Investor Responses to IMF Program Suspensions: Is Noncompliance Costly?

Martin S. Edwards, Texas Tech University

Objective. This article reinvestigates the “catalytic” effects of IMF programs on investment controlling for the implementation of the program, allowing us to ascertain if capital markets respond to the mere announcement of a Fund program or its sustained positive implementation. Methods. Using a panel of data based on the experience of 106 developing countries entering Fund programs between 1979 and 1995, I employ both probit and tobit estimations to assess the effects of the announcement of a Fund program (and performance under it) on inflows of portfolio investment. Results. I find mixed evidence that good implementation serves to slow capital flight. I find consistently strong evidence that failed Fund programs produce portfolio flight. Conclusion. The findings suggest that we should think about the Fund’s influence (both positive and negative) in light of how capital markets respond to its signals.

One of the most visible international economic organizations is the International Monetary Fund (IMF or Fund). IMF programs in developing countries have become the focus of protest and controversy. The Fund has often been pilloried by politicians as ineffective or causing harm, but there have been few systematic attempts to understand the broader influence of IMF programs on developing countries. This article aims to address this shortcoming by focusing on the effects of Fund program implementation on the behavior of potential investors.

To understand the influence that the Fund has over developing countries, I focus on how investors respond to signals from the IMF. The Fund claims that its programs have “catalytic” effects, and that signing a program opens the door for inflows of investment, which are an added benefit for states to use the IMF. Unfortunately, there has been little empirical evidence suggesting this effect actually exists.

*Direct correspondence to: Martin S. Edwards, Department of Political Science, Texas Tech University, 17 Holden Hall, Lubbock TX 79409 (martin.edwards@ttu.edu). Data available from the author on request. Thanks are due to Robert Kaufman, Thomas Willet, and Michael Bordo for their comments on previous drafts. This project would not have been possible without the support of the Institute for the Study of World Politics and the IMF’s Archives and Records Services Division. Additional thanks are due to Jean Marcouyeaux, Kahinde Mbanefo, and Madonna Gaudette for their assistance during the author’s visit to the Fund.

A previous study (Edwards, 2003) found evidence that IMF programs deter inflows of portfolio investment in the first program year. The previous study, however, did not evaluate the long-run effects of programs on flows, nor did it include the degree of program implementation. This article offers a richer picture of the IMF’s influence. I find that breaching IMF programs serves as a powerful signal about a state’s commitment to adjustment. Investors learn that a noncompliant state is not an appropriate climate for investment and respond to the program suspension with capital flight. Significantly, the amount of capital flight is greater than a counterfactual non-program baseline, suggesting that the magnitude of losses exceeds those that would have occurred had a state not entered a program. The evidence is less clear that states with good performance under a Fund program are able to entice measurable positive inflows.

**Background**

Before we can proceed to the heart of the matter, more setup of the problem is warranted. IMF programs are designed to address disequilibria in a state’s balance of payments. Thus, it is not surprising that studies point to a consistent number of factors as predictors of IMF programs: low reserves, high levels of debt, high current account deficits, and, often, high inflation, budget deficits, and rapid growth of the money supply (Knight and Santella, 1997; Conway, 1994; Joyce, 1992).

Why are IMF programs thought to have catalytic effects? The central claim is that conditionality serves as a signal that allows external observers to distinguish between “committed” and “uncommitted” reformers and allocate their assets accordingly. What makes conditionality work as a signal is its contractual nature. The receipt of portions of the Fund’s loan (referred to as tranches) are contingent on a state’s commitment to austerity. State compliance with the program is assessed quarterly, and breaches of limits on the growth of the money supply or expenditure can force the Fund to suspend lending. In other words, observers see that a country signs a letter of intent, and this informs them that a country is adopting credible policies (Rodrik, 1995; Dhonte, 1997). As a result, one would expect an increase in flows of capital and investment following the decision to sign a Fund letter of intent. IMF policy documents refer to this effect as an added benefit of conditionality (Fischer, 1999; Kohler, 2000; IMF, 2001).

