions about its mission. While this chapter clearly suggests that future progress in making conditionality more effective requires taking politics “out of the error term,” we must understand that this idea in practice poses dangers as well as opportunities.

---

<sup>123</sup> In light of the logic developed above, the Fund should regard these states as committed reformers, yet does not reward them. After all, they are more likely to enter its programs and they exhibit better fiscal and monetary performance under them. Following Table 6-7, Morocco and Romania should be Fund exemplars.

## Appendix One: Alternative Institutional Models

In the field of economic policy reform, there has been a laundry list of arguments about the effects of institutions on outcomes. How well do these suggested hypotheses hold up in a large sample of states? Rather than attach a long series of statistical results, the table below summarizes the results for the regression models in Tables Four and Eight (Performance and Sanctioning) for an array of alternative institutional specifications. The entries in the table denote the sign and significance for each.

<b>Table 8-11: Summary of Alternative Institutional Models</b>			
Institutional Variable	Effects on Fiscal Performance	Effects on Monetary Performance	Effects on Sanctioning
Presidential Regime Dummy	Positive, Sig	Positive, NS	Positive, NS
Parliamentary Regime Dummy	Negative, Sig	Negative, NS	Negative, NS
Military Regime Dummy	Positive, NS	Negative, NS	Negative, NS
Veto Players Count	Negative, NS	Positive, Sig	Positive, NS
NS = "Not Significant" Sig = "Statistically Significant"			

For each potential explanation, we found little empirical support. At the performance stage, these institutions either had contradictory effects, implying either fiscal improvement and monetary deterioration or the opposite, or had no noticeable effect. At the sanctioning stage, we also saw no statistically significant differences between these states and the population at large. Further followup tests interacting presidential system,

parliamentary system, and number of veto players with regime type produced similar null findings.

## Chapter Nine: Conclusions

Conditionality is an increasingly vilified component of IMF operations, and calls from the left and the right agree on the need for its overhaul (Calomiris and Meltzer 1998, Meltzer 1998, Bandow and Vazquez 1994, Killick 1995, 1998). I argue that the problem with conditionality is one of information. The IMF makes decisions regarding its borrowing states—whether to offer agreements and whether to enforce them by suspending the loan—under uncertainty. It does not know whether a state that requests a letter of intent is a committed reformer or a reform minimizer, and its prior beliefs about these states do not seem to be supported by existing evidence.

Several consequences follow from this argument, which formed the basis for the empirical chapters. First, markets do not accept the Fund's endorsement as a "seal of approval" and make added loans and investments to a country following the announcement of an IMF loan. Second, the Fund's lending is inefficient, in that it tips the balance only under certain domestic conditions. For nondemocracies, increasing the number of parties in a legislature increases the demand for Fund programs, but this effect does not hold for established democracies. Here, the higher costs of Fund-supported austerity cancel out the need to use the Fund to strengthen reformers. Finally, we considered the effects of domestic institutions and noted that systematic mismatches between performance and sanctioning exist. While multiparty democracies do not exhibit poorer performance under Fund agreements, they are consistently more likely to be sanctioned by the Fund. The Fund seems to assume that involuntary defection is the sole source of the problem that these states face, and makes conditionality significantly



tougher as a result. However, voluntary defection is a greater problem, and the Fund consistently sanctions without evidence that democracies with a high number of parties are poorer performers. Thus, conditionality can be made to be more effective if the Fund were to become better informed about the types of states that it faces and use that information in its operations.

The goal of this chapter is to recast this argument in light of recent events and discuss the broader significance of the findings in light of modern theoretical debates. I outline how the Fund would operate differently in a world of high info conditionality. I begin by addressing a key objection focusing on recent IMF reforms. Then, I address the broader implications of this information failure argument.

### **Objections in Light of Recent Developments**

One could conceivably argue that my depiction of the IMF as an organization that operates under uncertainty is a historical artifact, and that the Fund is actually moving toward acquiring better information and making informed decisions about lending based on a borrower's type. Thus, the notion that the Fund operates in a low information equilibrium in fact has no basis. A skeptic would point to three pieces of evidence to make this argument. First, the Fund opened an Independent Evaluation Office as of July 2001, which is intended to enhance the IMF's ability to learn more about how its programs should be designed. Second, one can also point to the Fund's increasing focus on "good governance" as a criterion in program oversight. Third, one can also focus on recent proposed changes in conditionality that have come out of the Fund's internal review process. Taken as a whole, these three pieces of evidence would seriously

challenge the notion that uninformed lending and enforcement is an equilibrium.

However, I will argue that each of these developments, while promising, represent at best a limited solution to the information problem.

### *Evaluation Office*

Following an extensive in-house discussion, the Fund's Executive Board approved the creation of an Independent Evaluation Office (IEO) in June 2001. This office was created after years of debate and a growing concern about the Fund's in-house evaluation structure, which consisted of self-evaluation by operational staff or evaluation by the Fund's Office of Internal Affairs. Though a great deal of self-evaluation has been done since 1996, much of which has become public, growing concerns over the credibility of these evaluations have become commonplace.<sup>124</sup> For example, the Fund's own Review of Experiences with Evaluation notes the following regarding the self-evaluations carried out in 1996-1997 (IMF 2000):

Some Directors expressed dissatisfaction with the tone of the reports, in particular, the entire report on lessons for surveillance from the Asian crisis and the Executive Summary of the review of programs in the Asian crisis. While many Directors commended the staff for the candor of these reports, a few Directors indicated that the staff could have been more critical of the Fund's performance. Moreover, a few Directors, in the discussion of the lessons for surveillance from the Asian crisis, observed that the report had not been sufficiently critical of the role of the Executive Board in not doing more to forestall the crisis.

Thus, to counter the notion of weakness, the IEO was created with an independent work program. It developed its work program through external consultations, and is presently developing a first set of reports involving the problem of prolonged use of Fund

---

<sup>124</sup> Numerous spirited academic critiques exist (Edwards 1989; Killick 1995: 76-86).

resources, issues of fiscal adjustment under Fund programs, and capital account crisis cases.

The creation of IEO may one day portend a restructuring of conditionality. At the present, however, this is unlikely. The potential subjects for its work program were made public. Here is a partial list of issues that were not included in the IEO work program for FY 2002-2005 (IMF 2001):

Has IMF support of a country's program had a positive "catalytic effect," in terms of generating additional external financing flows within a specific timeframe?

The nature and effectiveness of conditionality and issues involving the "ownership" of national/IMF-supported programs.

Why do many IMF-supported programs remain uncompleted and what difference does it make? Are there particular aspects of program design (e.g. optimism of projections, extent of conditionality) that have a strong influence on the probability of completion? Do outcomes depend on the extent to which programs are completed and what lessons can be learned from uncompleted programs?

Do staff papers on country programs contain the necessary information and analysis for the Board to make an informed judgment on the probability of success?

Each of these unasked questions point to issues raised in the preceding pages, and each issue raises questions about the amount of information the Fund has ex ante in program design and enforcement, as well as the information it transmits ex post to international markets. Thus, while IEO may make a difference, whether it develops the means to reshape conditionality is another issue altogether.<sup>125</sup> It will certainly not develop this capacity by not studying the above issues.

### *Good Governance*

In 1997, the Fund's Executive Board developed a new focus on what was termed "good governance" and advocated a proactive approach to incorporating governance concerns in program design. This policy change was in response to a growing consensus

---

<sup>125</sup> By contrast, the World Bank's Operations Evaluation Department publishes detailed records of program success or failure. The differences between these two organizations is a subject for future research.

that corruption can be harmful to economic growth. Thus, incorporating governance concerns was part of the Fund's mandate to safeguard its resources, and in practice this meant both incorporation of governance issues into Fund surveillance, as well as a proactive approach to reducing the potential for corruption in countries under Fund programs. The mandate for governance was to be confined to two principal areas: public sector resource management and development of private sector regulatory systems. Both are areas in which governance issues could jeopardize an IMF program.

Thus, it should be clear that the mandate to focus on governance issues is a limited one. Point 7 of the Guidance Note captures the limits of governance concerns succinctly: "Specifically, the Fund's judgements should not be influenced by the nature of a political regime of a country, nor should it interfere in domestic or foreign politics of any member." While large instances of corruption have led to the suspension of ESAF programs in Kenya and Cambodia (IMF 2001:37-38), the issues raised in these cases are of limited scope. The Fund's efforts in this area do not presage the development of better attempts to acquire information about a borrower's ability to commit to a program ex ante. Indeed, adding more conditions to the menu of conditionality, in light of what we already know about its effectiveness, raises questions about how effective governance conditionality can be (Kapur and Webb 2000). Again, lacking the means to distinguish borrowers by type and design conditionality appropriately, good governance does not represent a solution to the Fund's low information equilibrium.

### *Conditionality Reform*

Following the Fund's extensive review of conditionality in the past year, new proposals have been made to reshape the so-called "modalities" of conditionality. This entails altering how conditionality is delivered, and involves two possible reforms. One of these is termed outcomes-based conditionality, and involves making lending conditional

on the results of programs rather than on adherence to specific performance criteria which are thought to produce these results. Thus, loan tranches would be released for good performance in a state's external balance, inflation, or growth (IMF 2002:8). Another possible reform involves the use of "floating" tranches, in which disbursements are not made according to a fixed timetable. These have been employed in some World Bank programs, and are thought to be more flexible since they reduce constraints on the borrower by allowing them to implement a program according to their own timetable. It has also been suggested that the World Bank's floating tranche programs may be more successful (Branson and Hanna 2000).

These reforms, while notable, also involve a reshaping of conditionality on the margins. They do not entail developing the capacity to design programs around the domestic constraints that borrowers face. They may entail added flexibility in assessing performance, but there is no evidence that more flexibility would produce better results than designing conditionality more appropriately to match a borrower's domestic constraints.

To recap, while these changes are notable, they do not challenge the argument developed throughout this project. In no way do these reforms represent the Fund's attempt to obtain better information about the type of country that it faces. Moreover, they do not represent an attempt to use that information to shape conditionality with respect to a state's domestic constraints. Thus, the low information equilibrium argument holds.

*Revising the Rules: Considering Informed Conditionality*

Suggesting how and why IMF programs fail is merely the first part of this project. Is it necessarily true that a more informed Fund would be a more successful Fund? The evidence in this project suggests this is indeed the case. But what does this mean in practice? This means that the Fund staff actually need to develop an in-depth understanding of the politics of adjustment. This holds in several senses. First, at the negotiating stage, IMF staff need to understand that the *politicians* that negotiate with them are attempting to balance domestic and international imperatives.<sup>126</sup> As a result, they are under cross pressures both to obtain external funding as well as minimize the costs of reform on their constituents. Appreciating this in reality means that conditionality should be designed differentially across states. As the analysis in Chapter Six suggests, if the Fund wants to be used to "tip the balance," then this means that it should make conditionality less onerous in democracies with a high number of legislative parties.

