

Signalling credibility? The IMF and catalytic finance

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International Monetary Fund (IMF) programmes are thought to function as a seal of approval to international markets although evidence suggests that Fund programmes do not attract capital inflows. Existing studies fail to address the effects of selection into IMF programmes, which raises questions about the robustness of the findings. Correcting for selection bias, I find that states under Fund programmes experience significant outflows of portfolio investment, which is a 'most likely case' for catalytic effects. The source of this capital flight is the 'medicine' (the Fund programme), not the 'disease' (the economic downturn necessitating the IMF programme). I argue that austerity deters portfolio inflows through its effects on future returns. These findings confirm previous studies and have broader implications for both the influence of IMF programmes and the politics of economic reform.

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Apart from its financial value, this accord is a passport allowing Romania to return to the international markets (Romanian Prime Minister Radu Vasile, 9 August, 1999).¹

International Financial Institutions (IFI) have been at the centre of public debate in recent years. In the policy community, the effects of International Monetary Fund (IMF) policies on developing countries have received close attention. Whether the focus is on the unintended consequences of Fund programmes and the danger of moral hazard (Calomiris and Meltzer 1998; International Financial Institutions Advisory Commission 2000) or the distributional consequences of Fund programmes (Pastor 1987; Danaher 1994; Thomas-Emeagwali 1995; Garuda 2000; Vreeland 2002), the increased scrutiny of IMF operations is the norm.

One justification that the IMF uses is that its programmes serve as a signal of borrower credibility (Guitian 1995; Dhonte 1997). By signing a letter of intent



and agreeing to implement it, the borrowing state sends a message to the outside world that it is adopting responsible economic policies. As a result, new inflows of loans and investment are thought to occur after countries sign IMF agreements (Fischer 1999; Kohler 2000; IMF 2001).

Existing studies find little evidence to support the notion that signing a Fund programme has a so-called catalytic effect on aid, investment and flows of new loans (Hajivassiliou 1987; Killick 1995; Rodrik 1995; Adji *et al.* 1997; Bird and Rowlands 1997, 2003; Rowlands 2001). The existence of weak findings for the catalytic effect serves to add to the chorus of criticisms of Fund programmes and to strengthen the claims of those who charge that the Fund is ineffective (Killick 1995; Bird 1996).

These studies, however, do not explicitly link theory and method. The Fund's endorsement is only sought in certain conditions, namely when states face balance of payments constraints, and these conditions by themselves may also be informative to potential lenders. In other words, the economic crisis that produces the decision to obtain assistance from the Fund may have independent effects on investment flows. To accurately assess the effects of the Fund programme, we have to separate the programme's effects from those of the balance of payments crisis. In other words, one needs to control for the effects of self-selection in order to generate reliable estimates of the parameters of interest. Many studies of catalytic finance recognize the importance of assessing the counterfactual to adequately assess the effect of the IMF (Bird and Rowlands 2002: 234; Cottarelli and Giannini 2002: 18; Morris and Shin 2005). Other studies of the effects of IMF programmes on other dependent variables have overwhelmingly employed corrections for selection bias (Przeworski and Vreeland 2000; Bordo and Schwartz 1999; Conway 1994). However, little empirical work has employed a correction to more clearly assess the effect of the IMF programme on capital flows.²

This article is designed to address this lacuna. In controlling for selection effects (the causes of why states enter IMF programmes) this allows us to tease out the effects of the 'medicine' (IMF austerity programmes) from the effects of the 'disease' (balance of payments crises). The findings here suggest that the first of these factors, and not the second, have significant negative effects on portfolio flows. I focus on portfolio flows because they are a most likely case for seeing evidence of catalytic effects. Investors respond to Fund programmes with significant levels of capital flight because Fund programmes can reduce the return on their investments. These findings, which evaluate the Fund's claims in a most likely case, support the received wisdom that a positive catalytic effect does not exist.

The findings have larger implications for the politics of economic reform. While politicians (such as the one in the epigraph) believe that a Fund programme provides an opportunity for improved financial market access,



this is not borne out in our data. It is in some sense little wonder that negotiations between developing countries and the Fund are divisive since a politician is faced with the certain costs of austerity and few external benefits in return.

The Case for Catalytic Effects

Before we can proceed to the heart of the matter, a more substantial setting up of the problem is warranted. IMF programmes are designed to address disequilibria in a state's balance of payments. Thus, it is not surprising that studies of the conditions in which these arrangements are initiated focus on a consistent number of factors: low reserves, high levels of debt, high current account deficits, and often high inflation, budget deficits and rapid growth of the money supply (McDonald 1986; Joyce 1992; Conway 1994; Knight and Santaella 1997).

Fund programmes require the introduction of fiscal and monetary austerity. This is because the Fund frames balance of payments problems as emerging largely from domestic profligacy (Polak 1957, 1991; IMF 1987).³ Thus, the Fund programme aims to alleviate the balance of payments constraint by providing reserves, and in exchange the state introduces policies designed to reduce the current account deficit. These take the form of fiscal and monetary restraint as well as exchange rate devaluation. In addition, so-called structural reforms such as privatization and the removal of export price supports and subsidies are often mandated to strengthen the competitiveness of the economy.

