The International Monetary Fund enforces the conditionality attached to its lending facilities by suspending disbursement of loan installments, or tranches, if borrowing countries fail to implement the associated conditions. The IMF Executive Board formally approves all disbursements of Fund resources. When a performance criterion is not implemented by its review date, this provokes an automatic suspension of the corresponding disbursement unless the Board decides to issue a waiver or modify the conditions. According to Staff, the requirement to seek Executive Board approval for changes to conditionality can constrain Management's discretion and consequently reinforce its bargaining position with country authorities. However, Management has discretion to recommend waivers or modifications to the Board or to adjust the schedule of reviews and disbursements, and in practice its recommendations are not overruled. The combination of Management discretion and consensus decision making make it easy for major shareholders to use their informal influence to urge Management to propose waivers for favored client states. Since the status quo outcome is that the program is suspended until every performance criterion is met, shareholders' informal influence has the effect of relaxing the enforcement of conditionality.

Management's dilemma is as follows. After a program goes off track, it is generally optimal to modify it, because the original macroeconomic forecast is no longer valid and key performance indicators may no longer be achievable. Furthermore, even when a program goes off track because the government has made political decisions not to implement its conditions, it is optimal ex post to renegotiate in order to give the government incentives to modify its policies when the original set of targets is no longer

realistic. The problem is that governments know that it is optimal for Management to renegotiate ex post, and this creates moral hazard: governments have weak incentives to implement conditionality if they anticipate that they will be rewarded with weaker conditionality when they renege on their commitments. The IMF Management tries to ameliorate the moral hazard problem by developing a reputation for enforcing conditionality rigorously, and resists efforts by shareholders to undermine its reputation; but it is ultimately understood by all of the participants that different rules, and a different reputation, apply to countries that have substantial influence in Washington (Stone 2002, 2004).

The key argument of *Lending Credibility* was that the IMF's reputation is built on differentiated strategies for enforcing reputation once a program goes off track (Stone 2002). Some countries face rigorous enforcement: no modifications or waivers; disbursements are delayed until conditions are implemented. Other countries are subject to a fluid set of conditions that are renegotiated periodically as the targets are missed. The first set of countries should miss their targets less frequently, but face lengthier program suspensions when they fail to implement their conditions. The second group of countries receives more waivers and faces shorter program suspensions, because there is pressure on the IMF Management to renegotiate their targets in order to bring them back on track. As a result, they have weak incentives to implement conditions, and they face frequent program suspensions.

The United States has used informal contacts to obtain waivers for a number of countries that play important roles in U.S. foreign policy, including Zaire and the Philippines in the 1980s, Egypt in the early 1990s, and subsequently Pakistan and

Turkey. Among the post-Communist countries, Russia and Ukraine have frequently received waivers because of direct contacts by U.S. officials, which have sometimes occurred at the highest level. My previous study of 26 post-Communist countries found that countries that received substantial amounts of U.S. foreign aid were subject to much shorter program suspensions when their programs went off track. They received waivers or their conditions were modified so that they could quickly get back into good standing. As a result of the weak incentives that they faced, their economic policies were more inflationary, and they failed to implement conditions and went off track more frequently (Stone 2002). Another study of 53 African countries revealed a similar pattern with respect to U.S. foreign aid, and also found that countries with close ties to France and Britain received similar treatment (Stone 2004). Using different samples, Edwards (2005) also finds that U.S. aid decreases the probability of program interruptions, and Pop-Eleches (2009) finds that states with voting patterns similar to the United States in the UN general assembly have a lower probability of program interruptions.

This chapter extends and revises these findings in several respects. First, it takes advantage of the data on conditionality in the MONA database to refine its statistical tests, as explained below. Second, it uses the theoretical model of informal governance to generate more precise hypotheses. As the previous chapter explained, the informal governance model predicts a conditional effect of measures of U.S. interests on IMF policies: U.S. intervention should only be observed when the borrowing country is important to the United States and has an intense need for IMF support. Third, this chapter explores the robustness of these findings by using multiple measures of U.S. interests: foreign aid, bank exposure, exports, UN voting affinities, and military alliance

patterns. These measures are only weakly correlated and capture different dimensions of U.S. foreign policy interests, so effects that are robust across measures are persuasive evidence of a pattern of deliberate intervention.

In addition, this chapter explores the collective governance of the Fund by examining the evidence of U.S. control as opposed to more widely shared influence by the G-5—the United States, Japan, Germany, the United Kingdom, and France—which are the largest IMF shareholders and the only countries to appoint (rather than elect) their own Executive Directors. The model of informal governance is consistent with informal control either by a single leading state or by a group of leading states, and other international organizations demonstrate each pattern, but I have argued that the IMF is an example of unusually strong U.S. control. An advantage of using multiple measures of U.S. interests is that some of the measures, particularly foreign aid and alliances, are weakly correlated with the corresponding measures for the other G-5 countries and consequently provide strong tests of U.S. vs. G-5 effects.

To foreshadow, the chapter finds strong evidence of U.S. influence over the enforcement of conditionality that is robust across measures, and a striking pattern that confirms that this influence is invoked only when countries are vulnerable to sudden stops of international financing. Consistent with the model in *Lending Credibility*, this influence is exerted after a disbursement is suspended and affects the duration of the suspension. Where comparative tests are possible, the evidence points to U.S. influence rather than collective governance by the G-5, although some of the results are inconclusive. The argument is illustrated with two illustrative case studies, Russia (1996-

98) and Argentina (2000-01), which represent distinct reasons for urgent U.S. intervention.

Data

The data cover 99 countries that participated in IMF programs between 1992 and 2002. During this period, 92 of the 99 countries experienced at least one program suspension, for a total of 752 program interruptions. In this set, 78 countries experienced short program interruptions of one or two months in duration an average of 4.7 times; 77 countries experienced suspensions of 3 to 8 months an average of 3 times; 54 countries experienced suspensions of 7 months or more an average of 1.5 times; and ten countries experienced very long interruptions of 24 months or longer.

The MONA data make it possible to overcome some important data limitations of previous studies of IMF program enforcement. First, previous work has had to rely on interpolation to determine when an IMF program was suspended. The IMF does not announce when a program goes off track, it simply suspends the next scheduled tranche of a loan facility. Thus, the only available measure of program suspensions was an interruption in the pattern of loan disbursements; but it was not always clear when the next scheduled disbursement was supposed to occur, or when disbursements were cancelled at the request of country authorities (as happened in Poland in 1994, for example) rather than because of non-compliance. The MONA data include the schedule of disbursements and its subsequent modifications, so it is possible to fix the exact date of program suspensions.

Second, previous studies had no independent measure of program compliance. Non-compliance was inferred when programs were suspended, and often the undisbursed portion of a facility was used as a proxy for partial compliance, but it was impossible to observe non-compliance that was not accompanied by a program suspension. The MONA data include the content of conditionality being tested at each review date and indicate which conditions were judged by the IMF to have been implemented, so it is possible to measure compliance with conditionality independently from enforcement. This avoids serious problems of inference. For example, if we observed in a previous study that democracies were subject to less lengthy program suspensions (Stone 2002, 2004), we could not be certain whether this occurred because democracies implemented their programs better and got back on track more quickly than autocracies, or because the IMF was less rigorous in punishing democracies.[1] More broadly, because studies of implementation and enforcement used the same dependent variables, it was impossible to determine whether any of the effects found were due to variations in implementation or to variations in enforcement—or whether non-findings were due to contradictory effects that cancelled each other out. Using independent measures of compliance and suspension, we can resolve these issues.

Third, previous studies were unable to control for variations in conditionality that might affect compliance and enforcement. As we saw in the previous chapter, the substantive scope of conditionality varies substantially, and there is no reason to expect it to be equally difficult to achieve compliance with narrowly defined programs and with sweeping reform plans, which typically involve many more structural benchmarks. In

[1] The results of the analysis reported below indicate that the rigor of enforcement does not depend upon Polity scores, which suggests that these effects had been correctly attributed to variations in implementation in these earlier studies.

addition, it is reasonable to expect the IMF to take the difficulty of compliance into account when determining whether to declare a program off track. As we will see below, the number of categories of conditions that have not been implemented is a strong predictor of the duration of program suspensions, while the number of categories of conditions being tested is strongly associated with leniency. Controlling for conditionality and implementation removes an important source of heterogeneity in the data, and also makes it unnecessary to control for domestic political factors that are related to implementation when we study enforcement.

The independent variables of interest in the analysis represent alternative measures of U.S. motivations to intervene on behalf of particular countries. The concept of informal influence is very general, as the United States could exert its influence on behalf of any country in which it perceives a critical interest, and the model does not define what those critical interests may be. Indeed, the model assumes that these interests change substantially over time, and are not fully predictable. The results will be robust, and therefore persuasive, to the extent that a range of subjectively valid measures that are not highly correlated perform similarly. Preferably, these measures should relate to a range of U.S. economic, political and military interests. For this analysis, I focus on U.S. foreign aid, the exposure of U.S. banks, U.S. exports, affinity in voting in the UN General Assembly, and similarity in military alliance profiles.

Foreign aid is a diffuse measure of influence because donors give aid for a variety of reasons, but it has the virtues of being an instrument that the United States controls directly, and of being a monetary measure with budgetary consequences. The distribution of aid across countries reflects the relative priority that donors attach to them,

and it can vary substantially as those priorities shift. Aid is not particularly effective at promoting goals such as economic or human development, which is consistent with the interpretation that the aid is tied to other agendas besides promoting development.[2] In the analysis that follows, I assume that foreign aid is an investment in a relationship with a leader or regime, representing a direct monetary measure of the importance of a particular recipient to a particular donor. Aid is measured in millions of U.S. dollars—not normalized by GDP—because it is used as a measure of U.S. interests expressed by aid, rather than the degree of the recipient's dependence.

