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Abstract: Are the lending programs of the International Monetary Fund bad for democracy? The conventional wisdom is that the implementation of IMF conditional lending programs triggers cycles of austerity, social protest, and government repression that result in democratic backsliding. We present evidence which suggests that the conventional wisdom is wrong. We use a statistical procedure – matching – to handle a major threat to inference about the direction and size of the effect of IMF programs on the level of democracy in borrowing countries: the non-random assignment of the “treatment” (IMF conditional lending arrangements) to cases. By pairing treatment and control cases that are very similar, we are able to generate more credible estimates of the impact of the IMF on democracy. Our results are surprising: IMF programs are associated with increases in the level of democracy. We find that the impact of the IMF is additive: countries that spend more time under the IMF lending programs experience big improvements in the level of democracy.

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Do the lending programs of the International Monetary Fund undermine the quality of democracy in the countries that make use of the institution's resources? Many thoughtful observers think that the answer is a resounding yes (Stiglitz 2002: 96). The IMF controls substantial resources: since 2002 the IMF has extended over \$550 billion in funds to needy members. To safeguard those resources, the institution conditions access to funds on policy commitments made by the borrower. IMF programs provide lifelines to governments struggling with all manner of economic problems, but they impose severe limits on the borrower's policy discretion that may restrict the accountability of governments to their publics. The kinds of policy changes that governments institute to meet the IMF's conditions – devaluations, tax and interest rate hikes, cuts in public programs, and the removal of consumer price supports – can trigger violent social protest. When Bolivian President Gonzalo Sanchez de Lozada announced a tough set of IMF-enforced austerity measures in 2003, “the popular reaction was swift, widespread, and unequivocal: protests and political marches broke out, leading to violent confrontations with the army” (Babb and Carruthers 2008: 13). Sanchez de Lozada was forced to flee the presidential palace and, eventually, the country under conditions of severe social and political upheaval (Dreher and Gassebner 2008).

The idea that IMF-mandated austerity programs increase the risk of social instability is not new, and it enjoys widespread currency with economic policymakers. Consider the exchange between Federal Reserve Governor Nancy Teeters and Fed Chairman Paul Volcker regarding the IMF-led bailouts of Mexico and other highly-indebted Latin American countries in the early 1980s:

Teeters: Isn't there also a problem of potential civil disorder in Mexico if they become too austere?

Volcker: Well, that's part of not being able to carry out the program. That is present in all these countries.¹

Stable, high-quality democracies are less prone to experiencing civil strife, military crackdowns, and irregular exits from power. If these dynamics are associated with IMF programs in even a small proportion of cases, then, on balance, the IMF's effect on the quality of democracy will be harmful. Kapur and Naim neatly summarize the conventional wisdom: "Everyone agrees that these consequences reach well beyond the economic realm and can have massive, all-too-concrete social and political effects, not least on the processes and institutions that make up the nerves and sinews of democracy" (2005: 90).

A handful of large-N studies have produced evidence that directly or indirectly supports the deleterious political effects of IMF lending. Barro and Lee's (2005) analysis suggests that greater IMF involvement produces lower democracy scores. Abouharb and Cingranelli (2009) find that human rights conditions worsen in the presence of IMF programs. A growing body of research further illustrates the relationship between IMF programs and the outbreak of a wide range of forms of political violence.² Brown (2009), working with a sample of Latin American countries observed between 1998 in 2003, finds evidence that more onerous loans put downward pressure on the level of democracy. Dreher and Gassebner (2008) find that IMF programs dramatically increase the likelihood of major government crises, which could in turn be expected to place undue pressure on fragile regimes.

¹ The exchange between Teeters and Volcker is drawn from the transcript of the December 21, 1982 meeting of the Federal Open Market Committee (FOMC 1982: 71).

² Auvinen 1996; Bienen and Gersovitz 1985, 1986; Franklin 1997; Haggard 1985; Haggard, Lafay, and Dessus 1995; Hartzell, Hoddie, and Bauer 2010; Morrison, Lafay, and Dessus 1994; Siddell 1988; Snider 1990; Walton and Ragin 1990.

We remain unconvinced by the conventional wisdom and the evidence marshaled in support of it for a couple of reasons. Figure 1 illustrates a pattern that does not sit easily with the claim that time spent under the IMF's watchful eye increases the chances of democratic backsliding: in the years between 1970 and 2000 the average level of democracy in the developing world skyrocketed – and so did the proportion of countries under IMF programs in each year.³

FIGURE 1 GOES HERE

This correlation has not gone unnoticed by policymakers. In March 2000 the International Financial Institution Advisory Commission (better known as the Meltzer Commission) issued a detailed report to the United States Congress on the activities and efficacy of the World Bank and IMF. The report was highly critical of the IMF and, unsurprisingly, highly controversial. The four members of the commission who dissented from the report offered the following view:

...[T]he report repeatedly argues that the IFIs undermine democracy by somehow precluding local governments from pursuing autonomous economic policies. The report is particularly critical of the Fund's role in Latin America, where virtually every country has become democratic during the very period when the IMF has been most active there. IMF conditionality is obviously not a roadblock to democracy. The allegations of the report simply fail to square with the facts of history (IFIAC 2000: 113).

The data in figure 1 are suggestive but cannot support any firm inferences about the direction of the relationship. Perhaps the upward trend in the level of democracy was entirely driven by the fraction of countries that spent very little time under IMF programs. We get a better grasp on the relationship by comparing the level of democracy in countries under and countries not under IMF programs, conditional on factors that are

³ We measure the level of democracy using the Polity and Freedom House scores; below we give more details on how the two indexes are constructed. The figure includes data from 110 developing countries.

known to affect the level of democracy. Some recent research that tries to control for factors that influence the level of democracy indicates that there is indeed solid evidence of a positive relationship between IMF programs and measures of democracy (Limpach and Michaelowa 2010; Nelson and Wallace 2005).

Any claim of a relationship between IMF programs and democracy – including the one we posit below – should be treated with caution for the simple reason that there are massive inferential obstacles endemic to this kind of research. Identifying the direction and size of the effect of conditional lending on democracy is complicated by the way in which IMF programs are distributed. If the IMF used a randomizing device to dole out conditional loans, we would be confident that the difference in democracy levels between the treated cases (those under IMF programs) and control cases (not under) was attributable to the intervention. IMF programs are not distributed randomly; some countries are more likely to receive the treatment, and the factors that increase the propensity to get the treatment may themselves be correlated with the outcome (in this case, the quality of democracy).

To handle this problem we make use of matching techniques. Matching does not (and cannot) replicate a randomized experiment. But it sharpens our estimates of the effect of the IMF on democracy by allowing us to pair a “treated” case with a very similar “untreated” case. As noted by Gilligan and Sergenti, matching has another virtue: “inferences are based entirely on the data. None of the results flow from arcane functional form assumptions or implausible arguments about valid instruments” (2008: 91). Matching is a more credible way of addressing selection problems than the alternative strategies employed in existing studies of the IMF and democracy.