Fund programmes are thought to serve a catalytic role for investors for three reasons.⁴ First, because the country under the programme is moving from a regime in which it adopted inappropriate policies to a regime of market-appropriate policies, this mere act of *policy redesign* should entice private inflows.⁵ Politicians face incentives to adopt policies that harm foreign investors such as expropriation, devaluation or capital taxation. Conditionality is intended to reorient governments to adopt responsible policies aimed at narrowing their current account deficits. Second, recent thinking about Fund programmes as inducing moral hazard make clear that another avenue to catalysis is through unintended consequences. Investors may enter a state under a Fund programme because they feel their investment will be *insured* by the IMF regardless of the state of the borrower's economy (Barro 1998; International Financial Institutions Advisory Commission 2000). One of the purposes of the programme is to provide liquidity and, by lending when others will not, the Fund is believed to entice inflows. Finally, it is thought that the act of IMF conditionality makes catalysis



possible *through commitment*. Statements by leaders about their policy stances are not to be believed because of the potential for time inconsistency. IMF programmes are a 'costly signal' (Fearon 1994) because they are a contract-like commitment. States under Fund programmes have to adopt fiscal and monetary austerity in order to remain eligible for future tranches of the loan. This commitment mechanism allows investors to reconsider their prior beliefs about countries.

The question of whether Fund programmes catalyze international finance is important for several reasons. First, this issue furthers our understanding of how and why states use international institutions: not only do they provide resources to help resolve pressing domestic problems, but they also help states to signal credibility.⁶ Evaluating the extent to which Fund programmes produce these catalytic flows thus tells us a great deal about the Fund's broader influence over markets.

Second, a state's ability to access international markets provides a means for policy-makers to produce growth. Levine (2001) argues that portfolio inflows improve a country's growth prospects by increasing the efficiency of stock markets. Reisen and Soto (2001) find a strong positive relationship in developing countries between capital inflows and growth. Thus, Fund programmes can be growth enhancing if the Fund's financing is supplemented by capital inflows.

Finally, assessing the effect of Fund programmes on markets helps to understand how more successful Fund programmes can be designed. Securing external financing to support the adjustment programme is part of the Fund's strategy for addressing macroeconomic imbalances. Fund programmes are designed with specific assumptions about how economic variables will behave months in advance. If an adjustment programme operates with the assumption that additional private investments will accrue as a result of the programme, and these fail to materialize, this makes meeting the other benchmarks of the programme more difficult. As a recent review of Fund conditionality noted, in a number of countries external private inflows have failed to materialize as projected (Schadler 1996: 15). A failure to attract private capital flows above pre-programme levels can pose problems if the current account deficit is not reduced to a sustainable level, thus necessitating further adjustment.

Previous Empirical Studies and their Limits

As noted, several previous studies have attempted to assess the catalytic effects of IMF programmes. What constitutes catalysis often varies with the specific study, as past studies have focused on official lending, public and



private debt and foreign direct investment. Though each of these studies differs in terms of the temporal domain and the number of observations, one thing is clear: there is very little evidence that IMF programmes have consistent positive catalytic effects. For example, Bird and Rowlands (1997) and Rowlands (2001) find that Fund programmes are positively related to official lending (which includes foreign aid). This result, however, is sensitive to operationalization as Bird and Rowlands (2002) find that only concessional Fund programmes (Structural Adjustment Facility/Enhanced Structural Adjustment Facility/Poverty Reduction and Growth Facility) produce significant inflows of official lending. The results seem more consistent when the focus is on the private sector, however. Bird and Rowlands (2002) find that only the high conditionality Extended Fund Facility (EFF) programme produces inflows of private debt; all other concessional IMF programmes produce statistically significant private debt outflows. Adji *et al.* (1997) find no effect for Fund programmes on Foreign Direct Investment (FDI), while Jensen (2004) finds that Fund programmes produce significant FDI outflows. Bird and Rowlands (2002) find that Fund programmes produce positive effects for both FDI and portfolio investment in middle-income countries, whereas the effects for both types of inflows are strongly negative in a sample of low- and middle-income countries.

These studies by and large differ from much of the existing scholarship on the effects of IMF programmes. While many studies recognize the non-random nature of IMF programmes (Goldstein and Montiel 1986; Ul Haque and Khan 1998) and adopt appropriate techniques to correct for potential selection bias (Bagci and Perraudin 1997; Bordo and Schwartz 1999), the above studies do not. This poses an important inferential problem. In reality, we know that states seek help from the IMF when they face balance of payments crises. Neglecting the non-random nature of IMF programmes makes it difficult to form a counterfactual. If we accept that both the presence of an economic crisis and the presence of a Fund programme can independently affect the behaviour of external observers, then collapsing these events can result in faulty inferences. Thus, we have to disaggregate the effects of the crisis that brought the country to the IMF from the effects of the programme, and then assess their independent effects on catalytic finance.⁷

A selection bias problem may exist in these studies if the same variables that affect financial flows, such as high levels of debt service or low growth, also affect whether or not a state goes to the Fund. This compromises one of the assumptions that we make in regression models, which is that the factors not in the model that are correlated with other independent variables do not systematically affect the dependent variable.⁸ Since one would suspect that the sample of Fund client states may be less likely to attract catalytic finance even if they did not go to the Fund (owing to their weak macroeconomic



fundamentals), a regression model that neglects selection bias will underestimate the effects of the Fund.

In the present literature on catalytic finance, there are two approaches to address issues of selection bias. Bird and Rowlands (2002) supplement their quantitative evidence with case studies and interviews in order to better address the counterfactual. Their qualitative evidence, which points in the same direction about the limited influence of the Fund over capital markets, suggests that selection bias may not be a threat to inference.

The solution adopted in this article and in Jensen (2004) is to explicitly model the sample selection problem, which allows us to better specify the causal mechanisms at work. By isolating the effects of the IMF programme from the effects of the economic fundamentals inducing the programme, the results clarify the sources of the problem. Is it the case that capital is deterred by weak fundamentals, or by the presence of the IMF programme itself? This approach provides a most likely research strategy to answer this question. If selection bias is a threat to inference, then we may see evidence of positive catalytic effects after controlling for this bias. The empirical tests below employ the techniques developed to handle non-random selection to better assess the effects of Fund programmes (Goldstein and Montiel 1986; Bagci and Perraudin 1997; Ul Haque and Khan 1998; Bordo and Schwartz 1999).