Unfortunately, there has been little previous evidence to suggest that IMF programs actually might have catalytic effects (Adji et al., 1997; Bird and

---

2The IMF agreements that I focus on in this project are termed stand-by arrangements and extended fund facility (EFF) arrangements. Stand-by agreements are generally 12–18 month arrangements, while EFFs are for a longer duration (24 months and up) and for a larger amount. Because these programs are designed for middle-income states, they comprise a strong test for catalytic effects.
Rowlands, 1997, 2000; Hajivassiliou, 1987; Rodrik, 1995; Rowlands, 2001). There are, in addition, three problems with previous studies. First, they disagree on what the dependent variable actually should be. Some authors focus on foreign direct investment (Adji et al., 1997), others focus on debt (Hajivassiliou, 1987), and others focus on official lending (Rowlands, 2001). This poses a problem because the logic of the catalytic argument implies a focus on the private sector, not the public sector. If IMF programs are a signal about a country’s commitment to reform, then it makes sense to measure their effects on market actors, since these actors have the greatest demand for new information and need the IMF to provide it.

The logic of the argument also necessitates a focus on the asset specificity of capital flows. For there to be evidence of catalysis, we have to be able to focus our study on mobile rather than fixed assets. After all, we might not see any evidence of the Fund program having effects, not because the program has no effects, but rather because we are studying its effects on immobile factors. This having been said, portfolio investment is a most likely case for catalytic effects. It is sufficiently mobile so that investors can shift their assets in response to new information (Sarno and Taylor, 1999; Maxfield, 1998). Second, existing studies disagree on the appropriate specification by ignoring program implementation. Program implementation matters because successful reform, which actually catalyzes capital flows, is never guaranteed. As numerous authors have noted, economic reform is politically difficult, and politicians have incentives to renege on their commitments (Nelson, 1984; Haggard, 1986; Haggard and Kaufman, 1992; Bates and Krueger, 1993). In addition, because Fund programs are often signed to “tip the scales” and use international leverage to bolster domestic reform, it is not always a given that the program will be successfully implemented. This makes the mere announcement of the Fund’s endorsement a noisy signal, since external observers still do not know whether a state is committed to reforms or not.

The existing studies on catalytic finance also neglect the nonrandom nature of IMF programs, and this also poses an inferential problem, since we know that the same variables that affect financial flows, such as high levels of debt service, also affect whether or not a state goes to the Fund. States seek

3Relatedly, the one study that shows evidence of a positive catalytic effect (Mody and Saravia, 2003) evaluates the effects of Fund programs on asset spreads on international bonds. This effect, however, is contingent on the degree of economic crisis, as spreads on bonds are lower only in those cases in which a state’s economic fundamentals have not significantly deteriorated.

4One could argue that bank lending might also be considered as part of a catalytic effect, though such claims need to be treated carefully. As Gould (2003) notes, bank lending is often negotiated as part of the IMF program, and these lenders often find conditions written into the letter of intent that benefit them. As a result, one cannot rely on bank loans as evidence of a catalytic effect, since these flows are not the result of new information.

5Bird and Rowlands (1997:971) acknowledge this possibility, but do not address it in their specification.
IMF programs for specific reasons, and we have to understand these reasons since they might also affect financial flows. Failure to do this compromises one of the assumptions that we make in regression, which is that variables that are correlated with other independent variables that are not in the model do not systematically affect the dependent variable. Since we would suspect that the sample of Fund client states will be less likely to attract catalytic finance even if they did not go to the Fund (owing to their weak macroeconomic fundamentals), a simple regression model can underestimate the effects of the Fund. To generate reliable parameter estimates, we have to take sample selection seriously. The empirical tests below employ the techniques developed to handle nonrandom selection to better assess the effects of Fund programs (Goldstein and Montiel, 1986; Ul Haque and Khan, 1998; Bordo and Schwartz, 1999; Bagci and Perraudin, 1997; Vreeland, 2002), which allows us to separate the effects of the state’s economic fundamentals from the effects of the letter of intent. In addition, we also need to control for the factors that affect whether or not a state is able to honor its commitments. Thus, we can think about this schematically in Figure 1. To assess the effects of the program on inflows requires that we generate and compare estimates of the values at Nodes A, B, and C. If implementation matters, then we should see evidence of catalytic effects in this most likely case.

