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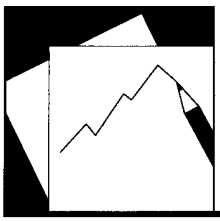

IMF Working Paper

IMF Lending and Banking Crises

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INTERNATIONAL MONETARY FUND

 WP/15/19

IMF Working Paper

Research Department

IMF Lending and Banking Crises[1]

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Authorized for distribution by Andrew Berg and Catherine Pattillo

January 2015

Abstract

This paper looks at the effects of International Monetary Fund (IMF) lending programs on banking crises in a large sample of developing countries, over the period 1970-2010. The endogeneity of the IMF intervention is addressed by adopting an instrumental variable strategy and a propensity score matching estimator. Controlling for the standard determinants of banking crises, our results indicate that countries participating in IMF-supported lending programs are significantly less likely to experience a future banking crisis than non-borrowing countries. We also provide evidence suggesting that compliance with conditionality and loan size matter.

JEL Classification Numbers: G01, F33, F34, O11
Keywords: Banking crises; IMF programs; Political economy; Conditionality

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1 Introduction

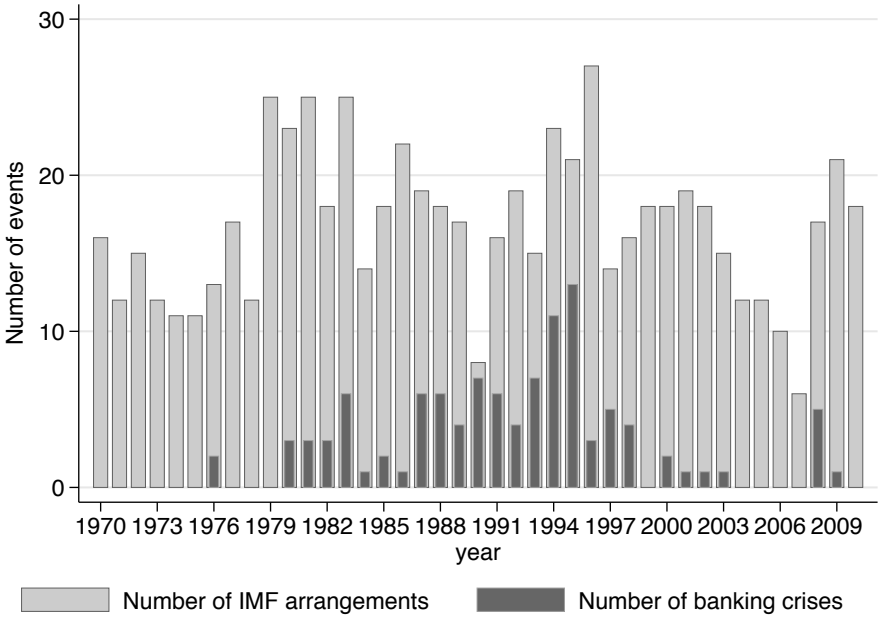
In this paper we look at the effects of International Monetary Fund (IMF or Fund) lending programs on the risk of banking crisis in borrowing countries and at the channels of influence of Fund interventions.

The mission assigned to the IMF by its founders in 1945 was limited to the promotion of exchange rate stability and the adjustment of external imbalances in member countries: to this end the IMF was to act as an intermediary between surplus and deficit countries and as an arbiter of changes in exchange rate par values between domestic currencies and the U.S. dollar. After the demise of the dollar exchange standard in 1973, the Fund had new sources of externalities to address and new public goods to provide (Bordo and James, 2000; Fratianni, 2003). The scope of IMF interventions expanded gradually, up to encompassing the much wider (and less well-defined) mission of preserving economic and financial stability in member countries. The Fund has pursued this objective both through its continuous country surveillance activity, involving policy recommendations and reform promotion, and by means of specific stabilization programs. The latter involved the disbursement of loans conditional upon the fulfillment of strict adjustment policies and economic reforms. According to the Fund's critics, however, the IMF lending policy is to be blamed for having imposed on recipient countries the inappropriate, ineffective and ideological economic recipes of the "Washington Consensus". In particular, the liberalization, privatization and austerity programs urged by the IMF in Mexico, South-East Asian countries, Russia and Brazil during the dramatic crises of the 1990s is blamed to have triggered massive capital outflows and severe banking crises (Radelet and Sachs, 1998; Sachs, 2002; Stiglitz, 2002).

In response to these developments, and given the simultaneity of currency and banking crises (Kaminsky and Reinhart, 1999), the Fund oriented its lending activity to the preservation of financial sector stability and the prevention of liquidity crises. These targets re-emerged dramatically onto the Fund's agenda during the global financial crisis of 2007-09 and the successive sovereign debt crises in the Eurozone. Many observers and scholars are now inclined to accept the idea that the IMF should be endowed with resources and instruments to act credibly as an international lender of last resort (Fischer, 1999; Rogoff, 1999). In this perspective, during the last fifteen years new lending programs have been introduced by the Fund to grant precautionary credit lines to prequalified countries. Upfront access to IMF resources is meant to mitigate the potential economic vulnerability of countries with appropriate policies and institutions to sudden liquidity crises and self-fulfilling bank runs, avoiding the accumulation of costly international reserves (Rodrik, 2006; Joyce and Razo-Garcia, 2011).

Despite the intense debate about the responsibility of the Fund in the banking crises of the 1990s

Figure 1: Banking crises and IMF lending arrangements



Notes: Calculations based on Laeven and Valencia (2013) data set and on data on IMF lending arrangements. The sample consists of 108 banking crises and 686 IMF lending arrangements observed in the 113 developing countries over the period 1970-2010 used in the empirical analysis (see Table A1 in the Appendix). We exclude country-year observations in which there are no data on banking crisis.

and the need to reshape its role and lending toolkit, we are not aware of any empirical study that analyzes the relationship between IMF-supported programs and the probability of systemic banking crises over the medium run.

At first glance, the data seems to indicate a positive correlation between banking sector instability and the Fund involvement in member countries, the very claim made by Fund critics. In Figure 1, we report the number of banking crises (the grey column) and IMF arrangements (the bold column) in developing countries during the period 1970-2010 considered in the empirical analysis. This figure displays a concentration of bank distress episodes in the early 1980s, in the 1990s and in the two years following the US subprime crisis, as well as a peak of IMF lending programs in the crisis years. A banking crisis occurs with a frequency of 3.1% of country-year observations in which an IMF program was in operation in the previous five years and 1.4% of country-years in which there was no IMF loans. However, since the Fund intervenes to prevent and sort out currency and financial crises its presence in a country as causal factor in triggering a banking crisis remains an open question, which we will investigate in the rest of the paper. In addition, conditional on a past financial crisis, the risk of a new crisis may be lower anyhow, if crises arrive in some unrelated stochastic manner or if they trigger conservative macroeconomic policies and financial reforms, strengthening the stability of the banking system. In this case, the causal factor is not IMF lending but rather the occurrence of a past crisis.

The Fund's involvement in a country may have an impact on the probability of a banking crisis through a number of contrasting channels. First, IMF support is associated with the mobilization of financial resources, that prevent banking crises from materializing. The increase in financial flows may be the result of IMF direct interventions, which are usually meant to provide credit to bolster liquidity for the economy concerned, and of a catalytic effect on other official and private lenders (Bird and Rowlands, 2002; Cottarelli and Giannini, 2006). However, IMF loans might also drive creditors to rush to recoup their credits from a limited pool of fresh liquidity, thus triggering a banking panic (Zettelmeyer, 2000).