Is such an action likely to have pernicious consequences down the road? Not at all. After all, the analyses in Chapter Eight suggested that these states were not noticeably poorer performers in both monetary and fiscal policy. However, they were consistently more likely to be sanctioned by the Fund, suggesting that the Fund operates with a low prior belief about the likelihood that these states will honor their promises. It is this prior belief that has to be revised, and this can only come about by the Fund taking the existing literature on institutions seriously. If the goal is to ask hard questions about whether the program that they propose will be actively implemented by the borrowing government,

---

<sup>126</sup> The emphasis here is to note a subtle but important distinction in IMF discourse. Fund documents inevitably refer to "the authorities" as if these individuals had full control over policy and were not accountable to others.

this is precisely a political matter. The sorts of arguments developed throughout this project thus have clear indications for how the Fund should design future programs.

Will such a strategy be forthcoming from the Fund in the future? Chapter Seven presents an argument demonstrating why a low-information approach to conditionality may in fact be an equilibrium. In this model, because the Fund had a dominant strategy to lend, this meant that it gains even from lending to states that it knows will breach the program down the road.<sup>127</sup> Thus, it has little incentive to become more selective. Becoming more selective is indeed an essential part of making conditionality more successful, but without an understanding of what selectivity entails, merely lending less is not necessarily lending better.

Again, the evidence gathered in this project suggests that the Fund's implicit notion of types requires revision. The game theoretic notion of types is a simplification that conflates both voluntary and involuntary defection. Just as we cannot design better conditionality without a better clarification of the strategic problem that the Fund faces, we cannot build policy by assuming that voluntary are not distinct problems. Our project found little evidence suggesting that democracies with a high degree of legislative fractionalization were not (or could not be) Committed Reformers. We saw the highest evidence of "cheating" (in the classic voluntary defection sense) among large quota states. Again, this is another problem for the Fund to solve. Not only does it need to stop assuming that certain types of states are more likely to break the commitment of

---

<sup>127</sup> Thus explaining why democracies with high levels of legislative fractionalization continue to receive IMF agreements, and why these agreements are not better designed in subsequent years.

conditionality, but it needs to see that the sources of cheating lie with borrower influence over it.

Thus, this project underscores two lessons. First, a world in which conditionality is correctly based on information is one likely to see more constructive outcomes. Not only will the Fund sanction states less, but this is not likely to result in poorer policy outcomes. At the same time, designing conditionality constructively—so as to match a borrower’s domestic constraints rather than add to them—will result in a higher likelihood of states actively using the Fund as it was designed to tip the balance in favor of reform.

At the same time, developing better Fund programs is also a matter of incentives. Since large Fund clients are the ones that breach their commitments, the Fund needs to be more selective in dealing with these states in the future. Granting these states more wiggle room does not make successful reform more likely.

### **Other Evidence and Implications**

This project has advanced an informational rationale for the failure of conditionality. What other evidence is consistent with this argument? Scholte (1998) notes that one of the challenges for the Fund in coming years is an increasing need to establish contacts with civil society representatives in its client states. Despite the advantages that can be gained from building partnerships with civic organizations that might otherwise complicate implementing a Fund program, Scholte notes that “the IMF-civil society dialogue has on the whole been only weakly institutionalized and haphazardly sustained.” Moreover, Scholte suggests that the Executive Board has not “formally articulated what purposes contacts with civil society should serve, nor has



management carefully considered what institutional mechanisms would best advance the dialogue.” Again, this is also reflective of the argument we have advanced. Building these contacts with civil society could be constructive in ascertaining the government’s degree of commitment to Fund-backed adjustment and also in ‘tipping the balance’ for reform in a sophisticated fashion. This requires more than lip service to concepts such as ‘ownership’ and ‘stakeholders,’ however. It requires actively engaging these actors to gauge the political balance for reform.

If conditionality is a low information equilibrium for the IMF, we might also expect other domestic-level hypotheses to be borne out. I discuss three below. First, if the Fund operates uninformed about state types, then electoral cycles are a certainty. We would expect that the approach of elections creates incentives for politicians to engineer fiscal and monetary policy expansions, and as a result this means a higher failure rate for states under Fund programs. This implies two hypotheses. First, that the Fund signs agreements without respect to the electoral calendar. Second, that the Fund does not design conditionality so as to reduce the incidence of pre-electoral expansions, producing a continual pattern of failures over time coinciding with electoral periods.<sup>128</sup>

The second implication follows for central bank independence.<sup>129</sup> We expect that central bank independence will not be a “magic bullet” for program compliance for two reasons. First, in those countries with strong central banks, politicians will be more likely

---

<sup>128</sup> Dreher 2001 presents evidence that suggests program breakdowns are less likely during election years, though the intuition here is that the Fund has incentives to prop governments up.

<sup>129</sup> Recall that the low number of usable observations prevented us from studying this at the present. Future plans call for a reanalysis using multiple imputation to address the data problem.

to engineer electoral expansions through fiscal policy (Clark and Hallerberg 2000), leading to repeated program failures. Second, the international constraints imposed by the size of the borrower still limit the Fund's influence. Thus, even if countries have institutional mixes designed to constrain opportunism, we might still expect "cheating" since the Fund is not likely to sanction these large borrowers.

A final implication concerns federalism. In the developing world, federalism has been viewed as creating severe problems for macroeconomic adjustment (Wibbels 2000). In recent years, issues regarding the effects of federalism on fiscal policy have emerged as a stumbling block in the Fund's relations with Argentina (Mussa forthcoming). Here the Fund's information failure unfolds in a slightly different manner: federal systems see a higher demand for Fund programs, lower performance under them, and a higher incidence of sanctioning. Again, we expect these trends to be constant over time. Thus, we expect evidence from other types of domestic institutions to corroborate our argument about the Fund's informational inefficiency.

The low information argument also carries with it implications for other types of IMF programs. A major development in IMF operations has been the growth of post conflict lending, which are loans to countries that are rebuilding in the wake of civil war. Of course, the post conflict setting is different from the Fund's standard lending to support balance of payments stabilization, and the challenge for the Fund is to aid the country's return to economic viability without jeopardizing the peace settlement. Though many recent observers have called for the Fund to take a more active role in peace enforcement (Ball and Halevy 1996; Boyce and Pastor 1998; Marshall 1997; Reinicke 1996), our low information equilibrium argument suggests that the Fund will have little

measurable impact in these settings. We expect little entrepreneurship in designing conditionality around the domestic constraints of borrowers, and little attention to the importance of solving the peace problem as a prerequisite for economic development. Moreover, we might even see a higher probability of sanctioning in these countries, in light of the Fund's ex ante failure to devise implementable letters of intent.

We also expect other international institutions to also be prone to problems stemming from low information. Clearly this analysis is relevant to the World Bank as well as other regional development banks. Some brief evidence on the World Bank suggests similar sorts of problems. Estimates of the extent of the World Bank's compliance problem seem to hover around 40% (Mosley, Harrigan, Toye 1992), though some have suggested that the fail rate for African programs is as high as 70% (IFIAC 2000).<sup>130</sup> Similarly, since the bulk of World Bank non-aid lending flows to larger countries that have access to capital markets, this provides some anecdotal evidence of the enforcement costs argument (IFIAC 2000). Internal critiques of the Bank, noted in the 1992 unpublished Wapenhans Report, suggested that the Bank's failure stemmed from the growth of a lending culture and an excessive focus on pushing money (Mosley, Harrigan, Toye 1992). This raises the larger question of why the Bank does not gather information about the types of clients that it faces, implying our low information equilibrium argument.

We might also expect that compliance problems in the regional development banks (Asian, African, and Inter-American) to also be profound. First, since they service

---

<sup>130</sup> The Commission report cites "evidence from the World Bank's website," but no follow up source is given.

only a restricted set of states, it is unlikely that they will face any incentives not to approve lending programs. Second, it is hard to imagine a regional development bank enforcing a loan on a neighbor following noncompliance. After all, an international institution can have the imprimatur to suspend a country's loan, but it is hard to imagine how a bank comprised of neighboring states can do the same thing.

### **Broader Theoretical Implications**

What is the 'value added' of this project in areas outside of the study of the international financial institutions? The findings here have clear relevance to a broad set of theoretical debates. Some of these are detailed below.

#### *Implications for the Study of Compliance*

This project addresses several existing questions in the debate about the sources of compliance with international agreements. Studying IMF agreements provides us with the perfect backdrop to answer these questions. A large sample of agreements provides us with the opportunity to employ necessary controls for selection bias and autocorrelation. In contrast to other studies, compliance with this set of international agreements is costly to leaders and government officials, since implementing austerity represents a change from the status quo.

In Chapter Seven, we contrasted several potential explanations for variations in compliance, and found some support for the proposition that variations in compliance are driven by enforcement costs. However, we found no evidence that moral hazard was a problem for Fund programs, since the rho coefficients for performance suggested that

these programs are not prone to borrower opportunism. Thus, voluntary defection is a limited problem for Fund programs; a function of the enforcement costs of the agreement and not a danger across the board for all borrowing states.

We found no evidence suggesting that involuntary defection is the source of IMF sanctions. Democracies with a high degree of legislative fractionalization did not produce measurably poorer performance, and nondemocracies with a high degree of legislative fractionalization actually produced better performance. This result stems from the differential effect of conditionality across regime types. In democracies, politicians face the constraints produced by the effects of austerity on the median voter. Politicians not in established democracies lack these constraints.

Our low information equilibrium argument suggests that the problem of IMF compliance stems from an incorrect prior belief that certain types of states are weak adjusters. Even after controlling for policy variables, the Fund is more likely to sanction democracies with high degree of legislative fractionalization. While we found no evidence that involuntary defection is really at work in these programs, the Fund seems to operate as if it is. Again, this mismatch between crime and punishment is a key implication of the low information equilibrium.

Thus, these findings suggest that future progress in studying compliance should also consider imperfections in the enforcement regime, and recognize that these imperfections may stem from both international and domestic sources. In this project, the inefficiency in Fund operations comes more from the international level (a failure to link crime and punishment) than it does from the domestic level (involuntary defection ala

Putnam). Furthermore, it does the field little good to develop alternative explanations and not test them in a head to head fashion (Underdal 1998).