The findings justify our skepticism about the conventional wisdom. Making use of thirty years of data from a large sample of developing countries, we find that IMF programs have modest through definitively positive effects on the level of democracy. We show that estimates from unmatched data are unreliable because of the dramatic differences in the distribution of important covariates between the treated and control samples. After we use the matching procedure to improve balance in the data, we find that treated units (years in which a country is enrolled in an active IMF program) are slightly more democratic than the control units. We then ask whether repeated exposure to IMF lending programs has an additive effect on the level of democracy. We find that our measure of recidivism – the cumulative number of years spent under IMF conditional loans – has a positive and significant impact on democracy scores in several different specifications.

*The IMF and the Politics of Hard Choices*⁴

There is a vibrant literature on how international institutions contribute to the consolidation of democracy around the world. Keohane, Macedo, and Moravcsik, for example, make the case that

Involvement with multilateral institutions often helps domestic democratic institutions restrict the power of special interest factions, protect individual rights, and improve the quality of democratic deliberation, while also increasing capacities to achieve important public purposes. Under some plausible circumstances international cooperation can thus enhance the quality of democracy even in reasonably well-functioning democratic polities (2009: 2).

The evidence for the democracy-promoting qualities of international organizations is impressive: engagement with institutions as varied as NATO (Epstein 2005), regional

⁴ This section of the paper draws on material from Nelson and Wallace (forthcoming).

organizations (Pevehouse 2002), and the WTO (Aaronson and Abouharb 2011) is linked to improvements in the quality and durability of democratic governance.

It is striking, then, that the IMF is the outlier among its peers. The conventional wisdom about the IMF's democracy-retarding effects is rooted in two main assumptions regarding the negotiation and implementation of IMF programs. First, when countries turn to the IMF for help negotiations frequently take place behind closed doors, and citizens' voices are as a result even less likely to be heard (Stiglitz 2002: 169-170). As Kapur and Naim remark "By their very nature, IMF conditions arise not from debate and discussion within a society, but come rather from unelected foreign experts." (2005: 9) By reducing transparency and centralizing decision making, the potential for input from a broader range of societal stakeholders is further undermined. According this view, it comes as no surprise that opponents turn to violence as their only viable course of action, and that governments respond in kind with more repressive policies.

Second, the actual implementation of IMF programs enforces strict limits on the policy discretion of borrowers and consequently has the potential to lead to harmful distributional consequences. Governments have to make hard choices about which societal groups will face uncompensated adjustment costs. As Hartzell, Hoddie, and Bauer put it, adjustment under the auspices of the IMF's conditional lending programs "lessens state actors' ability either to compensate or confront the losers produced by economic liberalization through budget cuts and the loss of other forms of control over the economy. The effect of this process is to increase the potential for violence by actors reacting negatively to their changed circumstances" (2010: 344). Thus countries fall into

the conflict-instability spiral described in the first section of this paper, with its attendant deleterious effects on the quality of democracy.

There is no doubt that IMF programs are costly. In exchange for access to financial resources, the IMF asks for painful, but presumably salutary, policy changes to correct the misguided practices that originally brought the prospective borrower to the institution's doorstep. While recent work to measure the extent of conditionality in IMF agreements reveals a surprisingly wide degree of variation (Copelovitch 2010; Gould 2003, 2006; Nelson 2009; Stone 2008) there is a common underlying model of the economy used by the IMF's staff to generate policy targets. Based largely on experiences in Mexico and Chile in the late 1940s and early 1950s, an economist in the IMF's research department named Jacques Polak proposed a simple economic approach that focused on excess domestic demand as the key source of balance of payments disequilibrium.⁵ The so-called "Polak Model" formed the intellectual grounding for the conditional lending facilities of the next half century.

For the IMF, targeting the monetary and fiscal sources of excess demand and structural policies that distort price signals is simply good textbook economics applied to the real world. But economic policymaking is unavoidably political. The IMF's policy agenda produces winners and losers within society. Groups that bear the brunt of the policy changes required by the IMF – small farmers who lose access to price supports, civil servants trimmed from bloated payrolls, urban workers who face rising food prices, among others – may turn against the government that entered into the loan agreement. In

⁵ Woods, 2006: 40-43; see also Babb, 2007; Barnett and Finnemore, 2004: 51-55. Each author notes that the attraction of the Polak model was enhanced by its simplicity and measurability: according to Woods (2006: 41), "the great advantage of Polak's new approach was that it used data on assets and liabilities in the banking system, which were more widely available and reliable than the national accounts data that other previous approaches to analyzing the balance of payments required."

response, the government may resort to extra-judicial killings and torture to control protestors. Evidence of a *repression effect* is provided in quantitative evidence assembled in a series of studies by Rodwan Abouharb and David Cingranelli (2006, 2007, 2009). Controlling for the nonrandom distribution of IMF programs using Heckman selection models, Abouharb and Cingranelli (2007, 2009) report a strong positive relationship between a variable that records the cumulative number of years spent under IMF programs between 1981 and 2003 and several indicators of human rights violations. The authors' focus is on government repression rather than the existence and quality of democratic institutions and practices, but indicators of civil rights violations should track indicators of democracy closely. If the repression effect holds, we will observe a strong negative correlation between indicators of IMF involvement and measures of democracy, a la Barro and Lee (2005).

A more nuanced view of the political consequences of IMF lending arrangements comes from Nooruddin and Simmons' (2006) work on how different regimes choose to bear the pain of IMF-imposed austerity. Observers sometimes joke that the IMF stands for "It's Mostly Fiscal" – and for good reason, considering that the core of nearly every lending arrangement involves strict limits on government spending. However, while the IMF sets stringent caps on the overall level of government expenditures, it generally avoids specifying *how* governments should meet spending targets. Deciding which areas of the budget get trimmed is left largely to the borrower.

In contrast to the repression model, Nooruddin and Simmons "believe that it is not entirely accurate to argue, as many critics do, that IMF programs are simply imposed on countries in economic turmoil and hence that the IMF is entirely culpable in whatever

negative effects these programs have on the poor” (2006: 1007). Rather, Nooruddin and Simmons propose that variation in national political institutions determines how spending cuts are distributed across the national budget. In democratic regimes, policymakers respond to pressure from organized interests channeled through representative institutions. Since their political survival depends on the support of well-organized and powerful interest groups, and the most vulnerable groups in society tend to be neither organized nor powerful, the public policies that most benefit the poor – namely, education and health – will be on the chopping block sooner than expenditures that go to politically powerful groups. While Nooruddin and Simmons’ analysis shows that public spending on health and education is generally higher in democracies, under IMF programs autocratic regimes tend to *increase* spending in these areas, whereas democracies cut social spending to meet targets.

