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Flows to Middle Income Countries?**

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## **Abstract**

This paper examines the claim that IMF agreements catalyze other private capital flows. We identify a series of propositions from the literature which we test using a treatment effects model to examine how private capital flows into middle-income countries respond to IMF agreements. The main result is that there is generally a negative association between signing an IMF agreement and subsequent private inflows, though the direction and significance of catalysis varies according to the type of flow and the circumstances of the country. The finding that catalysis is complex and nuanced has important implications for policy that are briefly discussed.

**Key words:** international capital flows; IMF; catalysis; middle-income countries

**JEL codes:** F32, F33, F34, F21

**Highlights:**

- Recent theory suggest IMF programs may catalyse additional private capital flows.
- Uses treatment effects analysis of 80 middle income countries 1975-2006.
- The catalytic effect is found to be generally negative, but the effect if complex.
- Catalysis varies across capital flow type, IMF agreement, and economic conditions. 1.

## **1. Introduction**

A central issue in discussions about the world's financial system is the nature of the relationship between the International Monetary Fund (IMF) and private international capital markets. On the one hand they may be viewed as substitutes, with the IMF becoming involved in countries that have little access to private capital, or where private capital outflows have created a short-term external financing vacuum. Those who subscribe to the idea of creditor moral hazard go further and claim that the prospect of future IMF lending in the event of a crisis encourages private lenders to underestimate risk and to over-lend, which in turn ultimately generates the crisis and IMF intervention. Viewed as complements, on the other hand, IMF lending is claimed to have a catalytic effect on private capital market lending. In this case, the IMF is presented as bailing in private capital through signalling, coordination, or coercion. Through catalysis the IMF can facilitate balance of payments adjustment with fewer of its own resources. The nature and size of the catalytic effect is therefore of considerable importance. If catalysis is overestimated it will lead to insufficient financing and excessive balance of payments adjustment.

Increasing attention has been paid to the catalytic effect of IMF programs on private capital flows. The survey of the literature by Cottarelli and Giannini (2002) traces the evolution of the concept, analyzes its possible operational mechanisms, and evaluates the available empirical evidence of its existence. We use more recent theoretical insights to guide an empirical examination of the IMF's potential catalytic effect.

## **2. Catalysis: Theoretical Underpinnings**

In their review of the catalytic effect, Cottarelli and Giannini (2002) claim that there are five channels through which it may work: policy design, information, commitment, screening, and insurance. IMF programs may lead to the design of superior policies that improve a country's future economic performance. Agreements may also reveal information about policies and

performance otherwise unavailable to private capital markets. Programs may signal a government's commitment to reform, and reduce the chances that there will be policy slippages, or they may screen out governments that are not serious about reform. Finally, the IMF may provide insurance by acting as a lender of last resort, though possibly at the risk of creating moral hazard.

Rodrik (1996) and Bird and Rowlands (1997) focused on conditionality as the principal modality for catalysis. The anticipation is that the catalytic effect of IMF agreements will be stronger when policy conditions are well designed and their implementation credible. However, when private markets believe that IMF programs will lead to recession or financial difficulties, or that good policies will not be implemented, then catalysis will be weak, non-existent or, in the extreme, negative. For example, 'conventional' IMF programs often lead to interest rate increases and exchange rate devaluation (Bird and Rowlands 1997). According to the asset market approach this policy combination might induce capital inflows by raising the return on invested capital and reducing future exchange rate risk. However, rising interest rates may also send a negative signal, which in credit rationing models might precipitate reduced capital inflows due to higher default probabilities and lower expected returns. Devaluation may also signal lower commitment to defending the value of the currency, raising the spectre of future exchange rate risk. Consequently Bird and Rowlands (1997) argue that the sign of the catalytic effect associated with IMF conditionality is theoretically unclear and that its true nature is essentially an empirical question.

In the aftermath of the East Asian crisis, researchers began to place more emphasis on the IMF as a provider of short-run liquidity, thereby inducing a stronger commitment to adjustment by making associated policies more sustainable politically and overcoming collective action problems that might otherwise lead to default. The consequent reduction in default risk could in turn encourage private capital inflows. Of course, in the absence of

conditionality, IMF liquidity may also allow governments to relax adjustment efforts, raising default probabilities and discouraging investment. The withdrawal of private capital would in turn be facilitated by the provision of IMF resources. In this context a number of papers have investigated the logic of partial as opposed to complete bailouts, with models of the former frequently generating multiple equilibria and an incentive for private creditors to foreclose and bring about default and crisis. When creditors are not co-ordinated, only a full bailout that completely fills the financing gap will restore confidence (Zettelmeyer 2000; Frankel and Roubini 2001; Jeanne and Wyplosz 2001).

Recent contributions to the theory of catalytic finance have concentrated on the impact of IMF lending on the probability of default. Morris and Shin (2006) continue the focus on IMF lending and default probabilities, using a global games framework to analyze the behavior of three players; short-term creditors, the IMF and debtor governments. In this co-ordination game with limited uncertainty, IMF lending may be necessary and just sufficient to supplement the financial resources a debtor country obtains from adjustment efforts, thereby encouraging short-term creditors to roll over their debt despite initial concerns about liquidity. Where a country has strong fundamentals and a government committed to stabilize the economy, there is no risk of illiquidity, no need for additional resources, and no role for IMF-induced catalysis. Where economic problems are fundamental and commitment to adjustment is weak, the IMF's relatively small resource contribution is unlikely to induce significant additional adjustment, and will thus fail to persuade creditors to roll over debts. Catalysis is therefore unlikely to exist here either, and the Fund's resources may be wasted. Catalysis may occur where countries on the margin of default receive just enough IMF resources and incentives to adjust to restore market confidence. Consequently, the catalytic effect will display context-specific non-linearities.

Similar conclusions emerge from Corsetti, Guimaraes and Roubini (2006). In their model, crises are caused by the interaction between poor fundamentals, self-fulfilling creditor runs, and the policies of investors, governments and the IMF. The model shows that the IMF can prevent a scramble for liquidity by co-ordinating agents' expectations and by increasing the number of creditors willing to lend for any given set of fundamentals. As in Morris and Shin (2006), IMF lending may induce stronger economic adjustment efforts. The Fund's influence increases with its lending level and the precision of its information, and default may be avoided even if IMF lending only partially fills the external financing gap.

In a related model, Penalver (2004) suggests that it is the implicit subsidy on IMF resources that encourages borrowing countries to exert adjustment effort and to avoid default. By preventing default, IMF lending increases the marginal rate of return to investment thereby encouraging capital flows. Penalver's analysis therefore builds an analytical bridge between debt rollovers and new capital flows.

This 'new wave' of finance models highlights the well-established observation that the IMF's catalytic effect relies largely upon its ability to reduce uncertainty and increase the probability of superior economic performance, either through lending (as these new models accentuate), or via conditionality. For countries with fundamental economic problems, IMF involvement is unlikely to alter the perceptions of private capital markets and may indeed trigger an outflow of private capital, facilitating its substitution with official resources. For more sound economies, IMF involvement will have either no impact or may transmit a negative signal to private capital markets. For countries between these extremes, IMF involvement may reduce uncertainty and provide assurances that the added liquidity and adjustment effort will be sufficient to move the economy forward. It is in these situations that, *a priori*, catalysis may be expected to be relatively strong. Where earlier informal approaches focused on return and risk as the underlying determinants of international capital



movements, these newer models usefully demonstrate how catalysis may be contingent on the nature and seriousness of the economic problems faced by IMF clients.