Methodologically, the findings here suggest that the decisions to make and honor commitments are linked, but in ways that differ from traditional lines of argument in the literature. First, in contrast to game theoretic accounts (Fearon 1999; Downs, Rocke, and Barsom 1996) that did not consider the effects of domestic institutions, we see little evidence that statesmen do not sign commitments that they cannot honor *ex ante*. We know that democracies with a high number of legislative parties are more likely to be sanctioned by the Fund, but we also know that this does not deter states from entering its programs. Recall that the coefficients on the interactive tests in Chapter Six were negative, but not significant; a result that was robust across specifications. We argued that the ‘deterrent’ effect here stemmed not from the higher likelihood of sanctioning, but from the effects of the program on domestic constituents, and their concomitant effects on leaders’ desire to enter these programs. Had it been the case that punishment deterred states from entry, then we would have seen democratic states with a high number of parties to be (significantly) less likely to enter Fund programs. These results were not borne out in the data.

Formally, I argued in Chapter Three that leaders have incentives to ‘pool’ on entering Fund programs even when the probability of sanctioning is high, since the alternative to a Fund program is a worsening balance of payments problem. In this case, even a partially implemented Fund program is better than doing nothing. Of course, the Fund also has incentives to accept agreements with these states, since making loans to countries is its job. We also noted in Chapter Seven that operating in a low information

environment with regard to country types is an equilibrium outcome, since committed reformers can become indifferent between reforming with an informed Fund that sanctions cheating and reforming without it.

Do these formal arguments undermine our econometric specifications? They do not. The use of a Heckman estimation is still fully appropriate here. Even though domestic political leaders have incentives to pool on entering agreements, this does not mean that other factors are present that can produce sample selection biases. Our review of the literature and the empirical results suggest two such explanations. First, states entering Fund programs face economic crises, and they are outliers relative to the population of states across a range of economic variables. Second, they also tend to be US allies, as US foreign aid and UN voting record affinity help explain why these states are granted programs. Thus, the presence of these other variables still necessitates a continued demand for econometric techniques that explain why commitments are made. The fact that these factors do not seem to be related to the presence of the enforcement regime does not mean that our estimation technique is ill-suited.

### *Implications for Paradigmatic Debates*

Throughout this project, we have sought to see how well realism, institutionalism, and public choice theory best account for the behavior of the IMF. Rather than assert the superiority of one of these to the alternatives, we sought to confront outcomes to data. The evidence in this project supports all three, to some extent.

For realist theory, we saw some evidence that suggests that US influence affects Fund behavior. States with close connections to the US are more likely to receive Fund

assistance. However, we saw much weaker evidence suggesting that US client states are more likely to breach their commitments, or are less likely to be sanctioned by the Fund. This suggests that major powers adopt a fire-alarm approach to overseeing the activities of the IMF, as US influence only affects whether states get agreements. Their performance under the agreement, or whether they are sanctioned, seems to be a matter of their own doing. This suggests that critiques that suggest that the IMF is the puppet of the US Treasury department are of little merit.

The findings here support an ‘incomplete delegation’ view of the links between the IMF and the US, which poses a major problem for Realist theory. The view of international institutions as the agents of great power principals here needs to be modified, for it certainly appears that there is a great deal of unexplained slack in the relationship. Thus, realists need to answer why such an arrangement would be an efficient one for a hegemon.

For Public Choice theory, we also saw mixed results. First, it is clear that the Fund faces a tradeoff between lending to states and securing policy reform in them—an exchange that Khan and Sharma (2001) termed the “Samaritan’s Dilemma.” The existence of this tradeoff is important, since it suggests that the Fund is not solely concerned with ramping up its loan portfolio. That having been said, we saw important connections between a borrower’s degree of influence over the Fund and its behavior—as states with large quotas exhibit poorer performance under Fund programs and are less likely to be sanctioned. These connections seem to better reflect enforcement costs rather than the constraints placed on the Fund by the openness of the world economy, and they reflect the old saw that “if you owe the bank a million dollars, the bank owns you, but if



you owe the bank a billion dollars, you own the bank.” This finding suggests that future Public Choice work in this area needs to adopt truer assumptions regarding tradeoffs between lending and reform. Fortunately, more sophisticated work of this sort is already underway (Willett 2000).

Finally, we saw some results that support an institutional interpretation of the Fund in the sense that the Fund does behave according to its own expressed rules—it accepts states that face balance of payments constraints, and it sanctions those states that breach its performance criteria. While in some sense these results are reflected in our findings, it is also clear that the institutionalist notion that international organizations are impartial public goods providers has severe problems. One of the unanswered questions in institutional theory concerns the extent to which IO autonomy can be compromised by either great power influence or a desire to treat some states differentially on the basis of their enforcement costs.

At the same time, the presence of a low information equilibrium also poses severe problems to the institutionalist understanding of international organizations. After all, if they are created to solve global problems, why would they not develop the wherewithal to make optimal decisions and separate potential borrowers by type? Thus, the notion that IFIs like the Fund are impartial providers of public goods is at odds with the notion that the Fund has no real incentives to obtain information about the quality of loans that it makes.

*Rationality and Constructivism*

Increasingly, the notion of choice-based explanations and norm-based explanations are being posited as contending explanations for the same phenomena (Keohane 1989; Checkel 2001; Caporaso 1992; Finnemore and Barnett 1999). While not explicitly designed as a head to head test, this project demonstrates a clear advantage to rationalist accounts over constructivist ones in terms of the clarity of the outlined causal mechanism.

As a consummate example of constructivist work, we rely on Finnemore and Barnett (1999:722-724) who use the Weberian conception of bureaucracy to derive a set of pathologies to describe dysfunctional behavior in international organizations. One such pathology that they would surely argue is relevant to the IMF is what they term insulation, which is an organizational inability to receive and process feedback about performance. They discuss the potential for insulated IFI bureaucracies to produce policy mistakes by noting that the IFIs tend to hire economists, and as a result the picture that they draw of developing countries, which is uniquely geared to their expertise, differs from what a historian or a demographer would see.

While this line of argument is productive in that it liberates us from a pollyannish view of what international institutions can do, it raises questions that a rationalist approach is better equipped to answer. Given that this insulation produces policy failure, why then is continued insulation an equilibrium? In other words, why does this organization continue to make the same mistakes across time? Finnemore and Barnett cannot answer this question without relying on path dependence. In contrast, another answer to this question appears in Chapter Seven, in which we argued through the use of

a game-theoretic model that the Fund does not gather information about the types of states that it faces because it may not improve program compliance. According to that model, committed reformers have no incentives to enter a Fund program, but opportunistic reform minimizers still enter Fund programs, still breach their commitments, and are still sanctioned. Thus, according to this model, low information conditionality remains an equilibrium because acquiring additional information does little to solve the problem. Through game theoretic tools, we can develop a more fine-grained understanding of this equilibrium without resorting to a path dependence argument.

Our argument also produces more directly relevant policy implications as well. In subsequent chapters, we argued that designing conditionality in an informed fashion by paying attention to a state's domestic constraints offered greater promise. To that end, the empirical work in Chapters Six and Eight suggest that paying attention to regime type and legislative organization promises to reduce the inefficiencies in conditionality. By devising letters of intent with sensitivity to the higher costs of adjustment in democracies, the Fund can become more efficient and tip the balance in these states. Moreover, by setting the thresholds for sanctioning more in line with institutional capacities, the Fund can better link crime and punishment and avoid punishing multiparty democracies for average performance. project this offers more direct policy relevant implications than constructivist work as well. This is a clearer set of policy recommendations than altering the culture of an institution, because it presents specific avenues for future reforms; or alternatively, exactly how this culture is to be transformed.

*Domestic Politics and International Outcomes*

For far too long, inquiry in IR had been conducted exclusively at the systemic level of analysis, which served to stultify analysis and prevent us from taking domestic politics seriously. As of late, research has swung the other way to focus on the domestic determinants of international outcomes. This project has demonstrated that both domestic and international factors determine whether IMF agreements are offered, the level of performance under them, and whether or not agreements are suspended by the Fund. Since the outcomes that we are concerned about emerge from strategic interaction, it is important that we consider the role of both the IMF and developing countries in producing these outcomes.

The policy implications of these results are profound. It suggests that the Fund's recent stress on ownership—a term that the Fund has yet to genuinely operationalize—is problematic as a solution to the compliance problem. First, it raises larger issues of what the Fund should actually be doing. The problem that the Fund faces is not merely how it separates good reformers from bad ones, but also given this information how it should lend accordingly.<sup>131</sup> The results from this project also suggests that focusing on the domestic sources of policy outcomes is fruitful, since it helps to uncover the inefficiencies in Fund operations. At the same time, the findings show that the Fund does have internal biases as well, since it is more likely to allow poor performance in (and less likely to sanction) large states. Thus, not only does the Fund need to address the low

---

<sup>131</sup> As noted earlier, without a discussion of the links between ownership and program selection, any analysis of ownership and program outcomes is likely to be biased.

information equilibrium, it needs to solve other biases in its operations that can be traced to traditional IR theory.

### *Implications for the Study of Domestic Institutions*

Our findings have relevance for debates about the effects of domestic institutions. First, the model of institutional effects on reform provision was weakly supported. Where we thought that the demand for the Fund would be highest and reform would be weakly implemented—in democracies with a high degree of legislative fractionalization—we found confounding results. These states are not more likely to seek Fund assistance, nor are they less able to implement Fund austerity. This suggests that the proper answer to certain types of political institutions affect the supply of public goods requires a more sophisticated model of the domestic ‘game,’ since the policy outcomes that we observe on the domestic level are themselves the product of strategic interaction between a legislature and an executive (Alesina and Rosenthal 1995). Thus, because executives have agenda setting power, they may be able to design around political constraints caused by a large number of legislative parties. Studies of central bank independence (Maxfield 1997) and budget institutions (Stein, Talvi, and Grisanti 1998) are based on this implicit agenda setter notion. Alternatively, it could be that social conditions also shape the effects of institutions. Rodrik (1998) develops this argument by framing democracy and the rule of law as institutions that manage conflicts, and finds that both the presence of these institutions and social cleavages explain the persistence of growth rates in developing countries. Regardless, it is clear that more work specifying how institutions matter is appropriate. Both approaches are subjects for future research.

Of course, one reason that the findings here differed from other results was context. Rather than study fiscal or monetary policy writ large, we focused on these outcomes under IMF programs. The presence of the Fund as a strategic actor surely affects the budget and monetary policy choices that actors make, and we saw evidence that the Fund augments the agenda setting power of actors under certain conditions. The Fund seems to push democratic states harder, especially in the area of fiscal policy formation. In nondemocratic states, policy outcomes improved with increases in fractionalization. This may be because the executives in these countries are more able to make ‘take it or leave it’ offers to the legislature, in which they are forced to choose between approving a budget or kicking the program into suspension. More work is necessary on this front as well.<sup>132</sup>

*Implications for the Study of International Institutions*

This project began with a paradox in that the IMF, one of the most influential international institutions, is confronted with substantial problems in enforcing the conditions of its agreements. Throughout this project, I have argued that low information lies at the heart of this inefficiency, explaining the Fund’s inability to ‘tip the balance,’ the weak response of international markets to the announcement of its programs, and its continuing willingness to suspend programs for states that show no evidence of weak performance.