What conclusions about the prospects for democracy follow from the evidence that established democratic regimes put the burden of adjustment on the poor to a greater degree than autocrats? If spending cuts lead to greater economic inequality, and inequality is harmful to democracy (Houle 2009), then we would expect to see measures of democratic quality among *established democracies* that seek IMF funding to decline. But what does the counter-intuitive finding that non-democracies under IMF programs allocate *more* resources to social programs imply for autocratic regimes?

One plausible possibility is that, absent the representative institutions and fair elections that are the hallmarks of democratic rule, non-democratic governments lack legitimacy and, consequently, rest on wobbly foundations. Going to the IMF involves paying a “sovereignty cost” (Vreeland 2003), since it implies the surrender of economic

management to an external authority. In order to compensate for this cost, perhaps authoritarian leaders under the IMF become more like populists: they seek to expand their base of support through selective spending increases that target wider swathes of the population. The problem for autocrats is that the pie is shrinking rather than growing, and they are simply shifting the sizes of the slices. If social spending is increased in the midst of an austerity program, it means that deep spending cuts have to be applied to other areas of the national budget. Interestingly, Nooruddin and Simmons report that spending on the military goes up in democracies and down in autocracies under IMF programs (2006: 1022-24). Over time, this pattern of spreading the pain of fiscal adjustment can undermine authoritarian governments' ability to retain power, since it weakens the coercive apparatus that is responsible for controlling pro-democracy forces.

We believe the implications of our argument are not necessarily inconsistent with the findings of Abouharb and David Cingranelli (2009) and others dealing with the effects of IMF programs on repression. Embattled borrowers may indeed turn to higher levels of repression, but the weakening of the security apparatus suggests repressive practices should become less effective in thwarting challenges to the regime. The mechanism posited here indicates that what makes sense in the short-term for autocrats can actually reduce their ability to stay in power in the medium term. In this model, authoritarians that spend a significant proportion of time under IMF programs face a higher risk of losing power in a democratic reversal because the ways in which these leaders prefer to manage the costs of IMF-imposed austerity saps the coercive power of the state over time.

To summarize, this section has presented two plausible, competing sets of mechanisms through which IMF programs affect democracy in low- and middle-income countries. Nooruddin and Simmons' (2006) work suggests that the repression model is too simplistic, since regimes can choose how to distribute the costs of adjustment. We draw on their work to propose a different avenue through which IMF lending behavior impacts the level of democracy: autocrats living under IMF programs respond to dual pressures to retain power and to meet fiscal targets by shifting resources away from the security forces and toward social services that reach a broader swathe of the population. Over time, this decision erodes the ability of the state to control the pro-democratic opposition forces, which hastens the regime's demise. Hence we should observe a positive relationship between IMF lending arrangements and the level of democracy in developing countries.

Addressing the Selection Problem

The theoretical overview in the previous section is necessarily truncated: we want to make it clear that while the “repression effect” underpins the conventional wisdom about the IMF-democracy relationship, there is at least one alternative pathway through which the IMF's lending programs might improve the prospects for democracy in the pool of countries that make use of its conditional lending facilities.⁶ We turn in the

⁶ There are many avenues through which IMF programs might influence democracy. For example, the IMF may have incentives to sweeten the typically harsh terms it imposes when dealing with democratizing regimes. Randy Stone's studies of IMF lending suggest that powerful countries – namely, the United States – frequently intervene in the Fund's operations to skew the design of programs in ways that favor their allies (Stone 2002, 2004, 2008, 2011). If democracy promotion is part of US foreign policy, then new democracies may be able to gain preferential treatment by the Fund – which, for economically vulnerable regimes, may serve as a bulwark against autocratic backsliding. Even if we view the IMF as relatively unconstrained by powerful member states, the institution may still deliver preferential treatment to democracies. The “democratic advantage” literature suggests that representative legislative institutions and regular elections make commitments by governments more credible (Schultz and Weingast 2003).

remaining sections to tackling the main purpose of the paper: developing more credible and reliable estimates of the impact of the IMF on democracy.

This is no easy task. In each year a set of countries are observed under IMF agreements. If we could access a parallel universe in which those same countries were *not* under IMF agreements, then it would be simple to establish the effect of the treatment: we would just compare democracy scores of those countries in the two universes. Our task is made more difficult by the fact that we have to compare country-year units under and not under IMF agreements in this universe. There may be confounding factors that distinguish the two populations and which make it appear that the IMF intervention is the cause of the observed differences in outcomes (in our case, democracy scores), when, in reality, any observed difference was driven by the pre-existing conditions of those countries that turn to the IMF in the first place.

Attention to possible confounders and how IMF borrowers may differ in significant ways from other developing countries points to the need to think seriously about two sets of counterfactuals for evaluating the IMF-democracy link. First, what would have happened in those borrowing countries if they had not received loans and been subjected to IMF conditionality? Second, what would have happened to those countries that did not fall under the purview of the IMF if they had instead become involved in IMF programs? Answers to these questions are crucial for testing the democracy-building potential, or lack thereof, of the IMF.

The most common way to handle the non-random assignment problem in studies of the IMF is to use some variant of multi-stage statistical models, such as a Heckman

selection model.⁷ Gilligan and Sergenti (2008: 90) describe how selection models work: “the researcher creates a model of the treatment-assignment (selection) process, uses that model to generate predictions of counterfactuals and then compares the factual cases to those predicted counterfactuals.” In the case of the IMF, the researcher specifies a model accurately predicting participation in IMF programs and then identifies the impact on democracy by comparing the treated and control cases after controlling for selection into IMF programs.

Selection models have the virtue of taking the counterfactual problem seriously, but the statistical fix can produce its own problems for a number of reasons. Heckman selection models often depend on strong distributional assumptions that are rarely met in practice and exhibit particular sensitivity to model specification, which leads to greater problems of inference compared to non-parametric methods (Simmons and Hopkins 2005). Furthermore selection models can generate unreliable findings if the distributions of the observed values of relevant covariates differ dramatically between the treatment and control groups (King and Zeng 2007). Take the example of the level of foreign reserves. We know that countries that have exhausted their store of reserves are more likely to end up going to the IMF for a loan. Consequently, the average level of reserves among the countries that went to the IMF is likely to be quite low and the distribution around the average may be very tight; on the other hand, countries that did not go to the IMF may have significantly higher average levels of foreign exchange reserves. It may be the case that the two distributions are so “disjoint” that they do not even overlap – there may be no examples of high-reserve countries that went to the IMF. If this is the case,

⁷ For example, Abouharb and Cingranelli (2009) use a three-stage least squares estimator to test the effect of the time spent under IMF programs on human rights; Brown (2009) uses a system GMM estimator; Hartzell, Hoddie, and Bauer (2010) use a bivariate probit model.

then predictions about what would have happened to the level of democracy if the IMF had concluded a loan agreement with a country possessing copious foreign exchange reserves “are extrapolations from the data. With no data on which to base such inferences causal claims are based on modeling assumptions rather than the data” (Gilligan and Sergenti 2008: 95; see also Lyall 2010: 173). As we describe in the next section, test statistics show that there are very significant differences in the distribution of confounding variables across country-year units under and not under IMF programs.

