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## WHAT DETERMINES THE IMPLEMENTATION OF IMF PROGRAMS?

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Summary

For many years analysis of IMF conditionality overlooked the extent to which it was implemented. However, more recently increasing attention has been paid to implementation. Theoretical contributions have focused on the importance of special interest groups, but empirical evidence has failed to provide compelling support for the theory. Indeed, empirical studies have reported mixed results that sometimes seem to be conflicting. This paper identifies a range of economic, political and institutional factors that may, in principle, influence implementation. Using various measures of implementation, it then tests an econometric model designed to capture these influences over 1992-2004 exploiting improved sources of data. The results suggest that significant determinants of implementation are trade openness, the existence of veto players and the amount of resources committed by the Fund. The paper offers an interpretation of the results and discusses the implications for policy.

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# 1. INTRODUCTION.

Although much has been written about IMF conditionality, for a long time research into the effects of IMF programs failed to distinguish between those that were implemented and those that were not. Recently, more attention has been paid to implementation. In principle the IMF's Executive Board only approves programs that it is confident will be implemented. But what is the scientific basis for this judgment? What determines the implementation of IMF programs? This is the central question that this paper seeks to answer.

Since the late 1990s advances have been made in our understanding of implementation on three fronts; theory, empirical estimation and policy. It is therefore an appropriate time to take stock of what we know and to offer new analysis based on the advances made.

What can reasonably be expected from such an exercise? Expectations should not be set unrealistically high. Just as studies of the determinants of IMF lending have been able to identify a range of variables that seem to be important in influencing the pattern of IMF arrangements, but have been less successful in explaining them in detail, so it may be that an investigation into the determinants of implementation will provide a list of factors that influence it, but will be less efficient at estimating the probability of implementation in individual cases (Conway, 1994; Bird and Rowlands, 2001, 2002).

Our objective in this paper is to assemble a list of factors that theory suggests may be important and to see which of them are shown to be statistically significant based on large sample regression analysis. Although our ambitions are modest, the endeavor is far from easy. Many of the independent variables that we would like to

test are only imperfectly captured by the available data. Moreover, the dependent variable – the implementation of IMF programs – can be measured in at least three ways. The results we report should therefore be viewed as suggestive and indicative rather than definitive.

The lay out of this paper is as follows. Section 2 discusses the measurement of implementation. Section 3 summarizes the record on the implementation of IMF programs. Section 4 examines the existing evidence on implementation. Section 5 provides a conceptual framework that builds on recent theoretical work and allows us to identify, in principle, a range of economic and political factors that may be expected to exert an impact on implementation. Section 6 undertakes new large sample regression analysis of implementation based on a political economy model. This section is divided into sub-sections that explain the econometric methodology adopted, the data used and the results found. It also interprets the results drawing on the theory of implementation. Finally Section 7 offers some concluding remarks and examines some of the policy implications of the empirical results.


## 2. MEASURING IMPLEMENTATION


There is no straightforward way of measuring the extent to which IMF conditionality is implemented; approximation is needed. Various proxies have been developed.


### (a) The Rate of Loan Disbursement


The most common proxy used is the extent to which a loan is disbursed (Killick, 1995; Mussa & Savastano, 2000; Joyce, 2003). The disbursement of IMF

loans is tied to the completion of program reviews, and thus to the fulfillment of conditionality. The IMF stops disbursing a credit if there is insufficient implementation.

The advantages of this proxy are that it provides continuous data and it is easy to measure. However, it also has disadvantages. Policy actions may be taken, but few or no resources drawn. Some countries choose not to draw down the full amount of an IMF loan. Programs may be precautionary. 'Failure' to complete a program as measured by the disbursement of Fund resources may reflect economic 'success' in as much as Fund financing is no longer needed.

However, and in contrast, a program may be completed in the sense that all the resources are used, while some of the conditions agreed at the initiation of the program may be left unimplemented. Waivers may be granted or conditionality may be modified.

*(b) Interruption Index*

An alternative approach to implementation is to see whether the program is interrupted. Mecagni (1999) defines interruption as either an interval of more than six months between IMF arrangements, or a delay of more than six months in completing a program review. Schadler *et al*. (1995) examine the quarterly reviews of IMF programs, and define non-compliance as situations where performance criteria are not met and loans are suspended.

Ivanova, Mayer, Mourmouras and Anayiotas (2003) offer two binary variables measuring reversible and irreversible program interruption. Reversible interruption is where the review of a program is delayed, but the program is revived subsequently.

Irreversible interruption is where the arrangement is eventually terminated. Although termination may be followed by a replacement program such that a relationship with the Fund is restored, even in these circumstances interruption does provide a signal of the poor implementation of conditionality.

*(c) Implementation Index*

A third measure of implementation uses the Fund's Monitoring of Fund Arrangement (MONA) database, which contains detailed information about program countries, and has been compiled since 1992 by the IMF's Policy Development and Review Department. Although this measure uses actual conditions, it relies on the subjective judgment of the Fund and weights individual conditions equally.

Furthermore, the MONA database only covers programs coming up for review by the Fund's Executive Board, and thereby excludes cancelled or interrupted programs. Excluded programs are likely to exhibit poor implementation, and, therefore, the MONA index overstates implementation. For these reasons the MONA based measure of implementation is our least preferred one. For completeness, we ran the regressions described later in the paper using it, but anticipated that it would generate poor results. It did, and we therefore do not report them (although they are available from the authors upon request).

The three measures of implementation described above are correlated with one another. But as shown in Table 1, the correlation coefficients suggest that the measures are picking up different dimensions of implementation. They also show that there is a closer correlation between disbursement and interruption than between the MONA implementation index and either of the other two measures.

[Insert Table 1]

In the empirical investigation that follows we focus on irreversible interruption since, in the light of the above discussion, this appears to be the best single measure of severe implementation problems. But we also check the robustness of our results by examining disbursement as well. Later in the paper we explore informally why results may differ as between the different measures of implementation.

## 3. THE RECORD ON IMPLEMENTATION

The record on the implementation of IMF programs has been explored in a number of studies. These are summarized in Table 2. Overall they span a period running from 1969 to 2002. Early studies focused on the implementation of fiscal conditionality or, in some cases, other components of conditionality. Killick (1995) was the first to examine the disbursement rate, using an 80 percent cut off point (20 percent or more undrawn) for judging whether a program had been fully implemented. He examined a large number of programs relative to earlier studies and discovered that only 47 percent of them were fully implemented. He also argued that case study research implied that his measure of compliance was a good proxy for judging the 'success' of programs. More recent studies have continued to use the disbursement rate but have also examined program interruption as well as the implementation index based on the MONA database. As anticipated in the previous section, interruption appears to be the most demanding test for implementation, with Ivanova *et al*. (2003) and Nsouli *et al.* (2003) reporting that only about 60 percent of programs passed this test.