The next two measures, bank exposure and exports, provide straightforward measures of economic interests as they relate to international finance and trade. A number of the countries that have been at the epicenter of international financial crises, such as Mexico, Brazil, Argentina and South Korea, are important to the United States because they are at the center of webs of financial transactions that imperil American financial institutions. Much of this debt is held by individuals and dispersed institutions in the form of securities, and bondholders have weak incentives to organize collectively, so aggregate debt is a poor instrument for U.S. financial interests. Banks, however, organize collectively, cooperate with national regulatory agents in refinancing debt, and represent concentrated interests, as well as playing a critical role as financial intermediaries at home. Consequently, bank exposure is the best available measure of financial interests (Copelovitch forthcoming). The data are from the Bank for International Settlements and are measured as the proportion of total U.S. bank lending to foreign countries and institutions owed by a particular country. Exports represent the commercial stake of the U.S. economy in prosperity abroad. Financial crises abroad

[2] See, for example, Burnside and Dollar 2000; Dollar and Pritchett 1998; Knack 2001.

reduce demand for U.S. products, and this is of greater concern to policymakers if the countries involved are major U.S. trade partners. Aggregate exports do not capture the ability of particular industries to organize collectively, however, so the effect of trade exposure is expected to be primarily a function of preemptive action by policymakers rather than a response to lobbying. Exports are measured in millions of U.S. dollars to provide a common metric of the relative importance of alternative export markets to U.S. producers.

A growing literature measures the political affinity of countries for potential foreign patrons by using measures of the similarity of their votes in the United Nations General Assembly (Barro and Lee 2002, Oatley and Yackee 2000, Thacker 1999). Two effects of political affinity are possible: it may be the case that countries that vote with the United States are favored; alternatively, it may be the case that habitual U.S. opponents in the United Nations are favored because the United States uses IMF loans as a way to buy support from skeptical countries during crises. The informal governance model does not speak to this question, since its prediction is about the intensity of U.S. interests, which varies over time, rather than about the source of these interests. If U.S. interests primarily relate to saving sympathetic regimes from the political consequences of financial crises, the former effect should predominate—the United States will lend its influence to prop up the fortunes of like-minded leaders. On the other hand, if they primarily concern the construction of ad hoc "coalitions of the willing" to achieve particular objectives, the latter effect should be more important, because countries with policy preferences close to those of the United States will not demand bribes in return for their cooperation. Either effect is consistent with the theoretical argument, because there

is no reason other than political manipulation to expect to find a correlation between UN voting and IMF lending. In either case, however, the theory predicts a conditional effect, so the hypothesis will be disconfirmed if the effect of affinity is not conditional on vulnerability to withdrawal of external financing. Political affinity is measured as an S-score, which summarizes the similarity between two voting profiles as the length of a line segment connecting two points in a multidimensional issue space, normalized to [-1,1] (Ritter and Signorino 1999, Gartzke, Jo and Tucker 1999).

Finally, alliance patterns measure a sharper dimension of alignment than UN voting, capturing formal military commitments. Countries located near the center of the web of U.S. military and strategic commitments can call upon the United States for support when they get into financial difficulties because issues that affect allies are associated with intense U.S. preferences. As U.S. allies or allies of close U.S. allies, their critical concerns become important national security issues. Furthermore, because of the military and diplomatic implications of formal alliances, U.S. allies have entre into levels of the U.S. government that other countries cannot reach, and have counterparts in the U.S. State and Defense Departments who can be called upon to argue their cases with the officials in Treasury who deal with the IMF. The similarity of each country's alliance portfolio to that of the United States is measured by an S-score that ranges from -1 (most dissimilar) to 1 (most similar).

These five dimensions of U.S. interests measure distinct reasons for the United States to have intense interests in a particular country, so it is unsurprising that they are not strongly correlated. The correlations for the set of countries that participated in IMF programs are presented in Table 7.1. [Table 7.1 about here]

Table 7.1 Correlations Among Measures of U.S. Interests

	U.S. aid	Bank exposure	U.S. exports	UN voting	Alliances
U.S. aid	1.00				
Bank exposure	-0.02	1.00			
U.S. exports	0.004	0.60	1.00		
UN voting	-0.08	-0.11	-0.10	1.00	
Alliances	-0.03	0.36	0.26	-0.02	1.00

Correlations calculated for countries under IMF programs, 7,053 observations

Foreign aid is a monetary measure of how much importance the donor attaches to a particular country or regime, but says nothing about why particular countries are important. Bank exposure and exports, on the other hand, measure narrow economic interests that motivate U.S. intervention on behalf of particular countries. Votes in the UN General Assembly capture the similarity of two countries' foreign policies. Alliance portfolios represent military commitments for mutual defense, which are associated with intense national security concerns. Taken together, these variables offer a nuanced view of the politics of enforcing IMF conditionality programs.

The analysis proceeds as follows. The key dependent variable is the duration of suspension episodes. The logic of *Lending Credibility* (Stone 2002) was that Board members exert influence after a program has been suspended to shorten the duration of suspensions. They do this by lobbying for waivers of conditionality or modification of its terms, which makes it easier and less politically costly to get programs back on track. In addition, the MONA data allow me to take a closer look at the substantive implications of

lax enforcement by analyzing the number of waivers that are granted when a suspended program comes back into good standing. This provides a second check on the logic of the argument. If it is true that informal influence over enforcement operates through the manipulation of Management's discretion about whether to seek waivers, this discreet influence should leave traces in the pattern of waivers.

Duration of Program Suspensions

The primary means for shareholders to exert influence on behalf of a borrowing country is to contact Fund Management after a program has been suspended. This should have the effect of reducing the duration of program suspensions. The dependent variable for tests of this hypothesis, therefore, is the duration of program suspensions in months. As discussed above, the theory does not pin down which measures of U.S. preferences should be associated with intense motivations to influence conditionality, so my approach is to use a range of subjectively plausible indicators that are weakly correlated with each other and that capture a variety of motivations. The theory of informal governance does make a precise conditional prediction, however: influence should be exerted when there is a combination of latent U.S. interest in the borrowing country and intense need for IMF financing on the part of the borrowing country. In short, the U.S. exerts its influence on behalf of a borrower only when a borrower that has a lot of chips decides it is time to cash them in. Consequently, each of the models that follow use interaction terms between measures of U.S. interests and measures of borrower-country vulnerability to sudden stops of external financing: trade/GDP, debt service/exports, and the percentage of debt held in short-term instruments.

I want to separate the rigor of enforcement from the government's record of implementing conditionality, so in the models that follow I control for the number of categories of conditionality in which the borrower has failed to implement conditions. I also control for the number of categories of conditions covered in the current review, on the assumption that Management makes allowances for the scope of conditionality when assessing country performance. Both of these measures are robustly significant across models: countries that have missed more categories of conditions have longer program suspensions, while countries that were required to implement more conditions have shorter ones. Because I control for conditionality and implementation, I do not have to control for political factors that might affect program suspensions through effects on conditionality or implementation. (It is better to control for the intervening variables.) However, I also control for the possibility that the rigor of IMF enforcement of conditionality systematically depends upon factors such as political regime, GDP per capita, or government capacity. Similarly, central bank reserves in months of imports is used to assess the possibility that bargaining power affects the rigor of enforcement. I find no systematic evidence that these variables affect enforcement once we control for the scope of conditionality and program implementation, although they have substantial effects on conditionality and implementation.

The results of models testing for interactive effects of U.S. aid, U.S. bank exposure, U.S. exports, UN voting affinity and alliance patterns are reported in Table 7.2. The models are parametric duration models using the Weibull distribution, which estimates a monotonically increasing or decreasing hazard rate with time-varying covariates. Observations of suspensions that had not ended by the end of 2002 are right-

censored. The estimated effects of the covariates are presented as hazard ratios in the proportional hazards metric, so hazard ratios greater than one indicate that a covariate

Table 7.2: Duration of Program Interruptions
Weibull regression -- proportional hazard metric