We use matching methods to reduce imbalance in our data and to sharpen our estimates of the impact of IMF lending programs on the level of democracy. Matching is a procedure that creates balanced datasets in which each treated unit is paired with an observationally similar control unit. Because the counterfactual comparisons are based entirely on the observed values of the confounding variables, estimates are not sensitive to “functional form assumptions about how to treat observations that lie outside the portion of the variable’s empirical distribution that is shared by the treated and control groups” (Lyall 2010: 173). Following Ho et al. (2009), we use matching as a pre-processing step; we then adjust for any imbalance that remains after the matching procedure by analyzing the data with a parametric (in our case OLS) model.

Data and Methods

Our sample consists of 110 developing countries observed between 1970 and 2000.⁸ We rely on two widely-used continuous measures of democracy: the Polity2 score and the Freedom House score. The Polity2 score ranges from -10 (least democratic) to +10 (most democratic). During “interregnum” and “transition” periods in which it was

⁸ Not all countries in the sample are observed for the full thirty year period; some observations are excluded due to missing data and several countries did not become independent until late in the observation window.

difficult for coders to measure the level of democracy in a country, the original Polity2 variable records a zero. Plumper and Neumayer (2010) show that this coding rule can produce misleading inferences; consequently, in the analysis below we use an amended version of the Polity2 variable that linearly interpolates values during the troublesome “interregnum” and “transition” periods. The Polity score combines information on the competitiveness of political participation, the extent of constraints on the power of the regime’s leader, and the openness and competitiveness of the process by which leaders are selected.

The Freedom House score is a composite of two indexes, one that measures respect for civil liberties and the other that measures political rights. We transform the composite Freedom House score so that it runs from 0 (least democratic) to 12 (most democratic). The Freedom House score is available from 1972 onward. The Polity and Freedom House scores are highly correlated (0.85).

Our key explanatory variable is the presence of an active IMF lending program in country i in year t . The presence of an IMF program is regarded as a dichotomous treatment variable in the first set of results.⁹ For the second set of statistical results, we create a variable that records the cumulative number of years that a country has spent under the auspices of the IMF from the beginning of the observation period up to year t .

We select a set of variables that are likely to be (positively or negatively) correlated with the probability that a country is under an IMF program *and* influences the

⁹ Note that, following Przeworski and Vreeland (2000) we do not differentiate between types of IMF programs, since the fundamental objectives of programs are broadly similar. See Limpach and Michaelowa (2010) for a different view on the value of disaggregating types of IMF loans. We believe concentrating on the effects of an overall IMF “treatment” is a useful first step given the contrasting empirical findings in the existing literature studying the political effects of IMF programs. We thus follow a similar tact to Gilligan and Sergenti (2008) in their decision to focus on a general UN peacekeeping treatment rather than disaggregating by mandate type or other possible dimensions.

level of democracy. One possible confounding factor is the nature of the regime. While political scientists have spilled much ink distinguishing between varieties of democratic regimes – presidential or parliamentary, for example – far less attention has been paid to differences between types of dictatorships (Geddes 1999). Not all dictatorships are alike, which has consequences for both foreign and domestic policies.¹⁰

Cheibub, Gandhi, and Vreeland (2009) distinguish between three types of autocratic rule: monarchic, military, and civilian. If we think of regime type as a continuum spanning the most repressive dictatorship to the perfect democracy, monarchic and military autocracies are generally closer to the most repressive pole than civilian dictatorships.¹¹ In the second section of the paper we sketched a mechanism which suggests that IMF programs erode the repressive capacity of autocrats. Given that autocratic regimes headed by monarchs or military leaders tend to be highly repressive, these regimes have the most to lose from going to the IMF. Our inferences about the effect of the IMF on democracy would be biased if the bulk of the autocratic regimes that sign IMF programs are less repressive, civilian-headed governments. We draw two dummy variables (*military autocracy* and *monarchic autocracy*) from the Cheibub, Gandhi, and Vreeland (2009) six-fold classification of regime types.

We also include a variable that records the sum of previous transitions to autocracy from 1946 to year t (Cheibub, Gandhi, and Vreeland 2009). We add the *previous transitions* variable because countries with a track record of unsettled, volatile

¹⁰ Notable examples include Peceny et al. 2002; Weeks 2008; Fjelde 2010.

¹¹ Indeed, monarchic and military autocracies are more autocratic than civilian-led dictatorships. The average Polity2 score for civilian autocracies in our sample is -2.44, which is significantly higher than average for military regimes (-5.859, difference of means is significant at $p = 0.0000$, t [14.28, 1037.37d.f.]) and the average for monarchic dictatorships (-8.219, $p = 0.0000$, t [18.47, 448.55d.f.]). We find very similar differences in the transformed Freedom House scores for military, monarchic, and civilian autocracies.

political systems may be forced to seek out a disproportionate number of IMF programs and may also have lower democracy scores on average.

Oil-rich countries are prone to boom and bust cycles, but they are less likely to obtain IMF loans than countries with little exportable oil.¹² Many scholars argue that reliance on oil is inimical to democracy (e.g., Ross 2001). Our dichotomous measure of oil wealth comes from Fearon and Laitin (2003) and takes a value of one if more than one-third of a country's export revenues come from the sale of fuels abroad.

We use four variables to account for potentially confounding economic factors. The level of reserves is an indicator of a country's economic health. It is well known that falling reserves increase the likelihood that a country will need to borrow from the IMF; in addition, the health of the economy is likely to have an impact on the level of democracy, particularly in fragile, "unconsolidated," democratizing regimes. We measure the ratio of reserves to gross national income (GNI); the data are constructed from the World Bank's Global Development Finance database. A country's average income per person and the size and direction of the annual change in per capita wealth are expected to influence both the probability that a country is under an IMF program (the relatively rich and fast-growing do not have much need to draw on the IMF's resources) and the prospects for democracy (the relatively poor and slow-growing countries are less likely to see gains and more likely to experience backsliding). The measures of GDP per capita and GDP growth come from the Penn World Tables version 6.3.