Implementation measured by the disbursement rate and by the MONA-based index occurred in over 70 percent of programs; although, as Ivanova *et al.* show, the implementation of macro conditions in the MONA-based index is superior to that of structural conditions.

Our own calculations, based on 218 programs over 1992 – 2004, confirm the results found by both Ivanova *et al.* (2003) and Nsouli *et al.* (2003). Not only do our results show that irreversible interruption is the most difficult test to pass, but they also suggest that implementation has not improved since 2002, even though the Fund's policy of 'streamlining' conditionality was in part intended to improve the rate of implementation (IMF, 2001). However, the results reported in Table 2 do perhaps suggest that, over a more protracted time period, there has been some improvement in implementation as judged by the disbursement rate. While Killick (1995) finds that 'full' implementation occurred in only 47 percent of programs during 1979 – 93, we find that during 1992 – 2004 it occurred in 74 percent of programs.

[Insert Table 2]

## 4. EXISTING EVIDENCE ON IMPLEMENTATION

The existing literature on the implementation of IMF programs is summarized briefly in Table 3. As shown here, the various measures of implementation, as well as various methodologies and time periods have been used. Areas of consensus have emerged, but there are also disagreements. Most studies concur that political factors are important in influencing implementation, although there is disagreement about the precise nature of the political influence.

Building on earlier research into the implementation of World Bank programs by Dollar & Svenson (2000), Ivanova *et al*. (2003) combine three measures of implementation using the 'Multiple Indicators and Multiple Causes' (MIMIC) model. They set out to discover whether implementation is affected by political conditions, initial economic conditions, and IMF conditionality and effort. They find that the political factors listed in relation to their study in Table 3 are significant but that other factors are not. In related research, Nsouli *et al.* (2004) find further evidence of the importance of political factors, and discover that there is better implementation where ethnic tensions are low, governments are stable and less corrupt and where the military are less involved in politics. They find that more time spent in previous IMF programs, and more financing (relative to a country's IMF quota) aid implementation. They also find that superior implementation contributes to superior macroeconomic performance in terms of inflation and ultimately balance of payments and fiscal outcomes.

[Insert  Table 3]

Studies by Dreher (2003) and Joyce (2003) emphasize the importance of the timing of elections and the openness of economies respectively. However, their studies fail to confirm some of the key results of earlier research, for example, finding no support for the claim that special interest groups play a central role. Further differences in the existing literature relate to the importance of initial conditions in general, as well as specific initial conditions in particular, and to the importance of democracy. For example, Edwards (1989) and Polak (1991) attribute poor implementation to adverse initial conditions and negative shocks. Dreher (2003) also

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finds some initial conditions to be relevant and Mecagni (1999) supports the idea that negative shocks impede implementation. Ivanova *et al.* (2003) discover some supportive evidence for the importance of adverse initial conditions, but this fails to pass tests of statistical significance. Whereas Ivanova *et al.* (2003) find that the degree of democracy is statistically insignificant, Joyce (2003) suggests that it is, and Dreher (2003) finds that democracy helps implementation at election times. Political cohesion is found to be significant by Ivanova *et al.* (2003) but not by Joyce (2003), although he does find that regimes that have been in power for longer are less likely to complete programs.

The existing literature leaves enough ambiguities that further empirical research is justified. New research can contribute by studying more recent evidence and better data, by drawing on ideas that have evolved from the literature, and by extending the methodologies adopted. The underlying purpose, however, remains to achieve a sound understanding of the implementation of IMF programs.

## 5. A CONCEPTUAL FRAMEWORK

With the kind of exercise to be undertaken in this paper the temptation is to embark on a data-mining approach with little theoretical motivation. The problem is that such an exercise frequently reveals results that vary significantly along with the specification of the equations estimated and the details of the sample. It is then difficult to draw firm conclusions. Unfortunately, formal theoretical modeling of implementation does not lend itself to specifying one particular and unique model that can be conveniently tested. Either models tend to be excessively narrow, or the data needed to test them are unavailable. Here we attempt to estimate a model that is

informed by political economy considerations, and includes variables that are likely, in principle, to exert an impact. In some cases *a priori* reasoning may lead to a clear expectation of the sign of the relationships. In others, there may be opposing forces at work such that it is unsafe to form a particular *a priori* view.

A first component of our model relates to *initial economic conditions* at the outset of programs, with these covering performance variables such as inflation, economic growth, the current account, and international reserve levels (which reflect the degree of currency misalignment), as well as policy variables such as fiscal deficits and monetary expansion. The most straightforward assumption is that the worse are the initial conditions, the less will be the degree of implementation. However, this need not necessarily be the case. It may be, for example, that the design of programs is modified to take initial conditions into account. Targets may be less ambitious where initial conditions are weak. Or it may be that where economic performance and policy have deteriorated to a low level, a government's commitment to reform becomes stronger. As a consequence, there may be less disharmony between the government and the IMF, and implementation may be better.

A second component of our model captures a country's *vulnerability to exogenous shocks*. For low income countries, shocks may emanate from the current account and result from their heavy reliance on export revenue from producing primary products whose prices are unstable, while for emerging economies they may also come from the capital account. It is tempting to assume that shocks will have a negative impact on implementation and will blow programs off course. At the very least they may create additional problems for economic management. Again, however, the relationship may be more nuanced than this. Shocks may be positive. Will such shocks necessarily improve implementation? They may instead allow

governments to disengage from the Fund with the result that programs lapse, as the country no longer needs to draw on IMF resources. Meanwhile, negative shocks may lead the Fund to grant waivers, with the result that programs remain uninterrupted and continue to be fully financed.

A third factor influencing implementation is *adjustment capacity*. In principle, it may be expected that implementation will be superior in economies that are more diversified and flexible, and possess greater scope for economic adjustment. This may be proxied by the *level of economic development*, which is itself proxied by per capita income. It may also be captured by the degree of *trade openness*. Yet again, however, openness may work both ways. From one point of view, economies that are more open may be expected to have higher foreign trade price elasticities, making conventional exchange rate policies more effective. Demand management policies may also have an effect on the current account at a lower cost in terms of domestic economic activity. However, greater openness may also make an economy more vulnerable to exogenous shocks. Moreover, economies that are less open may have greater scope to benefit from trade liberalization.