---

<sup>132</sup> Future projects call for unpacking the fiscal policy outcomes into expenditure and revenues to better assess the institutional effects.

These findings suggest that we should be circumspect about the influence that international institutions have, and appreciate the constraints under which they operate. While we saw some evidence supporting Realist theory, the more pressing barrier the Fund faces is not great power interference, but uninformed lending and enforcement. Thus, though conditionality has acquired numerous detractors in recent years, future progress in improving the rate of program compliance requires that the Fund develop serious answers to difficult questions. It requires that the Fund develop a better understanding of how domestic institutions operate and how they affect decisions to enter and honor IMF programs. At the same time, it also requires that the Fund base its decisions on this information. This means that the Fund has to end the lip service that it pays to the politics of adjustment and take the effects of domestic institutions seriously. It also requires that the Fund realize the contingent nature of its influence, and that it can be held hostage by large borrowers.

Thus, while it is clear that we need to appreciate the limits of the IMF's influence, our findings make clear that one pathway to devising more credible adjustment programs lies in addressing the information problem. The solution to this problem means that the Fund must not only address difficult questions about how political institutions matter, but also devise appropriate policies that strengthen reformers rather than undercut them.

## Sources Cited

- Achen, Christopher H. 1986. *The Statistical Analysis of Quasi-Experiments*. Berkeley: University of California Press.
- Adji, Slamet Seno, Y.S. Ahn, Cheryl M Holsey, and Thomas D. Willett. 1997. Political Capacity, Macroeconomic Factors, and Capital Flows. In Marina Arbetman and Jacek Kugler, eds., *Political Capacity and Economic Behavior*. Boulder: Westview Press.
- Ahmed, Masood, Timothy Lane, and Marianne Schulze-Ghattas. 2001. Refocusing IMF Conditionality. *Finance and Development* 38(4).
- Akerlof, George A. 1970. The Market for 'Lemons': Quality Uncertainty and the Market Mechanism. *Quarterly Journal of Economics*. 84(3):488-500.
- Alesina, Alberto. 1994. Political Models of Macroeconomic Policy and Fiscal Reforms. In Stephan Haggard and Steven Webb, eds. *Voting for Reform* New York: Oxford University Press.
- Alesina, Alberto and Allen Drazen. 1991. Why Are Stabilizations Delayed? *American Economic Review* 81:1170-1188.
- Alesina, Alberto and Howard Rosenthal. 1995. *Partisan Politics, Divided Government and the Macroeconomy*. London: Cambridge University Press.
- Axelrod, Robert. 1984. *The Evolution of Cooperation*. New York: Basic Books.
- Axelrod, Robert and Robert O. Keohane, 1986. Achieving Cooperation Under Anarchy: Strategies and Institutions. In Kenneth A. Oye, ed., *Cooperation Under Anarchy* Princeton: Princeton University Press, pp. 226-254.
- Bagci, Pinar and William Perraudin, 1997. The Impact of IMF Programmes. Working Paper Number 24, ESRC Global Economic Institutions Series.
- Ball, Nicole and Tammy Halevy. 1996. Making Peace Work: The Role of the International Development Community. Overseas Development Council Policy Essay Number 18.
- Bandow, Doug and Ian Vásquez. 1994. *Perpetuating Poverty : the World Bank, the IMF, and the Developing World*. Washington: Cato Institute.
- Barnett, Michael and Martha Finnemore. 1999. The Politics, Power, and Pathologies of International Organizations. *International Organization* 53(4):699-732.



- Bartilow, Horace A. 1997. *The Debt Dilemma: IMF Negotiations in Jamaica, Grenada, and Guyana* London: Macmillan Caribbean.
- Bates, Robert H. 1994. Comment. In John Williamson ed. *The Political Economy of Policy Reform* Washington: Institute for International Economics, pp. 29-34.
- Bates, Robert H., and Paul Collier, 1993. Politics and Economics of Policy Reform in Zambia. In Robert H. Bates and Anne O. Krueger., eds. *Political and Economic Interactions in Economic Policy Reform* New York: Blackwell pp. 444-473.
- Bates, Robert H. and Anne O. Krueger 1993. Generalizations Arising From The Country Studies. In Robert H. Bates and Anne O. Krueger, eds., *Political and Economic Interactions in Economic Policy Reform* Cambridge: Blackwell.
- Bates, Robert H. and Anne Krueger, eds., 1993. *Political and Economic Interactions in Economic Policy Reform*. New York: Blackwell.
- Bayne, Nicholas. 1997. What Governments Want From International Economic Institutions and How They Get It. *Government and Opposition* 32(3):361-379.
- Beck, Nathaniel, and Jonathan Katz. 1995. What To Do (And Not To Do) With Time-Series Cross-Section Data. *American Political Science Review* 89(3):634-648.
- Beck, Nathaniel and Jonathan Katz. 1997. Nuisance vs. Substance: Specifying and Estimating Time Series Cross Section Models. In John R. Freeman, ed., *Political Analysis* Volume 6 Ann Arbor: University of Michigan Press, pp. 1-36.
- Beck, Nathaniel, Jonathan Katz, and Richard Tucker. 1998. Taking Time Seriously: Time Series Cross Section Analysis with a Binary Dependent Variable. *American Journal of Political Science* 42(4):1260-1288.
- Beck, Thorsten, George Clarke, Alberto Groff, Philip Keefer, and Patrick Walsh, 2000. New Tools and New Tests in Comparative Political Economy: The Database of Political Institutions. World Bank Policy Research Working Paper 2283.
- Berg, Eliot. 1997. Dilemmas in Donor Aid Strategies. In Catherine Gwin and Joan Nelson, eds., *Perspectives on Aid and Development* Johns Hopkins University Press, pp. 79-94.
- Berk, Richard A. 1983. An Introduction to Sample Selection Bias in Sociological Data. *American Sociological Review* 48(3):386-398.
- Beveridge, W. A., and Margaret R. Kelly 1980. Fiscal Content by Financial Programs Supported by Stand-By Arrangements in the Upper Credit Tranches, 1969-1978. *International Monetary Fund Staff Papers* 27(2):205-249.

- Bienen, Henry and Mark Gersovitz. 1985. Economic Stabilization, Conditionality, and Political Stability. *International Organization* 39(4):729-754.
- Bird, Graham and Timothy Orme. 1981. An Analysis of Drawings on the International Monetary Fund by Developing Countries. *World Development* 9(6):563-568.
- Bird, Graham and Dane Rowlands. 1997. The Catalytic Effect of Lending on the International Financial Institutions. *World Economy* 20(7):967-991.
- Bird, Graham, Mumtaz Hussain and Joseph P. Joyce. 2000. Many Happy Returns? Recidivism and the IMF. Wellesley College Department of Economics Working Paper 2000-04.
- Bird, Graham and Dane Rowlands. 2000. The IMF's Role in Mobilizing International Capital: Is There A Catalytic Effect? Paper presented at the Claremont-Georgetown Conference on Improving the Credibility of IMF Programs, January 10, 2000.
- Bird, Graham, 1996. *IMF Lending to Developing Countries* London: Routledge.
- Bird, Graham. 1998. The Effectiveness of Conditionality and the Political Economy of Policy Reform: Is It Simply a Matter of Political Will? *Policy Reform* 2(1):89-113.
- Bird, Graham. 2000. IMF Programs: Is There a Conditionality Laffer Curve? Paper presented at the Claremont-Georgetown Conference on Improving the Credibility of IMF Programs, January 10, 2000.
- Bordo, Michael and Anna Schwartz. 1999. Measuring Real Economic Effects of Bailouts: Historical Perspectives on How Countries in Financial Distress Have Fared With and Without Bailouts. Paper prepared for the Carnegie-Rochester Conference on Public Policy, November 19-20, 1999.
- Boughton, James. 2001. Comments from "IMF Conditionality: How Much is Enough?" An Economic Forum Washington D.C. Wednesday, December 19, 2001. Available at <http://www.imf.org/external/np/tr/2001/tr011219.htm>
- Boyce, James K. and Manuel Pastor. 1998. Aid for Peace: Can International Financial Institutions Help Prevent Conflict? *World Policy Journal* 15(2):42-49.
- Branson, William and Nagy Hanna. 2000. Ownership and Conditionality. World Bank OED Working Paper #8, June 1, 2000.
- Breen, Richard. 1996. Regression Models: Censored, Sample Selected or Truncated Data Sage Quantitative Applications in the Social Sciences Paper 111. Thousand Oaks, CA: Sage Publications.

- Callaghy, Thomas M. 1989. Toward State Capability and Embedded Liberalism in the Third World: Lessons for Adjustment. In Joan M. Nelson ed., *Fragile Coalitions: The Politics of Economic Adjustment* Washington: Overseas Development Council, pp. 115-138.
- Callaghy, Thomas. 1990. Lost Between State and Market: The Politics of Economic Adjustment in Ghana, Zambia and Nigeria. In Joan Nelson ed., *Economic Crisis and Policy Choice* Princeton: Princeton University Press.
- Calomiris, Charles W. and Allan H. Meltzer. 1998. Fixing the IMF. *The National Interest* 56:88-96.
- Calomiris, Charles W. 1998. The IMF's Imprudent Role as a Lender of Last Resort. *The Cato Journal* 17(3):275-295.
- Camdessus, Michel. 1998. Concluding Remarks at the Closing Joint Session of the Annual Meetings, Washington, D.C., October 8, 1998. Available at [www.imf.org/external/np/speeches/1998/100898.HTM](http://www.imf.org/external/np/speeches/1998/100898.HTM).
- Caporaso, James A. 1992. International Relations Theory and Multilateralism: The Search for Foundations. *International Organization* 46(3):599-632.
- Carnegie Corporation. 1997. *Preventing Deadly Conflict*. New York: Carnegie Commission on Preventing Deadly Conflict.
- Carr, E. H. 1946. *The Twenty Years Crisis*. London: Macmillan and Co.
- Chayes, Abram and Antonia Handler Chayes. 1993. On Compliance. *International Organization* 47(2):175-205.
- Chayes, Abram and Antonia Handler Chayes. 1995. *The New Sovereignty*. Cambridge: Harvard University Press.
- Checkel, Jeffrey T. 2001. Why Comply? Social Learning and European Identity Change. *International Organization* 55(3):553-588.
- Cheibub, Jose Antonio 1998. Political Regimes and the Extractive Capacity of Governments. *World Politics* 50(3):349-376.
- Clark, William Roberts and Mark Hallerberg. 2000. Mobile Capital, Domestic Institutions, and Electorally-Induced Monetary and Fiscal Policy. *American Political Science Review* 94(2):323-326.
- Collier, Paul, Patrick Guillaumont, Sylviane Guillaumont, and Jan Willem Gunning. 1997. Redesigning Conditionality. *World Development* 25(9):1399-1407.