Countries that seek IMF funding are in many (if not most) cases experiencing severe economic distress. We have to account for the crisis conditions that brought the

¹² For instance, the least frequent users of IMF resources are countries from the Middle East and North Africa.

country to the IMF in the first place, since crises have been linked the breakdown of both democratic (Gasiowski 1995) and autocratic regimes (Pepinsky 2009). Consequently we use a measure of exchange market pressure (*currency crash*) which, following Frankel and Rose's (1996) widely-used definition, takes a value of one for years in which a country experiences a nominal devaluation in its exchange rate of at least thirty percent that is also at least a ten percent hike in the rate of depreciation compared to the previous year.

Other potential confounding factors that we take into account in the analysis include the size of a country's population and the level of political violence. Population is a common covariate included in quantitative studies of democratization. Bigger developing countries might be more difficult to govern, and hence have lower democracy scores. Larger countries may also be able to mobilize more internal resources and thus have a lower need for IMF funds. The population size variable is drawn from the Penn World Tables volume 6.3.

Countries that are wracked by severe internal violence or engaged in intense cross-border conflicts are less likely to be able to muster the resources to formulate a credible reform program in consultation with the IMF; in the worst episodes, the state may wither to the point that key economic policy positions in the finance ministry and/or central bank are vacant. The IMF cannot send a mission to a country that does not have the basic infrastructure to support loan negotiations. We are unlikely to see many episodes of IMF loans going to failed or failing states, and democracy is similarly unlikely to flourish in these environments. We include a new index of political violence

which records the intensity of annual episodes of intra- and interstate conflict (Marshall 2010). The political violence index ranges from 0 to 13.

Finally, we account for neighborhood effects by including regional variables. Banking crises often spill across borders into neighboring countries; we speculate that the presence of an IMF program in country A raises the probability that country A's neighbors will also end up with IMF programs, either as a precautionary measure to reassure market actors or as an attempt to restore stability once the crisis has spread. In addition, there is convincing evidence of regional dynamics in the spread of democracy (Bunce and Wolchik 2009; Brinks and Coppedge 2006). We place countries into one of six regional classifications: Middle East and North Africa, Latin America and the Caribbean, East Asia and Pacifica, Post-Communist, sub-Saharan Africa, and South Asia.

Results, part I

We used Jasjeet Sekhon's genetic matching routine to generate balanced subsamples for both of our outcome variables (Plumper and Neumayer's corrected Polity2 score and the transformed Freedom House score).¹³ Each treated case is paired with a control case via one-to-one nearest neighbor matching with replacement. Each case was matched on all of the confounding covariates described in the previous section. Table 1 reports descriptive statistics for each of the covariates before and after matching when the Polity score is the outcome variable; table 2 gives the same balance statistics for the Freedom House sample.¹⁴ We report several common measures of balance, including the standard mean differences between treatment and control cases, test statistics for t-

¹³ See the "Matching" package for R, available at <http://sekhon.berkeley.edu/matching/>.

¹⁴ The sizes of the samples diverge slightly because of differences in missing values across the two measures for democracy. All post-matching analysis was performed using Stata 11.

tests, as well as p-values for the Kolmogorov-Smirnov test, which assesses the similarity of distributions of continuous variables across treatment and control populations.

TABLE 1 GOES HERE

TABLE 2 GOES HERE

It is clear from the measures included in the tables that there are dramatic differences between the treatment and control groups. In the unmatched data we have assembled, treated cases are less likely to be military or monarchical autocracies, are less likely to depend on revenues from oil exports, tend to have a history of reversions to autocracy, are poorer and less economically healthy (based on mean values of the level of reserves and economic growth), are more likely to experience currency crises, and have lower levels of political violence than control cases. By almost every measure the two groups significantly differ in ways that are likely consequential for the level of democracy. While the matches are far from perfect, the matching procedure does an impressive job reducing imbalances across the two samples.

We start with the analysis of the Polity2 measure of democracy. A simple difference-of-means test on the matched data indicates that the 1,252 cases under IMF programs are slightly more democratic (Polity2 = -0.42) than 1,252 paired control cases (Polity2 = -1.59).¹⁵

TABLE 3 GOES HERE

We can reduce imbalance further by running a parametric model with the confounding variables included. Table 3 reports OLS regression estimates of the covariates on the Polity2 score for both the unmatched and matched datasets. We find

¹⁵ $p = 0.0000$, $t (-4.396, 2501.9 \text{d.f.})$

that the estimate of the impact of the IMF on democracy in the unmatched sample is positive but not statistically significant. This suggests that failing to correct for selection into IMF programs systematically underestimates the positive effect of lending arrangements on the average level of democracy in developing countries. The estimate from the matched sample reveals that the difference between countries under and not under IMF programs is about one point, which represents a modest though statistically significant effect.

TABLE 4 GOES HERE

We get very similar results when the Freedom House score is the dependent variable. The average Freedom House score for the 1,208 treated cases is 5.31 compared to 4.78 for the 1,208 control cases in the subsample.¹⁶ Comparing the results from the regressions on the unmatched and matched datasets, we see that failing to control for selection would lead analysts to conclude that IMF programs have no significant effect on democracy, when, in fact, countries that obtain conditional lending programs experience small but noticeable improvements in their Freedom House scores.

Results, part II

In the previous section we presented findings which suggest that the conventional wisdom is mistaken. Far from being bad for domestic politics, the IMF actually promotes democracy through its conditional lending facilities. We used matching to minimize differences across the treatment and control cases, and found that the portion of the sample under IMF programs consistently scored higher on two continuous measures of democracy when compared to very similar cases that were not under an IMF program.

¹⁶ $p = 0.0000$, $t (-4.312, 2413.3 \text{d.f.})$

If IMF programs are on average good for democracy, does repeated exposure to the treatment produce an additive effect? Some countries are habitual IMF borrowers. Do those recidivist countries have higher democracy scores after controlling for confounding factors? We tackle that question in this section of the paper. We construct an indicator of the intensity of IMF involvement by counting the cumulative number of years spent under active conditional lending programs between 1970 and 2000. Matching is much more difficult when the treatment variable is continuous, so the analysis in this section is conducted using the original, unmatched Polity and Freedom House datasets. Countries vary widely in terms of not only the presence of IMF programs, but also the extensiveness of time spent relying on conditional lending. Across all observations in the unmatched sample the mean value for time spent under IMF auspices is a little over six years, a far from unsubstantial duration, with countries covering the gamut from no borrowing episodes to almost constant borrowing from the Fund during the entire thirty-year period of our study.

Tables 5 and 6 present the association between the cumulative measure of IMF exposure and democracy scores, conditional on the confounding factors described in the previous sections. To make the test more difficult, we analyze several different specifications. Our models incorporate both country and year fixed effects, as well as a lagged dependent variable.