Distinct from addressing problems of selection bias, previous studies provide a second opportunity for further empirical testing through appropriate operationalization of the dependent variable. Many scholars argue that not all capital flows are equal, as differences exist in the motivations behind different types of investments (Winters 1994; Armijo 1998; Maxfield 1998; Reich 1998; Sarno and Taylor 1999). Bird and Rowlands (2000: 956) suggest that conditionality will have a stronger impact on short-term capital flows than long-term capital flows. If this is correct, then this implies that studying the effects of Fund programmes on portfolio investment constitutes a most likely case for catalytic finance.

In this article portfolio investment is operationalized as purchases of equities, depository receipts, and country funds by foreign investors are measured as a percentage of Gross National Product (GNP). Our justification for focusing on portfolio investment is twofold. The most stringent case for catalytic effects is to be found by looking to the private sector rather than the public sector. IMF officials, as noted above, claim that private markets respond positively to the announcement of Fund programmes. Thus, testing the effects of programme announcements on private market behaviour allows us to assess the merits of this claim. As a result, I focus on investment rather than foreign aid or bank loans, which may not respond to the IMF's signal because they have different determinants.⁹ Importantly, the volume of private



flows has increasingly swamped the level of IFI and other public flows (Knight 1998). The volume of net FDI and portfolio investment in emerging markets has exceeded that of external borrowing for every year during the 1990s (IMF 1997, 2001). As portfolio flows have become an increasingly more important issue in the world economy, understanding the Fund's role in shaping them is critical.

Moreover, the logic of the argument necessitates a focus on the asset specificity of capital flows. For evidence of catalysis to be there, the focus of the study should be on mobile rather than fixed assets. After all, there might not be any evidence of the Fund programme having effects, not because the programme has no effects, but rather because the study focuses on immobile factors.¹⁰ 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 (Maxfield 1998; Sarno and Taylor 1999). Investors will be poised to respond to the announcement of a programme by allocating assets accordingly.

The above section produces two lessons for future evaluations of the IMF's catalytic effect. First, to evaluate the Fund's claims on the fairest grounds possible a research design has to separate the effects of the Fund programme on flows from the effects of those variables that necessitate Fund programmes on flows. Second, follow-up research needs to identify appropriate dependent variables that are most likely to uncover evidence of catalytic effects. These two lessons guide the research design outlined below. This study thus comprises a critical test for catalytic effects; if IMF programmes produce strong positive effects on capital flows, then we should see them in this case.

Research Design

In order to shed some light on these issues, I created a data set of 126 states that entered a total of 346 Stand-by or Extended Agreements between 1979 and 1995 (Table A1).¹¹ Stand-by agreements generally last 12 to 18 months, while the EFF is around 36 months in duration. It should be noted that, while this does not comprise the entire panoply of Fund programmes, this is not a threat to inference. Other Fund facilities exist, such as the Extended Structural Adjustment Fund facility (now renamed Poverty Reduction and Growth Facility) but these are not included in our analysis for several reasons. First, these programmes are concessional, and are geared toward countries with low incomes. Since these states are less likely to attract external investment, focusing on middle-income countries avoids this bias and builds a most likely case for catalytic flows.¹² Moreover, because PRGF programme states have



very low incomes and weak administrative capacity, including these states also invites problems owing to missing data. Focusing on Stand-bys and EFF programmes thus allows us to better confront the Fund on its home turf (Table A2).¹³

In order to proceed in this project, I outline the factors that cause states to select Fund agreements and address how to control for the effects of selection. Second, I develop a model of investment flows.

Understanding selection and outcomes

In order to assess if Fund programmes have effects, I first develop a set of independent variables that predict this. Previous research has identified a number of variables to predict whether a state will be under a Fund programme: debt service ratio, Gross Domestic Product (GDP) growth, per capita GNP, changes in terms of trade, and reserves measured in months of imports. I also use the log of inflation to capture the country's policy stance, regional dummies to capture heterogeneity, and United States (US) foreign aid to capture great power influence.¹⁴ These variables reflect the conventional wisdom of what factors drive states to the Fund, and are supported by a number of studies (Conway 1994; Knight and Santaella 1997; Thacker 1999; Przeworski and Vreeland 2000). All variables are lagged by one year to avoid simultaneity. To address the potential for autocorrelation, I included a series of temporal splines (Beck *et al.* 1998).

The implicit assumption here is that the IMF and developing countries come together when the developing countries need loans to solve their current account problems. Thus, the IMF accepts agreements either because its very job is to make loans, or that it is driven by budget maximizing incentives along the lines of the public choice school (Vaubel 1991; Dreher 2004). It could be argued that this is a simplification of the IMF's preferences. Since the IMF cares about reform as well as lending, it could be argued that the process of lending is shaped in part by beliefs about programme implementation. However, this takes us away from the question at hand. Rather than attempt to explain programme selection, the goal here is to explain how markets respond to these programmes. Regardless, I address potential concerns about the effects of programme implementation by discussing follow-up work below (Edwards 2005).