However, different types of capital will have different risk and return profiles, and may respond differently to IMF programs. Progress here is impeded by the fact that the theory of capital movements is too imprecise to exploit these differences in a large sample econometric model. Much of the literature on the determinants of foreign direct investment, for example, focuses on decision making within multinational enterprises; there is no well-established theory of FDI viewed from the perspective of the recipient country, nor do good data exist for dyadic flows. We also lack a well-established theory of bank lending, bonds or portfolio investment upon which estimations of IMF catalysis may be confidently based.

Yet informal theorizing does suggest some hypotheses. For example, bank lending is likely to be short or medium term in nature, pro-cyclical, and influenced by proximate economic variables such as short term interest rates and liquidity. In contrast, foreign direct investment will depend more on the long term prospects for economic growth, policy relating to the repatriation of profits, and political stability. Portfolio investment may share some of the features of both of the above. Bond market responses are likely to vary with their maturity profile and the extent to which they are credibly guaranteed.

If the determinants of private capital flows are complex and differentiated, it follows that IMF programs may be expected to affect different types of capital in different ways. To the extent that stand-by arrangements (SBAs) are associated with increased macroeconomic stability they may be expected to have a positive effect on all capital flows, as theories of catalysis frequently assume. However, while the rise in interest rates and the elimination of currency overvaluation (thereby diminishing the likelihood of future devaluation and associated adverse balance sheet effects) might exert a positive effect on bank lending, some studies have found that SBAs reduce growth (Vreeland, 2003), and this may discourage

FDI. Moreover, the infusion of credit associated with an SBA may be expected to be more relevant for PPG bank lending and bonds than for non-guaranteed inflows such as FDI.

The IMF's Extended Fund Facility programs (EFFs) might, in contrast, be expected to have more profound effects on longer term flows. To the extent that such programs are seen as correcting structural deficiencies and exerting a positive effect on long run economic growth, they should have a positive impact on FDI. However EFFs may also be interpreted as signalling the need for fundamental economic reform. Here, a previously good record of implementation may be required to encourage catalysis.

This discussion allows us to formulate a number of testable propositions about the relationship between IMF programs and private capital flows.

The *first* proposition is that the catalytic effect will vary across different types of private capital flow. For example, short-term creditors may be more concerned about macroeconomic stability and liquidity while long-term investors may look for evidence of structural adjustment.

*Second*, IMF programs are more likely to be successful if initial conditions are not severely adverse. The catalytic effect will therefore tend to be weak in situations where economic fundamentals are very poor, and may be negative if capital outflows are facilitated by IMF financing. For countries with relatively strong fundamentals, IMF arrangements may again exert little catalytic effect, and may even deter capital inflows by transmitting a negative signal. It is between these two extremes that a positive catalytic effect may be expected to be strongest. Consequently, it will depend non-linearly on the initial country conditions.

*Third*, catalysis should be stronger if IMF conditionality is well designed and effectively implemented. There is a large literature and extensive debate about the design of IMF programs. Critics suggest that conditionality emphasizes policies favored by

international capital markets.<sup>1</sup> If so, the key uncertainty for catalysis pertains to implementation, which is affected by a complex mixture of economic and political factors (Arpac, Bird and Mandilaras 2008). Creditors may thus be expected to wait to see whether governments keep to their policy commitments, and there may be no signalling-related catalytic effect. Alternatively, capital markets may consider a country's past track record of implementation as a reasonable guide to the future.

*Fourth*, there is a reasonable presumption that the prolonged use of IMF resources implies either the existence of intractable economic or political problems, or a serial reluctance to implement programs. It follows that the catalytic effect will be weaker for prolonged users of IMF resources.

*Fifth*, the type of facility under which resources are borrowed from the Fund gives a further indication of the nature of the economic problems being encountered. SBAs tend to be associated with short-term stabilization, while EFFs are associated with more fundamental and longer-term structural adjustment. Indeed, EFF agreements are generally seen as having more extensive conditionality as they often include both stabilization and adjustment elements. It follows that SBAs may be expected to exhibit a stronger catalytic effect on shorter term flows, while EFFs may have a greater impact on longer-term flows such as FDI.

*Sixth*, where countries encounter problems of illiquidity, it may be expected that catalysis will be stronger when IMF lending is large relative to a country's financing needs, particularly in the case of short term lending. With IMF credits being conditional, the incentive to implement programs may also be expected to be higher in these cases, further enhancing catalysis. However, it may be a country's precautionary borrowing potential that is important; the actual use of these resources may negatively affect catalysis.

What support for the above propositions exists in the empirical literature?

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<sup>1</sup> See Bird (2007) for a recent review.

### **3. Existing Evidence on Catalysis**

The existing empirical evidence on the catalytic effect has been summarized at some length elsewhere (Bird and Rowlands 2002; Cottarelli and Giannini 2002; Hovaguimian 2003). The methodologies used have included large sample econometric estimation, case studies and attitude surveys of market participants. The general consensus from this body of research is that there is relatively little evidence in support of the catalytic effect of IMF lending on private capital flows, at least not as a universal, strong and positive phenomenon. Individual studies do, however, discover evidence of a significant positive effect on some flows in specific circumstances. These conflicting results raise questions as to when the catalytic effect may be positive and when it may be negative. Unfortunately the existing literature does not include a comprehensive disaggregated analysis of catalysis.

The propositions in the previous section systematically identify the circumstances in which a catalytic effect may be expected to be at its weakest or at its strongest. Some of the existing empirical research is relevant in this context. In a study of the bond market, Mody and Saravia (2006) find evidence that countries with weak fundamentals do not experience catalysis while those where the fundamentals are less weak encounter a significant positive catalytic effect. Bordo, Mody and Oomes (2003) report similar results, but find a ‘dip and recovery’ pattern. They conclude that IMF programs are most valuable “for countries in vulnerable, as distinct from extreme distress, situations” (Bordo, Mody and Oomes, 2003: 444). Jensen (2004) and Edwards (2006) find results less favorable to catalysis for foreign direct investment and portfolio flows respectively. Bird and Rowlands (2002) also find that disaggregation across time periods, samples, individual lending facilities, and the degree of conditionality, previous experience with the IMF and types of capital flow, make a difference. They recount a complex story for the effect of IMF programs on private flows. However, while these studies have shed important light on the catalytic effect, they do not set

out to test specific propositions such as those formulated in the previous section. The remainder of this paper attempts to fill this gap.

#### **4. Estimations, Methods and Data**

In estimating the catalytic effect, the challenge is to isolate the impact of the IMF so that observed changes in capital movements can be attributed to its involvement. For example, countries experiencing some form of economic crisis may be expected to turn to the IMF for assistance. They may also be expected to experience a capital outflow. It is, however, unsafe to draw a direct causal connection between the IMF's involvement and the loss of capital. There is a potentially serious selection problem.

Countries are generally anticipated to go to the IMF for a program when their balance of payments is unsustainable. The distribution of program and non-program countries is non-random; the fragile economic conditions that drive a country to the Fund will also affect independently its subsequent economic performance and ability to attract new capital. Consequently, the estimated magnitude of the catalytic effect will be biased downwards if uncorrected for non-random selectivity. Earlier empirical work on catalysis avoided formal selection correction procedures (Bird and Rowlands 1997, 2002; Rowlands 2001), occasionally adopting less formal approaches instead (Bird and Rowlands, 2002). More recent work, for example Mody and Saravia (2006) and Edwards (2006), has incorporated formal selection corrections for some aspects of catalysis,

However, the correction procedure and the extent of any bias is itself open to debate. While there is a growing consensus about what factors contribute to a country's propensity to turn to the IMF, equations attempting to identify these factors have proven somewhat disappointing, and there is mixed evidence about the necessity of selection correction procedures. While Vreeland (2003) finds that the selection problem introduces bias when

studying the effects of the IMF on growth and inequality, the results reported by Edwards (2006) when examining the catalysis of portfolio flows appear much less sensitive to it.