TABLE 5 GOES HERE

TABLE 6 GOES HERE

The results suggest that the IMF's repeat customers experience significant increases in their levels of democracy. To give a sense of the substantive impact of the cumulative

number of years spent under the watchful eye of the IMF, the result in model 3 – a very tough test that includes both country and year fixed effects to control for unmeasured unit-specific heterogeneity and sample-wide time trends in the level of democracy – implies that a one standard deviation increase in the cumulative IMF programs variable (approximately 6 years) is associated with a 1.44 [0.36, 2.50] point increase in the Polity2 score and a 0.9 point [0.31, 1.50] increase in the Freedom House score. Moving from the minimum to maximum value of the cumulative measure (0 to 29 years) is associated with a 6.6 point [1.7, 11.5] increase in the Polity2 score and a 4.29 point [1.45, 7.13] increase in the Freedom House measure of the level of democracy. While only preliminary, these findings suggest the IMF does not have a one-shot effect on new borrowing countries, but rather that the benefits for democracy tend to accumulate the more countries turn to the Fund's lending facilities.

Conclusion

The general consensus is that across a wide range of economic and social indicators the IMF has in fact made things worse rather than better for most borrowing countries (Vreeland 2003). Notwithstanding the deleterious impact of the IMF across numerous areas, when looking at the specific question of promoting democracy our argument points to some salutary political effects emanating from Fund activities. Our research design confirms widespread suspicions that borrowing countries differ in significant respects from non-borrowing countries, which affects both the likelihood of receiving a loan from the IMF, as well as future prospects for democracy. We believe the empirical difficulties inherent in taking into account these baseline differences between countries in and out of IMF programs helps to account in part for the inconsistent and

sometimes somber findings regarding the relationship between the IMF and democracy. Once these baseline country characteristics are properly taken into account, we find overwhelming positive effects for IMF programs across multiple measures of democracy. Furthermore, the political benefits are not simply a function of the mere presence of IMF lending, but tend to accrue the longer a country finds itself borrowing from the institution.

Our findings nevertheless remain preliminary and raise a number of questions and implications for future research. By taking a broad first cut, we focus on identifying average effects of the treatment on the treated (in other words the democratic consequences for those countries entering into IMF programs) to the detriment of obscuring potentially important regional and temporal dynamics. As several studies have demonstrated, IMF practice and relations with debtor countries have varied dramatically across different regions of the world, which cautions against inferring any uniform trends in the impact of the institution's activities (Pop-Eleches 2009). Of particular note, during the period of our study Latin America and Eastern Europe experienced some of the most notable improvements in democracy, but also the most intense involvement in IMF programs. Disentangling the impact of the IMF relative to concurrent regional processes thus remains an important task for assessing the overall consequences, political or otherwise, arising from conditional lending.

Furthermore, in our design we chose to concentrate on the immediate short-term effects on IMF activities to the neglect of potential longer-term dynamics. We incorporated temporal elements to a certain extent by taking into account the amount of time countries spent under IMF programs, but we readily admit that our results still

mostly focus on the immediate consequences. If our conjecture regarding the domestic distributional implications of IMF conditionality is correct, however, then we might also expect the political consequences of IMF programs to likely vary over time. Future work could thus benefit from examining how the effects of the IMF on democracy may change, and perhaps even create legacies lasting well after the end of the institution's formal involvement in borrowing countries.

Questions concerning the implications of the redistribution of scarce government resources return us to the question of what exactly accounts for the democratizing effects of IMF programs. While we identified a robust positive relationship between the IMF and democracy promotion in borrowing countries, our design does not allow us to directly test the many plausible mechanisms linking IMF activity to domestic politics in the developing world. This is especially important since there may be good reason to be skeptical of whether it is the unique behavior of the IMF that leads to the observing of improved democracy, or rather if it is a general process of economic liberalization that is driving the finding.¹⁷ Tracing in a more in depth manner the precise pathways through which IMF programs eventually generate democratic changes on the ground would be helpful for assessing competing claims. Nevertheless, to the degree to which the IMF continues to embody one of the primary catalysts for widespread economic reforms in developing countries, we believe our results suggest a corresponding autonomous political impact as well.

¹⁷ For instance, Hartzell et al. 2010 largely claim the IMF represents a proxy for economic liberalization in borrowing countries.

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Appendix: Figures and Tables

Figure 1: Average Democracy Scores and Proportion of Countries under Active IMF programs, 1970-2000