Second, Fund intervention may impact on the stability of the domestic banking industry through conditionalities that influence domestic economic and financial policies and reforms.

Finally, IMF lending could raise the risk of a banking crisis by inducing moral hazard on the part of both the borrowing country and its private creditors and through bad signaling.

In sum, whether and how IMF involvement affects the probability of a systemic banking crisis is an empirical issue which might have different answers, depending on different sources of heterogeneity regarding loan size, conditionalities, lending arrangements and country's institutional environments. In this context, our contribution extends the recent empirical literature on the IMF's role in mitigating financial instability. This literature has investigated the effect of IMF-supported programs on sudden stops of financial capital flows (Eichengreen *et al.*, 2008), on currency crises (Dreher and Walter, 2010), on sovereign debt crises (Jorra, 2012), and on the spread of the 2007-08 global financial crisis (Presbitero and Zazzaro, 2012).

Taking advantage of a large dataset covering 113 low- and middle-income countries over the period 1970-2010, we estimate the determinants of banking crises across countries, focusing on the effect of the presence of IMF programs in previous years. The endogeneity of the Fund intervention is addressed by adopting an instrumental variable strategy, in which the degree of political similarity between IMF borrowers and the G-7 and the presence of past elections are taken as main instruments for the likelihood of a country signing an IMF lending arrangement in the five years before the crisis. Our main finding is that countries participating in IMF-supported programs are significantly less likely to suffer a future banking crisis than other comparable non-borrowing countries. This result is confirmed using a propensity score matching estimator and it also holds when considering exclusively either countries that suffered at least one systemic crisis over the sample period, or IMF programs that were not associated with financial crises – to exclude the possibility that past financial crises are negatively correlated with the probability of future banking crises, regardless of any IMF intervention.

We document that the negative correlation between IMF interventions and the likelihood of a

future banking crisis is statistically significant only above a given loan threshold. This result is consistent with the positive effect that IMF-provided liquidity has on banking sector stability, rather than with a *seal of approval* channel. We also find that the effect of the Fund in reducing the incidence of banking crises is significantly stronger when recipient countries are compliant with IMF conditionalities. This result is consistent with the positive effect of Fund-imposed reforms on the domestic banking sector, and is further supported by the evidence of similar positive effect from World Bank structural adjustment loans, having equivalent conditionalities as the IMF program, albeit smaller in loan size.

The remainder of the paper proceeds as follows. Section 2 frames the research question in the theoretical and empirical literature about IMF support and financial stability. Section 3 describes the main variables and illustrates some stylized facts, while Section 4 describes the empirical model to estimate and the identification strategy. Sections 5 and 6 discuss the baseline results and present a number of extensions and robustness exercises. Section 7 summarizes our main findings and concludes.

2 Related literature

The bulk of the theoretical and empirical literature on the role of the IMF in preventing financial crises has referred to the effects of IMF-supported programs. In what follows, we briefly review this literature with special attention to the channels through which IMF lending programs may affect the probability of banking crises. At the risk of some over-simplification such channels can be grouped in three broad categories. First, the credit availability channel fueled by both direct IMF lending and catalytic effects. Second, the reforms channel nourished by macroeconomic, structural and financial sector initiatives made possible by both the effect of direct IMF conditionality and the so-called scapegoat effect. Third, bad signals and moral hazard effects, which may increase risk taking and poor policies.

2.1 IMF support and banking crises: the channels of influence

2.1.1 Credit availability

A first strand of studies points out the positive effects of credit availability and countercyclical lending due to IMF intervention. First, once the program has been approved and credit disbursed, the amount of resources available to the country to be used to build up a certain level of emergency liquidity provision increases, thus reducing the probability of crises caused purely by illiquidity problems (Haldane, 1999; Miller and Zhang, 2000). Second, exploiting its position and reputation as

most effective international lender of last resort (Rogoff, 1999; Fischer, 1999), a catalytic effect is indirectly exerted both towards other official lenders and the private sector. The existence of an IMF program acts as a *seal of approval*, reassuring investors and depositors and reducing the probability of withdrawing funds from the domestic banking sector. Similarly, IMF lending and partial bailouts may induce lenders to roll over their loans if the macroeconomic fundamentals of the member country are not too weak (Corsetti *et al.*, 2006), and if the IMF intervention does not crowd out the adjustment effort of the member country government (Morris and Shin, 2006).

Several studies have explored empirically whether and how IMF-supported programs affect private capital flows, reaching mixed results (see Bird (2007) and Steinwand and Stone (2008) for a review). Bird and Rowlands (2008, 2009) cast doubt on the catalytic role of IMF loans by documenting that net private capital inflows are negatively correlated with the presence of an IMF lending agreement, even if the average effect is heterogeneous across different capital flows and the initial conditions of recipient countries. In a similar vein, van der Veer and de Jong (2013) show that if one limits the analysis to countries that have not restructured their debt in the same year as their signing of an IMF program, the Fund's catalysist effect on private capital flows is significantly positive.[1]

2.1.2 Conditionalities and financial reforms

Since the 1990s, IMF conditionalities have increasingly concerned to policy actions directly related to financial reforms and capital account liberalization (Joyce and Noy, 2008). Financial reforms sponsored by the Fund have comprised measures aiming to increase financial liberalization and improve the regulatory and supervisory framework. Whereas introducing financial liberalization without adequate banking sector surveillance might also contribute to banking sector fragility because it might increase opportunities for excessive risk-taking and fraudulent behavior, creating a more effective control system should instead make the banking sector more resilient, hence reducing the likelihood of systemic banking crises. Moreover, once a country has adopted a significant structural financial reform, the introduction of further reforming initiatives should become easier, through a sort of learning effect (Abiad and Mody, 2005).

Consequently, provided that conditionalities are correctly identified and properly implemented, the stability of the banking sector should be positively affected as a result of IMF intervention and countries more compliant with conditionalities should be less likely to experience banking crises.[2]

[1] A different strand of literature has investigated whether the existence of an IMF-supported program modifies interest rate spreads, both on commercial bank loans and on international bonds, and countries' debt maturity (Mody and Saravia, 2006; Saravia, 2010). Chapman *et al.* (2012) find that increasing the scope of conditionality attached to IMF programs reduces the yield on government bonds.

[2] In addition to what may be envisaged in the attached conditionality to a specific program, the IMF might facilitate the national authorities' effort to promote special financial reforms which, in the absence of IMF support, could be politically too difficult to implement due to opposition at home. Consequently, governments of member countries, by using the

However, the positive role of conditionalities has been questioned both in theory and practice. According to this critical view, the IMF would not have access to all relevant information needed to design optimal policies in time of crises and, most important, the Fund policy advice would be influenced by the vested interests of its main shareholders. In this case, compliance with IMF conditionalities could trigger the spread of the crisis and aggravate the post-crisis collapse, as happened during the Southeast Asian financial crisis of the 1990s (Radelet and Sachs, 1998; Feldstein, 1998; Stiglitz, 2002).

Indirect and mixed evidence on the importance of reform channel is provided by Demirguc-Kunt and Detragiache (1998) and Kaminsky and Reinhart (1999), who find that financial liberalization is a determinant and a predictor of future banking crises. A more nuanced picture is provided by De Haan and Shehzad (2009), who find that most dimensions of financial reforms that enhance liberalization reduce the probability of systemic banking crises strictly conditional on adequate banking supervision.