- Collier, Paul. 1997. The Failure of Conditionality. In Catherine Gwin and Joan Nelson, eds., *Perspectives on Aid and Development* Johns Hopkins University Press for Overseas Development Council, pp. 51-77.
- Conway, Patrick. 1994. IMF Lending Programs: Participation and Impact. *Journal of Development Economics* 45(2):365-391.
- Cornelius, Peter. 1987. The Demand for IMF Credits by Sub-Saharan African Countries. *Economics Letters* 23:99-102.
- Cortell, Andrew and James Davis. 1996. How do International Institutions Matter? The Domestic Impact of International Rules and Norms. *International Studies Quarterly* 40(4):451-478.
- Cottarelli, Carlo. 1993. Limiting Central Bank Credit to the Government: Theory and Practice. International Monetary Fund Occasional Paper 110.
- Council on Foreign Relations Independent Task Force Report. 1999. Safeguarding Prosperity in a Global Financial System: The Future International Financial Architecture. Washington: Institute for International Economics.
- Cowhey, Peter. 1990. States and Politics in American Foreign Economic Policy. In John Odell and Thomas D. Willett, eds., *International Trade Policies* Ann Arbor: University of Michigan Press, pp. 225-252.
- Cowhey, Peter F. 1993. Domestic Institutions and the Credibility of International Commitments: Japan and the United States. *International Organization* 47(2):299-326.
- Cukierman, Alex, Steven B. Webb, and Bilin Neyapti. 1992. Measuring the Independence of Central Banks and its Effect on Policy Outcomes. *World Bank Economic Review* 6(3):353-399.
- Cukierman, Alex. 1992. *Central Bank Strategy, Credibility and Independence* Cambridge: MIT Press.
- Danaher, Kevin, ed. 1994. *50 Years Is Enough: The Case Against the World Bank and the International Monetary Fund*. Boston: South End Press.
- De Vries, Margaret Garritsen. 1986. *The IMF in A Changing World, 1945-1985*. Washington: International Monetary Fund.
- Dell, Sidney. 1981. On Being Grandmotherly: The Evolution of IMF Conditionality. Princeton Essays in International Finance 144, October 1981.
- Dhonte, Pierre. 1997. Conditionality as an Instrument of Borrower Credibility. IMF Paper on Policy Analysis and Assessment PPAA 97/2 (February 1997).

- Dollar, David and Jakob Svensson 1988. What Explains the Success or Failure of Structural Adjustment Programs? World Bank Policy Research Working Paper 1938, June 1998.
- Downs, George W. and David M. Rocke. 1995. *Optimal Imperfection? Domestic Uncertainty and Institutions in International Relations*. Princeton: Princeton University Press.
- Downs, George, David Rocke, and Peter Barsoom. 1996. Is the Good News about Compliance Good News about Cooperation? *International Organization* 50:379-406.
- Drabkin, Eric Joshua. 1993. The Political Economy of Macroeconomic Policy Implementation: Lessons from IMF High Conditionality Programs. Ph.D. diss. Department of Economics, UCLA.
- Dreher, Axel. 2001. The Influence of Elections on Compliance with World Bank Conditionality and IMF Program Interruptions. Institut für Volkswirtschaftslehre und Statistik No. 604-01, Universität Mannheim.
- Dubin, Jeffrey and Douglas Rivers. 1989. Selection Bias in Linear Regression, Logit and Probit Models. *Sociological Methods and Research* 18:360-390.
- Eckhaus, R.S. 1986. How the IMF Lives with its Conditionality. *Policy Sciences* 19:237-252.
- Economist Intelligence Unit Country Reports, Various.
- Edin, Per-Anders. and Henry Ohlsson. 1991. Political Determinants of Budget Deficits: Coalition Effects versus Minority Effects. *European Economic Review* 35(8):1597-1603.
- Edwards, Sebastian and Guido Tabellini. 1991. Explaining Fiscal Policies and Inflation in Developing Countries. *Journal of International Money & Finance* 10:16-48.
- Edwards, Sebastian and Julio Santaella. 1993. Devaluation Controversies in the Developing Countries: Lessons from the Bretton Woods Era. In Michael D. Bordo and Barry Eichengreen, eds., *A Retrospective on the Bretton Woods System* Chicago: University of Chicago Press pp. 405-455.
- Edwards, Sebastian. 1989. The IMF and the Developing Countries: A Critical Evaluation. In Karl Brunner and Allan H. Meltzer, eds., *IMF Policy Advice, Market Volatility, Commodity Price Rules and Other Essays*, Carnegie-Rochester Conference Series on Public Policy #31 Amsterdam: North-Holland, pp. 7-68.
- Evans, Peter, Harold K. Jacobson, and Robert Putnam, eds., 1993. *Double Edged Diplomacy*. Berkeley, University of California Press.

- Fearon, James D. 1994. Domestic Political Audiences and the Escalation of International Disputes. *American Political Science Review* 88(3):577-592.
- Fearon, James D. 1998. Bargaining, Enforcement, and International Cooperation. *International Organization* 52(2):269-305.
- Feldstein, Martin S. 1998. Refocusing the IMF. *Foreign Affairs* 77:20-33.
- Fernandez, Raquel and Dani Rodrik. 1991. Resistance to Reform: Status Quo Bias in the Presence of Individual-Specific Uncertainty. *American Economic Review* 81(5):1146-1155.
- Finch, David. 1988. Let the IMF be the IMF. *The International Economy* 126-128.
- Finch, C. David. 1989. The IMF: The Record and the Prospect. *Princeton Essays in International Finance* 175.
- Finnemore, Martha. 1996. *National Interests in International Society*. Ithaca: Cornell University Press.
- Fischer, Stanley. 1997. Applied Economics in Action: IMF Programs. *American Economic Review* 87(2):23-27.
- Fischer, Stanley. 1998. Press Briefing by Teleconference with Hong Kong SAR and Singapore, October 15, 1998.
- Frey, Bruno S. 1984. The Public Choice View of International Political Economy. *International Organization* 38:199-223.
- Frieden, Jeffrey A. and Ronald Rogowski. 1996. The Impact of the International Economy on National Policies. In Robert O. Keohane and Helen V. Milner, eds., *Internationalization and Domestic Politics*. New York: Cambridge University Press, pp. 25-48.
- Friedrich, Robert J. 1982. In Defense of Multiplicative Terms in Multiple Regression Equations. *American Journal of Political Science* 26(4):797-833.
- Fry, Maxwell J. 1995. *Money, Interest, and Banking in Economic Development*. Baltimore: Johns Hopkins University Press, 2<sup>nd</sup> Edition.
- Garrett, Geoffrey and Peter Lange 1996. Internationalization, Institutions, and Political Change. In Robert O. Keohane and Helen V. Milner, eds., *Internationalization and Domestic Politics*. New York: Cambridge University Press, pp. 48-79.

- Gartzke, Eric, D.J. Jo and Richard Tucker. 1999. The United Nations Affinity Data Project. Version 1.17 August 1999. Available from <http://www.vanderbilt.edu/~rtucker/data/affinity/un/similar/>
- Gates, Scott Geoffrey. 1989. *The Limits of Conditionality*. Ph.D. diss. Department of Political Science, University of Michigan.
- Gaubatz, Kurt Taylor. 1996. Democratic States and Commitment in International Relations. *International Organization* 50(1):109-139.
- Geddes, Barbara. 1994. *Politician's Dilemma*. Berkeley: University of California Press.
- Geddes, Barbara. 1995. Challenging the Conventional Wisdom. *Journal of Democracy* 5:104-118.
- General Accounting Office. 1999. International Monetary Fund: Approach Used to Establish and Monitor Conditions for Financial Assistance. Report GAO/GGD/NSIAD-99-168, June 1999.
- Gold, Joseph. 1970. *The Stand-By Arrangements of the International Monetary Fund*. Washington: International Monetary Fund.
- Gold, Joseph 1979. Conditionality. IMF Pamphlet 31 Washington: IMF.
- Goldstein, Judith. 1996. International Law and Domestic Institutions: Reconciling North American "Unfair" Trade Laws. *International Organization* 50(4):541-564.
- Goldstein, Morris and Peter Montiel. 1986. Evaluating Fund Stabilization Programs with Multicountry Data: Some Methodological Pitfalls. *IMF Staff Papers* 33:304-344.
- Goldstein, Morris. 2000. IMF Structural Programs. Paper prepared for NBER Conference on "Economic and Financial Crises in Emerging Market Economies" Woodstock, VT October 19-21, 2000.
- Gourevitch, Peter A. 1986. *Politics in Hard Times*. Ithaca: Cornell University Press.
- Gourevitch, Peter A. 1996. Squaring the Circle: The Domestic Sources of International Cooperation. *International Organization* 50:349-373.
- Greene, William C. 1997. *Econometric Analysis*. New York: Prentice Hall, 1997.
- Grilli, Vittorio, Donato Masciandaro, and Guido Tabellini. 1991. Political and Monetary Institutions and Public Financial Policies in the Industrial Countries. *Economic Policy* 13:341-392.

Grindle, Merilee and Francisco Thoumi. 1993. Muddling Toward Adjustment: The Political Economy of Economic Policy Change in Ecuador. In Robert H. Bates and Anne O. Krueger., eds., *Political and Economic Interactions in Economic Policy Reform* New York: Blackwell, pp. 123-179.

Guitian, Manuel. 1994. The Role of Monetary Policy in IMF Programs. In J. Onno de Beaufort Wijnholds, Sylvester C. W. Eijffinger and Lex H. Hoogduin, eds., *A Framework for Monetary Stability* Dordrecht: Kluwer Academic, pp. 185-209.

Guitian, Manuel. 1995. Conditionality: Past, Present, Future. *IMF Staff Papers* 42(4):792-835.

Gylfason, Thorvaldur. 1987. Credit Policy and Economic Activity in Developing Countries with IMF Stabilization Programs. *Princeton Studies in International Finance* 60.

Haggard, Stephan and Robert R. Kaufman, 1989. The Politics of Stabilization and Structural Adjustment. In Jeffrey Sachs, ed., *Developing Country Debt and Economic Performance* (Chicago: University of Chicago Press, 1989).

Haggard, Stephan and Robert Kaufman. 1992. Institutions and Economic Adjustment. In Stephan Haggard and Robert Kaufman, eds., *The Politics of Economic Adjustment* Princeton: Princeton University Press, pp. 3-37.