	Haz. Ratio	P>z	Haz. Ratio	P>z	Haz. Ratio	P>z	Haz. Ratio	P>z	Haz. Ratio	P>z
Unmet conditions	0.77494	0.00	0.77633	0.00	0.77646	0.00	0.78396	0.00	0.78067	0.00
	0.01980		*0.02003*		*0.02010*		*0.02025*		*0.02039*	
Scope of conditions	1.07802	0.00	1.08183	0.00	1.08345	0.00	1.07880	0.00	1.08477	0.00
	0.01598		*0.01611*		*0.01618*		*0.01616*		*0.01662*	
U.S. aid	0.99522	0.00								
	0.00167									
U.S bank exposure			3.6E+05	0.29						
			4.4E+06							
U.S. exports					0.99880	0.18				
					0.00089					
UN voting (S-U.S.)							3.90066	0.00		
							1.75808			
U.S. alliance portfolio									1.24201	0.85
									1.45390	
GDP per capita	1.00006	0.03	1.00003	0.36	1.00005	0.16	1.00006	0.03	1.00003	0.35
	0.00003		*0.00003*		*0.00003*		*0.00003*		*0.00003*	
Democracy	0.98676	0.14	0.99072	0.31	0.98992	0.28	0.99248	0.42	0.98959	0.27
	0.00898		*0.00916*		*0.00921*		*0.00929*		*0.00938*	
Missing scale	1.16062	0.70	1.64416	0.19	1.47503	0.31	1.61328	0.23	1.43665	0.35
	0.45038		*0.62440*		*0.56595*		*0.64101*		*0.55647*	
Reserves/imports	0.99163	0.33	0.98638	0.14	0.98655	0.15	0.98912	0.36	0.98615	0.15
	0.00856		*0.00921*		*0.00916*		*0.01177*		*0.00961*	
Trade/GDP	0.99525	0.01	0.99689	0.07	0.99571	0.02	0.99670	0.05	0.99822	0.71
	0.00181		*0.00171*		*0.00175*		*0.00170*		*0.00479*	
Debt service/exports	1.00019	0.96	1.00070	0.87	0.99756	0.58	0.99601	0.33	0.98947	0.29
	0.00369		*0.00428*		*0.00444*		*0.00405*		*0.00984*	
Short-term debt	0.98447	0.25	0.99231	0.54	0.99511	0.69	0.97824	0.15	0.96236	0.09
	0.01327		*0.01245*		*0.01233*		*0.01477*		*0.02198*	
Short-term debt^2	1.00015	0.48	1.00011	0.60	1.00004	0.84	1.00041	0.17	1.00011	0.58
	0.00021		*0.00020*		*0.00020*		*0.00030*		*0.00020*	
Interactions with:										
Trade/GDP	1.00002	0.30	1.14447	0.35	1.00001	0.27	0.99054	0.04	0.99733	0.80
	0.00002		*0.16593*		*0.00001*		*0.00458*		*0.01049*	
Debt service/exports	1.00009	0.11	1.04745	0.73	1.00002	0.05	0.95270	0.00	1.02286	0.15
	0.00006		*0.13796*		*0.00001*		*0.01338*		*0.01611*	
Short-term debt	1.00020	0.02	0.53971	0.24	1.00000	0.96	0.96763	0.20	1.06691	0.11
	0.00008		*0.28187*		*0.00002*		*0.02496*		*0.04261*	
ln(ρ*)*	0.38294	0.00	0.37105	0.00	0.36929	0.00	0.37607	0.00	0.37292	0.00
	0.02471		*0.02441*		*0.02448*		*0.02466*		*0.02466*	
Number of obs	3401		3401		3401		3401		3401	

Note: Standard errors in italics. Regional fixed effects not reported.

reduces the expected duration, and hazard ratios lower than one increase the expected duration. [Table 7.2 about here]

The important conclusion to draw from Table 7.2 is that the interaction effects between each of the measures of U.S. interests and the measures of countries' vulnerability to external financing shocks run consistently in the direction predicted by the theory. Of the fifteen possible interaction effects, three are statistically indistinguishable from zero, and have such small substantive effects that they do not influence the effects of their corresponding measures of U.S. interests (bank exposure x debt service/exports, U.S. exports x short-term debt, and alliances x trade/GDP). One, which will be discussed further below, runs in the opposite direction and has a strong effect (bank exposure x short-term debt). The others run in the predicted direction and have substantively significant results that influence the significance of their respective measures of U.S. interests.

The results of a table such as this are mainly useful for researchers, who can use the table to understand how the models were specified and replicate the results. To understand the substantive significance of the results—whether they are big or small—we have to calculate predicted probabilities or marginal effects, which are presented in Table 7.3. In addition, since the quantities of interest in this case are interactions between variables, the hypothesis tests represented by the significance levels reported in the table are not particularly informative. The significance test for the coefficient for U.S. aid, for example, is only valid when trade/GDP, debt service/exports, and short-term debt are equal to zero. In order to test the hypothesis that U.S. aid has a significant effect when countries are vulnerable to disruptions on international financial markets, I test the joint

effect of the coefficient of U.S. aid and the coefficients of its interactions with trade/GDP, debt service/exports, and short-term debt, evaluated at the desired levels of vulnerability.[3] These are the hypothesis tests reported in Table 7.3. [Table 7.3 about here]

Table 7.3: Duration of Program Suspensions: Substantive Effects

		Median duration (months)	Change in duration (months)	Percentage change	*p*
All variables at means		5.8			
U.S. aid	**Vulnerability**				
	High	3.4	-2.4	-41.4%	0.03
	Mean	5.4	-0.4	-6.9%	0.97
U.S. bank exposure	**Vulnerability**				
	High	4.2	-1.6	-27.6%	0.05
	Mean	4.2	-1.6	-27.6%	0.00
U.S. exports	**Vulnerability**				
	High	3.8	-2.0	-34.5%	0.15
	Mean	6.2	0.4	6.9%	0.52
UN voting opposition	**Vulnerability**				
	High	3.4	-2.4	-41.4%	0.00
	Mean	5.0	-0.8	-13.8%	0.02
Alliance patterns	**Vulnerability**				
	High	3.4	-2.4	-41.4%	0.05
	Mean	5.0	-0.8	-13.8%	0.09

Effect on the median duration of changing the bolded variable by one standard deviation, conditional on the levels of trade/GDP, debt service/exports and short-term debt (%). High signifies one standard deviation above the mean.

[3] The coefficient in the table represents the partial derivative of the duration of program suspensions with respect to U.S. aid, but the total effect of U.S. aid is the total derivative. This differs from the partial derivative because the effect of U.S. aid depends upon the values of the variables with which it is interacted.

Table 7.3 reports the substantive effects calculated from the models reported in Table 7.2. In each case, the effects shown are for a one-standard deviation change in the values of the measures of U.S. interests. "High" levels of vulnerability to external financial shocks are one standard deviation above the sample means, respectively, on each of the three vulnerability measures. When all variables are held at their means, the median predicted duration of a program suspension is 5.8 months. When U.S. foreign aid is increased by one standard deviation and vulnerability measures are at their mean, there is only a small decrease in the predicted duration, to 5.4 months, and the effect is not statistically significant. As the borrower crosses into the top third of the vulnerability distribution, however, the effect of U.S. foreign aid becomes statistically significant, and the expected duration drops 41 percent to 3.4 months. The patterns across the other measures of U.S. interests are broadly similar.

Increasing U.S. bank exposure to a single country by one standard deviation—about 2 percent of total foreign exposure of U.S. banks—decreases the median suspension duration by 27.6 percent to 4.2 months. For the sake of comparison, this represents a quadrupling of the average level of U.S. bank exposure to a particular country, but is substantially lower than the peak levels of exposure in the sample to Argentina (8.8%), Brazil (14.6%) and Mexico (18.5%). The effect of bank exposure on suspensions reported in the table does not appear to vary significantly as external vulnerability changes, but further examination (below) indicates that this is misleading. The effect of U.S. exports has only marginal statistical significance, even at high levels of external vulnerability, but this is not because the estimated effects are small—in fact, the effects of a standard deviation of exports are comparable to those of a standard deviation

of foreign aid, reducing the median duration 34.5 percent, to 3.8 months. Rather, the weak significance is due to the very large standard errors around the estimated effect. The correct interpretation of this result is not that the effect is near zero, but rather that the effect is probably quite large, but cannot be estimated with enough precision to be certain that it is not zero. The estimated effects become substantially larger as the level of vulnerability rises, but so do the standard errors, so that significance levels improve slowly.

UN voting has a strong association with the duration of punishment, and it varies sharply with vulnerability. When borrowers have average exposure to external shocks, the effect of UN voting is to reduce the length of suspensions modestly, by less than one month; when external vulnerability increases by one standard deviation, however, expected suspensions shorten by 41.4 percent, to 3.4 months. As vulnerability drops further, the effect of UN voting becomes insignificant, and when vulnerability drops to very low levels the effect switches signs and significantly extends the length of suspensions. These effects occur, perhaps surprisingly, when countries oppose rather than support U.S. votes in the United Nations. That is, the United States appears to reward recalcitrant regimes by intervening on their behalf—but only when they are vulnerable to financial crises. Recalcitrant regimes that are not vulnerable to sudden stops of external financing do not benefit from U.S. patronage, and in fact have substantially longer than average program suspensions. For countries that oppose the United States in the UN General Assembly one standard deviation more than average, the expected suspension duration is two months longer than average, an increase of almost 35 percent (significant at p=.01).

To put this finding in perspective, consider the names of some of the countries that play important roles in U.S. foreign policy but generally vote against the United States in the UN General Assembly. The average UN S-score in the sample is -.085 on a scale of -1 to 1 (the United States votes in the minority much more often than in the majority on substantive issues, but many procedural votes are uncontested), and drops to -.11 among IMF program participants. The standard deviations are .37 and .31, respectively. Pakistan, which has been an important recipient of U.S. foreign aid and a key partner in U.S. security policy, votes consistently against the United States on issues involving the Middle East, and is approximately one standard deviation more oppositional than the average country at -.41 (minimum -.52, maximum -.25). There can be little doubt that Pakistan's close cooperation with the United States on security issues has been achieved in spite of, rather than because of, the general foreign policy preferences of both its elected and military leaders. Pakistan is a country that has repeatedly failed to fulfill the conditions attached to its programs, and has turned to the United States for help managing the Fund on numerous occasions. Its programs were interrupted for non-compliance seventeen times between 1990 and 2002, or roughly 1.5 times per program year, and the length of those interruptions ranged from very short—1 to 2 months eight times—to two very long suspensions. The last long suspension from IMF and World Bank financing occurred when the Clinton administration punished Pakistan for developing nuclear weapons, and was brought to a close when the Bush administration required Pakistan's assistance in the 2001 war against Afghanistan.

A similar example is Egypt, which has an average S-score of -.37 (minimum -.56, maximum -.18), and frequently opposes the United States on votes involving Israel.