2.1.3 Bad signals and moral hazard

Another strand of literature has focused on debtor and creditor moral hazard effects. First, country authorities might view IMF financial support as a substitute for their own adjustment effort (Jeanne and Zettelmeyer, 2001). In this case, IMF emergency loans weaken the incentives of national policy makers regarding their own adjustment effort, leading to laxer economic policies, IMF dependency (Vaubel, 1983; Goldstein, 2001), and in turn potential negative consequences on banking stability.

Similarly, some sort direct and indirect creditor moral hazard may occur. First, to the extent that the IMF rescue package does not provide the member country with unlimited financial resources, the investors might have incentives to liquidate their positions in the country and withdraw deposits from domestic banks (Zettelmeyer, 2000; Jeanne and Wyplosz, 2003). Second, anticipating a possible IMF bail-out in case of a crisis may lead markets to underprice sovereign risk in bond and equity markets, and investors to excessive risk-taking, making the crisis a more likely event (see Dreher (2004b) and Conway (2006) for two excellent surveys of creditor moral hazard related to IMF lending). Finally, borrowing countries might suffer from some sort of stigma effect and turning to the IMF for crisis prevention might be interpreted by markets as signalling more severe troubles than hitherto publicly recognized.

international financial institution as a scapegoat (Vreeland, 1999), may want to delegate responsibility for carrying out domestic unpopular reforms to the politically unaccountable IMF, deflecting towards the latter the possible blame for the resulting social and political costs (Haggard and Kaufman, 1995; Vreeland, 2003).

2.2 IMF support and financial crises

To the best of our knowledge, there are no previous econometric studies analyzing the association between IMF-supported programs and banking crises. However, a limited number of recent papers have dealt with the role of the IMF in pursuing financial stability and preventing financial crises.

Eichengreen *et al.* (2008) document that countries, especially those with strong fundamentals, are less likely to experience sudden stops in international capital flows in the years following participation in an IMF program. The stabilizing role of the IMF emergency liquidity provision holds even after controlling for reverse causality.

Another piece of evidence in favor of the positive role of IMF on the financial stability of member countries is presented by Dreher and Walter (2010) who find that the existence of an IMF-supported program in the previous five-year period reduces the probability of a future currency crisis. The authors analyze 68 developing countries over the period 1975-2002 and show that it is the lending agreement *per se* which drives the result, rather than the amount of the disbursed loan or the degree of compliance with conditionality.

Conversely, Jorra (2012), focusing on 57 developing and emerging economies over the period 1975-2008, shows that IMF-supported programs significantly increase the average probability of subsequent sovereign defaults by 1.4 percentage points. This is a meaningful effect, given a sample frequency of defaults of 4.8 percent. Like in Dreher and Walter (2010), however, this result does not seem to be due to lack of compliance with conditionality, but it reflects the effects of IMF interventions *per se* and suggests that the IMF intervention could trigger debtor moral hazard.

Finally, Presbitero and Zazzaro (2012) investigate whether, during the 2008-10 financial crisis, IMF lending was directed at preventing the risk of contagion, and whether participation in IMF programs was sensitive to the political-economic interests of the IMF's main shareholders. Their findings are mixed: on the one hand, political similarity with G7 countries is positively correlated with the probability of signing a loan agreement; on the other hand, the IMF has channeled more financial resources to those countries where the economic crisis was more severe independent of the existence of balance of payment imbalances.

3 Data and stylized facts

The empirical analysis is based on a panel dataset covering 113 developing countries over the period 1970-2010. The dependent variable is a dummy equal to one for country-year observations in which there was a systemic banking crisis ($BANKING\ CRISIS$). We follow the widely-adopted methodology proposed by Laeven and Valencia (2013) and define a banking crisis episode as sys-

temic when in a given year there are: 1) significant signs of financial distress in the banking system, and 2) major banking policy intervention measures in response to significant losses in the banking system.[3]

In our sample (Table A1 in the Appendix) there are 108 systemic banking crises, the first occurring in 1976 and the last in 2009. The episodes of banking sector instability tend to concentrate in the early 1980s, in the 1990s, and then in 2008 in correspondence to the global financial crisis. The occurrence of banking crises is correlated with past event of financial instability (see Table 3, panel A). The frequency that a country incurs in a banking crisis, conditional on not having experienced banking crises in the previous eight years is 3.3%, while this frequency becomes significantly smaller (1.8%) for countries that experienced at least one banking crisis in the past eight years, suggesting that past banking crises could trigger more conservative financial and macroeconomic policies and reduce future occurrences of the same phenomenon irrespective of any IMF intervention. By contrast, sovereign and currency crises in the past are associated with a higher frequency of banking crises. The latter increases from 2.8% to 4.8% depending on not having or having experienced a debt crisis in the previous eight years, in line with the contagion hypothesis between sovereign and bank default risk (Acharya *et al.*, 2014). Finally, consistent with the twin crises narrative (Kaminsky and Reinhart, 1999), there is a statistically significant positive difference between the likelihood of a banking crisis conditional on the occurrence of a currency crisis (3.7%) in the previous eight-year period and the likelihood of a banking crisis in the absence of a past currency crisis event (2.5%).

During our sample period 686 IMF-supported programs have been agreed for low- and middle income countries (276 and 410 respectively; see Table 3, Panel B). IMF activity generally peaked around the crisis years. However, it is interesting to note that the Fund's financial support remained sustained (but declining) even after the late 1990s, without any significant banking distress episodes until the 2007 financial crisis (Figure 1). This is the period during which the focus of the Fund's activity on the reforms and stability of the financial sector became dominant. Hence, the figure would suggest that the increased attention of IMF structural conditionality on banking sector stability and regulation after the 1997 Asian crises may have brought positive effects in terms of less vulnerability to systemic banking crises (Giustiniani and Kronenberg, 2005).[4] More generally, about 70% of the IMF programs in our sample do not follow a financial (banking, currency or sovereign) crisis that happened in the previous three years. However, the frequency of signing a Fund lending arrange-

[3] See Laeven and Valencia (2013, section I) for more details on the actual definition of banking crisis episodes. Their extensive dataset dates 147 systemic banking crises over the period 1970-2011, and also lists 211 currency crises and 61 sovereign crises over the same period.

[4] Giustiniani and Kronenberg (2005, p.11) note that "comparing the periods before (1995-96) and after (1997-2003) the Asian crisis, the share of banking sector conditionality has expanded from 65 percent to 80 percent of total financial sector measures [...and that this] is indicative of a growing and more comprehensive attention of IMF programs, and hence of IMF conditionality, to the functioning of the banking industry".

ment conditional on having gone through a financial crisis in the previous three years is significantly larger (52.2%) that the same probability in tranquil times (39.8%).

Panel B of Table 3 points out the positive association between banking crises and IMF-supported programs. In the whole sample of low- and middle-income countries used in the empirical analysis, the likelihood of a banking crisis is equal to 5% in country-year observations in which the IMF is lending compared to 2% in country-year observations in which there is no IMF loan agreement, and the difference between these two probabilities is statistically significant. The same pattern holds when we split the sample into low-income and middle-income countries.