Haggard, Stephan and Robert R. Kaufman. 1995. *The Political Economy of Democratic Transitions*. Princeton: Princeton University Press.

Haggard, Stephan and Beth Simmons. 1987. Theories of International Regimes. *International Organization* 41: 491-517.

Haggard, Stephan and Steven Webb, eds. 1994. *Voting for Reform*. New York: Oxford University Press.

Haggard, Stephan 1986. The Politics of Adjustment: Lessons from the IMF's Extended Fund Facility. In Miles Kahler, ed., *The Politics of International Debt* Ithaca: Cornell University Press, pp. 157-186.

Haggard, Stephan. 1990. The Political Economy of the Philippine Debt Crisis. In Joan Nelson, ed., *Economic Crisis and Policy Choice* Princeton: Princeton University Press, pp. 215-256.

Hajivassiliou, Vassilis A. 1987. The External Debt Repayments Problems of LDCs: An Econometric Model Based on Panel Data. *Journal of Econometrics* 36:205-230.

Hale, David. 1998. The IMF, Now More Than Ever: The Case for Financial Peacekeeping. *Foreign Affairs* 77(6):7-13.



- Heckman, James J. 1979. Sample Selection Bias as a Specification Error. *Econometrica* 47:153-161.
- Heller, William B., Philip Keefer, and Mathew D. McCubbins. 1998. Political Structure and Economic Liberalization: Conditions and Cases from the Developing World. In Paul W. Drake and Mathew D. McCubbins, eds., *The Origins of Liberty* Princeton: Princeton University Press, pp. 146-179.
- Hellman, Joel. 1998. Winners Take All: The Politics of Post-Communist Reform in Postcommunist Transitions. *World Politics* 50(2):203-234.
- Hovi, Jon. 1998. *Games, Threats, and Treaties*. London: Pinter.
- Iida, Keisuke. 1993. When and How Do Domestic Constraints Matter? *Journal of Conflict Resolution* 37:403-426.
- Iida, Keisuke. 1996. Involuntary Defection in Two-Level Games. *Public Choice* 89:283-303.
- Ikenberry, G. John and Charles Kupchan. 1990. Socialization and Hegemonic Power. *International Organization* 44(3):283-315.
- Inter-American Development Bank. 1997. *Latin America After a Decade of Reforms*. Baltimore: Johns Hopkins University Press for Inter American Development Bank.
- International Financial Institution Advisory Commission. 2000. Final Report. Available at <http://www.house.gov/jec/imf/meltzer.htm>
- International Monetary Fund Central Files, Various.
- International Monetary Fund. 1977. *The Monetary Approach to the Balance of Payments*. Washington: International Monetary Fund.
- International Monetary Fund. 1987. Theoretical Aspects of the Design of Fund-Supported Adjustment Programs. Occasional Paper No. 55.
- International Monetary Fund. 1996. *Financial Programming and Policy: The Case of Sri Lanka*. Washington: IMF Institute.
- International Monetary Fund. 1997. The ESAF at Ten Years: Economic Adjustment and Reform in Low-Income Countries. Occasional Paper No. 156.
- International Monetary Fund. 1997. IMF Adopts Guidelines Regarding Governance Issues. News Brief No. 97/15, August 4, 1997.

International Monetary Fund. 1999. *Annual Report of the Executive Board for the Financial Year Ended April 30, 1999*. Washington: International Monetary Fund.

International Monetary Fund. 1999. *External Evaluation of Fund Surveillance: Report by A Group of Independent Experts*. Washington: International Monetary Fund.

International Monetary Fund, Evaluation Group of Executive Directors. 2000. Review of Experience with Evaluation in the Fund. (March 14 2000).  
<http://www.imf.org/external/np/eval/2000/031400.HTM>

International Monetary Fund. 2001. Conditionality in Fund Supported Programs: External Consultations. (July 17, 2001).  
<http://www.imf.org/external/np/pdr/cond/2001/eng/collab/econ.htm>

International Monetary Fund. 2001. IMF Reviews Strengthening Country Ownership of Fund Programs. Public Information Notice No. 01/125 December 14, 2001.

International Monetary Fund Policy Development and Review Department. 2001. Conditionality in IMF Supported Programs–Policy Issues. February 16, 2001

International Monetary Fund Independent Evaluation Office. 2001. Defining the 2002-2005 Work Program. October 19, 2001

International Monetary Fund Policy Development and Review Department. 2001. Review of the Fund's Experience in Governance Issues. March 28, 2001

International Monetary Fund Policy Development and Review Department. 2002. The Modalities of Conditionality–Further Considerations. January 8, 2002.

International Monetary Fund Independent Evaluation Office. 2002. Prolonged Use of Fund Resources: Terms of Reference for the Evaluation. March 15, 2002.

Jagers, Keith, and Ted Robert Gurr. *Polity III: Regime Change and Political Authority, 1800-1994* [Computer file]. 2nd ICPSR version. Boulder, CO: Keith Jagers/College Park, MD: Ted Robert Gurr [producers], 1995. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [distributor], 1996.

James, Harold 1996. *International Monetary Cooperation Since Bretton Woods* New York: Oxford University Press.

James, Harold. 1998. From Grandmotherliness to Governance: The Evolution of IMF Conditionality. *Finance and Development* 35:4.

Jervis, Robert 1978. Cooperation Under the Security Dilemma. *World Politics* 30(2):167-214.

- Johnson, John H. and Sulaiman S. Wasty. 1993. Borrower Ownership of Adjustment Programs and the Political Economy of Reform. World Bank Discussion Paper 199.
- Joyce, Joseph P. 1992. The Economic Characteristics of IMF Program Countries. *Economics Letters* 38(2):237-242.
- Kahler, Miles. 1989. International Financial Institutions and the Politics of Adjustment. In Joan M. Nelson, ed., *Fragile Coalitions: The Politics of Economic Adjustment* Washington: Overseas Development Council, pp. 139-159.
- Kahler, Miles. 1990. Orthodoxy and Its Alternatives: Explaining Approaches to Stabilization and Adjustment. In Joan Nelson, ed., *Economic Crisis and Policy Choice*. Princeton: Princeton University Press, pp. 33-63.
- Kahler, Miles. 1990. The United States and the International Monetary Fund: Declining Influence or Declining Interest? In Margaret P. Karns and Karen A. Mingst, eds., *The United States and Multilateral Institutions*. Boston: Unwin Hyman, pp. 91-114.
- Kahler, Miles. 1992. External Influence, Conditionality, and the Politics of Adjustment. In Stephan Haggard and Robert R. Kaufman, eds., *The Politics of Economic Adjustment*. Princeton: Princeton University Press, pp. 89-139.
- Kahler, Miles. 1993. Bargaining with the IMF: Two-Level Strategies and Developing Countries. In Peter Evans, Harold K. Jacobson, and Robert Putnam, *Double Edged Diplomacy* Berkeley, University of California Press, pp. 363-394.
- Kapur, Devesh and Richard Webb. 2000. Governance Related Conditionalities of the International Financial Institutions. G-24 Discussion Paper Series, United Nations, August 2000.
- Kapur, Devesh. 1998. The IMF: A Cure or A Curse? *Foreign Policy* 114:114-126.
- Katzenstein, Peter ed., 1978. *Between Power and Plenty*. Madison: University of Wisconsin Press.
- Kaufman, Robert R. Carlos Bazdresch, and Blanca Heredia. 1994. Mexico: Radical Reform in a Dominant Party System. In Stephan Haggard and Steven B. Webb, eds., *Voting For Reform* New York: Oxford University Press, pp. 360-411.
- Kaufman, Robert R. and Barbara Stallings. 1991. The Political Economy of Latin American Populism. In Rudiger Dornbusch and Sebastian Edwards, eds., *The Macroeconomics of Populism in Latin America* Chicago: University of Chicago Press.
- Kaufman, Robert R. 1985. Democratic and Authoritarian Responses to the Debt Issue: Argentina, Brazil, Mexico. *International Organization* 39:473-503.

Kaufman, Robert R. 1991. *Stabilization and Adjustment in Argentina, Brazil, and Mexico* in Joan Nelson ed. *Economic Crisis and Policy Choice* Princeton: Princeton University Press, pp. 63-112.

Kearns, Ian. 1994. Eastern and Central Europe in the World Political Economy. In Richard Stubbs and Geoffrey R.D. Underhill, eds. *Political Economy and the Changing Global Order* New York: St. Martin's Press, pp. 378-390.

Keohane, Robert O. and Lisa Martin. 1995. The Promise of Institutional Theory. *International Security* 20:39-51.

Keohane, Robert O. 1984. *After Hegemony* (Princeton: Princeton University Press, 1984).

Keohane, Robert O. 1993. Institutional Theory and the Realist Challenge After the Cold War. In David Baldwin, ed. *Neorealism and Neoliberalism* New York: Columbia University Press, pp. 269-300.

Khan, Mohsin S. and Sunil Sharma. 2001. IMF Conditionality and Country Ownership of Programs. IMF Working Paper 01/142, September 1, 2001.

Khan, Mohsin. 1990. The Macroeconomic Effects of Fund-Supported Adjustment Programs. *IMF Staff Papers* 37(2):195-231.

Killick, Tony, ed. 1984. *The Quest for Economic Stabilization* New York: St. Martin's.

Killick, Tony. 1995. Conditionality and the Adjustment-Development Connection. *Pakistan Journal of Applied Economics* 11(1-2):17-36.

Killick, Tony. 1995. *IMF Programs in Developing Countries: Design and Impact*. London: Routledge.

Killick, Tony. 1996. Principals, Agents, and the Limitations of BWI Conditionality. *World Economy* 19(2):211-229.

Killick, Tony. 1998. *Aid and the Political Economy of Policy Change*. London: Routledge.

King, Gary, Robert Keohane, and Sidney Verba. 1994. *Designing Social Inquiry*. Princeton: Princeton University Press.

King, Gary. 1989. *Unifying Political Methodology*. London: Cambridge University Press.

King, Gary, James Honaker, Anne Joseph, and Kenneth Scheve. 2001. Analyzing Incomplete Political Science Data: An Alternative Algorithm for Multiple Imputation. *American Political Science Review* 95(1):49-69.