There is also the question of how to model empirically the selection bias. There are multiple approaches. A Heckman correction procedure can be used, but this does not estimate directly the effect of the program and may lose some efficiency by estimating program and non-program samples separately; it is more helpful when examining the effects of variables that may interact with the presence or absence of a program. Barro and Lee (2005) use an instrumental variables approach to correct for selection, though the functional form for the selection equation does not reflect well the dichotomous nature of IMF programs.

In this paper we use a treatment effects model as the primary estimation procedure. More commonly used in the medical and evaluation literatures, treatment effects models are similar to standard instrumental variables estimations except that they impose a more realistic dichotomous variable structure on the selection equation.<sup>2</sup> In this case, we use the signing of an IMF program as the treatment, allowing us to focus on the initial signalling effect of a new agreement while correcting for the endogenous choice of entering such an agreement. For the primary estimations we use a maximum likelihood procedure that provides for robust variance-covariance estimation by permitting observations on each country to be treated as potentially dependent.

The data used for the estimation are unbalanced panels of between 1048 (1985-2006) and 1523 (1975-2006) observations (depending on the selection equation) on 80 different middle-income countries (as currently defined by the World Bank) that were eligible to sign an IMF agreement. Low income countries are excluded from the study as the catalytic effect for them is traditionally dominated by official flows, and the evidence suggests that the

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<sup>2</sup> See Brock and Durlauf (2001) and Greene (2003) for details of the likelihood functions and statistical properties. Estimations were conducted using Stata.

influences on their participation in IMF programs differs significantly from middle-income countries (Bird and Rowlands 2007, 2009).

The dependent variable measuring the catalytic effect is a country's net private capital flow as a proportion of gross domestic product (GDP). We use net flows rather than gross flows since our concern is with the impact of IMF programs on countries' overall external financing positions and the consequences for the resulting speed of economic adjustment; gross flows are affected by exogenous repayment schedules while net flows are a more accurate reflection of whether investors wish to increase or decrease their exposure.

Table 1: Average annual net flow-to-GDP percentages 1975-2006

Type of capital flow	All middle income countries that could sign an IMF agreement	All middle income countries that could sign an IMF agreement but did not	All middle income countries that signed an IMF agreement
All private net flows	5.94	6.36	4.16
Private short term net flows	0.62	0.67	0.43
Net foreign direct investment	3.44	3.69	2.42
Net portfolio flows	0.51	0.56	0.32
Net Portfolio Bonds	0.40	0.43	0.25
Net Portfolio Equity	0.12	0.12	0.07
Net PPG total debt	0.92	0.98	0.68
Net PPG bank debt	0.37	0.39	0.26
Net PPG bond debt	0.33	0.36	0.20
Net PPG other private debt	0.23	0.23	0.21
Net PNG total debt	0.43	0.46	0.32
Net PNG bank debt	0.36	0.38	0.27
Net PNG bond debt	0.07	0.08	0.04
Number of Observations	1491	1204	287

We examine various categories of capital flows: total flows, short-term debt, foreign direct investment, portfolio flows (bond, equity and total), public and publicly guaranteed (PPG) debt flows (bank, bond, other private, and total), and private non-guaranteed (PNG) debt flows (bank, bond, and total). The averages for these different flows are summarized in Table 1 for all eligible countries in the sample, and for those that signed, or did not sign, an IMF agreement the year before. The basic data suggest that inflows into IMF eligible

countries are lower in the following year for the group that signs an IMF agreement than for the group that does not. This observation does not take into account the conditions associated with signing an agreement, however, and thus cannot be linked causally to the actual IMF agreements themselves.

The IMF treatment variable is an indicator of the signing of any conditional IMF program. In the largest estimation sample (1523 observations) there is a total of 325 cases where at least one program is signed. In total there are 233 SBAs, 57 EFFs, 4 Structural Adjustment Facility agreements (SAFs) and 38 Enhanced Structural Adjustment Facility or Poverty Reduction and Growth Facility agreements (ESAF/PRGFs) signed; in seven of these cases multiple agreements are signed in the same year. While our main interest is in the non-concessional SBA and EFF agreements, we estimate using all conditional lending facilities to avoid the problem of predicting that an IMF agreement is expected but a concessional SAF, ESAF or PRGF agreement is signed in place of the SBA or EFF agreements that are more typical of the middle-income country group.<sup>3</sup>

To take into account the potential selection bias, a secondary estimation of the dichotomous treatment variable is specified using a set of explanatory variables commonly found in the literature estimating IMF program participation<sup>4</sup>. These economic and political variables are all lagged one year from the year the agreement is signed to minimize

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<sup>3</sup> For the most part eligibility is determined by income cutoffs associated with eligibility for International Development Association concessional lending. Some countries received concessional arrangements from the IMF despite having fairly high per capita income levels, especially former Soviet Republics or Yugoslav states in transition. We also do the analysis using only SBA and EFF agreements while excluding from the sample instances where a concessional program was signed; the results are qualitatively very similar to those reported here, though the magnitude of the (negative) catalytic effect is even larger than when concessional programs are included in the treatment.

<sup>4</sup> See Moser and Sturm (2011) for a recent review.



endogeneity. The explanatory variables include economic growth, reserve adequacy, indicators of recent debt rescheduling and impending debt rescheduling, and indicators of recent IMF agreements in preceding years. Two additional versions of the core economic model are also estimated. The first variant adds an indicator of legislative elections; this version is the selection equation used for the main base model estimations reported in the paper. The third version adds both the indicator for legislative elections and a measure of a country's UN voting proximity to the United States, which has become a popular variable in IMF selection equations because of its interpretation as a measure of US influence over Fund decisions (Thacker 1999; Dreher, Sturm and Vreeland 2009).<sup>5</sup>

The results of these three probit equations for selection are presented in Table 2, and they perform reasonably well with pseudo-R<sup>2</sup> values above 0.23.<sup>6</sup> In general, countries with current debt rescheduling needs (which are linked institutionally to the need for an IMF program) and a history of IMF involvement tend to sign agreements with the Fund. Countries that are growing more rapidly, have relatively high reserves, and that have recently rescheduled debt, are less likely to adopt an IMF program. On the political side a legislative election is associated with a higher propensity to sign an agreement the next year, and there is weak evidence that voting at the UN in a manner similar to the United States is also positively associated with IMF agreements.

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<sup>5</sup> Using the UN voting patterns reduces the sample by 300 observations, as the data are only available starting in 1983. The inclusion or exclusion of the UN variable (and of the various political variables we tried) had no significant effects on the core results, and in most cases the estimated coefficient for the UN variable remained insignificant. A useful and current review of the literature on the geo-political dimensions of IMF behaviour appears in Dreher and Vreeland (2011).

<sup>6</sup> By contrast, recent studies such as Moser and Sturm (2011) report pseudo R<sup>2</sup> measures that are below 0.15 even for much smaller samples, and often below 0.1.