Egypt has been a mainstay of American foreign policy in the Middle East, however, and was explicitly promised U.S. aid in return for peace with Israel in the Camp David Accords. Egyptian troops played a prominent psychological role in the first Gulf War in 1991, and Egypt was rewarded with a dramatic debt reduction package and briefly became the leading recipient of U.S. foreign aid. Egypt supports U.S. initiatives in spite of, rather than because of, the inclinations of its leaders and the pressures of public opinion, and Egypt has been a stark example of the failure to enforce IMF conditionality. In contrast, there are two groups of countries that receive IMF programs and are one standard deviation more supportive than average of U.S. positions in the UN: small former Communist countries, and small, poor countries in sub-Saharan Africa. Most of these countries have no particular political weight and occupy no strategic position that could allow them to extract favors from U.S. policymakers.

Alliance patterns show a strong substantive effect: U.S. allies and countries that are closely allied with U.S. allies have program suspensions that are sharply reduced in length. A country one standard deviation closer than average to the U.S. alliance network has an expected duration that is reduced by over 40 percent to 3.4 months. This effect, again, depends upon vulnerability. The duration rises to five months and the effect of alliance portfolios becomes only marginally significant when vulnerability to external shocks drops to mean values, and durations continue to rise while the effects of alliances become ever more insignificant as vulnerability falls below the mean. These results are easy to interpret. U.S. allies have a constant claim on U.S. attention. Security issues demand high priority, and allies can rely on their counterparts in the U.S. defense and foreign policy bureaucracies to interpret their needs to Treasury. These ties and

relationships are not drawn on lightly, however, so they only have a practical influence on IMF enforcement of conditionality when the borrowing country is sufficiently vulnerable to reverses of international capital flows to prioritize its interactions with the IMF in its foreign policy.

Robustness and discrimination

Statistical results are convincing to the extent that measures have conceptual validity, data are of high quality, results are robust to alternative specifications, and tests discriminate effectively between alternative theories. I have addressed conceptual validity and data quality above, and now turn to assessments of robustness and discrimination. Robustness can be thought of in three ways: measurement robustness, specification robustness, and methodological robustness.

Measurement robustness is important in cases where there exists no single, obvious measure of the quantity of interest. In this case, the intensity of U.S. interests in a particular country has no obvious metric, and can be expected to vary over time and across countries with respect to different issues. Some countries are important because of their economic ties and others because of their strategic military locations, and the balance between these sources of U.S. interests shifts over time with the salience of security and economic crises. The results presented above demonstrate an impressive degree of measurement robustness.

I noted that the strongest exception to the pattern of evidence regarded bank exposure, which, although it has a strong and significant effect in reducing the duration of program suspensions, did not appear to have an effect that varied as a result of

vulnerability. Closer examination reveals that the pattern is in fact consistent with the broader pattern when vulnerability is measured in terms of trade/GDP and debt service/exports—interactions with these variables strengthen the effects of bank exposure that reduce the length of suspensions—but the pattern reverses for short-term debt. Short-term debt weakens the effect of bank exposure and counteracts the effects of the other two vulnerability variables, so the combined effect of increasing all three is insignificant. Debt service/exports has a weak interaction with bank exposure, but increasing trade/GDP while holding short-term debt constant has the familiar effect of strengthening the effect of bank exposure on duration. The expected duration falls by two months, or almost 35 percent, when bank exposure and trade/GDP are high and all other variables are at their means. These results are presented in Table 7.4. [Table 7.4 about here]

Table 7.4: Substantive Effects of Bank Exposure
Effect of an increase in U.S. bank exposure by 2.1% (one standard deviation)

	Median duration (months)	decrease (months)	% decrease	*p*
Trade, debt service & short-term debt high	4.2	1.6	27.6%	0.05
Trade, debt service & short-term debt low	4.2	1.6	27.6%	0.00
Trade/GDP high	3.8	2.0	34.5%	0.02
Trade/GDP low	4.6	1.2	20.7%	0.07
Debt service high	4.2	1.6	27.6%	0.00
Debt service low	4.2	1.6	27.6%	0.01
Trade & debt service high	3.8	2.0	34.5%	0.02
Short-term debt high	5.0	0.8	13.8%	0.02
Short-term debt low	3.8	2.0	34.5%	0.02

High and Low indicate one standard deviation above or below the mean;
all other variables held at their means.

What is striking about this result is that the variable that runs in the wrong direction relates to something that directly affects bank interests as well as the interests of the borrower country, the maturity structure of the debt. When bank exposure to a

developing country takes the form of short-term debt, the banks are substantially protected against the risk of default, because they can refuse to roll-over their loans or demand higher interest rates if the risks change. Under those circumstances, banks have weak incentives to coordinate their actions or to lobby their governments to bail out their debtors. When bank exposure is in long-term debt, on the other hand, banks are stuck with depreciating assets and they have strong incentives to act collectively. Therefore, the assumption that short-term debt, or roll-over risk, affects the intensity of borrower interests but not U.S. interests does not hold for the case of bank exposure—loan maturity enters directly into the banks' calculations—although it does hold for the other measures of U.S. interests. The hypothesis that country vulnerability that does not directly affect U.S. interests intensifies the effects of those interests on IMF enforcement of conditionality, however, does hold for bank exposure. Banks do not care about the ratio of trade to GDP, but that ratio affects the risk calculations of borrower countries and gives them incentives to lobby the U.S. government, which is receptive to lobbying from countries to which U.S. banks are heavily leveraged.

A second form of robustness check is specification robustness. The results have been subjected to a range of alternative specifications, and they are broadly consistent across specifications. First, I have experimented with dropping control variables and including additional control variables related to the domestic politics of borrowing countries, and while significance levels and substantive effects vary, the qualitative effects do not change. Robustness appears to be provided by the strong anchoring effect of controlling for conditionality and implementation, which soaks up the effects of domestic politics on the duration of suspensions. Second, I have nested these models in

larger models that include all of the measures of U.S. interests and their interactions or subsets of them, and the results are consistent with those presented above for U.S. foreign aid, bank exposure, UN voting and exports. The effect of alliances becomes statistically insignificant in models that include U.S. aid, but the pattern of increasing effects with increasing vulnerability remains. Here, robustness appears to be provided by the low correlations among most of the measures of U.S. interests, which suggests that they really capture distinct reasons for the United States to have strong interests in particular countries rather than simply representing different measures of the same underlying latent variable.

A third kind of robustness check is methodological. The results presented here are produced by a parametric Weibull duration model, and alternative methods for analyzing the data are possible. The Weibull model estimates a parameter that determines how rapidly the baseline hazard rate increases or decreases over time, but assumes a monotonic rate of change. The exponential model, for example, is a special case where the hazard rate does not change as a function of time, and the Weibull is more general. A parametric hazard model is appropriate if we have prior beliefs about the shape of the hazard. In this case, I expect a monotonically decreasing hazard, because unobserved variation in the difficulty of implementing conditionality across countries should screen the sample of programs that are off track until only the hard cases are left, and this is consistent with the estimated results.

If I use a non-parametric Cox model instead of Weibull, the results are qualitatively the same, but significance levels drop: U.S. aid and bank exposure are only marginally significant, exports and alliances become insignificant, and only UN voting

remains strongly significant. There is, unfortunately, no direct test of the hypothesis that the Weibull results fit the data better than the Cox results, or vice versa, because the likelihood functions computed by the models are not conformable.[4] A likely explanation for the differences between the Weibull and the Cox models is that the Cox model allows for a very flexible and non-monotonic hazard function, so if some countries consistently have longer durations than others—which is the essence of the argument made here—the Cox model may attribute these cross-sectional variations to the shape of the hazard function rather than to the independent variables.

Another form of methodological robustness check is to include fixed effects. Country-specific fixed effects are impractical because there is not enough variation in the dependent variable to calculate all of the country coefficients, and in any case country fixed effects would absorb all of the cross-sectional variation in the importance of particular countries, which are the quantities of interest, into theoretically uninformative country effects. However, the results are robust to the inclusion of a variety of specifications of regional fixed effects, and the results reported include the most important regional effects—sub-Saharan Africa, Latin America, and the countries of the former Soviet Union.

Another approach is to estimate a frailty model. Parametric hazard models such as the Weibull model make a proportional hazards assumption, which is analogous to the OLS assumption of no omitted variables: if there is unmeasured heterogeneity in the data this assumption does not hold and the results are biased. Of course, there is always

[4] They effectively treat the definition of observations differently, so their likelihoods cannot be compared meaningfully. This rules out comparisons based on the Akaike information criterion or the Bayesian information criterion, and also rules out tests of non-nested model fit such as the Vuong test or the Clarke test.

unmeasured heterogeneity in the data, so a common approach is to estimate a frailty model, which estimates an additional parameter to correct for the resulting bias. All of the results are robust to estimation of a frailty model with shared regional parameters over nine regions.[5]

In summary, the results are very robust to alternative measures, alternative specifications, and alternative models. Every measure is not statistically significant in every specification of every model, but this is nearly the case. UN voting is significant across the board, and U.S. aid and bank exposure are almost always significant and always at least marginally significant. The pattern holds across the board that measures of U.S. interests have substantially stronger effects when countries are highly vulnerable to external financial shocks, and in most cases those effects are only significant when countries are in the top third of the distribution on at least one dimension of vulnerability.