6In other words, we assume that \( E(u_i|X) = 0 \). The problem is that unobserved factors induce a correlation between the error term and the independent variable (Berk, 1983; Achen, 1986).
Testing the Argument

To test this argument we estimate simulated values for each node in Figure 1. To do this, we first estimate the factors that determine whether or not states enter Fund programs. From this equation, we generate an instrument to capture the potential selection effect. This is traditionally known as the hazard rate or lambda, and represents the probability of being under a Fund agreement over time. We form the hazard rate from the predicted values from this model, and add this variable to a model of investment flows, thus allowing us to control for the effects of program selection. A pair of models is estimated; one for those states actually under Fund programs, and one for those that are not. We use only one of these (the nonprogram baseline) as our estimate for the value of Node A. Following this, we repeat the process, now estimating not whether a state is under a program, but whether a program is suspended. Again, we create a hazard rate to capture the unobserved correlates, and add this to a model of investment flows to control for both selection and program suspension. This allows us to generate a pair of estimates for programs under good status and suspended ones. The mean of these values are the values for Nodes B and C.

I employ two statistical techniques to accomplish these estimations. First, I use a probit with corrections for temporal dependence (Beck, Katz, and Tucker, 1998) to assess the determinants of program selection. The independent variables are those that are standard in previous studies: debt service, reserves, growth, changes in terms of trade, and the monetary policy stance (Cornelius, 1987; Joyce, 1992; Conway, 1994; Knight and Santaella, 1997). The flows model is a tobit, which takes into account the fact that the dependent variable is censored at zero.

The dependent variable for the flow model is portfolio investment, which is operationalized as purchases of equities, depository receipts, and country funds by foreign investors measured as a percentage of gross national product (GNP). Importantly, the volume of net foreign direct investment (FDI) and portfolio investment in emerging markets have exceeded that of external borrowing for every year during the 1990s (IMF, 1997, 2001). Thus, accessing portfolio flows has become more crucial for countries, and understanding the responses of portfolio investors to Fund program announcements and suspensions is a critical test for catalytic effects.

7 Other scholars have employed a bivariate probit, which estimates a Fund equation and an LDC equation (Knight and Santaella, 1997; Vreeland, 2003). I chose not to use this technique because reducing the number of observations by looking at program entry and suspension makes model convergence for a bivariate probit increasingly problematic.

8 One could argue that the most appropriate measure for catalysis is a price measure such as interest rates on government bonds rather than a capital flow measure. However, there are important data limitations here. The previously cited study by Mody and Saravia relies on proprietary data. In addition, the IMF’s International Financial Statistics only has data on interest rates on 10-year government bonds (a measure used in Mosley, 2003a for the OECD) for 17 developing countries.
These findings confirm the conventional wisdom: states enter IMF programs under low growth, high debt, and low reserves. Importantly, trade shocks and the monetary policy stance do not affect whether a state is under an IMF program. Adding other variables to proxy for great power influence, including U.S. foreign aid and a state’s quota with the Fund, did not improve model fit, nor were these measures significant.

Using this model, we form the hazard rate from the predicted values and then use this and other independent variables to estimate a model of portfolio investment flows. The independent variables used to predict portfolio flows are per-capita GNP, growth, exports, and U.S. borrowing spreads. This last variable controls for “push factors” from developed countries (Calvo, Leiderman, and Reinhart, 1993, 1996).

These findings confirm a key role of wealth and growth in attracting portfolio flows, as increases in per-capita GNP and growth produced increases in portfolio investment. This effect for these variables holds regardless of whether a Fund program is present. Trade flows, measured as exports as a percentage of GNP, were negatively related to portfolio flows in both models, though neither were significant.