The close interactions between financial crises and subsequent IMF interventions and between past and current financial crises complicate the identification of the IMF participation effect on the probability of banking crises. The challenge to identify the additional contribution of IMF lending programs to the risk of future banking crises is to tease out the effect of past financial crises, which called for an IMF intervention, but also made the country (and the domestic banking system) more conservative and/or more fragile anyhow, reducing the risk of future crises.

4 The empirical strategy

4.1 The empirical model

The empirical literature on the determinants of banking crises is quite extensive and, starting from the influential paper by Demirguc-Kunt and Detragiache (1998), has identified some key variables which are correlated with the probability of banking crises.[5] In Table 1 we briefly summarize the results from a non-exhaustive list of recent papers, pointing out also the sample covered and the methodology used. Almost all the studies consistently show that the likelihood of a banking crisis is higher when real interest rates and inflation are higher, after episodes of credit boom and when real GDP growth and the stock of international reserves are lower. This strand of literature has looked at the effect of several other explanatory variables, also considering the role of domestic institutions. However, so far it has ignored the potential role that International Financial Institutions (IMF, World Bank, etc.) could play in affecting the degree of domestic financial stability.

Henceforth, to assess the effect of IMF lending agreements on the probability of the occurrence of banking crises, we estimate the following linear probability model:

$$Pr(BANKING\ CRISIS_{i,t}) = \alpha IMF\ PRESENCE_{i,(t-1,t-5)} + \\ + \sum_{j=1}^{3} \beta^j \mathbf{PAST\ \ CRISES}_{i,(t-1,t-3)} + \sum_{j=1}^{n} \gamma^j \mathbf{CONTROLS}^j_{i,t-1} + \epsilon_{i,t} \quad (1)$$

[5] For a recent review of this strand of literature, see Demirguc-Kunt and Detragiache (2005) and Kauko (2014).

where the dependent variable is a dummy equal to one if a banking crisis occurred in country i at time t. Following a large literature (Demirguc-Kunt and Detragiache, 1998; Beck *et al.*, 2006; De Haan and Shehzad, 2009; Jorra, 2012), we estimate a pooled model without including country fixed-effects, as they wash-out much of the cross-sectional variation in the data that we would like to utilize (on this, see also Noy, 2004; Aizenman and Noy, 2013).[6] Hence, differently from empirical models that rely on the use of a fixed-effect estimator (Joyce, 2011; Gourinchas and Obstfeld, 2012; Catão and Milesi-Ferretti, 2014), we can interpret the coefficient α in a cross-sectional dimension, and not as the the effect of the IMF intervention on the likelihood of a future banking crisis in a given country.

The key explanatory variable is the presence of IMF loan arrangements in the country between $t-5$ and $t-1$ ($IMF\ PRESENCE$); we use a five-year window to account for possible medium-term effect of IMF programs on financial sector stability, mainly due to the the time required to implement some of the reforms required by Fund conditionalities. However, in one of the robustness exercises we validate our findings using different lag structures. In the baseline model, the presence of the Fund is measured by a dummy variable equal to one if country i has signed at least one IMF lending arrangement in the five-year period before the onset of the banking crisis ($IMF\ ARRANGEMENT$). Alternatively, both for robustness and for investigating the importance of the credit availability channel, we consider a continuous measure, defined as the logarithm of the ratio between the amount of loan agreed between $t-5$ and $t-1$, and the country GDP ($IMF\ LOAN/GDP$). In our sample, the ratio between the loan agreed and GDP is extremely variable, ranging from 0.1% to 44.8%: the average (median) loan-to-GDP ratio is equal to 3.2% (2.2%), and half of the loans are between 1.2% and 4.1% of GDP. The $IMF\ LOAN/GDP$ variable is set to zero if the country has not signed any agreement in the five-year period. For countries with more that one lending arrangement between $t-1$ and $t-5$, the loan amount (as a share of GDP) is the sum of the loan-to-GDP ratios.[7] Finally, to explore the reform channel we check on the degree of compliance with IMF conditionalities by distinguishing between compliant and non-compliant countries according to the amount of the agreed IMF loan that remained undrawn at program expiration (below or above a 25% threshold; see Table 2 and Section 5.3).

To allow for the possibility that past financial crises (**PAST CRISES**) can affect the current likelihood of a banking crisis (see Section 3), we include three dummies, each set equal to one if the country experienced at least one banking crisis ($BANKING\ CRISIS$), one sovereign crisis

[6] In unreported regressions we find that adding country-fixed effects significantly weakens the identification strategy.

[7] We also used the ratio of the total amount of loan arrangements agreed with the Fund in previous five-years period to the average value of GDP in the period, finding almost identical results. In addition, unreported regressions also show that results are unaffected measuring loan size as the amount of actual disbursement rather than the agreed quantity. For robustness, we have also measured the ratio between IMF loan and country quota, as published in the IMF's historical data set. Results are not reported for the sake of brevity but they are available upon request.

(*DEBT CRISIS*) or one currency crisis (*CURRENCY CRISIS*) between $t-8$ and $t-1$. The timing of these three dummies allows for taking into consideration both the effect that past crises could have on the likelihood of a future crisis and on the probability that the country and the Fund sign a loan agreement. Finally, the model controls for the effect that a set of standard macroeconomic variables (**CONTROLS**), all one-year lagged, can have on the likelihood of a banking crisis. Namely, we control for real per capita GDP, real GDP growth, the share of short-term debt over total external debt, credit growth, exchange rate depreciation, the ratio of M2 to reserves, a measure of openness and three indicators of *de facto* financial integration, as measured by the ratios over GDP of: (i) portfolio equity liabilities, (ii) debt liabilities, and (iii) foreign direct investment liabilities. Then, we extend the analysis by including a number of institutional characteristics of the countries and their domestic banking and financial systems. The list of variables, their labels, definitions, sources and statistics are reported in Table 2.

As several macroeconomic variables, such as output growth, public debt, and credit to the private sector, may be affected by the unfolding of the crisis, leading to a post-crisis bias (Bussière and Fratzscher, 2006). To minimize the impact of the banking crisis on the right-hand side variables we follow Gourinchas and Obstfeld (2012) and Catão and Milesi-Ferretti (2014) and we drop from the sample the years during which the crisis is spreading.[8]

4.2 The identification strategy

As already mentioned, the identification of the causal effect of participation in a Fund program on the probability of banking crisis using a simple linear probability model can be biased because of the potential endogeneity of IMF arrangements. Fund's presence in a country is more likely in crisis years or just before the onset of a crisis, when the government may ask the Fund for technical and financial assistance, than in tranquil periods. Moreover, the "crisis resolution role is at the core of IMF lending".[9] In this case, the observed positive correlation between IMF lending and future banking crises would not imply any causation running from the former to the latter.

In order to estimate the average treatment effect of IMF programs – the "IMF participation effect", as in Atoyan and Conway (2006) – we would need to be able to compare the outcome variable across two countries which are identical but for the participation into an IMF-supported program. However, the treatment ("the IMF loan agreement") is not randomly distributed across countries, but it is correlated to some country characteristics. As a result, differences in the likelihood of a banking crisis between countries which have an IMF lending program ("the *treated* group") and the

[8] We are helped in this task by the fact that Laeven and Valencia (2013) indicate in their data set the starting and ending year of each crisis. For the episodes which started in 2008, we assume that they are still ongoing in 2010, if no ending year is specified.