- Knight, Malcolm and Julio Santaella. 1997. Economic Determinants of Fund Financial Arrangements. *Journal of Development Economics* 54(2):405-436.
- Koh, Harold H. 1997. Why Do Nations Obey International Law? *Yale Law Journal* 106(8):2599-2659.
- Kontopoulos, Yianos and Roberto Perotti. 1999. Government Fragmentation and Fiscal Policy Outcomes: Evidence from OECD Countries. In James M. Poterba and Jurgen von Hagen, eds., *Fiscal Institutions and Fiscal Performance* Chicago: University of Chicago Press, pp. 81-102.
- Kotowitz, Y. 1989. Moral Hazard. In John Eatwell, Murray Milgate, and Peter Newman, eds., *Allocation, Information, and Markets* New York: W.W. Norton, pp. 207-214.
- Krasner, Stephen D. 1985. *Structural Conflict: The Third World Against Global Liberalism* Berkeley: University of California Press.
- Krasner, Stephen D. 1991. Global Communications and National Power: Life on the Pareto Frontier. *World Politics* 43:336-366.
- Krueger, Anne O. and Ilter Turan. 1993. The Politics and Economics of Turkish Policy Reforms in the 1980s. In Robert H. Bates and Anne O. Krueger., eds., *Political and Economic Interactions in Economic Policy Reform* New York: Blackwell, pp. 333-387.
- Krugman, Paul. 1979. A Model of Balance-of-Payments Crises. *Journal of Money, Credit & Banking* 11(3):311-325.
- Laakso, Markku and Rein Taagepera. 1979. The "Effective" Number of Parties: A Measure with Applications to West Europe. *Comparative Political Studies* 12(1):3-28.
- Lake David A. and Robert Powell, eds. 1999. *Strategic Choice and International Relations* Princeton: Princeton University Press.
- Lake, David A. 1993. Leadership, Hegemony, and the International Economy: Naked Emperor or Tattered Monarch with Potential? *International Studies Quarterly* 47:459-489.
- Lake, David A. 1996. Anarchy, Hierarchy, and the Variety of International Relations. *International Organization* 50(1):1-34.
- Lal, Deepak and Sylvia Maxfield. 1993. The Political Economy of Stabilization in Brazil. In Robert H. Bates and Anne O. Krueger., eds., *Political and Economic Interactions in Economic Policy Reform* New York: Blackwell, pp. 27-78.

Lane, Timothy, Atish Ghosh, Javier Hamann, Steven Phillips, Marianne Schulze-Ghattas, and Tsidi Tsikata. 1999. IMF Supported Programs in Indonesia, Korea, and Thailand: A Preliminary Assessment. International Monetary Fund Occasional Paper 178.

Leeds, Brett Ashley. 1999. Domestic Political Institutions, Credible Commitments, and International Cooperation. *American Journal of Political Science* 43(4):979-1002.

Leone, Alfredo. 1991. Effectiveness and Implications of Limits on Central Bank Credit to the Government. In Patrick Downes and Reza Vaez-Zadeh, eds., *The Evolving Role of Central Banks* IMF: Washington, pp. 363-413.

Levy, Jack S. 1989. The Causes of War: A Review of Theories and Evidence. In Philip Tetlock, Jo Husbands, Robert Jervis, Paul Stern, and Charles Tilly, eds. *Behavior, Society, and Nuclear War* New York: Oxford University Press, pp. 209-333.

Levy, Jack S. 1996. Contending Theories of International Conflict. In Chester Crocker and Fen Osler Hampson with Pamela Aall, eds. *Managing Global Chaos* Washington: United States Institute of Peace Press, pp. 3-24.

Lipson, Charles. 1984. International Cooperation in Economic and Security Affairs. *World Politics* 37(1):1-23.

Lipson, Charles. 1991. Why Are Some International Agreements Informal? *International Organization* 45(4):495-538.

Lohmann, Susanne 1993. Electoral Cycles and International Policy Coordination. *European Economic Review* 37(7):1373-1391.

Long, J. Scott. 1997. *Regression Models for Categorical and Limited Dependent Variables* Thousand Oaks: Sage Publications.

Manning, Willard G, Naihua Duan, and W. H. Rogers. 1987. Monte Carlo Evidence on the Choice between Sample Selection and Two-Part Models. *Journal of Econometrics* 35(1):59-82.

Marshall, Katherine. 1997. Emerging from Conflict: What Roles for the International Development Finance Institutions? Harvard Institute for International Development Discussion Paper 587.

Martin, Lisa L. and Beth A. Simmons. 1998. Theoretical and Empirical Studies of International Institutions. *International Organization* 52(4):729-757.

Maxfield, Sylvia 1997. *Gatekeepers of Growth* Princeton: Princeton University Press, 1997.

- McCubbins, Mathew and Thomas Schwartz. 1984. Congressional Oversight Overlooked: Police Patrols versus Fire Alarms. *American Journal of Political Science* 28(1):165-179.
- McDonald, Judith Ann. 1986. Factors Influencing Countries' Decisions to Use Credit From the International Monetary Fund. Ph.D. Diss. Department of Economics, Princeton University.
- Mearsheimer, John J. 1994/1995. The False Promise of International Institutions. *International Security* 19:5-49.
- Mecagni, Mauro. 1999. The Causes of Program Interruptions. In Hugh Bredenkamp and Susan Schadler, eds. *Economic Adjustment and Reform in Low Income Countries* Washington: IMF, pp. 215-276.
- Meltzer, Allan H. 1998. Asian Problems and the IMF. *The Cato Journal* 17(3):267-275.
- Milner, Helen. 1997. *Interests, Institutions, and Information* Princeton: Princeton University Press.
- Mercer-Blackman, Valerie and Anna Unigovskaya. 2000. Compliance with IMF Program Indicators and Growth in Transition Economies. IMF Working Paper WP/00/47.
- Millimet, Daniel. 2001. Endogeneity Versus Sample Selection Bias. Available from <http://www.stata.com/support/faqs/stat/bias.html>
- Mitchell, Ronald B. 1994. *Intentional Oil Pollution at Sea : Environmental Policy and Treaty Compliance* Cambridge: MIT Press.
- Mo, Jongryn. 1995. Domestic Institutions and International Bargaining: The Role of Agent Veto in Two-Level Games. *American Political Science Review* 89(4):914-924.
- Moravcsik, Andrew. 1993. Integrating International and Domestic Theories of International Bargaining. In Peter B. Evans, Harold K. Jacobson, and Robert D. Putnam, eds., *Double-Edged Diplomacy* Berkeley: University of California Press, pp. 3-42.
- Moravcsik, Andrew. 1997. Taking Preferences Seriously: A Liberal Theory of International Politics. *International Organization* 51(4):513-553.
- Morgenthau, Hans J. 1985. *Politics Among Nations: The Struggle For Power and Peace* Sixth Edition New York: Knopf.
- Morrow, James D. 1989. Capabilities, Uncertainty, and Resolve: A Limited Information Model of Crisis Bargaining. *American Journal of Political Science* 33(4):941-973.
- Morrow, James D. 1994. Modeling the Forms of International Cooperation: Distribution versus Information. *International Organization* 48(3):387-423.

Mosely, Paul, Jane Harrigan, and John Toye 1991. *Aid and Power: The World Bank and Policy-Based Lending* London: Routledge.

Mosley, Paul. 1987. Conditionality as Bargaining Process: Structural Adjustment Lending, 1980-1986. Princeton Essays in International Finance 168.

Mosley, Paul. 1992. A Theory of Conditionality. In Paul Mosley ed., *Development Finance and Policy Reform* New York: St. Martin's Press, pp. 129-154.

Mussa, Michael and Miguel A. Savastano. 1999. The IMF Approach to Economic Stabilization. IMF Working Paper 99/104.

Mussa, Michael. Forthcoming. Argentina and the Fund: From Triumph to Tragedy. Institute for International Economics Policy Analyses in International Economics 67.

Myers, Robert J. 1987. *The Political Morality of the International Monetary Fund* New Brunswick: Transaction Books.

Myerson, Roger B. 1993. Incentives to Cultivate Favored Minorities under Alternative Electoral Systems. *American Political Science Review* 87(4):856-870.

Nelson, Joan. 1984. The Political Economy of Stabilization: Commitment, Capacity, and Public Response. *World Development* 12(10):983-1006.

Nelson, Joan, ed., 1991. *Economic Crisis and Policy Choice* Princeton: Princeton University Press.

Nelson, Joan. 1996. Promoting Policy Reforms: The Twilight of Conditionality? *World Development* 24(9):1551-1559.

North, Douglass C. and Barry R. Weingast. 1989. Constitutions and Commitment: The Evolution of Institutions Governing Public Choice in Seventeenth-Century England. *The Journal of Economic History* 49(4):803-832.

North, Douglass. 1990. *Institutions, Institutional Change, and Economic Performance* New York: Cambridge University Press.

Oatley, Thomas, and Robert Nabors. 1998. Redistributive Cooperation: Market Failure, Wealth Transfers, and the Basle Accord. *International Organization* 52(1):35-54.

Pauly, Louis. 1997. *Who Elected the Bankers? Surveillance and Control in the World Economy* Ithaca: Cornell University Press.

Polak, Jacques. 1957. Monetary Analysis of Income Formation and Payments Problems. *IMF Staff Papers* 6:1-50.



- Polak, Jacques. 1991. The Changing Nature of IMF Conditionality. Princeton Essays in International Finance 184.
- Powell, Robert. 1994. Anarchy in International Relations Theory: The Neorealist-Neoliberal Debate. *International Organization* 48(2):313-344.
- Przeworski, Adam. 1991. *Democracy and the Market* Cambridge: Cambridge University Press.
- Przeworski, Adam and James Raymond Vreeland. 2000. The Effect of IMF Programs on Economic Growth. *Journal of Development Economics* 62:385-421.
- Putnam, Robert and Nicholas Bayne. 1987. *Hanging Together: Cooperation and Conflict in the Seven-Power Summits* Cambridge: Harvard University Press.
- Putnam, Robert. 1988. Diplomacy and Domestic Politics: The Logic of Two Level Games. *International Organization* 42(2):427-460.
- Reed, William. 2000. A Unified Statistical Model of Conflict Onset and Escalation. *American Journal of Political Science* 44(1):84-93.
- Reichmann, Thomas M. and Richard T. Stillson. 1978. Experience with Programs of Balance of Payments Adjustment: Stand-By Arrangements in the Higher Tranches, 1963-1972. *IMF Staff Papers* 25(2):293-309.
- Reinicke, Wolfgang. 1996. Can International Financial Institutions Help Prevent Violence? The Sources of Ethno-National Conflict in Transitional Societies. In Abram Chayes and Antonia Handler Chayes, eds., *Preventing Conflict in the Post-Communist World* Washington: Brookings Institution, pp. 281-338.
- Remmer, Karen L. 1986. The Politics of Economic Stabilization: IMF Standby Programs in Latin America, 1954-1984. *Comparative Politics* 19:1-24.
- Rodrik, Dani. 1989. Promises, Promises: Credible Policy Reform via Signalling. *Economic Journal* 99:756-772.
- Rodrik, Dani. 1991. Policy Uncertainty and Private Investment in Developing Countries. *Journal of Development Economics*. 36(2):229-242.
- Rodrik, Dani 1995. Why is There Multilateral Lending? National Bureau of Economic Research Working Paper W5160.
- Rodrik, Dani. 1996. Understanding Economic Policy Reform. *Journal of Economic Literature* 34(1):9-41.