Waiver of Conditionality

A further test of the robustness of the results is to generate additional testable implications of the model that predict effects on additional dependent variables. An alternative way to measure lax enforcement of conditionality is to count the substantive concessions, or waivers, that are granted to countries when their programs come back on track after a suspension. When program suspensions are cut short because of informal influence, this should show up in adjustments to conditionality, which may take the form of waiving performance criteria. The measure used here is derived from the MONA dataset and counts the number of types of conditions, ranging from zero to a possible

[5] The nine regions, coded by Przeworski et al. 2000 and extended, are Latin America, the Caribbean, sub-Saharan Africa, North Africa and the Middle East, South Asia, South-East Asia, East Asia, Eastern Europe and the former Soviet Union, and Oceania.

maximum of 19, on which waivers were granted in a particular review. Observations are recorded when a program suspension ends. The dataset contains 688 instances when a suspended program came back on track after a successful review, and in 35 percent of those cases the Executive Board granted at least one waiver, for a total of 546 waivers. Two or more waivers were granted in 21 percent of program resumptions, three or more were granted in 12 percent of cases, and four or more were granted in 6 percent of cases. The maximum number of waivers granted in a single successful review was eight, which occurred in Russia in 1998 and in Zambia in 1999.

Since the dependent variable is an event count and I expect to observe over-dispersion—countries that receive one waiver in a given review are more likely to receive additional waivers—the appropriate statistical model is a negative binomial count model. The hypothesis of over-dispersion is confirmed in the analysis to a high degree of confidence. The regressors specified in the models are identical to those for the duration models presented in the previous section. The results of the analysis are presented in Table 7.5. [Table 7.5 about here]

The main conclusions to be drawn from the table are that the results generally support the theoretical predictions, but do not conform to the theoretical predictions as closely for waivers as for the duration of suspensions. First, while the effects of alliance patterns on the duration of suspensions strongly supported the hypothesis of U.S. influence, the results for waivers are not consistent with the model. The interaction effects between alliance portfolios and measures of international financial vulnerability are in the wrong direction. Second, the effects of one of the three measures of vulnerability, debt service/exports, are consistently in the wrong direction across

Table 7.5: Frequency of Waivers
Negative binomial regression

	Coef. *Std. Err.*	*p*	Coef. *Std. Err.*	*p*	Coef. *Std. Err.*	*p*	Coef. *Std. Err.*	*p*	Coef. *Std. Err.*	*p*
Unmet conditions	0.27 *0.04*	0.00	0.26 *0.04*	0.00	0.26 *0.04*	0.00	0.27 *0.04*	0.00	0.25 *0.03*	0.00
Scope of conditions	0.17 *0.03*	0.00	0.15 *0.02*	0.00	0.16 *0.03*	0.00	0.16 *0.03*	0.00	0.15 *0.02*	0.00
U.S. aid	1.3E-03 *2.4E-03*	0.58								
U.S bank exposure			1.0E+02 *2.7E+01*	0.00						
U.S. exports					6.4E-03 *2.4E-03*	0.01				
UN voting (S-U.S.)							-1.39 *0.80*	0.08		
U.S. alliance portfolio									3.52 *1.88*	0.06
GDP per capita	-1.1E-04 *6.3E-05*	0.07	-3.6E-04 *9.5E-05*	0.00	-1.6E-04 *6.3E-05*	0.01	-1.3E-04 *6.3E-05*	0.04	-6.4E-05 *6.1E-05*	0.29
Democracy	-0.01 *0.01*	0.28	-0.01 *0.01*	0.47	-0.01 *0.01*	0.52	-0.01 *0.01*	0.44	0.00 *0.01*	0.85
Missing scale	0.98 *0.50*	0.05	0.86 *0.48*	0.07	1.08 *0.48*	0.03	1.16 *0.52*	0.03	0.86 *0.49*	0.08
Reserves/imports	-0.01 *0.02*	0.44	-0.02 *0.02*	0.27	-0.02 *0.02*	0.20	-0.01 *0.02*	0.51	-0.03 *0.02*	0.17
Trade/GDP	-0.01 *0.00*	0.01	0.00 *0.00*	0.35	0.00 *0.00*	0.19	-0.01 *0.00*	0.01	0.01 *0.01*	0.29
Debt service/exports	-0.01 *0.01*	0.04	-0.01 *0.01*	0.07	-0.01 *0.01*	0.25	-0.01 *0.01*	0.35	-0.01 *0.02*	0.38
Short-term debt	-1.6E-03 *2.0E-02*	0.94	4.0E-03 *1.9E-02*	0.84	-1.8E-02 *1.9E-02*	0.34	3.3E-02 *2.5E-02*	0.18	1.0E-01 *3.8E-02*	0.01
Short-term debt^2	-8.9E-05 *3.4E-04*	0.80	-1.7E-04 *3.3E-04*	0.61	1.1E-04 *3.3E-04*	0.74	-1.0E-03 *5.6E-04*	0.06	1.1E-04 *3.3E-04*	0.74
Interactions with:										
Trade/GDP	4.8E-05 *2.6E-05*	0.07	-1.25 *0.35*	0.00	-6.8E-05 *2.5E-05*	0.01	-4.5E-03 *6.7E-03*	0.50	-0.04 *0.02*	0.02
Debt service/exports	-2.2E-04 *1.4E-04*	0.11	-2.24 *1.04*	0.03	-1.8E-04 *6.2E-05*	0.01	0.07 *0.03*	0.01	-0.01 *0.03*	0.70
Short-term debt	-9.4E-05 *1.1E-04*	0.41	2.02 *1.12*	0.07	1.3E-04 *5.7E-05*	0.03	0.10 *0.05*	0.04	-0.27 *0.09*	0.00
Constant	-1.41 *0.37*	0.00	-1.27 *0.35*	0.00	-1.48 *0.36*	0.00	-1.65 *0.37*	0.00	-2.48 *0.82*	0.00
ln(⁄)	-0.32 *0.19*		-0.46 *0.20*		-0.38 *0.20*		-0.34 *0.19*		-0.41 *0.20*	
⁄	0.73 *0.14*		0.63 *0.13*		0.68 *0.14*		0.71 *0.14*		0.66 *0.13*	
Number of obs	608		608		608		602		608	
LR chi2(17)	221.72		243.96		228.68		223.28		233.12	
Prob > chi2	0.00		0.00		0.00		0.00		0.00	
Pseudo R2	0.15		0.16		0.15		0.15		0.16	
Log likelihood	-635.36		-624.24		-631.88		-628.59		-629.66	

Note: Standard errors in italics. Regional fixed effects not reported.

measures of U.S. interests. However, U.S. foreign aid, U.S. bank exposure, U.S. exports and UN voting patterns each interact with some of the measures of international vulnerability in ways predicted by the model. Joint tests of significance for these

variables and the measures of vulnerability that have the predicted effect, with all other variables held at their means, are presented in Table 7.6. [Table 7.6 about here]

Table 7.6: Effects of U.S. Influence on Waivers
Negative binomial regressions

Vulnerability	IRR	Std. Err.	*p*
	U.S. foreign aid		
High trade/GDP*	1.0024	0.0012	0.04
High trade/GDP	1.0013	0.0013	0.34
Mean	0.9995	0.0010	0.60
	U.S. bank exposure		
High short-term debt	5.6E+09	5.5E+10	0.02
Mean	10.9	113.9	0.82
	U.S. exports		
High short-term debt	1.0014	0.0004	0.00
Mean	1.0001	0.0004	0.80
	UN voting		
High trade/GDP	1.5673	0.5780	0.22
Mean	1.8583	0.6590	0.08
	Alliances		
High trade/GDP, debt service/ exports & short-term debt	0.0017	0.0028	0.00
Mean	0.1314	0.1245	0.03

Note: Incident rate ratios (IRR) greater than one indicate an increase in the number of waivers.
*Restricted model does not include interactions with debt service/exports or short-term debt.

Table 7.6 presents a mixed picture, but introduces substantial additional evidence that U.S. informal influence reduces the enforcement of conditionality. Alliance patterns are statistically significant in the wrong direction, and UN voting is insignificant, but U.S. foreign aid, U.S. bank exposure and U.S. exports have significant effects that increase the incidence of waivers. The substantive effects of these three measures of U.S. interests on the incidence of waivers are substantial when countries are highly vulnerable to sudden reversals of external financing. A one-standard deviation increase in U.S. aid increases the incidence of waivers by about one-third (the confidence interval ranges

from one percent to 64 percent). The estimated effect of bank exposure is very imprecise, so the point estimate is not informative, but the effect is clearly substantial. Using the lower end of the 95 percent confidence interval, the effect of a one-standard deviation increase in U.S. bank exposure (about two percentage points of total U.S. foreign exposure) is to increase the incidence of waivers by at least 50 percent. Increasing U.S. exports by one standard deviation, or by $684 million, increases the incidence of waivers by an estimated 93.4 percent (the confidence interval ranges from 37 to 150 percent).

Discrimination

A final consideration is discrimination among alternative theories. The results have two significant advantages in terms of theory discrimination. First, the specification of an interactive effect between measures of U.S. interests and measures of borrowing country vulnerability to external financial shocks is consistent with the theory of informal governance, but seems too specific to be subject to ad hoc explanations or explained by omitted variables. Second, only the theory of informal governance explains a common pattern of interactive effects across variables that measure U.S. economic, political and strategic interests.