<table>
<thead>
<tr>
<th>TABLE 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sources of IMF Programs</td>
</tr>
</tbody>
</table>
| Debt\(_{-1}\) & 0.0123***
| (0.0031) |
| Reserves\(_{-1}\) & –0.0639***
| (0.0186) |
| Growth\(_{-1}\) & –0.0273***
| (0.0078) |
| Terms of trade\(_{-1}\) & 9.41e-15
| (8.06e-15) |
| Change in net domestic credit\(_{-1}\) & –0.00011
| (0.00036) |
| Constant & 0.8543***
| (0.0927) |
| \(N\) for cubic spline segments & 1,206 (99 countries) |
| \(\chi^2\) & 0.0000 |

*p<0.05; **p<0.01; ***p<0.001.

Model \(\chi^2\): 0.0000. Robust standard errors in parentheses. Percent correctly predicted: 83.83 percent.

Fiscal policy variables are not included here because of missing data concerns.

These results were unchanged with the addition of variables such as debt service, inflation, and investment.

The coefficients on the other independent variables are affected by the lagged dependent variable on the right-hand side (Achen, 2000). The main results of the article, however, are unaffected by the omission of the lagged portfolio flows term.

9Fiscal policy variables are not included here because of missing data concerns.
10These results were unchanged with the addition of variables such as debt service, inflation, and investment.
11The coefficients on the other independent variables are affected by the lagged dependent variable on the right-hand side (Achen, 2000). The main results of the article, however, are unaffected by the omission of the lagged portfolio flows term.
Controlling for push factors was a wise move, as increases in the U.S. interest rate spread lead to inflows in portfolio investment, regardless of whether a state was under an IMF program. In both instances, investors appeared willing to move to emerging markets when U.S. markets seemed less profitable.

Finally, we see that unobserved variables associated with being under a program do not have independent effects on flows. Because the hazard rate is formed from the predicted values in the probit model, it does not have a ready interpretation. Here, however, we can think of it as capturing investment-specific factors as well as unobserved economic fundamentals. This is somewhat counterintuitive, since we would expect these factors to have an independent effect on flows. One tentative explanation here is that this variable does not capture program implementation, which would clearly be a concern for investors operating with foresight.

What is the exact effect of the program on flows? To estimate this, we take the difference in the average level of flows for each sample. This is detailed in Table 3.

These estimates suggest that Fund programs can be beneficial. The average state under a program experiences less of an outflow of portfolio investment compared to those that are not under a Fund program. Thus, these findings support the notion that states seek Fund programs to signal credibility, and that there are important benefits from using IMF conditionality. This difference amounts to about $30 million in new portfolio

### Table 2

**Effects of IMF Programs on Portfolio Flows**

<table>
<thead>
<tr>
<th>Dependent variable</th>
<th>States Under Fund Programs</th>
<th>States Not Under Fund Programs</th>
</tr>
</thead>
<tbody>
<tr>
<td>( t_{-1} )</td>
<td>1.155***</td>
<td>0.7465***</td>
</tr>
<tr>
<td>( \text{GNP per capita}_{t-1} )</td>
<td>(0.2323)</td>
<td>(0.1373)</td>
</tr>
<tr>
<td>( \text{Growth}_{t-1} )</td>
<td>3.13e-06***</td>
<td>2.59e-06***</td>
</tr>
<tr>
<td>( \text{Exports}_{t-1} )</td>
<td>-0.000043</td>
<td>-0.000056</td>
</tr>
<tr>
<td>( \text{U.S. interest rate spread}_{t-1} )</td>
<td>0.01711***</td>
<td>0.0164***</td>
</tr>
<tr>
<td>Hazard rate</td>
<td>0.0042</td>
<td>-0.000463</td>
</tr>
<tr>
<td>Constant</td>
<td>-0.0616***</td>
<td>-0.0611***</td>
</tr>
</tbody>
</table>

\( N = 524 \) (Under), \( 635 \) (Not Under). Model \( \chi^2 \): 0.0000 (for both). Coefficients identified with ***, **, and * are significant at 0.05, 0.01, and 0.001 levels respectively.
investment annually. IMF programs thus seem to slow capital flight, but they do not eliminate it.