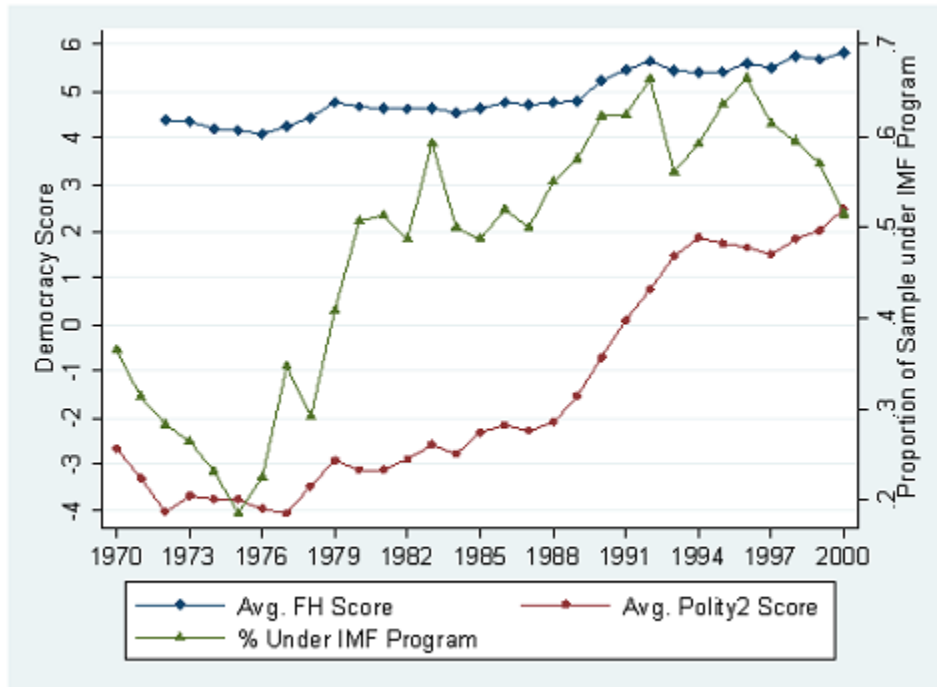


Table 1: Balance Statistics, Polity2 Sample

<i>Covariates</i>		Mean Treated	Mean Control	Std. mean difference	T-Test (p- value)	K-S Test
Military Autocracy	Before matching	0.307	0.332	-5.256	0.190	–
	After matching	0.307	0.315	-1.730	0.307	–
Monarchic Autocracy	Before matching	0.028	0.090	-37.486	0.000	–
	After matching	0.028	0.036	-4.843	0.032	–
# Previous Transitions	Before matching	0.590	0.425	16.627	0.000	0.000
	After matching	0.590	0.574	1.604	0.037	0.82
Oil Producer	Before matching	0.108	0.187	-25.63	0.000	–
	After matching	0.108	0.108	0	1	–
Reserves/GNI	Before matching	0.087	0.111	-31.232	0.000	0.000
	After matching	0.087	0.082	5.795	0.04	0.04
Population	Before matching	27393	51984	-29.747	0.000	0.004
	After matching	27393	25623	2.141	0.07	0.35
GDP Per Capita	Before matching	2380.5	2507.6	-6.2704	0.168	0.000
	After matching	2380.5	2338.5	2.064	0.055	0.34
GDP growth	Before matching	0.737	1.796	-16.365	0.000	0.000
	After matching	0.737	0.722	0.230	0.847	0.71
Currency Crash	Before matching	0.135	0.080	16.194	0.000	–
	After matching	0.135	0.135	0	1	–
Political Violence	Before matching	0.827	1.124	-17.192	0.000	0.000
	After matching	0.827	0.818	0.555	0.631	0.99
Middle East/N. Africa	Before matching	0.067	0.144	-30.584	0.000	–
	After matching	0.067	0.067	0	1	–
Latin Am & Caribbean	Before matching	0.286	0.211	16.629	0.000	–
	After matching	0.286	0.285	0.177	0.564	–
East Asia & Pacifica	Before matching	0.062	0.119	-23.308	0.000	–
	After matching	0.062	0.075	-5.285	0.029	–
Post-Communist	Before matching	0.094	0.023	24.233	0.000	–
	After matching	0.094	0.094	0	1	–
Sub-Saharan Africa	Before matching	0.428	0.432	-0.723	0.856	–
	After matching	0.428	0.416	2.420	0.047	–

South Asia	Before matching	0.062	0.072	-3.937	0.338	–
	After matching	0.062	0.062	0	1	–

Table 2: Balance Statistics, Freedom House Sample

<i>Covariates</i>		Mean Treated	Mean Control	Std. mean difference	T-Test (p- value)	K-S Test
Military Autocracy	Before matching	0.302	0.335	-7.091	0.087	–
	After matching	0.302	0.315	-2.883	0.052	–
Monarchic Autocracy	Before matching	0.027	0.090	-38.668	0.000	–
	After matching	0.027	0.026	0.508	0.317	–
# Previous Transitions	Before matching	0.589	0.434	15.430	0.000	0.000
	After matching	0.589	0.569	1.987	0.011	0.78
Oil Producer	Before matching	0.110	0.190	-25.502	0.000	–
	After matching	0.110	0.110	0	1	–
Reserves/GNI	Before matching	0.088	0.113	-32.356	0.000	0.000
	After matching	0.088	0.082	7.562	0.011	0.017
Population	Before matching	27724	53889	-31.172	0.000	0.009
	After matching	27724	26204	1.810	0.012	0.252
GDP Per Capita	Before matching	2437.1	2629.5	-9.33	0.045	0.000
	After matching	2437.1	2384	2.578	0.010	0.232
GDP growth	Before matching	0.630	1.689	-16.361	0.000	0.000
	After matching	0.630	0.529	1.552	0.316	0.372
Currency Crash	Before matching	0.137	0.084	15.358	0.000	–
	After matching	0.137	0.134	0.961	0.045	–
Political Violence	Before matching	0.839	1.159	-18.442	0.000	0.005
	After matching	0.839	0.816	1.286	0.196	0.99
Middle East/N. Africa	Before matching	0.065	0.144	-31.754	0.000	–
	After matching	0.065	0.065	0	1	–
Latin Am & Caribbean	Before matching	0.276	0.214	13.743	0.000	–
	After matching	0.276	0.276	0	1	–
East Asia & Pacifica	Before matching	0.062	0.121	-24.544	0.000	–
	After matching	0.062	0.062	0	1	–
Post-Communist	Before matching	0.098	0.025	24.436	0.000	–
	After matching	0.098	0.098	0	1	–
Sub-Saharan Africa	Before matching	0.436	0.423	2.585	0.526	–
	After matching	0.436	0.436	0	1	–

South Asia	Before matching	0.063	0.072	-3.727	0.378	–
	After matching	0.063	0.063	0	1	–

Table 3: IMF Programs and Democracy (Polity2 Score)

<i>Covariates</i>	(1) Unmatched dataset	(2) Matched dataset
Military Autocracy	-5.827* (0.744)	-5.896* (0.853)
Monarchic Autocracy	-11.581* (1.545)	-9.987* (1.285)
Previous Transitions	0.466 (0.431)	0.357 (0.529)
Oil Producer	-2.613* (1.201)	-2.980 (1.532)
Reserves/GNI	8.283* (2.417)	9.793 (5.257)
Population	-2.2x10 ⁻⁶ (4.3x10 ⁻⁶)	-1.4x10 ⁻⁶ (4.4x10 ⁻⁶)
GDP Per Capita	0.007* (0.002)	0.0006* (0.0002)
GDP growth	-0.002 (0.013)	-0.016 (0.022)
Currency Crash	0.413 (0.365)	0.672 (0.501)
Political Violence Index	0.006 (0.137)	-0.182 (0.167)
Under IMF Program	0.374 (0.368)	0.976* (0.439)
Sub-Saharan Africa	-5.367* (1.371)	-5.539* (1.250)
East Asia & Pacifica	-3.803* (1.767)	-4.314* (1.525)
Post-Communist	-3.980* (1.844)	-4.248* (1.856)
Latin America & Caribbean	-2.524 (1.523)	-2.101 (1.402)
Middle East & North Africa	-4.098* (1.905)	-3.103 (1.711)
Number of observations	2533	2504
R-squared	0.52	0.47

Robust standard errors in parentheses below coefficients. * = p < 0.05

Table 4: IMF Programs and Democracy (Freedom House Score)

<i>Covariates</i>	(1) Unmatched dataset	(2) Matched dataset
Military Autocracy	-2.760* (0.330)	-2.670* (0.333)
Monarchic Autocracy	-3.501* (0.893)	-1.330* (0.630)
Previous Transitions	0.176 (0.184)	0.182 (0.187)
Oil Producer	-1.021* (0.504)	-1.223* (0.528)
Reserves/GNI	3.724* (1.344)	2.396 (2.586)
Population	-1.57x10 ⁻⁶ (2.38x10 ⁻⁶)	8.16x10 ⁻⁷ (2.32x10 ⁻⁶)
GDP Per Capita	0.0002 (0.0001)	0.0003* (0.0001)
GDP growth	0.0007 (0.007)	0.0001 (0.010)
Currency Crash	0.267 (0.177)	0.284 (0.238)
Political Violence Index	-0.131 (0.073)	-0.188* (0.067)
Under IMF Program	0.239 (0.170)	0.470* (0.188)
Sub-Saharan Africa	-2.431* (0.734)	-2.094* (0.623)
East Asia & Pacifica	-1.218 (1.034)	-1.033 (0.833)
Post-Communist	-1.688 (0.930)	-1.975* (0.870)
Latin America & Caribbean	-0.067 (0.852)	0.265 (0.832)
Middle East & North Africa	-1.450 (0.904)	-1.106 (0.715)
Number of observations	2403	2416
R-squared	0.50	0.49