A separate question is how well the results discriminate between the hypothesis of U.S. informal control of the IMF and collective control by the G-5, and this calls for additional analysis. Up to this point, all of the hypotheses tested have measured U.S. interests. Some of the measures are well suited to discriminating among U.S. preferences and those of other G-5 countries, because they are not highly correlated across countries,

Table 7.7: Correlations of Measures of U.S. and G-5 Interests

	U.S. aid	UK aid	French aid	German aid	Japanese aid
U.S. aid	1.00				
UK aid	0.12	1.00			
French aid	0.29	-0.05	1.00		
German aid	0.15	0.08	0.27	1.00	
Japanese aid	0.15	0.10	0.05	0.11	1.00

	U.S. bank exposure	UK bank exposure	French bank exposure	German bank exposure	Japanese bank exposure
U.S. bank exposure	1.00				
UK bank exposure	0.96	1.00			
French bank exposure	0.72	0.73	1.00		
German bank exposure	0.47	0.44	0.68	1.00	
Japanese bank exposure	0.36	0.45	0.54	0.29	1.00

	U.S. exports	UK exports	French exports	German exports	Japanese exports
U.S. exports	1.00				
UK exports	0.36	1.00			
French exports	0.36	0.81	1.00		
German exports	0.34	0.84	0.73	1.00	
Japanese exports	0.38	0.45	0.36	0.25	1.00

	U.S. UN voting	UK UN voting	French UN voting	German UN voting	Japanese UN voting
U.S. UN voting	1.00				
UK UN voting	0.82	1.00			
French UN voting	0.84	0.96	1.00		
German UN voting	0.78	0.89	0.96	1.00	
Japanese UN voting	0.76	0.87	0.91	0.94	1.00

	U.S. alliances	UK alliances	French alliances	German alliances	Japanese alliances
U.S. alliances	1.00				
UK alliances	-0.55	1.00			
French alliances	-0.56	0.9996	1.00		
German alliances	-0.56	0.9988	0.9985	1.00	
Japanese alliances	-0.64	0.95	0.95	0.95	1.00

Sample: Countries under program suspensions, 3,724 observations

and others are less able to provide sharp tests. The correlations across measures are presented in Table 7.7. [Table 7.7about here]

As Table 7.7 indicates, bank exposure and UN voting provide weak discrimination among national interests because they are so highly correlated among the G-5 countries. Replications of the analysis of the duration of program suspensions using average bank exposure rates and average UN voting scores for the G-5 countries other than the United States—Japan, Germany, the United Kingdom and France—are qualitatively identical to the results presented above using U.S. measures, and are not reported. Because the other G-5 countries vote similarly to the United States, countries that vote similarly to the United States must also vote similarly to the other G-5 countries. Bank exposure to particular countries varies quantitatively across G-5 countries, but the other G-5 countries' banks are highly exposed to most of the same countries as U.S. banks. There are important regional variations, however: French banks are more highly invested in Africa, and Japanese banks are more highly invested in East Asia.

Foreign aid and alliances provide strong comparative tests of U.S. versus G-5 control. As Table 7.7 shows, foreign aid correlations within the sample are quite low: the correlation with U.S. foreign aid ranges from .12 for Britain to .29 for France. Replications substituting total aid from other G-5 countries for U.S. aid find that the predicted pattern does not hold for aid donors other than the United States. The results are reported in Table 7.8. [Table 7.8 about here]

Table 7.8: Effects of U.S. vs. G-5 Aid
Accelerated failure time metric

	IRR	*p*	95% Conf. interval	
U.S. Aid				
High vulnerability	0.9974	0.03	0.9951	0.9998
Mean	1.0000	0.97	0.9994	1.0006
Other G5 Aid				
High vulnerability	0.9996	0.21	0.9989	1.0002
Mean	0.9996	0.01	0.9994	0.9999

At average levels of vulnerability, U.S. foreign aid has no statistically significant effect. However, program suspensions shorten as aid recipients become more vulnerable to external financial shocks, and this effect is highly significant for the top third of the distribution of vulnerability. In contrast, aid from other G-5 countries is associated with a substantively marginal but statistically significant decrease in the length of program suspensions at average levels of vulnerability, but this effect disappears as vulnerability increases. Aid from other G-5 countries does not reduce the duration of program suspensions for the countries that have the strongest incentives to ask their patrons to lobby the Fund, which suggests that the mechanism of informal governance is generally limited to the United States.

Alliances reveal a similar pattern. As Table 7.7 demonstrated, S-scores for alliances with the United States are negatively correlated with S-scores for alliances with the other G-5 countries. The United States is allied with the other G-5 countries, of course, but the correlations reflect the fact that U.S. alliance commitments are flung widely around the world, and do not generally coincide with those of even its closest allies. Results of replications of earlier models that substitute alliance portfolios of other

G-5 countries for those of the United States are reported in Table 7.9. [Table 7.9 about here]

Table 7.9 Effects of U.S. and G-5 Alliances
Accelerated failure time metric

	IRR	p
U.S. Ally		
High vulnerability	0.2577	0.04
Mean	0.4623	0.09
U.K. Ally		
High vulnerability	0.7170	0.67
Mean	0.5283	0.17
French Ally		
High vulnerability	0.7588	0.72
Mean	0.5377	0.18
German Ally		
High vulnerability	0.7170	0.67
Mean	0.5283	0.17
Japanese Ally		
High vulnerability	4.8867	0.06
Mean	1.4961	0.42

Table 7.9 reports the results of tests of the joint significance of alliance portfolios and their interactions with trade/GPD, debt service/exports, and short-term debt, evaluated at the means of the three vulnerability levels and at one standard deviation above the mean. The results for U.S. alliances are calculated from the model reported in Table 7.2, and the other results are calculated from analogous models that substitute the alliance portfolio of a different G-5 country and its interactions for the corresponding U.S. variables. Only the U.S. variables are statistically significant in the predicted direction. U.S. alliances are marginally associated with reduced length of program suspensions at mean levels of financial vulnerability, and the effects become stronger and highly significant as vulnerability increases. In contrast, none of the other alliance portfolio measures is

associated with reduced duration of program suspensions; only Japanese alliance portfolios have any discernible effect, and their effect takes the opposite of the predicted direction.

Discrimination between the hypotheses of U.S. influence and G-5 influence becomes sharper when we turn to the analysis of waivers. Replications of the analysis of

Table 7.10: Effects of other G-5 Interests on Waivers
Negative binomial regressions

Vulnerability	IRR	Std. Err.	*p*
	Other G-5 Aid		
High trade/GDP*	1.0003	0.0006	0.57
High trade/GDP	1.0003	0.0008	0.76
Mean	0.9996	0.0004	0.28
	Other G-5 Bank Exposure		
High short-term debt	1.8E+13	3.1E+14	0.07
Mean	0.5162	7.4186	0.96
	Other G-5 Exports		
High short-term debt	1.0001	0.0004	0.79
Mean	0.9994	0.0004	0.13

Note: Incident rate ratios (IRR) greater than one indicate an increase in the number of waivers.
*Restricted model does not include interactions with debt service/exports or short-term debt.

the number of waivers granted when programs are allowed to come back on track indicate that none of the statistically significant results survive when measures of U.S. interests are replaced by analogous measures for the other G-5 countries. The results are summarized in Table 7.10. [Table 7.10 about here]

In each case, the results presented replicate the models presented above, which also represent the ones that are most favorable for the hypothesis of G-5 influence. The models substituting total aid from Britain, France, Germany and Japan for U.S. foreign aid do not approach statistical significance, although the results gradually move in the predicted direction as trade increases as a percentage of GDP. Average G-5 bank

exposure comes closest to mimicking the effect of the parallel measure of U.S. interests—the average bank exposure of Britain, France, Germany and Japan correlates with U.S. bank exposure at .73, so this is unsurprising—but the G-5 measure is only marginally significant. The substantive effect is large, so the best interpretation of this result is that G-5 bank exposure probably has an effect that is similar to that of U.S. bank exposure; however, the standard error is so large that it is impossible to determine with a high degree of certainty whether an effect of G-5 bank exposure exists, whereas the effect of U.S. bank exposure is clear. Exports, on the other hand, are able to discriminate more clearly between the U.S. influence and G-5 influence hypotheses, because the correlation between U.S. exports and exports from the other G-5 countries for the set of program participants is only .45. The effect of total exports from Britain, France, Germany and Japan is statistically insignificant. At high levels of vulnerability these exports are estimated to have an effect that is in the same direction as the effect of U.S. exports, but the estimated effect of U.S. exports is 11.6 times higher.

In summary, these results support the hypothesis of U.S. influence rather than the hypothesis of G-5 influence whenever the measures of national interests differentiate U.S. interests from G-5 interests sufficiently clearly to permit a sharp test. For the analysis of the duration of program suspensions, the two measures of interests that allow a sharp comparative test of the hypothesis of U.S. versus G-5 control, foreign aid and alliance portfolios, support the conclusion that the United States exercises disproportional informal influence over the duration of program suspensions. The results of replications using bank exposure and UN voting to measure interests are equally consistent with the interpretation that the G-5 exercise collective control over program suspensions and that

the United States exercises sole control—the measures are simply too highly correlated to permit a comparative test of these hypotheses. The results for waivers are more unequivocal. Replications replacing the three variables for which measures of U.S. interests were found to have significant effects with parallel measures for G-5 countries—total aid, average bank exposure, and total exports of Britain, France, Germany and Japan—reject the hypothesis that these countries influence waivers at the .05 level. The results for G-5 bank exposure were significant at .07, and the substantive effects were large, so the verdict with respect to bank exposure is uncertain, rather than rejected. As noted above, this uncertainty is to be expected, because G-5 bank exposure is highly correlated with U.S. bank exposure. However, the effect of U.S. bank exposure is clearly established, and the effect of the exposure of other G-5 banks is less clear. With respect to aid and trade, on the other hand, the results clearly establish that U.S. measures are associated with the issuance of waivers, and G-5 measures are not.