Thinking about the economic logic helps make this clearer. Capital is drawn to countries that have implemented fiscal adjustment. Corbo and Hernandez (1996) and Calvo, Leiderman, and Reinhart (1996:127) suggest that capital inflows in Argentina, Mexico, Thailand, and Chile were preceded by reductions in fiscal deficits, which in turn helped to lower inflationary expectations. Moreover, IMF medicine in the form of tight money can also lure capital from abroad, since this means higher interest rates and higher rates of return (Bird and Rowlands, 1997). Significantly, our decision to separate program effects from omitted variables makes clear that the program is having this effect, and not changes in fundamentals. After all, for both models in the above table, the hazard rate is not significant.

How reliable is this result? The above means are the average for all the states in the sample, thus the mean for nonselected programs is a counterfactual as if those states that were under a program were not. Table 3 shows the 95 percent confidence intervals for these estimates, and a paired $t$ test suggests that we can reject the null that these means are different from zero.

The above findings, however, omit the degree of program implementation, and in so doing fail to distinguish between successful programs and unsuccessful ones. If potential investors evaluate the presence or absence of an IMF program in making their decisions, then clearly the success or failure of these programs will also shape their investment decisions. The previous discussion makes clear that implementation matters because it allows investors to screen out good Fund clients (i.e., committed adjusters) from bad ones.

The one previous study that addressed this question, that of Bird and Rowlands (2002), found mixed results. A history of uncompleted programs was found to reduce portfolio investment in a sample of both low- and middle-income countries, but it did not affect portfolio investment in a middle-income country sample. They measured nonimplementation by the number of agreements in the past four years of which more than 20 percent of the total arrangement was left undrawn. The problem with this measure is

### TABLE 3

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Selected programs</td>
<td>$0.0266$</td>
<td>($0.0274$–$0.0258$)</td>
</tr>
<tr>
<td>Nonselected programs</td>
<td>$0.0275$</td>
<td>($0.0282$–$0.0267$)</td>
</tr>
<tr>
<td>Difference</td>
<td>$0.00094$</td>
<td>($0.0007$–$0.0011$)</td>
</tr>
</tbody>
</table>

$N = 1,526$. Variable is measured as a percentage of GNP. Results from paired $t$ test: selected vs. not selected: $t = 8.589$ ($p < 0.0000$).
that it conflates the existence of a compliance problem with the number of arrangements that a country has in a given year. For example, if a state has four agreements in four years and one fails, this is coded the same as a state that has one agreement in four years that fails. One might think that this second state would be better managed than the first, and that investors would evaluate the two states quite differently. Thus, it is not quite clear that the measure Bird and Rowlands used accurately captures the concept.

I elected to operationalize this variable more simply and avoid the confusion of tying compliance with the number of programs in a given amount of time. I gathered information on whether a program was suspended for programs between 1979 and 1995. In 138 of the 347 programs signed between 1979 and 1995, states were not eligible for all of the drawings either because they missed performance criteria and were unable to obtain a waiver from the Fund or they failed a quarterly review (Schadler et al., 1995:2).12

Thus, our model of Fund program suspensions includes variables that are often the basis of Fund performance criteria: the growth of the money supply and the level of reserves (Beveridge and Kelly, 1980; IMF, 1987; Guitain, 1995).13 As control variables, we also include a state’s level of trade openness (defined as exports plus imports as a percentage of GNP). We also include two measures designed to capture great power influence: U.S. foreign aid as a percentage of GNP and the state’s quota in the Fund. These results are shown in Table 4.

This model confirms our understanding of surveillance in Fund adjustment programs. These findings suggest that Fund programs break down because states are unable to implement policies of monetary restraint; as the growth in the money supply increases, the program is more likely to be suspended by the Fund. Similarly, states unable to accumulate reserves of currency also face the danger of suspension. We note that major power influence matters here, as states that are U.S. allies or large program recipients are less likely to have a program suspended by the Fund. This result confirms those of previous studies (Stone, 2002).