Table 2: Probit results for estimating the propensity scores

Explanatory Variable	Model 1	Model 2	Model 3
	Coefficient estimate (Normal statistic)	Coefficient estimate (Normal statistic)	Coefficient estimate (Normal statistic)
GDP growth (lag = 1)	-0.0304*** (-3.73)	-0.0342*** (-4.21)	-0.0423*** (-4.00)
Reserve adequacy (lag = 1)	-2.08*** (-3.21)	-2.10*** (-3.21)	-1.56*** (-2.74)
Debt Rescheduled (lag = 1)	-0.830*** (-5.69)	-0.780*** (-5.23)	-0.724*** (-3.87)
Debt Rescheduled	1.10*** (8.09)	1.00*** (7.29)	1.05*** (5.77)
Past IMF program (lag = 1)	0.675*** (4.51)	0.648*** (4.01)	0.584*** (3.52)
Past IMF program (lag = 2)	0.305* (2.23)	0.297* (2.02)	0.303 (1.79)
Past IMF program (lag = 3)	0.210 (1.79)	0.186 (1.54)	0.189 (1.32)
Legislative election (lag = 1)	-	0.229*** (2.55)	0.244*** (2.43)
UN voting behaviour	-	-	0.105 (1.80)
Constant	-1.09*** (-11.62)	-1.06*** (-10.91)	-1.25*** (-10.76)
Number of observations	1700	1493	1177
Pseudo R-squared	0.233	0.232	0.246
Years in sample	1974-2006	1976-2006	1984-2006

Robust normal test statistics appear in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 2%, 5%, and 10% levels for two-tailed tests.

The explanatory variables included in our capital flows equations reflect basic economic conditions, and are lagged by one year to reduce the likelihood of endogeneity. They include: per capita GDP, investment rate, export to GDP ratio, real international interest rates, real exchange rate depreciation, reserve adequacy, debt-service ratio, and the ratio of PPG debt to GDP. We selected these variables because they have been commonly included in the empirical literature on capital flows. The expectations about how they should affect capital inflows are relatively clear and we do not discuss them in detail; those variables that suggest robust economic conditions should attract capital, while indicators of difficulty or

weak fundamentals will presumably deter additional inflows. The precise definitions of all variables appear in the Appendix.

In order to examine various other dimensions of how IMF programs may affect capital flows, three Fund-related explanatory variables are included in the primary capital flow equation. First, we examine the amount of resources drawn from the IMF as a share of imports and debt service payments, in order to separate out the effects of program conditionality and signalling from any associated liquidity effects. Second, we examine the number of months in which a country was previously under an agreement (weighted more heavily for more recent years) to determine whether or not past program experience induces or deters additional capital flows. Third, we include a measure of the number of recently incomplete IMF programs to see if a history of poor implementation affects capital flows.<sup>7</sup> Finally, in addition to estimating the standard treatment effects model, we also estimate a fixed-effects version of the treatment effects model by including dummy variables for each country in the sample (and suppressing the constant).

## **5. Results.**

The full results of the treatment effects estimation (using the second selection equation as the base model) on total net flows appear in the first results column of Table 3. For brevity we present only the results for the sample that includes legislative elections in the selection equation but not UN voting. In addition we do not restrict the sample by excluding cases

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<sup>7</sup> The variance inflation factors and bivariate correlations indicate no serious multicollinearity amongst the variables in the main estimating equation. In the selection equation there is evidence of some multicollinearity between the two variables measuring impending and past rescheduling, and amongst the lagged variables indicating past IMF program. When variables with possible multicollinearity are removed the results of the selection equation and the main treatment effects equation do not change significantly.

where there were particularly high capital flows relative to GDP.<sup>8</sup> The results of the fixed-effects version of the base treatment effects model appears in the last column of Table 3.

Table 4 presents the coefficient estimates for the IMF signing treatment variable derived from the different capital flow equations, and presents some summary statistics for the performance of the equations. The last three columns of table 4 present these statistics for sub-samples of countries identified in the selection equation as having a low, medium, or high propensity to sign an agreement.

To focus our analysis we examine in detail only those variables that shed light on the propositions outlined previously. The results of the estimations are presented in two stages. First, we examine the main estimations and selection equations to see how well they perform relative to our prior expectations. Second, we present the results in terms of the six propositions presented in section 2.

### **5.1 Equation performance**

From tables 3 and 4 we can see that with the exception of the portfolio equity flows and PPG and PNG bond flows, all equations have an acceptable level of joint coefficient significance for the full sample estimations; in the smaller sub-samples reported in Table 4 some other categories of flows also have poorly performing equations.

In terms of goodness of fit, the equations perform reasonably well.<sup>9</sup> For total net flows (Table 3) three of the non-IMF variables have statistically significant coefficient estimates, indicating that higher investment levels, more exports, and lower global interest

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<sup>8</sup> We did conduct estimations on samples restricted to cases where the capital flows are not excessive. In the full sample there are only a few cases of very large capital flows (over 75 percent of GDP). However even when we restrict the sample to cases where capital flows are less than 10 percent of GDP, the key results remain qualitatively unchanged.

<sup>9</sup> In one of the robustness checks we estimated the equations using panel data techniques (random effects, though the fixed effects model yielded comparable results). The resulting  $R^2$  values were between 0.12 and 0.13 for the total net flow equations.

rates are all associated significantly with higher net capital inflows.<sup>10</sup> The estimation results of the fixed-effects version of the model are largely comparable to those of the standard treatment effects estimation, though the exports-to-GDP variable no longer has a significant coefficient estimate, while the coefficient estimates for income per capita and reserve adequacy become statistically significant.

Table 3: Base-line Treatment Effects Estimation Results on Total Net Flows

Explanatory Variable (lagged one year)	Model 1 (main model)		Model 2 (fixed-effects version)	
GNI per capita	0.226	(1.26)	0.540***	(2.84)
Investment	0.219***	(4.62)	0.187***	(3.69)
Exports/GDP	0.0583***	(2.56)	0.0238	(0.82)
Real LIBOR	-0.486***	(-5.13)	-0.370***	(-4.39)
Depreciation	0.00228	(1.17)	0.00361	(1.72)
Reserve adequacy	-0.0179	(-1.15)	-0.0218***	(-2.80)
Debt Service	0.0609	(1.57)	0.0277	(0.98)
Debt/GDP	-0.00789	(-1.44)	-0.00531	(-1.01)
IMF Flows	-0.297***	(-2.68)	-0.202**	(-2.38)
Past IMF months	0.156***	(3.29)	0.109*	(2.11)
Failed IMF programs	-0.515	(-0.81)	-0.558	(-0.81)
IMF Treatment	-5.50***	(-3.42)	-5.65***	(-3.69)
Constant	-2.52	(-1.48)	-	
Selection equation for IMF agreement signings (lagged 2 years)				
GDP growth	-0.0392***	(-5.17)	-0.0406***	(-4.97)
Reserve adequacy	-0.0221***	(-2.93)	-0.0214***	(-2.93)
Past rescheduling	-0.757***	(-4.86)	-0.729***	(-4.34)
Imminent rescheduling	1.02***	(6.38)	0.981***	(5.41)
Past IMF program (lag 2)	0.661***	(4.03)	0.659***	(4.09)
Past IMF program (lag 3)	0.311**	(2.29)	0.289*	(2.19)
Past IMF program (lag 4)	0.151	(1.25)	0.165	(1.37)
Legislative election	0.213**	(2.30)	0.231***	(2.58)
Constant	-1.00***	(-8.13)	-1.00***	(-8.33)
No. of obs.	1339		1339	
P(all $\beta$ s = 0) ( $\chi^2$ test)	0.00		0.00	
$\rho$	0.349		0.394	
P( $\rho$ = 0) ( $\chi^2$ test)	0.00		0.00	

Robust normal test statistics appear in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 2%, 5%, and 10% levels for two-tailed tests. The parameter  $\rho$  is the covariance between the estimating and selection equation. If  $\rho = 0$ , then the selectivity correction is not statistically important.

<sup>10</sup> In order to allow sufficient space to focus on catalysis we do not discuss here the magnitude of the effects that the non-IMF variables have on capital flows.

Table 4: Estimated coefficients for the IMF treatment variable for different capital flow categories and different agreement signing propensities.