[9] See the IMF website at: http://www.imf.org/external/about/lending.htm.

others ("the *control* group") can be due to systematic differences across the two groups of countries rather than to the treatment. The literature has generally dealt with the selection bias adopting either an Instrumental Variable (IV) or a matching estimator (see Atoyan and Conway, 2006, for a review of these strategies applied to the evaluation of IMF programs).

Our baseline results are based on an IV approach, but we also test the robustness of our main findings using a propensity score matching estimator, which relies less on the exogeneity of the instruments (see Section 6.5).

4.2.1 The IV approach

The instrumental variable (IV) approach has been widely used both in the literature on IMF lending (Barro and Lee, 2005; Eichengreen *et al.*, 2008; Dreher and Walter, 2010) and in the literature on banking crises (De Haan and Shehzad, 2009). This methodology relies on the existence of some instruments, which are good predictors of the probability that a country signs an IMF arrangement, but do not affect the likelihood of a banking crisis other than through receiving IMF loans. The literature has identified a number of good predictors of IMF lending arrangements (see Sturm *et al.*, 2005; Moser and Sturm, 2011, for recent reviews). Here we use three political variables, which have been shown to be correlated with the probability of the IMF intervention and yet arguably exogenous to the future occurrence of a banking crisis.

First, we exploit the political proximity between borrowers and the IMF major shareholders that has been found to be a significant predictor of participation in a Fund loan agreement (Thacker, 1999; Oatley and Yackee, 2004; Barro and Lee, 2005; Barnebeck Andersen *et al.*, 2006; Dreher *et al.*, 2009; Presbitero and Zazzaro, 2012). The motivation of such correlation can be explained by the behavior of the Group of Seven (G7) governments, which trade their influence on the IMF Executive Board in exchange for support on important foreign policy issues discussed by the United Nations. The voting pattern in the United Nations General Assembly (UNGA) is taken as a proxy of the degree of similarity in foreign policy between IMF member countries and the Fund's major shareholders, namely the US and the other G7 countries. Hence, countries which prove to be aligned with the interests of G7 governments would be more likely to be rewarded by IMF assistance. We calculate the average alignment score with G7 countries computed on all UNGA regular votes ($PROXIMITY$).[10]

[10]For a detailed description of the dataset, see Kilby (2009b). The data set also includes identification of important votes as declared by the US State Department. However, since this information is not available for the whole time span, we cannot construct the alignment scores based on important UNGA votes. Therefore, we cannot use the difference between the alignment score in important votes in the UNGA and the same score in all other UNGA votes, a measure introduced by Barnebeck Andersen *et al.* (2006). As in Thacker (1999) and Dreher and Jensen (2007), the alignment score of country Y with country X is measured considering, for each vote, that country Y scores 1 if it follows X, 0.5 if it abstains or is absent when X votes (or vice versa), and 0 if it opposes X. Political similarity with the G7 is built by averaging the pairwise annual alignment scores.

Then, exploiting the observation that the probability of signing an IMF-supported programs is higher following a change in the government (Przeworski and Vreeland, 2000; Vreeland, 2002; Harrigan *et al.*, 2006; Moser and Sturm, 2011), we build a second instrumental variable that identifies years in which there have been political elections in the country ($ELECTION$). The intuition is that government officials have an incentive to strike a Fund lending arrangement early in their terms to minimize potential stigma effects that may compromise the chances of being re-elected.

Finally, given that US aid flows could capture the strength of economic and political proximity between borrower countries and the US (Dreher, 2004a), we use, in the robustness analysis, the share of US aid in total foreign aid inflows ($US\,AID$) as a third instrument.[11]

To take into account some extraordinary voting pattern and strategic behavior (see below) in the political process of concessions and rewards, the measure of political proximity is averaged over the five-year period $(t-6, t-10)$ before the five-year period $(t-1, t-5)$ in which we allow for the possibility of a Fund lending program. Similarly, the other two instruments are measured over the same time frame: (i) $ELECTION$ is a dummy equal to one if the country had at least an executive election between $t-10$ and $t-6$, so to take into account the effect of lagged elections on future agreements, and (ii) $US\,AID$ is averaged over the same time period.

The *relevance* of the instruments should not be an issue, given the robust evidence mentioned in favor of a positive influence of political similarity between a country and the G7 governments (or the US) and the likelihood of signing of an IMF-supported program in that country in the following years, and the existing evidence supporting a correlation between election and future IMF programs. These correlations are confirmed also in our data set and, as we will show when discussing our results, the first-stage F-statistic is generally above the Stock and Yogo (2005) rule of thumb of 10, suggesting that we do not have a weak instrument problem.

There could be a question on the *exogeneity* of the "political" instruments as they apply to banking crisis context. Political variables have been widely used as instruments in growth regressions. In those cases, the excluding restriction is that the similarity in foreign policy orientation and elections should not have a direct effect on economic growth. When looking at financial stability, the excluding restriction is less tenable. In fact, it may be conceivable that political proximity with the US and IMF shareholders also has direct influence on the stability of the financial sector in prospective recipient countries. If a reward mechanism exists, such that the US and G7 governments trade support in exchange of aligned votes in the UNGA, this support may take different forms besides the influence on the IMF Executive Board. Another possible form would be exerting influence on the domestic

[11] Some authors have used US military aid as a proxy of the country's economic and strategic importance to the US (Oatley and Yackee, 2004). We experiment with such indicator, but it does not prove to be a relevant instrument in the first-stage regression.

monetary and regulatory authorities or on foreign creditors in order to support and provide the banking system of G7 foreign policy friends with liquidity (Copeletovich, 2010a,b).[12] Similarly, in the case of the share of the US aid in total aid flows, one could argue that the importance of a country for the US would translate in support other than just aid, mitigating the vulnerability to banking crises. However, we are not aware of any evidence supporting a linkage between US aid and financial stability. Furthermore, in our sample there is no evidence that past flows of US foreign aid (measured as a share of foreign aid) are significantly different between years with and without a banking crisis.[13]

A second source of concern is related to a possible strategic behavior of potential recipients of IMF-supported lending. Some countries could move closer to the US and the G7 governments when they expect future financial troubles, reverting their foreign policy alignment once they obtain the financial assistance from the Fund. Under this strategic friendship, our foreign policy variables could be correlated with future financial instability. However, this particular type of endogeneity would bias results against the positive impact of IMF interventions on bank stability. To the extent that countries that are on a brink of a banking crisis actually alter their voting patterns at the UNGA in order to obtain the Fund support, the positive correlation between IMF programs and banking crises probability would be amplified rather than mitigated. Be that as it may, in our sample there is no evidence of such a strategic behavior on the part of IMF borrowing countries. To support this statement, in Figure 2 we plot the average evolution of our political proximity instrument in the 5-year periods before and after each IMF loan agreement in our sample. For comparison, we show the evolution of $PROXIMITY$ in a control group. This has been chosen randomly selecting a sample of country-year observations in which no Fund-supported lending agreements were signed. Rather than a downward trend after the lending arrangement, the diagrams show a small increase in the measure of proximity in the 11-year window, and this trend is common to the control group.