Using this information, we can then form a hazard rate from this model that addresses the effect of program suspensions. This variable in turn is included in a model of flows identical to the model in Table 2. As before, we rerun the tobit model of flows, but here we divide the sample between those states that had a suspension in the prior year, and those under good status in the prior year. These results are detailed in Table 5.

12 Information on Fund program compliance was obtained through three sources: Schadler et al. (1995), quarterly reports of the Economist Intelligence Unit, and IMF archives. I was unable to find information on an additional 22 programs.

13 These variables are not lagged because contemporaneous increases in the money supply or decreases in reserves suggest that the Fund program is not being implemented and can lead to program suspension (Leone, 1991; Guitain, 1994; Cottarelli, 1993; Gylfason, 1987). Fiscal policy variables were not included here because of missing data concerns.
Table 5 suggests that increases in exports also reduce flows, but only for states with successful programs. As before, controlling for push factors was important, as high U.S. interest rate spreads lead to portfolio inflows. Significantly, this was not contingent on a state’s status with the Fund, suggesting that push factors can be more influential than a state’s performance under the program.

In both models, the coefficient for the hazard rate was significant. This suggests that unobserved variables have a positive effect on portfolio flows for both suspended programs and programs in good status. Again, this could represent the beneficial effects of IMF conditionality. Fiscal and monetary austerity, as noted earlier, can serve to entice portfolio flows. Since both states suspended programs and those with programs in good standing have made at least some commitment to both of these, this could be what the coefficients are capturing. It might also represent investment-specific factors not captured in the above model. This provides an indication that controlling for selection effects was a wise move.

Using these results, we can generate selection and suspension corrected estimates of portfolio flows. These are shown in Table 6, along with our earlier estimate of the nonprogram baseline, which is the counterfactual estimate of portfolio flows for states that did not enter an IMF program. Table 6 thus compares the estimated level of flows for states under programs with both good and bad performance as well as those states that did not

\[ N = 453, \text{Percentage correctly predicted: 71.08 percent. Model } \chi^2: 0.0000. \chi^2 \text{ results for cubic spline segments: 0.0004.} \]

**Table 4**

<table>
<thead>
<tr>
<th>Probit Model of IMF Program Suspension</th>
</tr>
</thead>
<tbody>
<tr>
<td>Change in net domestic credit, ( t-1 )</td>
</tr>
<tr>
<td>(0.0652)</td>
</tr>
<tr>
<td>Log of reserves, ( t-1 )</td>
</tr>
<tr>
<td>(0.0637)</td>
</tr>
<tr>
<td>Openness, ( t-1 )</td>
</tr>
<tr>
<td>(0.0027)</td>
</tr>
<tr>
<td>IMF quota, ( t-1 )</td>
</tr>
<tr>
<td>(0.00012)</td>
</tr>
<tr>
<td>U.S. foreign aid, ( t-1 )</td>
</tr>
<tr>
<td>(3.469)</td>
</tr>
<tr>
<td>Hazard rate (from selection model)</td>
</tr>
<tr>
<td>(0.3186)</td>
</tr>
<tr>
<td>Constant</td>
</tr>
<tr>
<td>(0.4335)</td>
</tr>
</tbody>
</table>

\[ n, \text{n}, \text{nn, nnn represent levels of significance at the 0.05 level, 0.01 level, and 0.001 levels, respectively.} \]

14The coefficient for the suspended programs model was significant at a 0.10 level.
enter a Fund program. As before, we can compare these three values using paired $t$ tests to assess if the differences between them are significant. The results from these tests are shown in Table 6.