The fourth and fifth factors that may be expected to influence implementation are the *level of financing from the Fund*, and the *ease with which future programs may be negotiated*. Higher levels of financing should provide greater incentives for governments to complete programs since there is a greater financial reward for accepting the perceived costs of IMF conditionality. More resources will be available to compensate the losers from economic reform. However, things may again be more complex than this simple idea implies. Other things being given, higher levels of financing may encourage governments to substitute out of adjustment. The question is

then the extent to which institutional arrangements surrounding conditionality allow them to do this.

Up to now we have covered only economic variables. What are the *political factors* that may, in principle, be expected to exert an impact on implementation and in what way? Once more, for many of them the *a priori* reasoning is ambiguous and unclear. The theory of policy implementation points to the importance of *special interest groups* (SIGs) or 'veto players' that have sufficient power to block reform (Drazen 2001; Mayer & Mourmouras, 2002, 2004)[1]. However, available data only allow imperfect measures to be used in empirical studies. For example, the role of SIGs may not be captured appropriately by simply looking at their number, although in general terms it may become more difficult to push through reform where there are many of them. Opposition may, however, be more effective where there is just one well organized and influential SIG as compared to a large number of disorganized ones. In principle, the strength of SIG opposition need not necessarily be reflected by parliamentary opposition if there are non-proportional voting systems or non-participation by SIGs in the parliamentary process. The opposition may, in any case, be *within* the government rather than outside it, as different ministries may have different views about the design of IMF programs. SIGs may furthermore influence the composition and nature of compliance rather than the level of compliance.

In a *democratic* society, opposition groups will have greater voice and influence and this may make implementation more difficult for an incumbent government as it seeks to enact unpopular policy. At the same time, if democracy implies greater involvement by civil society in policy decision-making and majority support for policy reform, a higher level of ownership may, in principle, make it more likely that programs will be implemented. *Powerful leadership*, whether under a

democratic or totalitarian regime, may be more relevant than the political system itself, but again this is difficult to capture empirically.

*Regime durability* may foster a consistent and coherent approach to economic policy, but it may also make economic reform less likely as special interests become more entrenched and able to resist reform that threatens any rents they receive under the *status quo*. Similarly *corruption* may also be anticipated to reduce the chances of implementation as distortions and rent-seeking occur. Although not beyond debate, there is also a broad consensus that corruption has a negative effect on economic growth which in turn makes it more difficult to implement reform.

The *stage of the electoral cycle* may also be expected to exert an effect on implementation, although this is another case where there may be opposing forces at work. Theories of the political cycle suggest that governments seek to pursue expansionary policies shortly before elections in order to raise consumption, reduce unemployment and garner popular support. These policies are likely to be inconsistent with IMF programs. After elections, governments may have to pursue counter-inflationary policies, which can either be blamed on the previous administration where there has been a change in the political party in power, or on the IMF where an IMF program is in place. This would be consistent with a political cycle of implementation based on the timing of elections, with implementation falling when elections are close. However, while incumbent governments may be anxious to demonstrate national sovereignty over policy in the build up to an election, they may not want to send out the negative signals about their economic management that may be associated with the failure to implement IMF programs. Similarly, the IMF may be reluctant to withdraw support shortly before an election for fear of being accused of trying to exert political influence. Much therefore depends on the circumstances in

which programs lapse. Is it the government that opts to disengage or the IMF that cancels or interrupts the program because of a country's failure to implement conditionality? Of course, if the Fund is seen as an agent of powerful capitalist countries – as it has been by some of its critics – it could be argued that the Fund's position will vary depending on the complexion of the incumbent government. Following an election, a new government may want to negotiate its own program with the Fund, one to which it is committed. If so, this would imply that implementation would not improve immediately after an election, but might improve after a lag of a few months.

The above brief conceptual survey illustrates why a neat, cut and dried theory of implementation is difficult to construct. It also suggests that while both political and economic variables may play a role, the precise nature of this role is theoretically unclear. One is therefore drawn towards empirical investigation. What factors in practice appear to significantly influence the implementation of IMF programs, and to what extent is it a political rather than an economic phenomenon?

## 6. NEW EMPIRICAL INVESTIGATION

### (a) Methodology and Data

We undertake regression analysis based on a pooled dataset in which each program is treated as an independent observation. Since the number of programs varies across countries the panel is unbalanced. Not all data are available for all countries or years and the number of observations for estimation depends on the choice of explanatory variables. The annual data cover the years 1992-2004 and

extend to 95 countries that participated in a Fund-supported program. All the variables, their definitions and the data sources are listed in Appendix I.

The macroeconomic data come from the *International Financial Statistics*, *World Economic Outlook* and *World Development Indicators* databases of the IMF and the World Bank. For the political economy data, the *Polity IV Dataset* (Marshall and Jaggers, 2002), the *Database of Political Institutions* (Beck *et al*., 2001) of the World Bank and the *International Country Risk Guide* datasets are used. The inherent subjectivity of some political datasets poses a limitation for the present study.

IMF programs include stand-by and extended programs, as well as programs under the concessionary facilities (Enhanced Structural Adjustment Facility and Poverty Reduction and Growth Facility) for low-income countries. Precautionary programs are excluded from the sample when the disbursement rate is used, since their inclusion would bias downwards the measurement of implementation. As mentioned earlier, in the results reported below we focus on the interruption measure of implementation but we use the disbursement measure to check for robustness. We do not use the MONA based measure[2].

Many studies of IMF programs encounter potential problems of selection bias, since IMF program countries are not randomly selected. Furthermore, endogeneity, where factors affecting the impact of IMF programs are generated by the programs themselves, and reverse causality, where the causal connections run in the opposite direction to the ones implied can make interpretation difficult. Even though we cannot claim full immunity from these methodological problems we believe that they are less important in the case of the research reported here. All the countries in our sample have chosen to sign agreements. They thus share this underlying characteristic. With regard to endogeneity, it may be that the factors that foster implementation can be

encouraged over time via IMF conditionality. To an extent, and in principle, they may

therefore be endogenous to the implementation of past IMF programs. But

endogeneity seems extremely unlikely for contemporary programs in terms of the

variables we include in our study. The implementation of contemporary programs will

not affect initial economic conditions, or primary product producing status. Nor will

many of the political variables we include be affected by contemporary

implementation. Given the specification of our model, it therefore seems reasonable to

assume that endogeneity and reverse causality will not be a problem. Nevertheless,

implementation of contemporary programs may depend on the incidence of past

programs, not just because these have encouraged openness and other forms of

economic liberalization but also because a better relationship may have been

established between governments and the IMF, leading to a greater commitment to

economic reform, or simply because there is a need to keep the IMF on side. By

including a measure of past involvement with the IMF, we therefore allow, to some

degree, for the possibility of endogeneity and reverse causality, although this variable

may also capture the probability as perceived by governments, that they will be

penalized for poor implementation by being excluded from future access to IMF

resources.