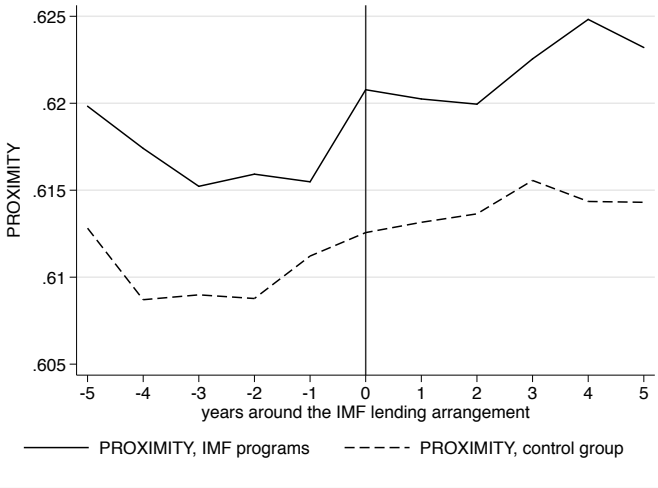
Endogeneity concerns may also arise when considering the election year variable. For example, Dinç (2005) documents that political influence on state-owned banks in emerging markets is particularly strong in election years, leading to a significant increase in lending by government banks, which may undermine financial stability. However, this destabilizing effect takes place in election years, but the fact that the $ELECTION$ variable is lagged at least six years before the crisis year should be enough to limit any direct effect between elections and subsequent banking crises.[14]

[12] It should be noted that a similar identification strategy based on friendships with IMF major shareholders has been followed to assess the impact of IMF-supported programs on the occurrence of other possible episodes of financial crisis, like sudden stops of capital flows, currency and sovereign debt crises, for which the plausibility of the excluding restriction is equally questionable (Eichengreen *et al.*, 2008; Dreher and Walter, 2010; Jorra, 2012).

[13] In our sample, the average value of the variable $US\ AID$ is almost identical (about 21%) considering crisis and non-crisis years.

[14] In our sample, the average value of the variable $ELECTION$ is even smaller in crisis than in non-crisis years, although the difference is not statistically significant.

Figure 2: IMF lending arrangements and foreign policy similarity



Notes: Calculations based on data on IMF lending arrangements and on foreign policy similarity (Kilby, 2009b). The original sample consists of 2,250 country-year observations (see Table 4, columns 1-3). To ensure the same balanced panel before and after $t = 0$, we have dropped all lending arrangements signed after 2003. This leaves us with 308 arrangements. The control group ($N = 368$) has been randomly selected among country-year observations in which no IMF lending arrangement was signed.

That said, we estimate an over-identified 2SLS linear probability model to test for the validity of the over-identifying restrictions. As we will show below, the statistical tests indicate that our instruments are valid, and thus we can apply IV strategy to equation 1. Furthermore, a simple falsification test shows that, in our sample, there is no direct correlation between each of the three instruments and the likelihood of a banking crisis.

5 Results

5.1 Main findings

The main results are reported in Table 4. In the first three columns we present the baseline model and compare a simple linear probability model with the 2SLS model, in which the binary indicator for the presence of at least one IMF lending arrangement between $t-1$ and $t-5$ ($IMF\ ARRANGEMENT_{t,t-5}$) is instrumented using $PROXIMITY$ and $ELECTION$. The OLS estimates show that having signed an IMF lending agreement is not statistically correlated with the incidence of banking crises in subsequent years (column 1). However, when we take into account the potential endogeneity of IMF support, the coefficient of $IMF\ ARRANGEMENT_{t-1,t-5}$ becomes negative and statistically significant (column 3). This result confirms the presence of a negative bias in the standard OLS model (because of the negative correlation due to the IMF intervention in "bad times") and indicates that the Fund's assistance actually lowers the probability of incurring a banking crisis from about 6 to 0.5 percent. The negative association between Fund intervention and the probability of banking crises

across countries suggests that the positive effects of the IMF-supported program, in terms of credit availability and reform stimuli, offset the bad signals and moral hazard effects.

The coefficients on the excluded instruments in the first-stage estimates (column 2) and the diagnostic tests generally confirm that the instruments are relevant. Foreign policy similarity and past elections between $t-10$ and $t-6$ show a positive and significant correlation with the dummy for the IMF presence in the country in the interval $(t-1, t-5)$. More importantly, the F-test for the weak identification test is equal to 18.5, well above the 10 value proposed by Stock and Yogo (2005) as a rule of thumb. As regards the exogeneity, since the model is over-identified, we can run a standard Sargan-Hansen test of overidentifying restrictions, which indicates that we cannot reject the null hypothesis that our instruments are uncorrelated with the error term. Finally, the Kleibergen-Paap rk LM-statistic suggests that the model is not underidentified.

A similar picture also emerges when we drop the global recession 2008-2010 period (columns 4-5), which significantly increased the number of banking crises and induced massive Fund interventions (see Figure 1). As anticipated, results are unaffected if IMF intervention is measured by the size of the loan (measured as a share of GDP, columns 6-7). In both cases, the first-stage F-statistics is lower than in the baseline, but still above the safe threshold of 10.

In columns 1-7, standard errors have been clustered at the country level, to deal with possible serial correlation in the error term within countries. The last two columns report the estimates obtained bootstrapping the standards errors (with 2,000 repetitions) and results are qualitatively identical.

Moving on to the set of control variables, our results are broadly consistent with the literature on banking crises. The occurrence of a banking crisis is robustly associated with low per capita GDP growth and rapid credit growth, similar to what was found by Gourinchas and Obstfeld (2012) in a panel of emerging economies. When looking explicitly at *de facto* financial integration, we find that foreign direct investment liabilities reinforce banking stability, while more volatile debt inflows raise the probability of a banking crisis, consistently with the evidence discussed by Joyce (2011) and Gourinchas and Obstfeld (2012) on a smaller sample of emerging markets. We also find that trade integration is associated with a lower incidence of banking crises. Interestingly, our findings confirm an association between sovereign and debt crises (Reinhart and Rogoff, 2009; Acharya *et al.*, 2014), while they do not indicate a similar pattern when considering currency crises.

On the contrary, we do not find any statistical association between the probability of banking crises and the fraction of short-term external debt, currency depreciation, and the ratio of M2 over international reserves, and real GDP per capita. This last finding is consistent with banking crises being an equal opportunity menace for poorer and richer countries (Reinhart and Rogoff, 2013).

5.2 The credit channel

As discussed in Section 2.1, the cross-country evidence of a positive influence of Fund intervention on banking sector stability may be due to credit availability and the implementation of macroeconomic policies and reforms or to a "seal of approval" effect that is related to the IMF presence *per se* more than to the size and conditionalities of the agreed arrangement. We will address the role of conditionalities and reforms in the next section. Here we focus on a direct liquidity provision effect, without explicitly testing for any catalytic role of IMF-supported program, and we assess whether there is some non-linearity in the IMF participation effect according to the size of the loan.

To test for the role of credit availability we consider the possibility that the positive effects of IMF interventions on the likelihood of banking crises holds only if the loan arrangement is sufficiently large. In fact, if the provision of IMF financial resources to increase were a necessary seal to nurture the confidence of creditors and create a safety net for the domestic banking system, the effect of participating in an IMF-supported program would be independent of its size. Otherwise, the intervention of the Fund is not sufficient to reduce the risk of banking crises, unless its size exceeds a certain threshold.