These tests are the comparison of the values of Nodes A, B, and C in Figure 1. They suggest that there is a statistical difference between Nodes B and C as well as between Nodes A and C. Suspending an IMF program not only causes portfolio outflows, but the magnitude of these flows is greater than the counterfactual value of the same state not being under the Fund program. Fund program suspensions do appear to send a clear signal to markets that a state is not an appropriate climate for investment, and so

### TABLE 5

Effects of IMF Program Suspensions on Portfolio Flows

<table>
<thead>
<tr>
<th></th>
<th>Suspended Fund Programs</th>
<th>Fund Programs in Good Status</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable$_{-1}$</td>
<td>14.381**</td>
<td>0.9318***</td>
</tr>
<tr>
<td>(5.442)</td>
<td>(0.1410)</td>
<td></td>
</tr>
<tr>
<td>GNP per capita$_{-1}$</td>
<td>-3.46e-07</td>
<td>9.89e-07</td>
</tr>
<tr>
<td>(3.46e-06)</td>
<td>(5.95e-07)</td>
<td></td>
</tr>
<tr>
<td>Growth$_{-1}$</td>
<td>0.000089</td>
<td>0.0003</td>
</tr>
<tr>
<td>(0.0008)</td>
<td>(0.0002)</td>
<td></td>
</tr>
<tr>
<td>Exports$_{-1}$</td>
<td>-0.000079</td>
<td>-0.000128*</td>
</tr>
<tr>
<td>(0.000355)</td>
<td>(0.000057)</td>
<td></td>
</tr>
<tr>
<td>U.S. interest rate spread$_{-1}$</td>
<td>0.1025**</td>
<td>0.00739***</td>
</tr>
<tr>
<td>(0.0338)</td>
<td>(0.00192)</td>
<td></td>
</tr>
<tr>
<td>Hazard rate (from suspension model)</td>
<td>0.0404</td>
<td>0.0091**</td>
</tr>
<tr>
<td>(0.02189)</td>
<td>(0.0033)</td>
<td></td>
</tr>
<tr>
<td>Constant</td>
<td>-0.2975**</td>
<td>-0.0353***</td>
</tr>
<tr>
<td>(0.0966)</td>
<td>(0.0071)</td>
<td></td>
</tr>
</tbody>
</table>

$N = 117$ (Suspended), 247 (Not Suspended). Model $\chi^2$: 0.0000 (both models). Coefficients identified with ***, *** are significant at 0.05, 0.01, and 0.001 levels, respectively.

### TABLE 6

Selection and Suspension Corrected Estimates of Portfolio Flows

<table>
<thead>
<tr>
<th></th>
<th>Mean</th>
<th>95% C.I.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Suspended programs$_{-1}$</td>
<td>$-0.1135$</td>
<td>$(-0.1193$–$-0.1077)$</td>
</tr>
<tr>
<td>Not suspended programs$_{-1}$</td>
<td>$-0.0235$</td>
<td>$(-0.0239$–$-0.0231)$</td>
</tr>
<tr>
<td>Nonprogram baseline</td>
<td>$-0.0275$</td>
<td>$(-0.0282$–$-0.0267)$</td>
</tr>
</tbody>
</table>

$N = 1526$. Variable is measured as a percentage of GNP. Results from paired $t$ tests: suspended vs. not suspended: $t = -32.78 \,(p > 0.0000)$; suspended vs. nonprogram baseline: $t = -32.51 \,(p > 0.0000)$; not suspended vs. nonprogram baseline: $t = 18.398 \,(p > 0.0000)$. 
investors pull their assets out of the country following bad news from the IMF. In addition, the previous results are confirmed, as states with programs that are in good performance are those that experience lower levels of capital outflows than if those states were not under a Fund program. Good performance under an IMF program, as in the first selection model, serves largely to slow capital flight rather than produce positive inflows.

In terms of magnitude, these results suggest that Fund programs have sizeable effects. As before, measuring the mean GNP for states in our sample, we find that the difference between Nodes A and B (nonprogram Baseline vs. suspended programs) is approximately U.S. $2.78 billion annually, the difference between Nodes A and C (nonprogram Baseline vs. not suspended programs) is approximately U.S. $129 million annually, and the difference between Nodes B and C (suspended vs. not suspended programs) is approximately U.S. $2.95 billion annually. Thus, these results expand the earlier findings. States under Fund programs that are successfully implemented perform better than states not under Fund programs. Those states that have their programs suspended by the Fund experience significant capital flight, exceeding that level had the state not entered the program.