*(b) Model Specification*


The choice of probit and tobit techniques in our preferred model and

robustness checks is guided by the need to make efficient use of the information

contained in the implementation measures and by the data available. The interruption

proxy used in the econometric analysis is a discrete binary random variable, whereas the disbursement proxy takes values between 1 and 100.

Our strategy is to relate the various indicators of the probability of implementation to the underlying political and institutional factors in the borrowing country, to institutional factors and to initial economic conditions. Although the probability of program implementation is unobservable, it is related to an observable implementation proxy. The model can be described as follows:

$$y_i^* = \beta' x_i + \varepsilon_i,$$

where $y_i^*$ is the unobservable probability of successful program implementation, vector $\beta$ contains estimated coefficients, matrix $x_i$ contains economic and political economy variables, and $\varepsilon_i$ is a stochastic disturbance term. We assume a normal distribution and, hence, estimate the above equation as a probit model when we use the interruption index as the dependent variable and as a tobit model when we use the disbursement index in our check for the robustness of our results[3].

To examine the influence of political and economic conditions on implementation, each of our chosen proxies is regressed on an explanatory variable set containing economic and political variables. To obtain our preferred specification of the model we use a "testing down" approach. The unrestricted model includes variables that have been identified in the literature as significant determinants of implementation. We then sequentially drop regressors that do not have significant coefficients at the 10 percent level. Likelihood ratio tests are employed to test the joint significance of dropped variables.

The following were included as potential explanatory economic variables: net foreign direct investment as a percent of GDP, the rate of monetary expansion, trade as a percent of GDP, a dummy for primary product exporting countries, real GDP

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growth, GDP per capita, the central government balance relative to GDP, the rate of inflation, the current account balance relative to GDP, and international reserves in months of imports.

Although there is an array of political variables that could be included in the regression analysis, including most of them at the same time would lead to collinearity problems and a loss of precision. On the other hand, omitting relevant institutional and political variables would lead to biased estimates. The political economy variables we include are: two election year dummies, one for pre-election years, and one for post-election years[4], the degree of democracy, the quality of the bureaucracy, corruption, ethnic tensions, the representation of special religious, nationalistic, regional and rural interests in parliament, regime durability, and the existence of veto players as captured by new DPI data.

The last two variables are worth a closer examination. The theory of implementation suggests that veto players or special interest groups are key determinants of program implementation. Various indexes have been employed in the literature to capture the influence of those whose agreement is necessary before policies can be changed. For instance, a political cohesion variable has been used in two studies; Joyce (2003) for the period 1975-99 and Ivanova *et al.* (2003) for the period 1992-98. Ivanova *et al.* (2003) find it to be significant, while Joyce (2003) finds it to be insignificant. The political cohesion variable takes the value of zero for a one-party government, of one for a coalition government with two parties, of two for a coalition government with three or more parties, and of three for a minority government. Based on Roubini and Sachs (1989), this variable does not distinguish countries according to the effectiveness of electoral checks on government decision makers. Nor does the variable take into account the degree of parties' control over

members. Weaknesses with the political cohesion variable led the World Bank to delete it in versions of its DPI database after 2000.

The veto players (checks) variable in the newer versions of the same database attempts to correct for some of the weaknesses of the political cohesion variable. It counts the number of veto players in a political system, adjusting for whether these veto players are independent of each other, as determined by the level of electoral competitiveness in a system, their respective party affiliations, and the electoral rules.

The veto players (checks) variable has been modified and improved over the years (DPI2000, DPI2004). Earlier versions of it counted parties as veto players as long as they were in the government coalition (in parliamentary systems), even when the party was not needed to give the government the majority of the votes (e.g., Albania in the early 1990s, Finland in 1978, 1979). The current veto players (checks) variable only allows parties to count as veto players when their votes are needed for the government to sustain a majority. It therefore captures more efficiently the essential notion of veto players.

Ivanova *et al.* (2003) used a variable measuring the strength of special interest groups in parliament by computing the maximum share of seats held by parties representing special interests (religious, nationalistic, regional, and rural). Joyce (2003) used a similar variable; a dummy indicating whether the government party represented a special interest group. Ivanova *et al.* (2003) found their SIG variable to be significant, but Joyce (2003) did not. We have updated the composite indicator of the strength of special interests used by Ivanova *et al.*, and used it in our own regressions. While it was found to be significant in some of our earlier regressions, the existence of veto players seems to exert a more significant influence over implementation. This may not be surprising since the number of seats that SIGs have

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in the parliament does not necessarily translate into effective veto power. On the other hand, the new veto players (checks) variable that we use directly measures effective veto power.

Several IMF-related variables were also included in our regressions to test whether the nature of IMF involvement influences the implementation of programs. The size of IMF credits relative to a country's quota and the incidence of past programs with the Fund were also examined.

*(c) Results*

Table 4 lists the country programs that were included in our study. Table 5 summarizes our probit estimation results, but only shows those coefficients that were found to be significant. Simple parameters as well as marginal effects are reported.

[Insert  Tables 4 and 5]

From amongst the economic variables included in the regressions, only the volume of trade is found to exert a significant effect. Open economies have a better chance of having uninterrupted access to IMF resources. This result was confirmed when we examined disbursement as a check for robustness. The insignificance of past IMF programs implies that our finding is not picking up reverse causality.

The size of programs, as measured by the amount of IMF financing in relation to a country's quota, emerges as exerting a significant effect on program interruption. Larger resources appear to assist implementation. However, this finding was not

confirmed by the disbursement measure of implementation where the size of IMF loans appeared insignificant.

Turning to the political dimension of implementation, the veto players variable is significant in explaining interruption. Our robustness check using the disbursement measure confirms this result. Powerful veto players do militate against the implementation of IMF programs. However, none of the other political variables emerge as being significant[5].