Variable	Full sample	Low probability of signing	Medium Probability of signing	High Probability of signing
Total flows	-5.50*** (-3.42) (1339,0,0)	-6.41** (-2.20) (1035, 0, 0.03)	9.83*** (2.85) (204, 0, 0.0054)	-0.476 (-0.16) (93, 0, 0.75)
Short term debt	0.062 (0.14) (1339, 0, 0.39)	-0.118 (-0.19) (1035, 0, 0.0.73)	-0.93 (-0.85) (191, 0.76, 0.57)	8.91 (1.04) (113, 0, 0.021)
FDI	-2.26** (-2.43) (1339, 0, 0)	-1.41 (-0.98) (1035, 0.002, 0.13)	2.27 (1.31) (191, 0.02, 0.31)	-3.13*** (-4.83) (113, 0, 0)
Portfolio	-0.77** (-2.39) (1339, 0, 0)	-1.02** (-2.26) (1035, 0.08, 0.07)	0.365 (0.47) (191, 0, 0.97)	-1.49*** (-3.30) (113, 0, 0)
Bond	-0.608* (-2.18) (1339, 0, 0)	-0.84** (-2.08) (1035, 0.08, 0.07)	-1.36*** (-5.56) (191, 0.0, 0.05)	0.032 (0.57) (116, 0.02, 0.52)
Equity	<i>-0.161* (-2.09)</i> <i>(1339, 0.22, 0.02)</i>	-0.168 (-1.81) (1035, 0.07, 0.05)	<i>0.103 (0.46)</i> <i>(191, 0.37, 0.68)</i>	-0.215*** (-3.84) (113, 0, 0)
PPG debt	-1.08*** (-3.43) (1339, 0, 0.0)	-1.01 (-1.75) (1035, 0, 0.03)	3.64*** (4.76) (191, 0, 0)	0.879 (0.62) (116, 0, 0.81)
Bank	-0.544*** (-2.79) (1339, 0, 0.01)	-0.346 (-0.64) (1035, 0, 0.37)	2.84*** (5.40) (191, 0, 0)	-0.106 (-0.06) (113, 0, 0.91)
Bond	<i>-0.517 (-1.91)</i> <i>(1339, 0.13, 0.01)</i>	<i>-0.752* (-1.97)</i> <i>(1035, 0.31, 0.07)</i>	-1.19*** (-5.27) (191, 0, 0)	0.457 (0.99) (113, 0.08, 0.24)
Other	-0.102 (-0.97) (1339, 0, 0.01)	-0.115 (-0.60) (1035, 0, 0.06)	0.197 (0.77) (191, 0.01, 0.67)	-0.615** (-1.78) (113, 0, 0)
PNG debt	-0.621** (-2.36) (1339, 0.05, 0.02)	<i>-0.749 (-1.57)</i> <i>(1035, 0.20, 0.38)</i>	-0.334 (-0.90) (191, 0, 0.35)	0.596 (0.88) (113, 0, 0.28)
Bank	-0.545** (-2.36) (1339, 0.04, 0.03)	<i>-0.675 (-1.61)</i> <i>(1035, 0.24, 0.39)</i>	-0.232 (-0.86) (191, 0, 0.34)	0.607 (1.13) (113, 0, 0.13)
Bond	<i>-0.0963 (-1.63)</i> <i>(1339, 0.16, 0.01)</i>	<i>-0.085 (-0.92)</i> <i>(1035, 0.106, 0.40)</i>	-0.426*** (-4.87) (191, 0, 0)	<i>-0.123***(-1.16)</i> <i>(113, 0.71, 0.04)</i>

Robust normal test statistics appear on the right in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 2%, 5%, and 10% levels for two-tailed tests. Figures in the second parentheses are, respectively, the sample size, the probability that the hypothesis that all regressors are jointly insignificant is falsely rejected, and the probability that the hypothesis that the parameter  $\rho = 0$  is falsely rejected. If  $\rho = 0$ , then the selectivity correction is not statistically important. Probabilities marked as 0 imply the probability is less than 0.005. Estimations where the hypothesis of all regressors being jointly insignificant have their results in italics and must be treated with caution.

For the most part, the equations correcting for selection perform consistently well. The diagnostic tests generally indicate that correcting for selection is important statistically in most of the full sample equations except for short term debt. (Table 4, full sample). As noted earlier, past studies such as Edwards (2006) raise questions about the statistical importance of selection in affecting estimations of the catalytic effect. For several of the smaller subsamples in the last three columns of table 4, however, selection corrections do not seem necessary.

## **5.2 Evidence on the propositions**

*Proposition one* suggests that different types of capital flow are motivated in different ways and that IMF arrangements may therefore exert different degrees of catalysis across them. We find ample evidence to support this proposition. Though not reported in detail, the estimating equations on the different capital flows reveal some variation in terms of important explanatory variables. For example, while high real international interest rates are generally associated with lower net inflows of capital, the estimated coefficient for this variable is not statistically significant for portfolio equity flows, PPG bank and other private debt flows, and PNG bond flows. Higher per capita income levels are associated with private bond lending and portfolio inflows, but not other capital flows. Short-term debt inflows are affected by exchange rate depreciation, while other flows are not. Bond flows generally seem less affected by economic fundamentals such as investment rates, exports, and debt obligations.

In terms of the catalytic effect, the first results column of Table 4 indicates that for the full sample estimations, signing an IMF agreement is generally associated with lower capital inflows in the following year. The magnitude of the catalytic effect varies from insignificant for short-term and “other” PPG debt flows, to weakly negative for PNG and PPG bond flows, to significantly negative for total flows (-5.5% of GDP), and the sub components of foreign direct investment (-2.3 percent), portfolio flows (-0.8%), PPG debt (-1.1%), and PNG debt (-0.6%). The negative effect on FDI is consistent with that found by Jensen (2004). Because

of these differential effects, it appears that IMF agreements are not only associated with a decline in net private capital inflows, but also a restructuring of them. In short, IMF agreements are associated with varied and nuanced effects on different types of private capital flow.

*Proposition two* claims that the strength of the catalytic effect should vary non-linearly with the strength of a country's economic fundamentals. Specifically, net capital flows to countries with either relatively weak or relatively strong fundamentals may not be catalyzed, and in the latter case there may even be a negative effect arising from an unexpected signal of problems. For countries experiencing some difficulties but not yet in distress, the IMF may have a favorable catalytic effect. We tested the proposition by splitting our sample into three groups on the basis of their estimated probability (or propensity scores) of signing an IMF agreement in order to reflect the extent of economic difficulties.<sup>11</sup> The summary catalytic effects appear in the last three columns of Table 4.

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<sup>11</sup> For comparability the boundaries for these sub-samples were kept the same as much as possible. Countries for which the estimated probability of an IMF agreement (as predicted by the selection equation) was less than 0.41 were identified as "low probability" (column 3 in Table 4). When this probability was between 0.41 and 0.585 the country was regarded as having a medium probability of signing (column 4), while the high probability cases were those above 0.585 (column 5). These levels were determined by trying to create the most balanced sample sizes for which the estimations converged. In ten cases the hypothesis of jointly insignificant estimated coefficients could not be rejected, and the results (marked in italics in table 4) must be viewed with some caution. In cases where convergence was not achieved, the sample was varied slightly to generate an equation from which inferences could be drawn with at least some confidence in their statistical properties; the extent of sample size variance can be seen from the reported number of observations for each sample. Due to the non-linearity of the estimation and the small sample sizes the results in the last three columns should be treated with some caution. In the cases of short-term debt and FDI the results for the high probability cases were sensitive to rather small variations in the sample. Most of the other cases appeared fairly robust.