Rodrik, Dani. 1998. Where Did All the Growth Go? External Shocks, Social Conflict, and Growth Collapses. National Bureau of Economic Research Working Paper W6350.

Rogowski, Ronald. 1987. Trade and the Variety of Democratic Institutions. *International Organization* 41:203-223.

Roubini, Nouriel and Jeffrey D. Sachs. 1989. Political and Economic Determinants of Budget Deficits in the Industrial Democracies. *European Economic Review* 33(5):903-933.

Roubini, Nouriel . 1991. Economic and Political Determinants of Budget Deficits in Developing Countries. *Journal of International Money and Finance* 10:49-72.

Sachs, Jeffrey D. 1989. Conditionality, Debt Relief, and the Developing Country Debt Crisis. In Jeffrey D. Sachs, ed., *Developing Country Debt and Economic Performance* Chicago: University of Chicago Press, pp. 255-298.

Sanger, David E. "IMF Agrees to Make Loan, but Not Send Cash to Russia" *New York Times* Thursday April 29, 1999, A12..

Santaella, Julio Alfonso. 1992. Stabilization Programs, Credibility, and External Enforcement. Ph.D. Diss., Department of Economics, UCLA.

Santaella, Julio. 1995. Four Decades of Fund Arrangements: Macroeconomic Stylized Facts Before the Adjustment Programs. IMF Working Paper 95/74-EA.

Sartori, Giovanni. 1976. *Parties and Party Systems* New York: Cambridge University Press.

Schadler, Susan et al. 1995. IMF Conditionality: Experiences Under Stand-By and Extended Arrangements, Part I: Key Issues and Findings. IMF Occasional Paper 128.

Schadler, Susan, ed., 1995. IMF Conditionality: Experiences Under Stand-By and Extended Arrangements, Part II: Background Papers. IMF Occasional Paper 129.

Schadler, Susan. 1996. How Successful are IMF Supported Adjustment Programs? *Finance and Development* 33(2):14-17.

Schelling, Thomas C. 1978. *Micromotives and Macrobehavior* New York: W.W. Norton.

Scholte, Jan Aart. 1998. The IMF Meets Civil Society. *Finance and Development* 35(3):42-45.

Schraeder, Peter J. and Steven W. Hook. 1998. Clarifying the Foreign Aid Puzzle: A Comparison of American, Japanese, French, and Swedish Aid Flows *World Politics* 50(2):294-324.

- Schultz, Kenneth and Barry Weingast. 1998. Limited Governments, Powerful States. In Randolph M. Siverson, ed. *Strategic Politicians, Institutions, and Foreign Policy* Ann Arbor, University of Michigan Press, pp. 15-50.
- Schultz, Kenneth A. 1998. Domestic Opposition and Signaling in International Crises *American Political Science Review* 92(4):829-845.
- Schweller, Randall and David Priess. 1997. A Tale of Two Realisms: Expanding the Institutions Debate. *Mershon International Studies Review* 41(1):1-32.
- Shepsle, Kenneth. 1991. Discretion, Institutions, and the Problem of Government Commitment. In Pierre Bourdieu and James Coleman, eds. *Social Theory for a Changing World* Boulder: Westview Press, pp. 245-262.
- Signorino, Curtis S. and Jeffrey M. Ritter. 1999. Tau-b or Not Tau-b: Measuring the Similarity of Foreign Policy Positions. *International Studies Quarterly* 43(1):115-144.
- Signorino, Curtis S. 1999. Strategic Interaction and the Statistical Analysis of International Conflict. *American Political Science Review* 93(2):279-300.
- Simmons, Beth A. 1994. *Who Adjusts?* Princeton: Princeton University Press.
- 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(4):819-835.
- Skidmore, Thomas. 1977. The Politics of Stabilization in Postwar Latin America. In James Malloy, ed., *Authoritarianism and Corporatism in Latin America* Pittsburgh: University of Pittsburgh Press.
- Smith, Alastair. 1995. Alliance Formation and War. *International Studies Quarterly* 39(4):405-426.
- Smith, Alastair. 1999. Testing Theories of Strategic Choice: the Example of Crisis Escalation. *American Journal of Political Science* 43(4):1254-1284.
- Snidal, Duncan. 1985. The Limits of Hegemonic Stability Theory. *International Organization* 39(4):579-614.
- Spence, A. Michael. 1974. *Market Signaling* Cambridge: Harvard University Press.

- Stein, Ernesto, Ernesto Talvi, and Alejandro Grisanti. 1998. Institutional Arrangements and Fiscal Performance: The Latin American Experience. National Bureau of Economic Research Working Paper 6358.
- Stein, Arthur A. 1982. Coordination and Collaboration: Regimes in an Anarchic World. *International Organization* 36(2):299-324.
- Stremmler, John and Francisco Sagasti. 1998. *Preventing Deadly Conflict: Does the World Bank Have a Role?* New York: Carnegie Commission on Deadly Conflict.
- Thacker, Strom C. 1999. The High Politics of IMF Lending. *World Politics* 52(1):38-75.
- Tsebelis, George. 1995. Decision Making in Political Systems: Veto Players in Presidentialism, Parliamentarism, Multicameralism, and Multipartyism. *British Journal of Political Science* 25(3): 289-325.
- Ul-Haq, Nadeem, and Mohsin S. Khan. 1998. Do IMF-Supported Programs Work? A Survey of the Cross-Country Empirical Evidence. IMF Working Paper 98/169.
- Underdal, Arild. 1998. Explaining Compliance and Defection: Three Models. *European Journal of International Relations* 4(1):5-30.
- Vaubel, Roland. 1991. The Political Economy of the International Monetary Fund: A Public Choice Analysis. In Roland Vaubel and Thomas D. Willett, eds. *The Political Economy of International Organizations* Boulder: Westview Press, pp. 204-244.
- Vaubel, Roland 1996. Bureaucracy at the IMF and the World Bank: A Comparison of the Evidence. *World Economy* 19(2):195-210.
- Vreeland, James Raymond. 2001. Institutional Determinants of IMF Agreements. Paper prepared for delivery at the 2001 Annual Meeting of the American Political Science Association, San Francisco, August 30-September 2, 2001.
- Walton, John and Charles Ragin. 1990. Global and National Sources of Political Protest: Third World Responses to the Debt Crisis. *American Sociological Review* 55:876-890.
- Waltz, Kenneth, 1979. *Theory of International Politics* New York: Addison-Wesley.
- Weaver, R. Kent and Bert A. Rockman, eds. 1993. *Do Institutions Matter?* Washington: Brookings Institution Press.
- Whitehead, Laurence. 1990. Political Explanations of Macroeconomic Management: A Survey. *World Development* 18:1133-1146.
- Wibbels, Erik. 2000. Federalism and the Politics of Macroeconomic Policy and Performance. *American Journal of Political Science* 44(4):687-703.

Willett, Thomas D. 2000. A Soft Core Public Choice Analysis of the International Monetary Fund. Working Paper, Claremont Graduate University.

Williamson, John and Stephan Haggard. 1994. The Political Conditions for Economic Reform. In John Williamson ed. *The Political Economy of Policy Reform* Washington: Institute for International Economics, pp. 527-596.

Williamson, John. 1994. In Search of a Manual for Technopols. In John Williamson, ed. *The Political Economy of Policy Reform* Washington: Institute for International Economics, pp. 11-28.

Williamson, Oliver. 1985. *The Economic Institutions of Capitalism* New York: Free Press.

Wilson, Charles 1987. Adverse Selection. In John Eatwell, Murray Milgate, and Peter Newman eds., *Allocation, Information and Markets* New York: W.W. Norton.

World Bank Development News, Friday April 30 1999.

World Bank. 1995. *Bureaucrats in Business: The Economics and Politics of Government Ownership* Washington: The World Bank.

World Bank. 1997. *World Development Report 1997: The State in a Changing World* New York: Oxford University Press.

World Bank. 1999. *Assessing Aid: What Works, What Doesn't, and Why* New York: Oxford University Press for the World Bank.

Young, Oran. 1979. *Compliance and Public Authority* Baltimore: Johns Hopkins University Press.

Young, Oran. 1999. *Governance in World Affairs* Ithaca: Cornell University Press.

## Martin S. Edwards

### EDUCATION:

- 1995-2002 Rutgers University, New Brunswick, NJ.  
 Major Field: International Relations  
 First Minor Field: American Politics  
 Second Minor Field: Comparative Politics
- Dissertation: "Institutions, Incentives, and International Monetary Fund Compliance"
- 1991-1992 Columbia University, New York, NY  
 M.A. in Political Science.
- 1987-1990 University of Michigan, Ann Arbor, MI  
 B.A. in Political Science.

### TEACHING EXPERIENCE:

- 2001-2002 University of Michigan, Ann Arbor. Visiting Instructor. Teaching responsibilities included International Political Economy, American Foreign Policy, and Seminars on American Foreign Policy and on the International Monetary Fund.
- 2001 Rutgers University, Department of Political Science. Adjunct Instructor. Taught Defense Policy.
- 1999-2000 Rutgers University, Department of English. Teaching Assistant, Writing Program. Taught two sections of Expository Writing I (Fall).
- 1998-1999 Rutgers University, Department of Political Science. Teaching Assistant. Led discussion sections for Introduction to International Relations (Fall) and Introduction to American Government (Spring).
- 1998 Rutgers University, Department of Political Science. Adjunct Instructor. Taught American Foreign Policy.
- 1996 Rutgers University, Department of Political Science. Adjunct Instructor. Taught Defense Policy (Spring and Summer).

### PUBLICATIONS:

- Contributor, "External Comments and Contributions on IMF Conditionality" International Monetary Fund, September  
 2001.<http://www.imf.org/external/np/pdr/cond/2001/eng/collab/comment.pdf>

"Reevaluating the Catalytic Effect of IMF Programs" Center for Global Security and Democracy Working Paper *Columbia International Affairs Online*, Columbia University Press. April 2001. <https://www.cc.columbia.edu/sec/dlc/ciao/wps/edm01/edm01.html>

"Continuity and Change in the Evolution of Warfare" (co-authored with Jack S. Levy and Thomas C. Walker) in Zeev Maoz and Azar Gat, eds., War in a Changing World (University of Michigan Press, Forthcoming).

Review of Lights, Camera, War: Is Media Technology Driving International Politics? by Johanna Neuman, *National Security Studies Quarterly* Spring 1997.