Table 5 also reports the predictive accuracy of our preferred model. Overall the model predicts accurately 65 per cent of the time. However, it is much better at explaining cases where programs are not interrupted than those where they are. Historical descriptive data suggests that about 60 per cent of programs proceed without interruption. A straight guess of non-interruption would therefore be accurate about 60 per cent of the time. Our model exhibits 82 per cent accuracy. We generate considerable additional explanatory power. For interruptions, however, our preferred parsimonious model performs much less well and no better than a straight guess based on past experience. In many cases where openness, veto players and the amount of IMF resources suggest that a program would be implemented without interruption, some other factor gets in the way. However, these other factors are not sufficiently systematic to show up in our large sample regressions; they appear to be largely idiosyncratic.

We can use the probit estimation to identify outliers. There are no clear outliers (using our definition of them) in the case of inaccurately predicted implementation. But there are with respect to interruption. To identify them we compare the predicted values for the probability of interruption with our binary indicator of interruption and compute a residual which is the actual value minus the

fitted probability[6]. If we define 'outliers' as cases where the residual is +/- 1.5, we find that the cases of severely unpredicted interruption were Indonesia in 1997, Guyana in 1998, Jordan in 1996, the Philippines in 1998, Kyrgyzstan in 1998 and the Congo in 1996. As would be expected, removing these outliers improves the predictive performance of our model, but certainly does not allow us to conclude that its overall poor performance when predicting interruption is because of the inclusion of a few exceptional cases.

*(d) Interpretation and Discussion*

The results reported in the previous section may usefully be interpreted in terms of the conceptual framework introduced in Section 5. It would seem that initial conditions, as reflected by a wide range of macroeconomic variables, exert no significant influence over the implementation of IMF programs. This is broadly consistent with what has been found in earlier studies. Relatively large current account or fiscal deficits do not foretell poor implementation. The view that there is a standard or conventional program with similar targets that will be more difficult to achieve where initial conditions are relatively weak is not supported by our evidence. One potential inference is that, in negotiating programs and designing conditionality, the size of the economic disequilibria that need to be corrected is taken into account.

From the economic variables we examine, only trade openness has a significant effect on implementation. This is a robust finding and applies irrespective of whether implementation is gauged by the interruption or the disbursement measure. It also confirms the findings reported by Joyce (2003). Open economies are more likely to implement IMF programs. So what is going on? It could be that conventional

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IMF-supported policy measures have a greater chance of being effective in open economies. Foreign trade elasticities, for example, may be higher making exchange rate adjustment or the management of aggregate demand more effective policy instruments. It may also be that, as suggested by Joyce (2003), greater openness reflects a closer proximity between the policy preferences of governments and those of the IMF. Openness has not uncommonly been used as a proxy for economic liberalization more broadly defined. Following this line of argument, our finding is consistent with the claim that implementation depends on commitment and on the degree of ownership of the program. Countries that have open economies may be more likely to accept and endorse the IMF's analysis and therefore carry through agreed programs.

There is, however, a potential downside to openness. Open economies may be more vulnerable to trade shocks. In principle, it could therefore be that open economies are not only more likely to implement IMF programs, but are also more likely to turn to the Fund for assistance. However, studies of the determinants of IMF lending do not identify openness as a significant factor (Bird, 1996). Moreover, in our own regression analysis we incorporated various measures of exposure to shocks, such as a primary product exporter dummy, and a measure of export concentration (not reported in this paper) and we did not find them to be significant in our preferred model. This may imply that the Fund has used waivers and program modifications to protect programs from interruption and to sustain the flow of IMF resources to the affected countries. Of course, as we noted in Section 5, shocks can be either positive or negative. In principle, non-disbursement of an IMF credit could be as much to do with a positive trade shock that raises export revenue and reduces the need for IMF support, as with a negative shock that makes it more difficult to comply with the

original conditionality. The connection between the incidence of shocks and the implementation of IMF programs is likely to be more subtle and nuanced that we allow for in this paper. And, as with some of the other variables we find to be systematically insignificant, there may be a more complex story to be told in which factors that are significant idiosyncratically, and in different ways, wash out in large sample regression analysis.

In the short-run, the IMF can do little about a country's exposure to shocks apart from encouraging it to pursue a development strategy based on efficient export diversification, although it can, and our evidence suggests that it has, offset the effects of shocks via waivers and modifications. It can also provide further protection against negative shocks by offering supplementary finance in the event of them reoccurring. This is a path down which the Fund is moving in terms of its recently adopted exogenous shocks facility, although experience with the now little used Compensatory Financing Facility is cautionary.

Our evidence suggests that the Fund can also affect implementation through the amount of financial assistance it provides or, perhaps more accurately, the amount of finance it stands ready to provide. It is tempting to interpret this finding in a simple fashion. According to this interpretation, IMF resources 'bribe' countries to pursue unpleasant and politically costly policies. The bigger the 'bribe' the bigger the incentive to implement the IMF program, since the losers from the reforms can be better compensated. However, the reality of what is going on may be more complex than this. In other studies of IMF operations, and in particular in some studies of IMF catalysis, it is reported that the significant effect that the size of IMF loans is found to have is conditional upon the loans not being fully used (Mody and Savaria, 2006). It would be appear that having the resources in reserve, but also showing that they are

not needed, is what is important. The way in which the size of loans affects implementation may again be more subtle that the simple interpretation suggests.

Our results strongly confirm that domestic politics exert a significant influence over implementation whether measured by interruption or by disbursement. This is a robust finding and one that provides evidential support for the theoretical analyses of implementation that emphasize the importance of powerful opposition to the reforms favored in the IMF program. Up until now empirical investigations into the importance of special interest groups (SIGs) have been hampered by poor data. The empirical proxies have only loosely reflected the theoretical ideas. For example, the number of political parties in parliament will be a very imprecise measure of the influence of SIGs. The important question is whether those opposed to the economic reforms embedded in IMF programs possess the power to disrupt implementation; are they 'veto players'. The data we use allows us to come closer than previous studies to capturing the influence of opposition groups by using data recently assembled by the World Bank's *Database of Political Institutions*. As a consequence we find that while conventionally used measures of SIGs generate insignificant results, confirming the results achieved by Joyce (2003) and by Dreher (2003), our measure of veto players is significant; a result that is robust across both the interruption and disbursement measures of implementation. The result not only provides empirical support for the theory of special interests as articulated by Drazen (2001) and others, but it also supports initiatives to promote national ownership. In a related paper one of us has argued that the political economy of implementation requires the IMF to temper the design of its programs in terms of technical economics (Bird and Willett, 2005) and the findings reported in this study further reinforce this suggestion. They also suggest

that the Fund needs to offer more effective support to governments in explaining the rationale of proposed economic reforms to civil society and opposition groups.