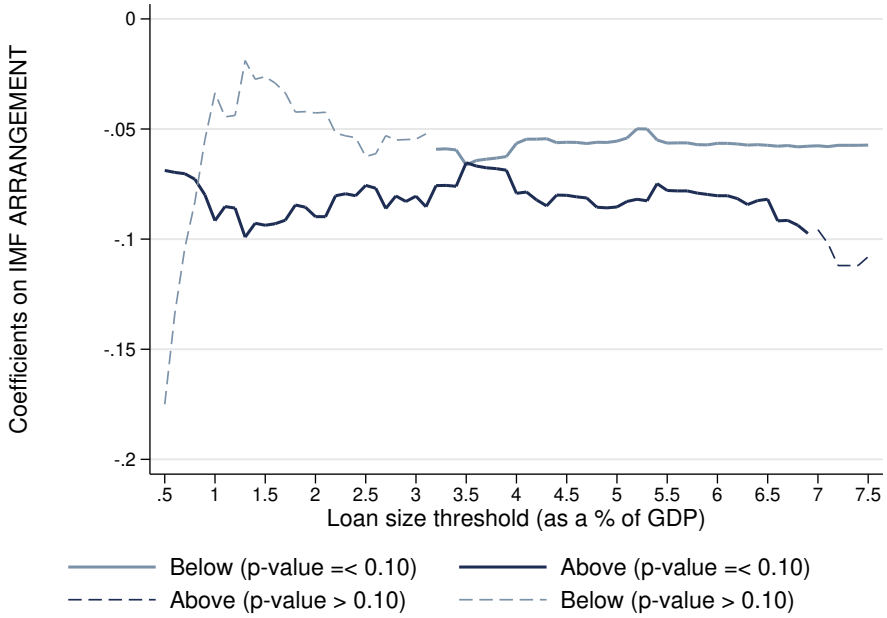
Empirically, we split the sample according to the size of the IMF loan at some specific exogenous thresholds. Given the non-linearity of our model and the endogeneity of the key regressor, we are unable to implement more sophisticated methods used to test for the presence of an endogenous threshold (Hansen, 2000). However, we try to mitigate the effect of the arbitrariness in the choice of the threshold, splitting the sample for any value of the loan amount (as a percentage of GDP) in the range $[0.5-7.5]$, with 0.1 percentage point increments.[15]

To save space, Table 5 reports the estimated coefficient α on $IMF\ ARRANGEMENT_{t-1,t-5}$ only for a few selected sub-samples, together with the first-stage coefficients on the excluded instruments. The results indicate that up to the 2% threshold the likelihood of a banking crisis is significantly smaller only for sufficiently large loans. By contrast, when considering loans equal or larger than 3% of GDP, IMF participation effect is negative and statistically significant both below and above the threshold. Thus, our results suggest that, as long as the loan amount is sufficiently small (below 2% of GDP), IMF lending does not reduce the probability of banking crises. In other words, only significantly large IMF loans are associated with a lower probability of banking crises across countries.

One may argue that this non-linearity is the result of a model mis-specification. However, the first-stage coefficients of the instrumental variables indicate that the statistical insignificance of the

[15] We choose such a threshold range since we are not interested in tails of the sample distribution of the loan size variable. A loan-to-GDP ratio of 0.5% (7.5%) roughly corresponds to the 5^{th} (95^{th}) percentile of the sample distribution of the $IMF\ LOAN/GDP$ variable.

Figure 3: Coefficients on IMF ARRANGEMENT below and above loan size thresholds



Notes: Calculations based on linear IV estimates of the baseline specification (Table 4, columns 4-5), splitting the sample for any value of the loan amount (as a percentage of GDP) in the range $[0.5-7.5]$, with 0.1 percentage point increments, similarly to what reported in Table 5. The diagram plots the coefficients on IMF ARRANGEMENT$_{t-1,t-5}$ disentangling between statistically significant (p-value ≤ 0.10) and non-significant coefficients.

lending arrangement dummy in the small loan samples does not depend on the limited power of the instruments. The insignificance of the IMF dummy may still depend on the sample being relatively small (see columns 1 and 3). However, by splitting the sample around the 4 and 5 percent thresholds (columns 8 and 10) we still get significant results, implying that the significance of the coefficients does not depend on the number of observations in each sub-sample.

Figure 3 provides further confirmation that a significant large loan size is required for the IMF-supported programs to be effective in forestalling future banking crises. There we plot the coefficients of $IMF\ ARRANGEMENT_{t-1,t-5}$ estimated in the subsamples restricted below and above the loan size thresholds (from 0.5 to 7.5% of GDP, at 0.1% interval). The figure shows that the coefficients estimated in the sample above the threshold are quite stable at around -0.8 and are always statistically significant, apart from very high values of the loss size threshold, when the sample size is substantially reduced. By contrast, the coefficients estimated in the sample below the threshold are imprecisely estimated, much less stable and smaller (in absolute value) than the ones above the threshold, up to a loan size threshold of 3.2 percent of GDP. Above this value, also the coefficients estimated in the sample below the loan size thresholds are quite stable and close to -0.6, confirming that IMF loans reduce the probability of future crises only as long as the loan size is sufficiently large.

To sum up, although in this paper we do not provide evidence on the impact of signing IMF agreements on the flow of private finance to the country or on interest rate spreads, usually considered as testing the *seal of approval* and catalytic effects (Bird and Rowlands, 2009; Saravia, 2010; Bal-Gunduz and Crystallin, 2014), the finding that only large enough loans have an effect in reducing the occurrence of banking crises can be read as corroborating the importance of the credit availability channel.

5.3 The reform channel

5.3.1 Compliance with conditionality

Other than credit availability, IMF intervention could affect banking sector stability thanks to the conditions and policy advice attached to IMF-supported programs. In particular, conditionalities targeted at financial sector reforms may affect the likelihood of a future banking crisis, making the financial sector more resilient to international capital flows and external shocks (see above, sub-section 2.1.2). However, the effectiveness of IMF conditionalities depends on the degree of compliance by the recipient country (Dreher, 2009). Focusing specifically on financial sector conditionalities, Giustiniani and Kronenberg (2005) find that compliance with IMF-supported banking sector reform strategies has contributed to an improvement in banking sector performance over the period 1995-2003.

Since the MONA data set classifies compliance by conditionality categories starting from 1992, we could examine financial-sector conditionalities only at the expense of a dramatic reduction of our sample. Therefore, we employ the approach proposed by Killick (1995),who takes IMF loans that were agreed but left undrawn at program expiration as an indicator of program performance. Drawing from Dreher (2003) and Dreher and Walter (2010), we code a country as compliant when at most 25% of the amount agreed under an IMF arrangement remained undrawn at program expiration. Interestingly, in almost 40% of the programs for which we have data countries can be considered as non-compliant with Fund conditionality.

The results shown in Table 6, indicate that the effect of IMF-supported programs on the probability of a future banking crisis critically depends on conditionality compliance. Unlike the evidence found on the outbreak of currency and sovereign debt crises, which point to a limited importance of the distinction between compliers and non-compliers (Dreher and Walter, 2010; Jorra, 2012), the effect of the IMF lending arrangement in reducing the probability of banking crises is much lower for not compliant countries. The effect of the compliance is economically significant. Considering the baseline estimates (column 1), the probability of a banking crisis is 20.9% lower for countries that sign an IMF lending agreement and comply with Fund conditionality. A lending agreement

folowed by non-compliance, instead, lowers the probability of a banking crisis only by 7.4%. The positive effect of compliance with conditionality on financial stability holds when measuring IMF participation with loan size (column 3). Given that in both specifications the first-stage F-statistics is below the critical value of 10, suggesting a possible weak identification, we report also the Limited Information Maximum Likelihood (LIML) estimates (columns 2-4), which are less precise but also less biased than the 2SLS ones. Again, we find that results are very similar.