Cases: Russia and Argentina

Two cases serve to illustrate the mechanisms by which the United States exerts its informal influence over the enforcement of IMF programs, the ways in which U.S. intervention depends upon the financial vulnerability of the borrowing country, and the diversity of motivations that the United States has for intervening on behalf of particular countries. The United States intervened extensively on behalf of both countries when they ran into difficulties with the IMF, but Russia and Argentina presented very different challenges to U.S. policy makers. Russia played a critical role in the Clinton administration's foreign policy, as the most important former Communist country and the

lynchpin of a regional security strategy based on reassurance after the end of the Cold War. This is reflected in the fact that Russia was a substantial recipient of U.S. foreign aid. At the height of its influence in Washington, in 1996, Russia received $416 million in U.S. economic aid, or approximately two standard deviations more than the average country that participated in IMF programs during this period. Russia did not rate very highly on most of the other measures of U.S. interests, however. Russia accounted for about 1 percent of foreign lending by U.S. banks and $278 million in U.S. exports, or about a quarter of a standard deviation above the mean on each variable, so economic interests provided only weak incentives for the United States to interfere in Russian relations with the IMF. Russia was about half of one standard deviation more supportive of U.S. votes in the United Nations than average, and was half a standard deviation further from the United States' system of military alliances than the average country, so these strategic dimensions of U.S. interests do not account for Russia's extraordinary treatment. Rather, Russia was important to U.S. interests for the collection of strategic concerns that motivated U.S. foreign aid, and this is captured in the statistical analysis.

On the other hand, Argentina has not been a substantial recipient of U.S. foreign aid since the end of the Cold War, but has been a very important economic partner. Throughout the sample period, lending by U.S. banks to Argentina averaged 4.5 percent of their total holdings of foreign assets, more than two standard deviations above the mean, and hovered between eight and nine percent of foreign assets between 1993 and 1996. Bank exposure declined from that high point but remained high relative to the sample, and Argentina became the most important emerging bond market by the year 2000. U.S. exports to Argentina were consistently higher than to Russia, averaging $339

million per year, and reaching $417 million in 2001, or half a standard deviation above the mean. Argentina was more critical of U.S. voting in the United Nations than Russia during most of the sample period, and was much more closely linked to the United States' network of military alliances—1.7 standard deviations more closely than the average IMF program participant. Argentina's influence was based on its economic importance, particularly to U.S. banks, on its close ties to other important U.S. allies—particularly Brazil, Chile, Mexico, Spain and Italy—and on its skepticism towards U.S. global leadership, which created an incentive for Washington to bolster sympathetic Argentine governments.

Russia

The failure to enforce conditionality in Russia has become emblematic of the broader IMF credibility problem. After the dissolution of the Soviet Union, Russia negotiated a Stand-By Agreement (SBA) in 1992, and no sooner was the ink dry on the agreement than the Central Bank of Russia tripled the money supply. Had Russia been an ordinary country, it would have been several years before the Fund tried to reengage—indeed, most of the mismanaged economies in the region, such as Ukraine and Bulgaria, had to wait—but the United States mobilized the G-7 to promote Russia's case, and the Executive Board created a new, low-conditionality facility, the Systemic Transformation Facility (STF), to accommodate Russia. Russia received an STF in 1993 whose major condition was an inflation target of 7-9 percent per month. Despite the leniency of these conditions, which staff in the European II Department protested, Russia went off track again, and the second disbursement of the STF was delayed; but again the United States

pressed Management for a waiver to allow the second disbursement to take place in 1994.[6] In the meantime, Russia's monetary policy had been tamed, but fiscal policy went off track, particularly in the form of weak tax collection and budgetary subsidies to enterprises.

After a collapse in the exchange rate in the fall of 1994, Russian policymakers forged a consensus around a new policy framework, which anchored monetary policy with an exchange rate band beginning in 1995. Russia and the IMF negotiated another SBA to accompany the new policy stance and incorporated the highly unusual precaution of requiring monthly monitoring of conditions. For about nine months, Russia appeared to be implementing its conditions. Fiscal policy slipped out of control after the 1995 parliamentary elections, however, which President Yeltsin's supporters lost to representatives of the far left and far right of the political spectrum, and the budget deficit expanded rapidly during the presidential campaign in the spring of 1996. Meanwhile, Russia was negotiating with the IMF to replace its expiring Stand-By with a three-year Extended Fund Facility (EFF), and President Clinton publicly urged that Russia's IMF support not be cut at the critical point.[7]

Looking backwards, it is hard to recapture the sense of crisis that reigned in the spring of 1996. The G-7 countries were convinced that Russia was at a turning point: if the Communist leader Gennadyi Zyuganov won the election, Russia's chances of consolidating democracy and market reform seemed lost. The dramatic fiscal expansion during the election campaign threatened a twin crisis on the exchange market and the market for government bonds (*Gosudarstvennye kratkosrochnye obligatsii*, or GKOs),

[6] Interview Hernandez-Cata, February 17, 1999, cited in Stone (2002).
[7] *New York Times*, January 31, 1996, 1.

and the Central Bank of Russia used hidden purchases of bonds and rubles to fend off the crisis. Under the circumstances, the Executive Board issued waivers covering Russian fiscal policy and debt, and the program was not suspended until the month after the election. Russian Central Bank officials believed that they had avoided a politically disastrous financial crisis by days in June and July 1996, while Yeltsin was hospitalized between the first and second rounds of the election.[8] There does not appear to have been any dissent within the G-7 about issuing these waivers; and although the Board only learned later about the secret manipulation of Russian reserves that spring, it was fully aware that Russia was not abiding by the key conditions of its program. Nor was the IMF Management unwilling to grant waivers in this case; even Staff, which usually took a more orthodox stance on Russia than Management, explained the decision in terms of geopolitics rather than macroeconomics.[9]

Had 1996 remained an exception, it might have been possible to avoid the financial crisis and partial default that occurred in August 1998. However, the program was suspended for only one month, and efforts to enforce conditionality throughout the rest of 1996 and 1997 were brief and inconsequential. In fact, although Russia's programs were suspended for non-compliance with key fiscal and monetary conditions once in 1995, three times in 1996, three times in 1997 and twice in 1998, these suspensions lasted only one or two months until the end of 1997. This accommodating stance prevented the IMF from exercising whatever leverage it had. Meanwhile, rapid capital inflows temporarily reduced Russia's reliance on IMF financing and blunted the effects of program suspensions. Russia became a high-yield emerging market and

[8] Interviews Dubinin, Aleksashenko and Potemkin, cited in Stone (2002).
[9] Interview Horiguchi, November 8, 1999, cited in Stone (2002), 138-40.

conducted a rapid expansion of fiscal policy that was financed by capital inflows into the booming stock market and the market for GKOs. It was not until Russian markets felt the contagion effects of the Asian crisis at the end of 1997 that President Yeltsin began to understand the urgency of fiscal reform, by which time it was too late to avert the crisis. The IMF delayed one disbursement by three months in late 1997 and one by four months in early 1998, but rapidly reversed itself when the Russian bond market was seized by panic in May. The IMF rushed to negotiate a new package of reforms and financing, announcing that it would lend up to $11.2 billion to Russia in 1998 as part of a two-year package of support including the World Bank and bilateral lenders totaling $17.1 billion.

By the time the eleventh-hour rescue package was announced on July 20, 1998, however, market actors had drawn conclusions about the seriousness of Russian reform promises and the credibility of IMF conditionality in Russia. Although the package was unprecedented in size and contained a reform agenda of extraordinary breadth, it appeared to improve market conditions for only a few days. Capital flight accelerated in early August in spite of frantic purchases of dollars by the Central Bank of Russia, and demand for government bonds vanished. On August 17 Russia ran out of funds, and the value of the ruble collapsed. Russian banks had taken advantage of interest rate differentials, borrowing heavily in dollars and lending in rubles, so the abandonment of the exchange rate band and the collapse of the bond market created a wave of insolvencies. The government and central bank declared a moratorium on debt service by private banks, and Yeltsin dismissed the government and the chairman of the central bank, ending Russia's last experiment with liberal politics.

Argentina

Weak enforcement of conditionality was less obvious in the case of Argentina than in the case of Russia, but it was pervasive throughout Argentina's relationship with the IMF in the 1990s, and it laid the groundwork for the subsequent crisis.[10] Argentina's remarkably successful financial stabilization in the 1990s was based upon a currency board arrangement embodied in the Convertibility Law, which fixed the peso to the dollar at a one-to-one parity and obligated the Central Bank to hold reserves equal to base money. This was more rigid that Russia's exchange regime, but shared the same underlying weakness: fiscal deficits and expanding debt would make the policy framework unsustainable in the long run, and made it acutely vulnerable to shifts in market sentiment. Argentina consistently missed IMF targets for its fiscal deficit, and the debt grew from 29 percent of GDP in 1992 to 41 percent in 1998, and rose to 50 percent by 2000. Staff repeatedly voiced objections to the planned EFF for Argentina in 1996-98 because of its weak fiscal provisions, but the Management overruled these concerns, and the staff reports to the Executive Board did not disclose these objections.[11] Fund insiders regarded the level of Argentina's debt burden as barely sustainable even in the presence of the toughest economically feasible fiscal policy, and only as long as market perceptions remained favorable. It was high enough to lead to exploding debt dynamics—debt so high that debt service drives it steadily higher as a percentage of GDP—if market sentiment became unfavorable.[12]

[10] Mussa 2002.
[11] IEO 2004, 36. This assessment was confirmed in interviews with IMF officials involved in the negotiations.
[12] Mussa 2002, 16-17.