We do find evidence of the non-linearities suggested by proposition 2. For countries that sign an IMF agreement despite a low predicted probability of doing so the results indicate a large and statistically significant negative catalytic effect, which then reverses and becomes strong and positive for countries with medium signing propensities. Where proposition 2 fails is in predicting negative catalysis for countries with a high predicted probability of an agreement; the associated coefficient is negative, but it is fairly small and statistically quite insignificant. These results are somewhat sensitive to the sample used in the estimation. Most notably, decreasing slightly the signing probability that divides the medium and high probability samples yields a large and positive catalytic effect for the latter group, suggesting that the main positive catalytic effect is observed primarily for a few cases around this cut off value.

By and large this pattern is reflected in the different components of total capital flows. For all of the different components the low signing probability samples exhibit negative catalysis, though only two of these are statistically significant. The groups with medium and high predicted probabilities of signing exhibit considerable variation across the different capital flows categories, with some of the results being sensitive to the chosen boundaries for these groups. For the medium probability group, IMF agreements seem to repel bonds but attract PPG bank debt. It is interesting to note that the results for the bond estimations are broadly consistent with those reported in Mody and Saravia (2006), who focus on bonds alone.

In addition, some countries on the boundary between these categories experience large short-term capital inflows, though the associated coefficient is not statistically significant. Finally, while the overall negative catalytic effect for countries with a high probability of signing is statistically insignificant, there are several categories of flows that have statistically significant negative coefficients on the IMF signing variable.

Given the small sample sizes for some of the estimations and the sensitivity of some results to the choice of differentiating between groups with medium and high probabilities of signing, some caution is warranted in interpreting the results. In general, however, proposition 2 receives significant support from our analysis.

*Proposition three* suggests that poor program implementation will weaken the catalytic effect. It is difficult to test all of the interpretations of this proposition, as implementation is complex and difficult to measure (Arpac, Bird and Mandilaras 2008; Bird 2007). Here we focus on the general effect that weak implementation has on catalysis by examining the estimated coefficient for the indicator of recent incomplete programs. Following the literature, we interpret substantial non-completion as an indication of program failure (Killick 1995; Bird 2007). In the total net flows estimation the estimated coefficient for our measure of program incompleteness is negative as expected, however it is not statistically significant. For PNG bank flows, PPG bank flows and portfolio bond flows, the estimated coefficient for past incomplete programs is negative and statistically significant as expected. While none of the other estimated coefficients for this variable are statistically significant, and most are negative, there is weak evidence that PPG bank flows may actually and paradoxically react positively to past incomplete programs.

Interacting program failure with the indicator of an IMF program signing did not generate any compelling evidence of a change in the catalytic effect. Overall, then, the non-completion of previous IMF programs does not appear to have a strong or consistent effect on subsequent capital flows, and proposition three cannot be accepted on the basis of the tests conducted here.

*Proposition four* suggests that a lengthy history of IMF programs, even when completed, will reduce the catalytic effect by signalling structural difficulties. Recidivism is generally associated with poorer countries (Bird, Hussain and Joyce, 2004), so here, and

given our focus on middle-income countries, we examine this proposition by looking at a shorter three-year history. The estimations for aggregate net flows reported in Tables 3 do not support the proposition. In fact, in most equations the presence of recent IMF programming in a country is associated with higher current inflows, including total flows, short term debt, foreign direct investment, and PNG bank debt. Consequently there is some evidence to suggest that private lenders may value the Fund's ongoing involvement with a country.

In the other categories of capital inflow there was no statistically significant coefficient estimate except for a negative one for other private PPG debt, and then only at 0.075 one tailed level of significance. In the total flows equation, an additional month of IMF programming in the previous year is associated with an increase in capital inflows equivalent to 0.16 percent of GDP. Subsequent tests interacting past IMF programming and current IMF agreement signing, and looking at a longer time horizon (previous six years), do not alter these findings.

*Proposition five* suggests that catalysis may vary with the type of IMF agreement. Short term investors may be looking for the stabilization policies associated with SBAs, while longer term investors are looking for the more numerous conditions and longer-term structural adjustment focus of EFFs. The results for all agreements (311 cases), all non-concessional agreements (276 cases) and SBAs (222 cases) are all essentially the same, reflecting the dominance of the stand-by arrangements. The only notable difference appears for the estimation of the EFF catalytic effect, which is similar to the other non-concessional agreements except for the absence of a statistically significant negative catalytic effect on FDI and portfolio flows. The sign of the coefficient estimate also becomes negative for short term flows, though it remains statistically insignificant. So there is some evidence that EFF agreements are less repellent to the non-debt flows.

Table 5: Estimated coefficients for the IMF, SBA and EFF treatment variables for different net capital flow categories.

Variable	All IMF	Concessional	SBAs and EFFs	SBAs only	EFFs only
Total	-5.50*** (-3.42)	5.04 (1.27)	-6.17*** (-3.94)	-5.96*** (-3.90)	-4.70** (-2.48)
Short term	0.062 (0.14)	-0.173 (-0.27)	0.253 (0.61)	0.373 (0.88)	-0.560 (-1.37)
FDI	-2.26** (-2.43)	9.16*** (4.75)	-2.84***(-3.79)	-2.86*** (-4.01)	-1.49 (-1.45)
Portfolio	-0.770** (-2.39)	-0.442 (-1.75)	-0.787* (-2.13)	-0.839** (-2.27)	-0.247 (-0.64)
Bond	-0.608** (-2.18)	-0.415 (-1.81)	-0.615 (-1.90)	-0.662* (-2.02)	-0.174 (-0.49)
Equity	<i>-0.161* (-2.09)</i>	-0.0643 (-0.88)	<i>-0.169* (-2.13)</i>	-0.183** (-2.32)	<i>-0.084 (-0.88)</i>
PPG debt	-1.08*** (-3.43)	-1.27***(-2.86)	-1.06*** (-3.10)	-1.02*** (-2.92)	-1.06* (-2.03)
Bank	-0.544***(-2.79)	1.73*** (4.04)	-0.526***(-2.73)	-0.481** (-2.53)	-0.820**(-2.49)
Bond	<i>-0.517 (-1.91)</i>	<i>-0.380 (-1.77)</i>	-0.521 (-1.65)	<i>-0.579 (-1.82)</i>	<i>-0.056 (-0.16)</i>
Other	-0.102 (-0.97)	-0.358**(-2.44)	-0.101 (-0.76)	-0.0470 (-0.32)	-0.249 (-1.07)
PNG debt	-0.621** (-2.36)	3.41*** (5.20)	-0.699** (-2.43)	-0.710** (-2.38)	-0.656* (-2.07)
Bank	-0.545** (-2.36)	3.05*** (5.49)	-0.620** (-2.45)	-0.638** (-2.42)	-0.566* (-1.97)
Bond	<i>-0.0963 (-1.63)</i>	-0.193 (-0.19)	<i>-0.102 (-1.60)</i>	<i>-0.0951 (-1.53)</i>	<i>-0.178 (-1.07)</i>
Number of signings	311	35	276	222	56

Robust normal test statistics appear in parentheses. \*\*\*, \*\*, and \* indicate statistical significance at the 2%, 5%, and 10% levels for two-tailed tests. Estimations where the hypothesis of all regressors being jointly insignificant have their results in italics and must be treated with caution.

There are also 35 cases of middle income countries in our sample signing concessional agreements. The estimated catalytic effects for these arrangements differ significantly from the non-concessional ones, and indeed provide the only evidence of positive catalysis. Three categories of capital flows appear to respond positively to concessional arrangements: FDI, and both PPG and PNG bank debt. These responses are generally large in magnitude: the coefficient on FDI indicates an inflow of 9 percent of GDP.