While we find empirical support for the role of veto players in explaining implementation, our results fail to find support for other political variables such as regime durability and the stage of the electoral cycle. Again, however, and given that other studies do report findings to suggest that these factors may be significant, it may be premature to eschew their potential influence at this stage. Regime durability has sometimes been interpreted to capture Olson's claim (Olson, 1993) that as the time in power of the incumbent regime increases, SIGs are better able to identify more clearly how they are affected by policy and to organize more effective opposition. Incumbent governments may find it progressively more difficult to alter the *status quo*, or may experience a higher degree of policy inertia that limits their ability to implement IMF conditionality. As far as the electoral cycle is concerned, Dreher (2003) finds, albeit limited, support for it. Some of the specifications of our overall model that were less satisfactory and are not reported here also hinted that the election cycle could be significant, although we found that whether an election was imminent or had very recently occurred was insignificant. The implementation of programs instead seemed to improve significantly only after some months had elapsed since an election. Perhaps new governments need a period of time to negotiate their own programs with the Fund. Thus, while our preferred model rejects the systematic significance of the electoral cycle, there may be a remaining suspicion that, on occasion, it may be important and that there may be opposing forces that cancel out in large sample studies.

Overall our results are consistent with what we anticipated. We find that implementation depends on country characteristics (trade openness), the amount of

IMF resources made available, and the strength of veto players in opposing reform. Narrow explanations based exclusively on economic variables fall short. Moreover, some conventional arguments that implementation is adversely affected by the size of initial macroeconomic disequilibria seem to be misplaced. Our findings also offer some value added over simple guesses based on the historical record of implementation. But this applies to non-interruption rather than to interruption. The inference could be that programs may be interrupted for a relatively wide range of reasons that are of idiosyncratic or occasional rather than systematic importance, as well as those that we identify as systematically significant. The next stage in our research is to examine the outliers identified by our probit estimation in order to discover what these factors may be. In any event, while we fail to provide a 'complete' explanation of implementation, we do discover a range of robustly significant determinants that exert a systematic influence. These in turn have important implications for the design of policy. Our findings suggest that policy initiatives within the Fund are moving in the right direction, but they may not yet have moved far enough.

## 7. CONCLUDING REMARKS

When assessing the effectiveness of IMF programs we need to take into account that not all of them are fully implemented. Moreover, implementation appears to matter when explaining the overall effects of IMF programs. What is the benefit of conditionality if it is not implemented? More specifically, if conditionality is designed to send out a signal that governments are committed to economic reform, poor implementation creates noise around the signal that makes it difficult to discern. If the

policy imperative is to improve the rate of implementation, it is important to understand the factors that influence it.

Theoretical analysis has identified a range of potential determinants, and empirical investigation is needed to clarify which of these are systematically significant in practice. A relatively small number of studies have provided some preliminary insights, but they have also illustrated the methodological challenges and data difficulties. There has been a range of sometimes conflicting results which transmit confusing messages for policy.

This paper contributes to this growing field of study. It draws on the most up-to-date theoretical work to isolate a group of factors that may be expected *a priori* to influence implementation. It suggests empirical proxies for these factors drawing on the most up-to-date data. It uses a range of economic and political variables and applies appropriate regression techniques to a large data set covering 95 countries over the period 1992 – 2004. It also tests for robustness by examining various measures of implementation.

In terms of economic variables, conventional indicators of macroeconomic performance and policy emerge as being insignificant. Instead, it is trade openness that makes a difference; economies that are more open have a better record of implementation.

Confirming recent theoretical contributions and using the best available data, we also discover that implementation is disadvantaged by the existence of veto players, although other political factors appear to be systematically insignificant.

Although the paper provides value added to our understanding of implementation, it does not as yet allow us to predict, with confidence, the probability that individual programs will or will not be implemented. But the results reported here

do suggest that predictions will be wide of the mark if they fail to include salient political factors. This suggests that the IMF needs to take domestic politics into account when forming a judgment as to whether programs will be implemented. The results also suggest that the Fund has been right to stress the importance of national ownership, although it remains to be seen whether the related policy initiatives will be adequate to improve implementation.

Further research also needs to focus on formulating a more sophisticated political economy approach to implementation. Given the shortcomings of large sample data relating to political variables, this is likely to involve collecting together a series of structured case studies that can more successfully capture idiosyncratic economic and political variables.

Acknowledgement

APPENDIX I. Variable Definitions and Data Sources

| **VARIABLE** | **DEFINITION** | **SOURCE** |
|---|---|---|
| PROXIES FOR THE DEPENDENT VARIABLE OF IMPLEMENTATION | | |
| Disbursement Ratio | Share of committed funds disbursed | IMF Country Reports |
| Program Interruption | Irreversible Interruption Dummy | Ivanova *et al.* (2003) |
| | | Nsouli *et al.* (2004) |
| MACROECONOMIC VARIABLES | | |
| FDI | Net Foreign Direct Investment as percent of GDP | WDI |
| Money Growth | Rate of monetary expansion | WDI |
| Trade Volume | Sum of imports and exported divided by GDP | WDI |
| Growth | GDP growth rate | WEO |
| Government Balance | Central Government Balance/GDP | IFS |
| Inflation | Inflation | IFS |
| Current Account | Current Account Balance/GDP | WEO |
| Reserve/Imports | Reserves to Imports in months | WEO |
| Primary Product Exporter | Dummy for Primary Product Exporter | WDI |
| GDP per capita | Initial GDP per capita (constant at 2000) | WDI |
| IMF RELATED VARIABLES | | |
| Loans to Quota | Approved IMF Credit in relation to a country's quota | Nsouli *et al.* (2004) |
| Fund months spent | Number of months spent in IMF-supported programs | Nsouli *et al.* (2004) |
| POLITICAL ECONOMY VARIABLES | | |
| Pre-election | Share of the year which is within twelve months prior to a national election | Dreher and Vaubel (2005) |
| Post-election | Share of the year which is within twelve months after to a national election | Dreher and Vaubel (2005) |
| Veto Players | Number of veto players in the legislature | DPI |
| Democracy | Indicator of type of regime. Includes measures of (a) competitiveness of political participation, (b) competitiveness of executive recruitment and, (c) constraints on the chief executive. Ranges from strongly autocratic (-10) to strongly democratic (+10) | Polity IV |
| Strength of Special Interest | Computed as the maximum share of seats in the parliament held by parties representing religious, nationalistic, regional and rural interest groups. | DPI |
| Regime Durability | Regime Durability, the number of years since the most recent regime change | Polity IV |
| Quality of Bureaucracy | Institutional strength and quality of the bureaucracy measured on a 4 point scale. | ICRG |
| Corruption | Corruption within the political system, 6 points. | ICRG |
| Ethnic Tensions | The degree of tension within a country attributable to racial, nationality or language division. Higher ratings suggest minimal tension, 6 points. | ICRG |