As for the dependent variable, I measured this differently than other studies. Most studies evaluate the effects of the programme on capital flows over the entire duration of the programme. In a sense, this takes us away from narrowing down the catalytic effect. If Fund programmes have an effect on market behaviour then one would expect new inflows to follow the *announcement* of a programme and not its duration.¹⁵ Thus, programme



Table 1 Baseline Selection Model (First Year Only)

Debt service _{<i>t</i>-1}	0.0142*** (0.0047)
Reserves _{<i>t</i>-1}	-0.1054*** (0.0346)
Growth _{<i>t</i>-1}	-0.0239* (0.0132)
Change in Terms of Trade _{<i>t</i>-1}	0.000032** (0.000015)
Log of Inflation _{<i>t</i>-1}	-0.00353 (0.0457)
Per Capita GNP _{<i>t</i>-1}	0.000064* (0.000038)
US Foreign Aid _{<i>t</i>-1}	2.4216 (4.180)
Latin America dummy	0.1633 (0.1575)
Eastern Europe/Former Soviet dummy	7.045*** (0.2015)
Constant	0.1180 (0.1911)

Model χ^2 -test: 0.0000.

Cubic spline χ^2 -test: 28.92 ($P > 0.0000$).

$N = 712$ observations (79 countries; 186 agreements).

Percent correctly predicted: 80.79%.

*, **, *** represent levels of significance at the 0.10 level, 0.05 level, and 0.01 levels, respectively.

selection is defined here as solely the first year of an IMF programme. Focusing on the first programme year allows us to isolate other confounding factors (such as the degree of programme implementation) and focus purely on the effects of the signal sent by the announcement of a programme. This is not to imply that programme implementation does not matter. In fact, I have addressed the issue of programme implementation in another paper, and I briefly discuss how these findings inform this project below. These results appear in Table 1.

The conventional wisdom, which suggests that debt service, reserves and growth lead politicians to enter Fund programmes, is supported here. The χ^2 -test on the cubic spline segments was highly significant, telling us that there certainly is autocorrelation in this model. States that have had Fund agreements in the past are likely to have them in the future. Including these splines in a regression model controls for the duration dependence in the data (Beck and Katz 1995; Beck *et al.* 1998). Shocks, captured by our terms of trade measure, are also significant determinants of whether a state is under an IMF programme. Differences in income across countries also affected the probability of receiving an arrangement from the Fund, as those countries with higher per capita incomes were more likely to receive an arrangement, though this result was not as strong. Inflation was not significant as shown in Table 1.

We also see regional effects here, as the dummy for whether the state was in Eastern Europe or the Former Soviet Union was significant. Dummies for Latin America or Africa, on the other hand, were not significant determinants of initiating agreements.



It should be noted that explanations for IMF decisions that are based on great power influence were not significant. The coefficient for lagged US foreign aid was not significant. Thus, in this model there is little evidence that influence from the US affects the probability of entering an IMF agreement.¹⁶

Finally, a battery of other specifications was attempted for this model, including more refined measures of the policy stance (lagged budget deficits, lagged domestic credit growth and lagged investment), as well as other political influence measures (democracy). Likelihood ratio tests led us to reject these more encompassing models.

Our conjecture is that the variables in the above model serve as a proxy for the overall robustness of the economy. As a result, they should affect the expected returns that potential investors receive, and therefore also affect the level of flows that occur after the state signs a letter of intent. In order to ascertain the effect of selection on outcomes, I create a variable known as the hazard rate from the predicted values from this model. Thus, the hazard rate is a variable that captures the effects of all of the independent variables on selection. When used as an additional variable in our estimation of the equation of interest, in this case, investment flows, this 'controls' for the effects of selection.¹⁷

It is worth noting that too much can be read into the accuracy of this model as a predictor of IMF programme initiation. While an 80% rate of prediction appears impressive, we must recall that states are not always under IMF programmes and, in fact, they initiate programmes even less frequently (Bird and Rowlands 2001: 252). This model does not answer all our questions about how states initiate programmes, for there is still a great deal of unexplained variance. However, it is worth noting that it was the best one that I could specify given the available data.

Finally, I need to develop independent variables that predict investment and capital flows. Here I build on the work of Montiel and Reinhart (2001) and Bird and Rowlands (2002). Our model of capital flows includes exports/GDP, capital controls, US interest rates, and the log of the black market premium. These variables address both developing country 'pull factors' and developed country 'push factors' (Calvo *et al.* 1993; Dooley *et al.* 1996; Taylor and Sarno 1997).¹⁸

Thus, Table 2 shows the models for portfolio flows with the hazard rate included as an independent variable on the right-hand side. Because the dependent variable is censored at zero, this is a random-effects tobit model.¹⁹ A fixed-effects estimation was not employed. Since in about 1/3 of the cases states are only observed for one state of participation, this makes generating a country-specific constant for the counterfactual case impossible. Even if this were remediable, unconditional fixed-effects estimates for tobit models are



Table 2 Effects of Fund Programme Selection on Portfolio Flows

	<i>States Under Fund Programmes</i>	<i>States Not Under Fund Programmes</i>
Dependent variable _{<i>t</i>-1}	0.6075 (0.4229)	0.3406*** (0.0591)
Exports _{<i>t</i>-1}	-0.00026 (0.00018)	-0.000014 (0.000029)
US interest rate spread _{<i>t</i>-1}	0.00934** (0.0031)	0.00165** (0.00063)
Capital controls _{<i>t</i>-1}	0.0104 (0.0069)	0.0011 (0.0011)
Log black market premium _{<i>t</i>-1}	-0.00264* (0.0017)	-0.00057** (0.00022)
Hazard rate	0.0013 (0.0034)	-0.000025 (0.0007)
Constant	-0.0379** (0.0119)	-0.0035 (0.0019)

Model χ^2 -test: 0.0000 (under), 0.0000 (not under).

Number of observations: 118 (under), 317 (not under).

Standard errors are in parentheses.

*, **, *** represent levels of significance at the 0.05 level, 0.01 level, and 0.001 levels, respectively.

biased, and only semi-parametric options would address this problem (Honore 1992).