Overall, PPG debt responds negatively to concessional agreements, due to outflows of PPG other debt and, of marginal statistical significance, PPG bond debt.

So, proposition five is generally supported by our results. If we consider that one of the main differences between these agreements is the stringency and nature of their conditionality, then a reasonable though complex story can be told about how the conditions of key IMF agreements affect differentially each specific type of capital flow.

*Proposition six* suggests that the liquidity effects of IMF programs may catalyze, in particular, short term capital, although there may be a difference between used and unused access to IMF resources. When the liquidity measure is taken as actual drawings in a year, the results indicate a substitution effect between IMF credit and almost all other capital flows. Measuring IMF resource use as drawings from the IMF, expressed as a proportion of imports and total debt service payments, gives a sense of the extent to which these resources are financing external outflows.<sup>12</sup> As these current account outflows are on average about 50 percent of GDP for the countries in the sample, the magnitude of the effect on total capital flows is that for every ten dollars of IMF credit used there is about a six dollar decline in other private flows, or a substitution effect of about -0.6.<sup>13</sup> This substitution effect is consistently negative across the different categories of capital flow, except for PPG bank and other private debt (insignificant and small positive coefficient estimates), and reflects particularly strong negative substitution effects for portfolio and PNG debt flows.

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<sup>12</sup> Estimations using the size of the original agreement as a percentage of GDP, indicating potential borrowing from the IMF, had the same qualitative effect. This result is not really surprising, as most of the countries do draw on their agreements very quickly, and these drawings will typically be a fixed proportion of the actual agreement size.

<sup>13</sup> The calculation comprises the -0.297 IMF flows coefficient in Table 2 multiplied by 2 to reflect the sample average ratio of GDP to exports and debt service payments.

Since the direct substitution effect is less than one, it follows that, in total, additional money is made available (especially to the government) when borrowing from the Fund occurs (though the effect may be offset or enhanced by the additional separate effects of new agreements, old ones, or the record on failed implementation). However, there is little to suggest that the provision of IMF liquidity induces additional capital inflows from other private sources, and the IMF may need to lend considerably more than a country's anticipated capital shortfall in order to support the desired blend of adjustment and financing. This result could be seen as being consistent with the claim by Mody and Saravia (2006) that private capital markets may, in some cases, want to see a Fund program in place irrespective of the amount of Fund credit involved, but they do not want to see countries having to use that credit.<sup>14</sup> On balance, however, our results do not support proposition six; the biggest negative responses to IMF inflows are for portfolio bond and PNG debt flows; other flows are not affected significantly. IMF resources tend to bail out rather than bail in private capital.

### **5.3 Sensitivity of the results.**

The basic results for the IMF effects reported above are reasonably robust across a range of sensitivity tests that we conducted. Alternative versions of the selection equation and the capital flow equation produce similar results, (see, for example, the results reported in the last column of Table 3).

Our first formal diagnostic procedure is to make sure that our data are stationary. We test each of the dependent and independent variables in the capital flows equation for

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<sup>14</sup> Mody and Saravia (2006) argue that having the Fund's resources available is seen as a positive signal by bondholders, but that actually drawing on the resources is indicative of financial problems that may discourage lending. Of course there is interesting endogeneity here: if the positive response to an agreement induces only limited capital inflows, a government may need to draw on its agreement and thereby possibly discourage further lending.

stationarity using the test developed by Levin, Lin and Chu (2002) for panel unit roots.<sup>15</sup> The hypothesis of non-stationarity is rejected for all variables except per capita income. Re-estimating the equations without this variable does not yield any significant changes in our results.

Our second test for robustness splits the sample into early (pre 1991) and late (post 1990) periods.<sup>16</sup> We find that the later sample period leads to a negative coefficient estimate for the IMF variable that is larger in magnitude (-5.31) and statistically very significant. By contrast the earlier period still has a fairly large negative coefficient estimate for the IMF signing variable (-2.13), but it is not statistically significant. Therefore the negative association between IMF programs and private capital flows seems to be a more recent phenomenon.

We also split the data on the basis of income per capita. The effect of signing IMF agreements on private capital flows into the poorer middle income countries remained negative but was smaller in magnitude and statistically insignificant once GDP per capita dropped below approximately \$5700 (approximately the median). In contrast the negative catalytic effect was much stronger in magnitude, and the associated coefficient estimate (-7.54) is statistically significant, for the higher income portion of our sample. Consequently the negative effects of IMF agreements on private capital flows seem largely confined to wealthier countries in the sample, and the processes determining capital inflows into poorer countries appear to be distinct from those of the wealthier group (see endnote 5). This result

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<sup>15</sup> The test requires a balanced and continuous panel, and was therefore conducted on 840 observations, the largest such data set we could construct.

<sup>16</sup> This division conveniently coincides with the pre- and post-Cold War periods, a test frequently of interest to international relations scholars. Whether the increasingly negative effect of IMF program signings indicated by the results is related to the end of the Cold War, the subsequent growth in private capital flows after the Brady Plan resolution of the middle-income country debt crisis, or other factors, is difficult to disentangle.

also holds true for levels of political and civil freedom. Dividing the countries into “more free” and “less free” (Freedom House, 2011) indicates that the negative estimated coefficient on the IMF agreement signing variable is statistically significant only for freer countries. Finally, splitting the sample by relative economic size (GDP as a share of all middle income country GDP) indicates that negative catalysis is particularly problematic for middle-sized countries that are relatively larger, but not the largest. The estimations on the smaller and very large middle income country groups have negative coefficient estimates for the IMF signing variable, but these coefficients are not statistically significant.

One problem with the data is its low frequency relative to developments in capital markets. Annual data obscures the reaction to IMF programs by treating programs signed early in a calendar year the same as programs signed at the end. To investigate whether this artifice of the data introduces a bias, we perform two sets of tests. First we re-weight the dependent variable as a blend of inflows over two years. For example, for an agreement signed in February the inflows of the first year would be weighted 10/12 while those of the second year would be weighted 2/12. The results are comparable regardless of whether we use inflows from the year of signing and the following year, or the following two years. The magnitude of the negative catalytic effect is larger for the weighted flows of the two years after an agreement (-5.21) than for the weighted flows of the year of the agreement and the following year (3.99); both estimated coefficients are statistically significant.

This result is reinforced by the second test in which we run the main estimation on two separate samples: those where countries sign early in a calendar year, and those where they sign later in the year. The estimated coefficient on the IMF agreement variable again remains negative and strongly significant statistically in both cases, and the magnitude of the effect is somewhat larger for agreements signed earlier in a year. These results suggest that



the negative effect of the agreement grows stronger for a period after the agreement is signed, rather than having an immediate and large negative effect that then begins to dissipate.

To test whether program size affects catalysis we split the sample into two according to program size relative to GDP and estimate the base-case treatment effects model on each. For both samples the estimated coefficient for the IMF agreement variable remains negative, statistically significant, and roughly the same magnitude (though the larger agreements have a slightly smaller negative effect). Our conclusion is that the size of the agreement is not particularly important in determining the catalytic effect.

We also test whether catalysis was affected by possible political interests of the United States, as measured by the similarity of a country's votes on key UN resolutions to the voting of the US. The base model equation (which does not include UN voting in the selection equation) is estimated on two samples, one where the country votes frequently the same way as the US, and a second where voting similarity is very low. The estimations for the two groups both indicate strong and statistically significant negative catalysis, though interestingly the magnitude of the estimated coefficient is much larger for countries that vote less often with the US than for those who vote with the US (-6.6 versus -3.75).