APPENDIX II. Robustness Checks

| | Probit Analysis of Program Interruption | | Logit Analysis of Program Interruption | | Tobit Analysis of Disbursement Rate | |
|---|---|---|---|---|---|---|
| | Coefficients | Marginal Effects | Coefficients | Marginal Effects | Coefficients | Marginal Effects |
| Trade Volume to GDP | -0.006 (0.003) * | -0.002 ** | -0.010 (0.005) * | -0.002 ** | 0.273 (0.137)** | 0.150 ** |
| Veto Players | 0.119 (0.063) * | 0.046 * | 0.195 (0.102) * | 0.047 * | -6.650 (2.349) * | -3.673*** |
| IMF Loans to Quota | -0.172 (0.096) * | -0.067 * | -0.306 (0.179) * | -0.074 * | | |
| Constant | -0.026 (0.339) | | -0.001 (0.561) | | 95.587 (13.205)*** | |
| No. of Observations | 145 | | 145 | | 112 | |
| Log-likelihood | -92.622 | | -92.503 | | -349.216 | |

Notes:   Standard errors are reported in parentheses.
*** indicates significance at 1 percent;
** indicates significance at 5 percent;
* indicates significance at 10 percent level.

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Table 1. Correlations of Implementation Measures

|  | Overall Implementation Index | Share of Committed Funds Disbursed | Programs Having Irreversible Interruptions |
|---|---|---|---|
| Overall Implementation Index | 1 |  |  |
| Share of Committed Funds Disbursed | 0.413 | 1 |  |
| Programs Having Irreversible Interruptions | -0.403 | -0.745 | 1 |

Notes:    Based on programs approved between 1992 and 2004. Each cell contains Pearson Correlation Coefficient.

Source:   Authors' own calculations based on IMF data.

Table 2. Record on Implementation

| | Programs | Span | Countries | Implementation (%) | Proxy |
|---|---|---|---|---|---|
| Beveridge and Kelly (1980) | 105 | 1969-78 | 105 | **60** | Fiscal Conditionality |
| Haggard (1985) | 30 | 1974-84 | 30 | **20** | Interruption and Conditionality |
| Zulu and Nsouli (1985) | 35 | 1980-81 | 35 | **61** | Objectives and Instruments |
| Edwards (1989) | 34 | 1983-85 | 34 | **31** | Fiscal Conditionality |
| Polak (1991) | 22 | 1988-99 | 22 | **50** | Fiscal Conditionality |
| Killick (1995) | 305 | 1979-93 | - | **47** | Disbursement Rate (80%) |
| Mecagni (1999) | 36 | 1986-99 | 28 | **28** | Non-interruption |
| Mercer-Blackman and Unigovskaya (2000) | 33 | 1993-97 | 33 | **51** | Structural Conditionality by MONA |
| Edwards (2001) | 347 | 1979-97 | - | **60** | Non-interruption |
| Ivanova *et al.* (2003) | 170 | 1992-98 | 95 | **57/71/73** | Non-interruption, Disbursement Rate, Conditionality by MONA |
| Nsouli *et al.* (2003) | 197 | 1992-2002 | 95 | **60/75/75** | Non-interruption, Disbursement Rate, Conditionality by MONA |
| *Current Research* | *218* | *1992-2004* | *95* | *60/74/75* | *Non-Interruption, Disbursement Rate, Conditionality by MONA* |

Table 3. Empirical Evidence on Implementation

| | Programs | Span | Countries | Proxy | Method | Results |
|---|---|---|---|---|---|---|
| Edwards (1989) | 34 | 1983-85 | 34 | Conditionality | Case | Negative shocks |
| Polak (1991) | 22 | 1988-99 | - | Conditionality | Case | Negative shocks |
| Killick (1995) | 305 | 1979-93 | - | Disbursement Rate | Probit | Export base, Debt, Size of IMF loan |
| Mecagni (1999) | 36 | 1986-99 | 28 | Interruption | Case | External shocks, domestic political economy factors |
| Ivanova *et al.* (2003) | 170 | 1992-98 | 95 | Combination of Interruption, Disbursement, MONA | MIMIC | Special interests, political cohesion, inefficient bureaucracy, ethnic fractionalization |
| Nsouli *et al.* (2004) | 195 | 1992-2002 | 95 | Interruption, Disbursement, MONA | Tobit, Probit | Political stability, military involvement, corruption, ethnic tensions, growth, size of IMF loan |
| Joyce (2003) | 384 | 1975-99 | 77 | Disbursement Rate | Tobit | Democracy, trade volume, length of tenure, ethnic fractionalization |
| Dreher (2003) | 104 | 1975-98 | 67 | Disbursement Rate | Probit | Democracy, elections, government consumption, short-term debt, GDP pc, budget deficit |
| *Current Study* | *218* | *1992-2004* | *95* | *Interruption, Disbursement* | *Tobit, Probit, Ordered Probit* | *Volume of Trade, Number of Veto Players, IMF Loans relative to Quota* |