Several findings from Table 2 are noteworthy. First, developed country ‘push factors’ matter for portfolio flows regardless of a state’s status with the IMF. Increases in the US interest rate spread lead to portfolio investment in developing countries. This variable is the interest rate charged by banks on loans to each other minus the commercially available interest rate. Thus, increases in this difference mean that commercial interest rates in the US are low and hence less attractive. This result reflects a search for revenue opportunities outside of the US instead of at home as commercial interest rates drop. Similarly, increases in a state’s black market premium deter investment regardless of status. Investors are not enticed by moving into states under IMF programmes if they have excessive black market premia.

The estimate for the hazard rate was not significant for either model. This suggests that there is no evidence that unobserved economic fundamentals shape portfolio flows. In other words, while Fund programme states and non-programme states may differ in terms of their unobserved fundamentals, these differences have no independent effect on portfolio flows. This is an important result. Above we argued that previous studies conflate the factors that enter Fund programmes with the effect of Fund programmes on investment flows. This raises an inferential question: is the failure of catalytic effects in these countries driven by the conditions when they enter a Fund programme or by the programme itself? Studies that do not control for selection bias suggest the second is the case, and not the first. This inference, however, does not follow from their statistical models. Controlling for selection, our findings suggest that the conditions producing the Fund programme do not affect capital flows.



Establishing this is essential to properly assess the unbiased effects of the programme on capital flows.

In addition, this finding challenges the notion that the weak evidence in support of the catalytic effect stems from model misspecification. Here we see no evidence that selection bias is a threat to inference. Had the coefficients for the hazard rates been significant, this would tell us that the omission of unobserved variables (in this case, the correlation in the error terms between the selection model and the capital flows model) has biased the result.

As before, numerous tests of specification were used on the flows model. Additional formulations of the flows model were tested by including variables such as debt service, inflation, investment, per capita GNP, growth, and less developed countries (LDC) interest rate spreads, as well as regional dummies for Eastern Europe, Latin America, and Africa, but likelihood ratio tests suggested that these variables added little to the model. I address further substantive robustness tests in a subsequent section.

To estimate the effect of IMF programmes, I use the predicted values from the above regression models to generate counterfactual estimates for investment flows for each country given that they are (or are not) under a Fund programme. Then I take the mean values of these estimates and compare across Fund programme and non-programme countries. This allows us to produce counterfactual estimates for each country for their flows given the presence or absence of a Fund programme. This produces an estimate of the effects of IMF programmes that controls for non-random selection. These results are shown in Table 3.

States not under Fund programmes have an estimated annual portfolio inflow of 0.12% of GNP, while states under Fund programmes have an estimated annual portfolio outflow of 1.4% of GNP. Measuring the size of this flow for states in our data set, this difference is approximately USD 503 million annually. To assess whether these two means are statistically significant I ran a paired *t*-test, which indicates that the differences in these means are not a result of chance. Thus, controlling for selection, IMF programmes deter

Table 3 Selection-Corrected Estimates of IMF Programme Effects on Portfolio Flows

	<i>Mean</i>	<i>95% Confidence interval</i>
Programme States	-0.01443	(-0.0151 to -0.0138)
Non-Programme States	0.00122	(0.00103 to 0.00141)
Difference	-0.0156***	

Estimates are scaled over GNP.

Number of observations: 824.

Paired *t*-test results: $t = -58.92$, $P > 0.0000$.



inflows of portfolio investment. These results suggest that the Fund's 'seal of approval' has a negative value. It should be stressed again that this work differs from predecessors in terms of inference rather than direction of the causal arrow. By controlling for selection, we ensure that the source of the capital flight is the IMF programme that states adopt, and not unobserved variables correlated with the decision to enter the programme in the first place. Not only did we correct for selection in this article but we also used a dependent variable most likely to respond to the IMF's signal and focused on the first programme year so as to isolate the pure effects of the Fund's endorsement. Our results supported the conventional wisdom rather than overturned it: Fund programmes produce capital flight over the short run.

These results merit some discussion. Above, I argued that the case for catalytic finance was based around three arguments: programme design, moral hazard, and commitment. How do our results reflect these explanations? The evidence suggests that IMF programme design is the source of weak catalytic effects.

The evidence first suggests that moral hazard type explanations are not at root here. The moral hazard argument implies that investors know that the IMF will cover their losses even in the event of an economic downturn, and as a result positive inflows follow IMF programmes. Our evidence suggests that investors do quite the opposite and that the presence of an IMF programme by itself (i.e. separate from economic fundamentals that might correlate with programme selection) conveys information that leads to capital flight. Investors do not feel that their risks are covered by the Fund since they leave rather than stay. This raises the possibility that the moral hazard argument is in fact potentially contingent on other factors, though I find no support for this claim in the subsequent robustness checks below.

One objection that can be raised to the discussion thus far is that the success or failure of catalytic finance can only be measured by considering both the long-term effects of Fund programmes as well as the degree of programme implementation, which are omitted in the present analysis. One can argue that the 'medicine' does not act instantaneously, and as a result a 1-year lag might not capture the response of markets. I re-estimated Model 2 with 2-year lags and the main result was unchanged. In addition, none of the lagged terms were significant.

Another objection is that I do not control for the degree of programme implementation, that is, the extent to which states actually take their medicine. To be clear, addressing these factors does take us afield so I have dealt with the question elsewhere (Edwards 2005). My analysis of this question finds that states that are successful in implementing IMF programmes still experience capital flight. Moreover, states with suspended programmes see portfolio outflows in the following year that exceed a non-programme baseline. States



are thus not rewarded for good implementation of IMF programmes, and they are punished for poor implementation with capital flight. These results contradict the commitment line of argument and suggest that the results are not an artifice produced by omitted variable bias. In addition, they suggest that our results are not compromised by a focus on only the first programme year. Regardless of whether we focus on the short or medium term, IMF programmes seem to harm portfolio investment.