I reassessed the robustness of the suspension model by adding additional variables for political institutions. Dummies for democracy, approaching elections, or military government were not significant, suggesting that the IMF does not treat states differently in terms of their internal political makeup.

How consistent are these results? I developed several robustness tests that ensure that these findings are not a result of model specification. I reestimated the model four times controlling for state interest rate spreads, democracy, the program type (a dummy for EFF programs), and a state’s level of stock market capitalization. In addition, I addressed potential confounds from regional and temporal effects by using a random effects tobit (to address heterogeneity across cases) and only postdebt crisis cases (by looking at cases only after 1989).

These results confirm the important informational role announced by an IMF program suspension. In all six models, states that have suspended programs have greater outflows than those states that never entered them. In addition, states that have suspended programs have greater outflows than those with successful implementation. In both the post-1989 model and the random-effects model, even those states with successfully implemented programs experience capital flight exceeding the nonprogram baseline. Thus, the beneficial effect of Fund programs for portfolio inflows seems to be a time-bound factor, and while it is present in the whole sample, the subsample from 1989 to 1995 and the random-effects model both indicate that even good implementation of Fund programs might not suffice to attract capital inflows.

15 These results are available from the author on request.
Further Implications

This article has found evidence that compliance with IMF programs is an important signal to portfolio investors. To better understand the effects of Fund programs, it is essential to evaluate the effects of successful program implementation rather than the mere announcement of a program. Our evidence suggests that investors withdraw their assets from countries following a Fund program suspension, as they learn that such a state is not a safe haven for their investments. The value of this capital flight is substantial, as it exceeds the counterfactual estimate for capital flows had the country not entered the program. At the same time, the evidence that investors respond positively to good performance under a program is less clear. The Fund thus functions as a "one-way" signal, in that while breaching programs produces clear costs, a successful Fund program does not produce consistent benefits.

These findings have larger implications for both future reform of the IMF and its influence. Some scholars (De Gregorio et al., 1999) argue that conditionality will be more influential if the IMF were more independent from major powers. Though this might affect the rate of program suspensions (as U.S. allies may be more likely to have their program suspended in the event of noncompliance), we see little evidence that this would improve the responses of portfolio investors to program suspensions. Irrespective of circumstance, program suspensions send powerful negative signals to investors.

Finally, this work illustrates that we have to think about the influence that international institutions have in a broader perspective (Simmons, 2000; Mosley, 2003b). Since the Fund does play a signaling role to international markets, we need to evaluate the added effects of private-sector behavior in addition to that of the IMF’s own resources in order to assess the IMF’s larger influence over countries seeking to adopt reform.

REFERENCES


**Appendix**

Economic variables drawn from World Bank’s *World Development Indicators*.

- **Fund Quota**: Represents state’s total borrowing privileges in the Fund in millions of SDRs. (IMF Annual Report, 1998).
- **Extended Fund Facility**: Dummy variable for program type taken from IMF Annual Reports.
- **Regime Type**: Taken from Polity IV data set (Polity IV Project, 2000).
- **Elections**: Dummy for an election in the following year. Taken from Beck et al. (2001).
- **Military Government**: Dummy for whether the chief executive was a military officer. Taken from Beck et al. (2001).

Dependent variables.

- **Portfolio Flows**: Portfolio equity flows are the sum of country funds, depository receipts (American or global), and direct purchases of shares by foreign investors. This measure is scaled over GDP and measured in U.S. dollars. Sources is the World Bank’s *Global Development Finance*. 
• **Agreement Selection**: Zero–one indicator noting if a country is under an IMF arrangement. This information is found in the annexes of the Fund’s Annual Reports.

• **Program Suspension**: Defined as the Fund’s decision to suspend a program either because a state missed performance criteria and was unable to obtain a waiver from the Fund or failed a quarterly review.

Codings for the dependent variable came from Schadler et al. (1995) and quarterly country reports of the Economist Intelligence Unit. Information about program compliance for programs prior to 1988 was obtained through a review of documents from the IMF archives.