Argentina failed to meet its targets under the EFF in 1997, and the program moved into a series of increasingly lengthy suspensions. However, continuing inflows of short-term capital financed Argentina's growing fiscal deficits and made it unnecessary for Argentina to draw on the IMF's resources—or to call on its shareholders to help unlock them. The burden of servicing the debt rapidly rose, from 4.9 percent of gross national income in 1996 to 9.9 percent in 2000, and this increased Argentina's vulnerability to sudden stops of external financing. Market sentiment shifted in the fall of 2000, and it became clear that Argentina would be compelled to abandon its pegged exchange rate and would probably be forced into default if a major rescue package were not forthcoming. Argentina had been treating its IMF program as precautionary in 2000, but it turned to the Fund with a request to draw on the program and asked for a substantial augmentation in November. Argentina missed its target for the fiscal deficit in September, and would have missed the target for the December review as well, had it not been modified. Staff was divided over whether it was advisable to expand the size of the loan facility under these circumstances, but Management strongly supported the program at U.S. urging. The United States represented the lone voice strongly favoring the program in the G-7, and the other members deferred in spite of their reservations.[13]

Staff in the IMF's Western Hemisphere Department believed that this was Argentina's last chance. A substantial package of financial support was assembled and the conditions of the precautionary program were revised: fiscal conditions were loosened to accommodate the poor performance of the economy, but structural conditionality was increased in an effort to compensate. Staff believed that a failure to implement the program, and particularly to meet the fiscal conditions that had already

[13] Blustein 2005, 101.

been weakened, would make the crisis inevitable. The consensus view was that if the program went off track after December 2000, the IMF should not extend itself further and Argentina should be allowed to fail.[14]

The program almost immediately went off track. Two finance ministers and the central bank governor resigned in rapid succession, and Argentina missed its targets for the first quarter of 2001 for the Federal fiscal deficit, the consolidated deficit of the public sector, the primary surplus, aggregate debt and short-term debt. The March review was delayed. At this point, the staff had serious misgivings, and the analysts closest to the front believed that the program should be suspended indefinitely. Short-term interest rates had jumped to 1000 basis points above U.S. Treasury bonds—to a level of about 14 percent—and the Argentine debt was no longer sustainable at those interest rates. In the context of a Management decision to go forward, however, Staff could not express a contrary view.[15] The Staff Report to the Executive Board for the May review used extraordinarily optimistic assumptions to build a case that the debt could still be sustainable, including growth rates that could not be attained at the current level of interest rates, primary surpluses (fiscal surpluses before debt service is included) that had not been reached even in the best years in the 1990s, and interest rates that were no longer being offered.[16] Misgivings were raised in the Executive Board, particularly by the British Alternate ED, but the decision to grant the waivers, further relax the conditions, and keep the program on track had been made informally at a higher level.[17]

[14] Interviews with IMF staff members.
[15] IEO 2004.
[16] Mussa, 77-81.
[17] Mr. Collins, the UK alternate ED, called for a "Plan B" in case the debt did not turn out to be sustainable and pointed out that "…the staff paper could have prepared a more in-depth analysis of the differences in the revised program from that agreed in January." Executive Board Minutes, May 21, 2001, EBM/01/53.

The most controversial stage of the Argentine program came in August 2001, when the government requested an $8 billion augmentation (discussed in Chapter 5). At this point the program was effectively taken out of the IMF's hands by the U.S. Treasury, which conducted negotiations with the Argentine government and won approval from skeptical members of the G-7.[18] Within the U.S. government, Treasury was heavily lobbied by officials from the National Security Council and the Departments of State and Defense, who were in contact with their Argentine counterparts and emphasized that the collapse of the Argentine economy would have broader negative consequences for U.S. policy in the region. The White House received calls from the presidents of Brazil, Chile and Mexico warning of the dire consequences for U.S. influence in Latin America—and for the influence of the IMF—if Argentina did not receive emergency assistance. Argentina pulled out all the stops.

The most optimistic Staff put the chance of success of the program at 20-30 percent at this point, and prominent voices including Kenneth Rogoff, the new head of the Research Department, were strongly opposed. However, none of this dissent was communicated to the Executive Board. A Mission returned to Washington from Buenos Aires shortly before the proposal went to the Board and brought back a pessimistic assessment about the unwillingness of provincial governors to go along with the new zero deficit law that was supposed to be the basis for meeting the program conditions in the fall. Although by September Staff in the Western Hemisphere Department believed that the probability that the authorities would be able to implement the key fiscal target of the program was very low, they could not raise doubts of this nature without solid evidence. In communications with the Executive Board, the benefit of the doubt goes to the country

[18] Taylor, 80-88; Blustein 2005, 145-51.

authorities.[19] The Executive Board, meeting a week before September 11, 2001, passed the review of the Argentine program together with the augmentation, but the meeting was memorable for its unusual degree of open criticism and several pointed abstentions from the final vote.

In the case of Argentina, as in the Russian case, the effect of a pattern of persistent non-enforcement of conditionality was that the sustainability of the foreign debt gradually deteriorated, and when the crisis came, the political system failed to respond. Far from tipping incentives in the direction of reform, the IMF weakened the incentives to come to grips with urgent problems. Argentina was no better able to reform its finances in September than it had been earlier in the year, and its policies failed to inspire confidence in the market. Capital flight accelerated in the fall; indeed, many observers inside and outside of the Fund argued that IMF financing had simply provided a brief breathing space that allowed individuals and foreign financial institutions to withdraw their capital. The IMF had lost its credibility as an arbiter of sound policies, and as a predictable source of soft financing, it diluted market discipline. The collapse of the exchange rate, the banking system and the government followed in January, and Argentina went into default on its foreign debt. Riots forced the resignations of two presidents. The country moved into a deep recession, investors lost much of their wealth, and wages and employment dropped sharply.

Conclusions

Russia and Argentina are extreme cases because these were countries that were able to call on significant leverage with the United States and that found themselves in such dire

[19] Interview 2.

circumstances that they were willing to cash in their influence. In short, they were textbook cases for the exercise of informal influence. They were not typical cases, but they provide ideal illustrations of the mechanisms and logical consequences of informal influence. The IMF is subject to cross-pressures from its biggest shareholder that lead to inconsistent enforcement of conditionality and interfere with its mission as a guarantor of market stability. In extreme cases, the Fund has no credibility, conditionality becomes almost meaningless, and the IMF becomes no better than a captive—and at worst a facilitator—of the policies that run national economies into the ground. In both cases, the pressure to relax the enforcement of conditionality came unambiguously from the United States. The U.S. motivations were different in the two cases—in Russia primarily strategic, in Argentina primarily economic—but the logic of a coincidence of powerful U.S. interests and intense interests in the borrowing country was the same.

Just as case studies are valuable for spelling out mechanisms and provide a weak basis for generalizing, statistical analysis is a powerful tool for generalizing and is poor at establishing causal mechanisms. The statistical analysis in this chapter demonstrates that the pattern of informal governance—the United States intervenes to relax enforcement in countries in which it has strong interests, but only when those countries perceive a strong interest in asking for U.S. intervention—is consistent with the evidence of a global dataset. These tests are stronger than previous ones in the literature because the sample is global rather than regional, the data provide better measures of the key concepts, and the data contain controls for implementation and conditionality. The results are robust to alternative measures of U.S. interests, alternative specifications, alternative methods, and two different dependent variables. The tests sharply discriminate the theoretical model of

informal governance from alternative explanations, because the model predicts an interaction between U.S. interests and borrower interests that is hard to account for with ad hoc explanations and that is unlikely to be due to omitted variables. U.S. foreign aid, bank exposure, exports, UN voting and alliance patterns are all associated with weak enforcement of conditionality, but only when borrowing countries are vulnerable to sudden reversals of international financing.

Some of these tests also discriminate between the hypothesis of U.S. influence and broader influence by the group of G-5 countries—including Japan, Germany, Great Britain and France as well as the United States—while others do not. Some of the variables measuring U.S. interests, particularly UN voting and bank exposure, are highly correlated with measures for other G-5 countries, so it is impossible to distinguish effects of U.S. interests from those of the other countries. Indeed, since some of these interests are common—the failure of a European bank affects the interests and stability of the U.S. banking system, for example—it may be impossible in principle to determine where one country's interests end and another's begin. However, some of these tests, notably those involving U.S. foreign aid and alliance patterns, strongly point to U.S. influence and not to the influence of other leading members of the IMF Executive Board. When the statistical evidence is less clear, as in the case of economic interests, the historical record helps with interpretation. For example, several European countries had stronger economic interests than the United States in avoiding a default by Argentina, but it was the United States that pushed the IMF to continue lending when Argentina repeatedly missed its budget targets. Similarly, Germany was more highly exposed to default by

Russia, but took a harder line on enforcing IMF conditions in Russia than the United States, particularly after 1996.

The evidence presented thus far indicates that informal influence is pervasive at every stage of the IMF product cycle, from surveillance, program initiation, and access to Fund resources (Chapter 7) to the design of conditionality (Chapter 8) and on to the enforcement of conditionality reviewed in the present chapter. The statistical patterns at each stage are consistent with informal influence: countries that are important to U.S. interests and vulnerable to reversals of external financing receive concessions from the IMF. Case studies illustrate the mechanisms by which the United States exercises influence at each stage, the motivations for doing so, and the consequences. Informal influence systematically weakens the incentives for the recipients of IMF financing to implement reform and prudent financial policies, and this imposes long-term costs because the United States has an interest in supporting the policies that the IMF promotes. The benefits to the United States of interfering in IMF governance are diverse, and are harder to measure than the costs, but appear to be compelling in the short term. The statistical evidence shows that the motivations involve foreign policy, economic interests and military security, and the case studies illustrate a wide range of reasons that operate in particular instances. The reasons are perhaps aptly described in the model as uniformly distributed temptations, however, because U.S. foreign policy tends to be dominated by short-term objectives and election calendars rather than by long-term strategic planning.