As noted earlier, one of the problems with the existing literature is a disjuncture between the empirical studies of motivations of investors and the results of studies of the Fund's effects. The literature on portfolio flows suggests that this type of investment would be deterred by the announcement of an IMF programme because the signing of an IMF programme conveys adverse information about the prospects for future returns. In this article, the dependent variable comprises purchases of equities.²⁰ Foreign actors in developing country equity markets are often motivated by the promise of short-term returns (Gooptu 1993; Howell 1993; Tesar and Werner 1993; Bohn and Tesar 1996; Maxfield 1998; Sarno and Taylor 1999).

What new information does the IMF programme convey to investors? Fund programmes entail monetary and fiscal austerity, which jeopardizes the short-term returns on equity markets in two ways. First, moving to a tight money stance can increase the cost of capital for LDC firms (through an increase in domestic interest rates). This, in turn, can make LDC firms less attractive to foreign investors. To the extent that tightening interest rates (and cuts in government spending) further produces a slowdown in the economy as a whole, this also limits the growth of firm profits and lower the price/earning ratios of firms (Gylfason 1987; Howell 1993), making these equities less attractive. In short, investors looking to maximize short-term gains will be unlikely to find opportunities in a country implementing an IMF programme.

These harmful effects of Fund programmes on portfolio investment can in turn be worsened by the timing of a Fund programme. Governments face incentives to delay implementing reform to avoid passing the costs onto their constituents (Alesina and Drazen 1991). As a result, the IMF is often brought in when an economic crisis is severe rather than at the first signs of crisis (Eichengreen and Fishlow 1998: 60–61). To the extent that this increases the necessity of painful corrective austerity measures, the arrival of a Fund negotiating team conveys clear information about the future course of economic policy; future returns are likely to decrease.

How sensitive are these results? Below, I discuss seven robustness tests aimed at clarifying these findings. First, I return to the moral hazard argument for catalytic effects. This argument might lead one to suspect that a greater willingness by the Fund to insure private actors might prove more



likely to induce inflows. Because IMF programmes often coincide with debt rescheduling, it could be that the presence of debt rescheduling in conjunction with an IMF programme affects inflows. To test this, I added a dummy variable for whether any amount of debt had been rescheduled in the past year to our models in Table 2, and there was no change in the overall results. The coefficient for rescheduling was never significant, and the overall result was unchanged. Thus, even if states reschedule their debt in conjunction with a Fund programme, this does not entice inflows. This raises further questions about the merits of the moral hazard argument for catalytic flows.

A second approach to assessing moral hazard looks at the type of IMF programme. It could be argued that the moral hazard argument is contingent on the type of IMF involvement, such that programmes with more extensive conditionality and longer durations are those that entice inflows. To assess this, I differentiated between Stand-by and Extended programmes. Extended programmes have more structural conditionality, are longer and the loans are larger.²¹ Those states with Extended programmes do attract slightly more portfolio investment, but the effect of the programme type is outweighed by other factors. Controlling for programme type, our main results are unchanged. Both these results cut strongly against the moral hazard claim for catalysis.

One other possible omitted variable that could affect these results is the exchange rate. Currency devaluations could have a significant effect on portfolio flows, but we lack reliable annual data on this. While we would ideally like data on devaluations, the model in Table 2 controls for black market premia which do change over time in response to events (Agenor 2000: 662) and, as noted, is a significant variable regardless of programme involvement. Thus, I employ two proxies to measure changes in the exchange rate. I employ Frankel and Rose's (1996) indicator of currency crashes, which is operationalized as a nominal depreciation of at least 25% that is also an increase in the rate of depreciation of at least 10%. In addition, I also employ Reinhart and Rogoff's (2004) designation of exchange rate regimes as 'freely falling'. This classification is given on the basis of two criteria. It is operationalized as a state with a floating exchange rate and inflation greater than 40%. These are states in which depreciations in the exchange rate are likely. Moreover, a state's exchange rate regime is coded as freely falling if a currency crisis marks a transition from a fixed regime to a floating regime, thus indicating a speculative attack. Importantly, our results are not changed when controlling for large movements in exchange rates. Those states with freely falling exchange rates that are under Fund programmes do experience inflows, but the magnitude of this coefficient is small. Regardless of how we operationalize changes in



the exchange rate, states under IMF programmes experience significant portfolio outflows.

We gain additional value through the use of these robustness checks. Mody and Saravia (2003) find evidence that IMF programmes can affect portfolio flows, though they evaluate spreads on bonds rather than aggregate investment flows. Their evidence is cautiously positive; Fund programmes matter for bond spreads only if states enter precautionary programmes or if states enter programmes before their economic fundamentals have appreciably deteriorated. Controlling for the conditions in place when states enter the programme thus allows us to double-check our results in the context of a more nuanced argument.

Two further robustness checks attempt to assess the sensitivity of these results. I assessed whether these results would hold for a subsample of programmes in the 1990s. This time period coincides with the resurgence of emerging markets, and a statistically significant difference remains between programme years and non-programme years for these countries. Finally, I sought to differentiate these results by country type to ascertain whether these results would still hold for a sample of middle-income countries. I relied on the World Bank's classification scheme, which specifies a country as lower-middle income if it has a per capita GNP of greater than USD 735. These results suggest no differences in our results.