Finally, we re-estimate the basic capital flows equations but include the lagged dependent variable. The results are reasonable and largely comparable to those already reported. For example, the lagged dependent variable coefficient estimates are all positive and statistically significant for all the different categories of capital flow, except for short term debt flows where the relationship is negative and statistically significant. The estimated coefficients for the IMF signing indicator generally remain comparable to the main results reported in Table 4, though their size and level of statistical significance are generally somewhat smaller and, in the case of PNG debt flows, there is evidence of positive catalysis.

Overall, therefore, our results raise serious doubts about the existence of a general and positive catalytic effect associated with the signalling effect of a newly-signed IMF program. In fact, it appears that the effect on subsequent capital inflows of signing such agreements is generally negative for middle income countries. Just as important, however, are the variations in this effect across different types of flows, time, and the economic state of the country.

## **6. Concluding Remarks.**

For many years the IMF has claimed that an important part of its role is to catalyze others to lend either by relieving immediate liquidity crises or by allowing governments to signal their commitment to reform through conditionality. Early empirical evidence seemed to be inconsistent with this claim, though these studies were not strongly grounded in formal theory and did not formally correct for selection bias. More recent research has dealt more formally with selection, but has focused on individual components of capital flows (such as bonds, portfolio flows, and FDI) in isolation. As their results point in different directions, making generalizations on the basis of any one of them is unsatisfactory. Formal theoretical models suggest that the degree of catalysis should depend on specific factors such as a country's economic fundamentals, while less formal theorizing has identified other factors that may influence the nature of catalysis. These theoretical insights have not been systematically explored in empirical analyses.

This paper has attempted to provide a comprehensive empirical analysis of catalysis that is guided by both formal and informal theoretical insights, and is informed by both the necessity of correcting for selection and the limitations of doing so. Using a treatment effects estimation procedure to deal with selection problems, it explores the empirical connection between IMF arrangements and private capital flows in middle-income countries. Unlike other recent studies it examines catalysis across a wide range of private capital flows as well

as across IMF facilities. Rather than simply presenting the resulting evidence, we relate it to a series of propositions concerning catalysis.

Our findings confirm that it is unwarranted to generalize from a sub-set of results. While there are some combinations of circumstances (such as economic fundamentals in the borrowing country, particular IMF facilities, and specific types of capital flow) in which catalysis seems to occur, there are others in which it does not, or in which the effect is negative. Indeed, on the whole there is much stronger evidence that catalysis is generally negative rather than positive. However the highly nuanced nature of the results points to the dangers of attempting to make general conclusions, especially on the basis of examining only parts of the whole.

Given the importance attached by the IMF and others to catalysis, and given that a belief in its efficacy has consequences for IMF lending and resources, the policy implications of our findings are important. First, policy needs to be much more subtle and cognizant of the circumstances in which IMF programs are either more or less likely to exert a catalytic effect on different types of capital inflows. Second, the limitations of catalysis need to be fully taken into account in the design of IMF programs and in thinking about alternative ways of mobilizing external capital in support of economic adjustment, as discussed in Bird and Rowlands (2004). Third, reforms to conditionality, such as those introduced following the global economic and financial crisis in 2008, need to be informed by the impact they may or may not have on private capital flows. The results reported in the paper illustrate just how complex catalysis is. Simplistic assumptions about it are theoretically and empirically unjustified, and policies based on these assumptions are unlikely to be effective.

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## Data Definitions and sources

The mean and standard deviation for the main sample of 1339 observations appear in parentheses. Source is the World Bank's *World Development Indicators*, unless stated otherwise.

**Dependent variables:** All original data in current \$US.

All flows: The total of all net PPG, PNG, Portfolio, FDI and short-term debt flows as a percentage of a country's GDP. (5.57, 8.51)

Short-term debt: Net short-term debt flows as a percentage of GDP. (0.583, 4.15)

FDI: Net foreign direct investment into a country as a percentage of GDP. (3.13, 5.01)

Portfolio (total): Purchases of shares and related assets by foreigners as a percentage of a country's GDP. (0.542, 1.90) (bonds only: 0.425, 1.76) (equity only: 0.117, 0.587)

PPG debt (total): Net public and publicly guaranteed debt flows as a percentage of GDP. (0.841, 2.78) (PPG bank debt: 0.289, 1.67) (PPG bond debt: 0.347, 1.68) (PPG debt from other private sources: 0.206, 1.30)

PNG debt (total): Net private non-guaranteed debt flows as a percentage of GDP (0.476, 2.06) (PNG bank debt: 0.398, 1.82) (PNG bond debt: 0.0784, 0.476)

### **Capital Account Equation Explanatory Variables (all lagged one year except for real LIBOR)**

GDP per capita: Real GDP per capita in constant thousands of 2005 \$US prices (Chain series). Source: Penn World Tables (6.61, 3.80)

Investment: Investment share of real GDP per capita as a percentage. Source: Penn World Tables. (25.2, 9.10)

Exports/GDP: Ratio of exports to GDP (all in current \$US). (38.1, 21.1)

Real LIBOR: The London Interbank Offered Rate on U.S. 6 month Treasury Bills (annual average) minus the rate of U.S. CPI inflation, as a percentage value. Source: IMF, *IMF Financial Statistics*. (1.73, 1.98)

Depreciation: The percentage annual change in the official number of domestic currency units per \$U.S. multiplied by the ratio of the U.S. consumer price index to the country's consumer price index. (2.84, 54.2)

Reserve adequacy: Total foreign reserves divided by total imports of goods and services and debt service obligations (all in current \$US), as a percentage. (33.2, 34.5)

Debt Service: Total long-term debt service payments divided by total exports of goods and services (all in U.S. dollars), as a percentage. (20.8, 17.1)

Debt/GDP: Total public and publicly guaranteed debt, divided by GDP (both in current \$US), as a percentage. (41.4, 52.4)

IMF Flows: Net borrowing from the IMF divided by imports and total debt service payments, as a percentage. (0.759, 2.19)

Past IMF months: Weighted number of months of the past three years in which a standby or EFF agreement is in effect (weighted 1, 0.75, and 0.5 for 1, 2 and 3 years previously, respectively). Source: IMF, *IMF Annual Report*, various years. (6.01, 8.23)

Failed IMF Programs: The number of agreements in the past two years which were “incomplete” according to the methodology of Killick, that is agreements with more than 20% of the commitment undrawn by the country at the time of expiry. Source: *IMF Annual Report*, various years. (0.146, 0.386)

IMF: A binary variable indicating if an IMF program has been in operation in the country in the previous year. Source: IMF, *IMF Annual Reports* (0.232, 0.422)

**Selection Equation Explanatory Variables: (all lagged two years)**

GDP growth: Annual GDP growth rate. (4.08, 6.41)

Reserve adequacy: Total foreign reserves divided by GDP (both in current \$US). (14.8, 16.3)

Past rescheduling: A binary variable indicating how many debt reschedulings it has undertaken in the previous two years (0.173, 0.379)

Imminent rescheduling: A binary variable indicating that debt rescheduling occurred in the current or coming year. (0.238, 0.426)

Past IMF: A binary variable indicating that an IMF program was operational for at least one month of the previous year. Source: IMF, *Annual Report*, various issues. Two year lag (0.319, 0.466); three year lag: ( 0.350, 0.477); four year lag: (0.355, 0.479).

Legislative elections: A binary variable indicating that a country had a legislative election in a given year. Source: World Bank, *Database of Political Institutions*. (0.211, 0.408)

UN voting behaviour: The number of times a country voted the same way as the United States on a key vote divided by the number of key votes in which it voted differently from the United States, abstained, or was absent. Source: United States Department of State, *Voting Practices in the United Nations*, various years. (0.841, 0.821)

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