Robust standard errors in parentheses below coefficients. * = p < 0.05

Table 5: Cumulative IMF Programs and Democracy (Polity Score)

<i>Covariates</i>	(1)	(2)	(3)	(4)	(5)
Polity Score _{t-1}				0.878*	0.706*
				(0.017)	(0.036)
Military Autocracy	-5.702*	-6.958*	-6.836*	-0.965*	-2.477*
	(0.755)	(0.769)	(0.788)	(0.236)	(0.494)
Monarchic Autocracy	-11.056*	-6.745*	-6.249*	-1.419*	-2.201*
	(1.497)	(1.303)	(1.123)	(0.295)	(0.345)
# Previous Transitions	0.387	-1.539	-1.691	0.014	-1.001*
	(0.413)	(1.098)	(1.117)	(0.055)	(0.500)
Oil Producer	-2.205	-1.168	-1.059	-0.172	-0.276
	(1.222)	(1.123)	(1.220)	(0.179)	(0.530)
Reserves/GNI	8.487*	2.520	0.827	1.364*	1.324
	(2.353)	(2.244)	(2.108)	(0.413)	(1.072)
Population	-1.9x10 ⁻⁶	-3.9x10 ⁻⁶	-9x10 ⁻⁶ *	-1.1x10 ⁻⁷	-5.6x10 ⁻⁷
	(4.4x10 ⁻⁶)	(3x10 ⁻⁶)	(4.1x10 ⁻⁶)	(6.5x10 ⁻⁷)	(1.5x10 ⁻⁶)
GDP Per Capita	0.0006*	0.0002	6.4x10 ⁻⁶	0.00006*	0.00004
	(0.0002)	(0.0002)	(0.0002)	(0.00002)	(0.00006)
GDP growth	-0.001	-0.006	-0.005	-0.004	-0.004
	(0.013)	(0.008)	(0.008)	(0.005)	(0.006)
Currency Crash	0.356	0.031	0.006	0.083	0.005
	(0.372)	(0.262)	(0.258)	(0.174)	(0.187)
Political Violence Index	-0.026	-0.131	-0.114	-0.015	-0.052
	(0.140)	(0.120)	(0.123)	(0.025)	(0.046)
Cumulative IMF programs	0.133*	0.348*	0.228*	0.036*	0.130*
	(0.045)	(0.052)	(0.085)	(0.009)	(0.024)
Sub-Saharan Africa	-5.161*			-0.365	
	(1.424)			(0.233)	
East Asia & Pacifica	-3.354			-0.236	
	(1.899)			(0.273)	
Post-Communist	-3.194			-0.261	
	(1.912)			(0.308)	
Latin America & Caribbean	-2.327			-0.014	
	(1.621)			(0.264)	
Middle East & North Africa	-3.832*			-0.206	
	(1.910)			(0.270)	
Country fixed effects	N	Y	Y	N	Y
Year fixed effects	N	N	Y	N	N
Number of observations	2533	2533	2533	2419	2418
R-squared	0.53	0.82	0.82	0.91	0.92

Robust standard errors in parentheses below coefficients. * = p < 0.05

Table 6: Cumulative IMF Programs and Democracy (Freedom House Score)

<i>Covariates</i>	(1)	(2)	(3)	(4)	(5)
Freedom House Score _{t-1}				0.874*	0.719*
				(0.017)	(0.029)
Military Autocracy	-2.738*	-2.868*	-2.908*	-0.444*	-0.992*
	(0.332)	(0.385)	(0.393)	(0.104)	(0.199)
Monarchic Autocracy	-3.408*	-1.643*	-1.690*	-0.469*	-0.399*
	(0.889)	(0.428)	(0.423)	(0.144)	(0.116)
# Previous Transitions	0.164	-0.908*	-0.852*	0.027	-0.547*
	(0.188)	(0.359)	(0.368)	(0.027)	(0.154)
Oil Producer	-0.941	-0.580	-0.602	-0.114	-0.240
	(0.506)	(0.449)	(0.461)	(0.078)	(0.145)
Reserves/GNI	3.731*	0.653	0.921	0.550*	0.590
	(1.372)	(1.229)	(1.226)	(0.217)	(0.543)
Population	-1.5x10 ⁻⁶	-6.8x10 ⁻⁶	-5.3x10 ⁻⁶	-1.2x10 ⁻⁷	-1.7x10 ⁻⁶
	(2.4x10 ⁻⁶)	(3.4x10 ⁻⁶)	(3.4x10 ⁻⁶)	(3.9x10 ⁻⁷)	(1.3x10 ⁻⁶)
GDP Per Capita	0.0002	-0.00005	-1.3x10 ⁻⁶	0.00002	-0.00001
	(0.0001)	(0.0001)	(0.0001)	(0.00002)	(0.00003)
GDP growth	0.0001	-0.001	-0.001	0.003	0.003
	(0.007)	(0.004)	(0.003)	(0.003)	(0.003)
Currency Crash	0.271	0.154	0.163	0.070	0.071
	(0.177)	(0.136)	(0.141)	(0.087)	(0.089)
Political Violence Index	-0.141	-0.217*	-0.218*	-0.038*	-0.089*
	(0.071)	(0.056)	(0.055)	(0.015)	(0.021)
Cumulative IMF programs	0.030	0.115*	0.148*	0.010*	0.037*
	(0.025)	(0.027)	(0.049)	(0.005)	(0.010)
Sub-Saharan Africa	-2.390*			-0.197	
	(0.743)			(0.130)	
East Asia & Pacifica	-1.134			-0.085	
	(1.058)			(0.170)	
Post-Communist	-1.468			-0.127	
	(0.969)			(0.179)	
Latin America & Caribbean	-0.034			0.045	
	(0.875)			(0.144)	
Middle East & North Africa	-1.405			-0.084	
	(0.904)			(0.147)	
Country fixed effects	N	Y	Y	N	Y
Year fixed effects	N	N	Y	N	N
Number of observations	2403	2403	2403	2289	2288
R-squared	0.50	0.79	0.79	0.89	0.91

Robust standard errors in parentheses below coefficients. * = p < 0.05