Lastly, I re-estimated the models with one additional push factor. Since portfolio flows might be more likely to be drawn to countries with more robust stock markets (Montiel and Reinhart 2001), I include a variable measuring each country's degree of stock market capitalization (from Demirguc-Kunt and Levine 2001). Even here, when controlling for this additional factor we see clear evidence that states under IMF programmes experience portfolio outflows. Importantly, in neither regression was the coefficient significant suggesting that this push factor fades in importance while states are under Fund programmes (Table 4).

Implications

These findings bring with them broader lessons for the study of international institutions and for analyzing the effects of Fund programmes more generally. First, in terms of the IMF the evidence points to an 'incredibility' effect in that, despite the Fund's repeated invocations to the contrary, the Fund's catalytic effect is negative rather than positive. Our analysis builds on the previous studies in this area and makes three changes to the research design evaluating the Fund's claim on the fairest grounds possible. I controlled for programme selection and chose a most likely dependent variable, and relied on judicious case selection by focusing on middle-income countries in the first programme


Table 4 Robustness Checks of IMF Programme Effects

	<i>Portfolio flows</i>
Programme (rescheduling dummy)	-0.0105
Non-programme (rescheduling dummy)	0.0018
Difference	-0.0123
Programme (EFF dummy)	-0.0133
Non-programme (EFF dummy)	0.0011
Difference	-0.01444
Programme (currency crash)	-0.0163
Non-programme (currency crash)	0.0011
Difference	-0.0174
Programme (freefalling exchange rate)	-0.0142
Non-programme (freefalling exchange rate)	0.00121
Difference	-0.0155
Programme (post 1989)	-0.0251
Non-programme (post 1989)	-0.00376
Difference	-0.0247
Programme (middle income only)	-0.0131
Non-programme (middle income only)	0.0019
Difference	-0.01497
Programme (market capitalization)	-0.0106
Non-programme (market capitalization)	0.0029
Difference	-0.0135

Estimates are scaled over GNP.

Number of observations: 359 (EFF), 358 (rescheduling), 800 (crash), 781 (freefall), 399 (post-1989), and 824 (middle income), 373 (market capitalization), 614 (property rights).

The significance level for differences in means between programme and non-programme groups is $P > 0.000$.

year. Despite this, the evidence suggests that portfolio investors respond to the arrival of an IMF programme with capital flight.

In the broader literature on the effects of Fund programmes, the findings here parallel those of Simmons (2000) who argues that markets respond to signals from the Fund and in so doing help to enforce the commitment to Article VIII. The results here suggest that markets do indeed respond to signals, but in ways that complicate Fund programmes rather than bolster them. As in Simmons' work (also Mosley 2000, 2003), international institutions have greater influence when they are able to obtain the cooperation of the private sector.



Finally, these findings also have important implications for the politics of selecting and complying with Fund agreements. Because Fund programmes deter portfolio investment, it is almost no wonder that negotiations between the Fund and developing countries are often protracted in the bargaining phase, or that ‘reform fatigue’ (Nelson 1991) can set in. This analysis confirms the findings of those who suggest that reform offers certain costs and uncertain benefits (Haggard and Kaufman 1992; Bates and Krueger 1993), as signing an IMF programme intended to bolster reform serves to make reform more difficult to sustain by inducing capital flight. The adverse effects of Fund programmes on the ability of countries to access international markets may also in turn make compliance with Fund programmes more problematic. Thus, returning to the theme in the epigraph, while the Prime Minister of Romania invoked the catalytic argument in order to justify the IMF programme, our evidence suggests that this passport may have little value, making it harder to sustain the commitment to austerity.

Notes

- 1 The epigraph comes from the *World Bank Development News*. Thanks are due to Tom Willett, Michael Bordo, Robert Kaufman, William Roberts Clark, Hugh Rockoff, Catherine Hovaguimian, David Leblang, and Axel Dreher for their comments. This research was supported by the Institute for the Study of World Politics.
- 2 The sole published exception is Jensen (2004), though his paper deals with FDI flows. Mody and Saravia (2003: 14–15) employ a Heckman estimation on the effects of IMF programmes on bond spreads but their selection bias correction is for the decision to offer bonds, not the decision to enter the programme.
- 3 The East Asian crisis was more a banking crisis or a private sector problem than a public sector problem. While increasingly important, the source of the balance of payments problem is not a key issue for this article since the domain of cases ends in 1995.
- 4 Cottarelli and Giannini (2002) argue there are five channels through which catalytic effects operate, although their focus is on official lending.
- 5 As the East Asian Financial Crisis demonstrated, the Fund might not always prescribe policies that all agree are optimal. Bird and Rowlands (1997) noted that some Fund policies may produce more harm than good; such as if interest rate hikes lead to bank failures.
- 6 The notion that international institutions solve these information problems is one of the cornerstones of institutional theory (Keohane 1984; Martin and Simmons 1998; Sobel 1999).
- 7 An example from Greene (1997: 981) should help make this clearer. Suppose that we want to understand the effect that a college education has on one’s earnings, and we run the following regression:

$$\text{Earnings} = Ax + B(\text{College attendance}) + \text{error} \quad (1)$$

The Ax term is shorthand for a vector of independent variables that predicts earnings. We use a dummy variable for whether or not an individual attended college. The key question that we have to answer is this: does B , our estimate for the effect of college attendance, accurately capture the causal effect we seek to estimate? Our answer depends on the counterfactual: what are we assuming about the typical individual that goes to college? If we assume that they would (owing to their above average determination) have high earnings



even if they did not go to college, then our regression will actually overestimate the effect of college on earnings.