## Table 4. Countries and Programs

| | | | |
|---|---|---|---|
| Albania | 1993, 1998, 2002 | Lao People's Dem. Rep. | 1993, 2001 |
| Algeria | 1994, 1995 | Latvia | 1993, 1995, 1996, 1997, 1999, 2001 |
| Argentina | 1992, 1996, 2000, 2003 | Lesotho | 1994, 1995, 1996, 2001 |
| Armenia | 1995, 1996, 2001 | Lithuania | 1993, 1994, 2000, 2001 |
| Azerbaijan | 1995, 1996, 1996 | Macedonia (FYR) | 1995, 1997, 2000, 2000, 2003 |
| Belarus | 1995 | Madagaskar | 1996, 2001 |
| Benin | 1993, 1996, 2000 | Malawi | 1994, 1995, 2000 |
| Bolivia | 1994, 1998 | Mali | 1992, 1996, 1999 |
| Bosnia & Herzergovina | 1998, 2002 | Mauritania | 1992, 1995, 1999, 2003 |
| Brazil | 1998, 2001, 2002 | Mexico | 1995, 1999 |
| Bulgaria | 1994, 1996, 1997, 1998, 2002 | Moldova | 1993, 1995, 1996, 2000 |
| Burkina Faso | 1993, 1996, 1999 | Mongolia | 1993, 1997, 2001 |
| Cambodia | 1994, 1999 | Mozambique | 1996, 1999 |
| Cameroon | 1994, 1995, 1997, 2000 | Nepal | 1992 |
| Cape Verde | 1998, 2002 | Nicaragua | 1994, 1998 |
| Central African Republic | 1994, 1998 | Niger | 1994, 1996, 2000 |
| Chad | 1994, 1995, 2000 | Pakistan | 1993, 1994, 1994, 1995, 1997, 1997, 2000, 2001 |
| Colombia | 1999, 2003 | Panama | 1995, 1997, 2000 |
| Congo | 1994, 1996 | Papua New Guinea | 1995, 2000 |
| Congo Democratic Republic | 2002 | Peru | 1993, 1996, 2001, 2002 |
| Costa Rica | 1993, 1995 | Philippines | 1994, 1998 |
| Cote D'Ivoire | 1994, 1998 | Poland | 1993, 1994 |
| Croatia | 1994, 1997, 2001 | Romania | 1994, 1997, 1999, 2001 |
| Czech Republic | 1993 | Russian Federation | 1995, 1996, 1996, 1999 |
| Djibouti | 1996, 1999 | Rwanda | 1998 |
| Dominican Republic | 1993, 2003 | Senegal | 1994, 1994, 1998 |
| Ecuador | 1994, 2000, 2003 | Serbia and Montenegro | 2001, 2002 |
| Equatorial Guinea | 1993 | Sierra Leone | 1994, 1994, 2001 |
| Egypt | 1993, 1996 | Slovak Republic | 1994, 1994 |
| El Salvador | 1993, 1995, 1997, 1998 | Sri Lanka | 2001 |
| Estonia | 1993, 1995, 1997, 2000 | Tajikistan | 1998 |
| Ethiopia | 1996, 2001 | Tanzania | 1996, 2000 |
| Gabon | 1994, 1995, 2000, 2004 | Thailand | 1997 |
| Gambia | 1998, 2002 | Togo | 1994 |
| Georgia | 1995, 1996, 2001 | Turkey | 1994, 1999, 2002 |
| Ghana | 1995, 1999 | Uganda | 1994, 1997 |
| Guinea | 1997, 2001 | Ukraine | 1995, 1996, 1997, 1998 |
| Guinea-Bissau | 1995, 2000 | Uruguay | 1996, 1997, 1999, 2000, 2002 |
| Guyana | 1994, 1998 | Uzbekistan | 1995 |
| Haiti | 1995, 1996 | Venezuela | 1996 |
| Honduras | 1992, 1999 | Vietnam | 1993, 1994, 2001 |
| Hungary | 1993, 1996 | Yemen | 1996, 1997, 1997 |
| Indonesia | 1997, 1998, 2000 | Zambia | 1995, 1995, 1999 |
| Jamaica | 1992 | Zimbabwe | 1992, 1992, 1998, 1999 |
| Jordan | 1994, 1996, 1999, 2002 | | |
| Kazakhstan | 1994, 1995, 1996, 1999 | | |
| Kenya | 1993, 1996, 2000 | | |
| Korea | 1997 | | |
| Kyrgyz Republic | 1993, 1994, 1998, 2001 | | |

Table 5. Estimation Results

| | Probit Analysis of Program Interruption | | Probit Analysis of Program Interruption (Without Outliers) | |
|---|---|---|---|---|
| | **Coefficients** | **Marginal Effects** | **Coefficients** | **Marginal Effects** |
| Trade Volume to GDP | -0.006 (0.003) * | -0.002 ** | -0.139 (0.004) *** | -0.005 *** |
| Veto Players | 0.119 (0.063) * | 0.046 * | 0.195 (0.071) *** | 0.071 *** |
| IMF Loans to Quota | -0.172 (0.096) * | -0.067 * | -0.490 (0.189) *** | -0.067 *** |
| Constant | -0.026 (0.339) | | 0.393 (0.400) | |
| No. of Observations | 145 | | 139 | |
| R-squared (McFadden) | 0.061 | | 0.155 | |
| Log-likelihood | -92.622 | | -93.300 | |
| Prediction success | | | | |
|   Interruption correctly predicted | 41% | | 45% | |
|   Non-interruption correctly predicted | 82% | | 80% | |
|           Total | 65% | | 66% | |

Notes:    Standard errors are reported in parentheses.
           *** indicates significance at 1 percent;
           ** indicates significance at 5 percent;
           * indicates significance at 10 percent level.

[1] For an earlier discussion of the rate of 'veto players' see Tsebelis (2001) and for a theoretical analysis of special interests see Grossman and Helpman (1994 and 2001).

[2] The IEO (2002) finds substantial errors and gaps in the MONA database for tracking performance under programs, especially with regard to data on outcomes. It concludes that "existing weaknesses in data on how programs have performed are an impediment to efforts to enhance the IMF's ability to learn from experience and to monitor the implementation and impact of its own policies".

[3] To check for the robustness of our conclusions, we use the logit model on the assumption that the cumulative distribution is logistic. The results of the logit regression confirm the findings of the probit regression. The estimated coefficients and marginal effects are very similar, with similar degrees of significance. See Appendix II for a comparison of the results.

[4] To control for the influence of elections, an index is used which measures the share of the year which is within twelve months prior to a national (executive or legislative) election. For example, if an election is in February, the pre-election index would take the value of 1/12 and the post-election index would take the value of 10/12.

[5] We have also created two other implementation indices by breaking down the disbursement rate into four and five intervals. The first index of implementation classifies program countries as non-compliers for 0 to 25%, poor compliers for 26 to 50%, weak compliers for 51 to 75% and good compliers for 76 to 100%. The second index breaks the disbursement rate into 5 intervals; 0-20%, 21-40%, 41-60%, 61-80% and 81-100%. Using the ordered probit model and following the same procedure of log likelihood tests, we find support for the significance of the veto players variable. It is significant at one percent and has a negative coefficient in both specifications.

[6] The precise computation of the residual taking account for the variation of the estimator is as follows;
$r_i = y_i - \hat{F}_i / \sqrt{1 - h_{ii}}$ , where $h_{ii} = F(\hat{\beta}'x_i)\left[1 - F(\hat{\beta}'x_i)\right]x_i'Est.Asy.Var\left[\hat{\beta}\right]x_i$, and $\hat{F}_i$ denotes the fitted probability estimator.