- 8 In other words, we assume that $E(u_iX) = 0$. The problem is that unobserved factors induce a correlation between the error term and the independent variable (Berk 1983; Achen 1986).
- 9 As Gould (2003), bank lending is often negotiated as part of the IMF programme, 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. Moreover, Eichengreen *et al.* (2005) argue that banks act as delegated monitors and thus have an informational advantage over markets, implying that the Fund's signal will have little effect on bank loans.
- 10 Relatedly, the one study that shows consistently positive evidence of catalysis (Mody and Saravia 2003) evaluates the effects of Fund programmes on asset spreads on international bonds rather than capital flows. 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.
- 11 Twenty states in this panel had no Fund programmes at all during this time period. Full information on the cases is included in the Appendix. The time period under question was chosen because this article is part of a larger project looking at the effectiveness of IMF programmes. It is difficult to get data on programme implementation for more recent programmes.
- 12 Bird and Rowlands (2002) adopt a similar strategy.
- 13 Bird and Rowlands (2002) also differentiate between states based on programme type.
- 14 Operationalizations of all variables are included in the Appendix.
- 15 Knight and Santaella (1997) offered a clear discussion of this issue. This is why some scholars use dynamic probit specifications so as to separate the decision to enter a programme from the decision to remain in the programme (Przeworski and Vreeland 2000).
- 16 This point tempers the analyses of Thacker (1999) and Stone (2002) but should not be interpreted to imply that these factors are never influential in IMF behaviour. In fact, they merely suggest that US influence or large borrower influence may affect the duration of a state's programme spell, or performance under them, but not selection.
- 17 It is for this reason that this process was originally referred to as a 'two-step' estimation following Heckman (1979). It should be noted that there are single equation estimators of treatment models but these are not used in this project because of the truncation issue and because they assume that the independent variables have the same effect on the dependent variable regardless of whether a state is under an IMF programme or not. Thus, the sole difference between those states that are under Fund programmes and those that are not is restricted to a dummy for programme status. Given that the Fund programme itself has effects on these independent variables, a split population model seems appropriate (following Millimet 2001).
- 18 See Eichengreen (2001) for a discussion of the limits of extant measures of capital controls. Other measures were not available for the number of years and countries required, and so were not employed.
- 19 There is one negative observation (Thailand 1994) and the vast majority of observations are zeroes. The results that follow are robust to specifying the lower bound for the regression at zero or at the value for this outlier.
- 20 While bond purchases are also included as part of portfolio investment, the World Bank has only recently begun to gather this data cross-nationally. This is an area for further research.
- 21 Goldstein (2003: 388) notes that EFF programmes average double the number of structural conditions compared to Stand-bys. Forty-six out of the 366 programme years in this study were EFF programmes (12.6% of total). As noted above, Bird and Rowlands (2002) also disaggregate flows by Fund programme type.



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Appendix

Independent Variables (Unless otherwise indicated, these come from World Bank's *World Development Indicators*):

Reserves: Gross international reserves measured in months of imports.

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

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

Interest rate spread (lending rate minus LIBOR): Interest rate spread is the interest rate charged by banks on loans to prime customers minus the interest rate paid by commercial or similar banks for demand, time, or savings deposits. US interest rate spread data comes from the IMF's *International Financial Statistics*.

GNP per capita (constant 1995 USD): GNP per capita is gross national product divided by mid-year population.

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

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

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

Extended Fund programme: Dummy variable for whether the Fund programme in question is a drawing from the Extended Fund Facility. The source is the IMF's *Annual Reports*.

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

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

Table A1 States in No Fund Programme Panel, 1979–1995

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


Table A2 States Under Fund Standby and EFF Programmes, 1979–1995

Albania	Georgia	Pakistan
Algeria	Ghana	Panama
Argentina	Grenada	Papua New Guinea
Armenia	Guatemala	Peru
Azerbaijan	Guinea	Philippines
Bangladesh	Guyana	Poland
Barbados	Haiti	Portugal
Belarus	Honduras	Romania
Belize	Hungary	Russia
Bolivia	India	Rwanda
Brazil	Jamaica	Senegal
Bulgaria	Jordan	Sierra Leone
Burma	Kazakhstan	Slovak Republic
Burundi	Kenya	Solomon Islands
Cameroon	Korea	Somalia
Central African Republic	Kyrgyz Republic	South Africa
Chad	Laos	Sri Lanka
Chile	Latvia	Sudan
China	Lesotho	Tanzania
Congo	Liberia	Thailand
Costa Rica	Lithuania	Togo
Cote D'Ivoire	Macedonia	Trinidad
Croatia	Madagascar	Tunisia
Cyprus	Malawi	Turkey
Czech Republic	Mali	Uganda
Czechoslovakia	Mauritania	Ukraine
Dominican Republic	Mauritius	Uruguay
Dominica	Mexico	Uzbekistan
Ecuador	Moldova	Venezuela
Egypt	Mongolia	Vietnam
El Salvador	Morocco	Western Samoa
Equatorial Guinea	Nepal	Yugoslavia
Estonia	Nicaragua	Zaire
Ethiopia	Niger	Zambia
Gabon	Nigeria	Zimbabwe
Gambia		

Rescheduling: Dummy variable for whether or not the state had any amount of debt rescheduled over the 1988–1995 time period only. Source: *Global Development Finance*.

Stock Market Capitalization: Measures the value of listed shares on a given country's stock exchange as a function of GDP. Source: Demirguc-Kunt and Levine (2001).

Black Market Premium: Calculated from data in Reinhart and Rogoff (2004).

Currency Crisis: Codings taken from Frankel and Rose (1996).



Dependent variable is from the World Bank's *Global Development Finance*:

Portfolio Flows: Portfolio equity flows are the sum of country funds, depository receipts (American or global) and direct purchases of shares by foreign investors. Scaled over GDP and measured in USD.

About the Author

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