5.3.2 World Bank lending

Another way to help to tease out the channels through which the IMF intervention affects financial stability is comparing the impact of IMF and World Bank (WB) lending programs. When dealing with the latter, we refer exclusively to structural adjustment loans, which typically have similar conditionalities as IMF programs (Easterly, 2005), but involve more limited amount of funds.[16] Therefore, if one were to find that a WB adjustment loan is less effective in reducing the likelihood of a banking crisis than an IMF arrangement would the hypothesis of a credit channel might be corroborated. By converse, if both programs were equally effective notwithstanding differences in the actual size of the loan, one would corroborate the reform channel.

As for the Fund, we measure the presence of the World Bank in a country with a dummy which is equal to one if the country received at least one structural adjustment loan from the World Bank in the five-year period before the onset of the banking crisis, and zero otherwise ($WB\ LENDING$). Then, we estimate a model similar to equation (1) in which we replace the variable measuring IMF participation with the one referring to the World Bank. Given that the empirical challenges are the same, we deal with the selection bias in the same way and instrument $WB\ LENDING$ with two instruments: one is the indicator for elections, and the second is the change the alignment score rather than its level, as the latter in not a relevant instrument for World Bank lending. In particular, in line with the political *movement* hypothesis, we consider the change in the average alignment score with G7 countries between t and $t-1$ ($MOVEMENT$).[17] This variable should capture the possibility that it is the change in foreign policy orientation which would be rewarded by the major IMF shareholders, in line with the evidence provided by Thacker (1999). Again, this indicator is averaged over the five-year period $(t-6, t-10)$ before the five-year period $(t-1, t-5)$ in which we allow for the possibility of a World Bank lending program.

For comparability, we use the same instruments over the same sample (truncated to 2005 because of data availability on World Bank lending arrangements) when considering the IMF lending

[16] For instance, Dollar and Svensson (2000) consider 182 adjustment loans disbursed between 1980 and 1995 and show that the average loan size is about USD 160 million; over the same period, the average size of the IMF-supported program in our sample is USD 345 million.

[17] A similar IV strategy is followed by Kilby (2009b)

variable. Results, reported in Table 6 (column 5-8), show that the involvement of World Bank is associated with a lower probability of a future banking crisis, having an impact similar in significance and magnitude to that of IMF interventions.[18] This is suggestive of the importance of reforms as a mechanism through which multilateral loans enhance financial stability.

6 Robustness checks and extensions

6.1 Past crises, IMF arrangement and future banking crises

One possible objection to our results is that controlling for the occurrence of past financial crises does not wash out the possibility that the IMF participation effect on banking stability could be due to the occurrence of a past crisis and not on the IMF involvement per se. A previous crisis could have triggered more conservative financial and macroeconomic policies so that, irrespective of any subsequent (or contemporaneous) IMF program, the country would experience a lower probability of a future crisis.[19] To take into account this possible source of bias, which could be not entirely eliminated by our IV strategy, here we zoom in exclusively on IMF programs that did not followed any financial crisis in the previous three years.

Interestingly, we find that even limiting our sample to Fund programs which were unrelated with previous crises, the IMF participation effect remains statistically significant and of the same order of magnitude than in the whole sample (Table 7). This is true if we condition the IMF presence variable to the occurrence of any financial crisis in the past (column 1), or exclusively to banking, currency, or sovereign crises (columns 2-4).

6.2 Other potential triggers of banking crises

Given that our model specification cannot exploit the within-country variation, the results discussed so far might be influenced by some unobserved country-specific characteristics which affect financial stability. To mitigate this concern, we control for the possible influence of different aspects of the macroeconomic and institutional setting on banking sector stability. Table 8 reports the 2SLS coefficients on IMF ARRANGEMENT$_{t-1,t-5}$, on the additional covariates and instruments. In sum, the significance of the IMF arrangement dummy variable is robust to the inclusion of many additional control variables and it provides further evidence that Fund-supported programs are effective in forestalling future crises.

First, we control for the presence of common regional factors and spillovers which may jointly

[18] Also in this case the F-statistics suggest the risk of weak identification, but the LIML estimates support the 2SLS ones.

[19] Indeed, in unreported regressions we find a negative association between past banking crises which were not preceded by any IMF arrangement in the previous three years and the likelihood of a current systemic banking crisis.

affect the probability of a banking crisis and the Fund intervention in a country, as happened during past financial crises in East Asia or Latin America, by adding a set of regional dummies. Eastern European and Central Asian countries are more likely to experience banking crises than other regions but, even controlling for common regional shocks, the effect of the Fund intervention in forestalling future crises remains positive (column 1).

Then, we include different measures capturing the degree of a country's institutional development (columns 2-5). First, we consider a measure of the degree of political, financial and economic riskiness ($COUNTRY\ RISK$). This risk rating is the Composite Index published by the International Country Risk Guide (ICRG).[20] Second, we use the index of access to sound money ($SOUND\ MONEY$), published by the Fraser Institute as part of the Economic Freedom of the World Index. This variable measures the growth rates of money supply and inflation, and the freedom to own foreign currency bank accounts domestically and abroad.[21] Third, we include the polity score from the Polity IV data set ($POLITY$), which ranks governing institutions from autocratic to democratic according to measures that record key qualities of executive recruitment, constraints on executive authority, and political competition (Marshall *et al.*, 2010). Fourth, we rely on the dummy $DEMOCRACY$, which identifies democracies and dictatorships according to the definition proposed by Cheibub *et al.* (2010). We find that only the access to sound money is an element which mitigates the vulnerability of the banking system to systemic crises (column 3). This result suggests that the probability of banking crises does not differ significantly according to the institutional and policy set-up and to the presence of an institutionalized democratic regime.

As a further robustness exercise we add a number of macroeconomic variables which may have an effect on banking sector stability. Controlling for inflation (measured by the GDP deflator), financial depth (measured by the ratio of credit to the private sector over GDP), debt service (as a share of exports) and the current account balance (as a share of GDP) does not affect the IMF participation effect, and only the debt service ratio turns out to be significant, suggesting that lack of liquidity raises the likelihood of a systemic crisis (column 8).

In columns 10 and 11 we control for a *de jure* measure of financial openness, using the Chinn and Ito (2010) index of capital account openness ($FINANCIAL\ OPENNESS$), and for the degree of financial liberalization ($FINANCIAL\ REFORMS$), using the index of financial reforms compiled by Abiad *et al.* (2008).[22] We confirm our baseline results on the positive impact of IMF lending

[20] The Composite Index is a risk rating based on a set of 22 components grouped into three major categories of risk: political, financial, and economic. The index ranges between 0 and 100, with higher values indicating lower levels of risk. For details, see http://www.prsgroup.com.

[21] See: http://www.freetheworld.com/index.php and Gwartney *et al.* (2012) for details.

[22] Abiad *et al.* (2008) build a database of financial reforms which covers 91 economies over the period 1973-2005. Financial policy changes are recorded along seven different dimensions: credit controls and reserve requirements, interest rate controls, entry barriers, state ownership, policies on securities markets, banking regulations, and restrictions on the capital account. Liberalization scores for each category are combined in a graded index that is normalized between zero

programs on domestic banking sector stability, while we find that the likelihood of banking crises do not differ across the degree of capital account openness and financial liberalization.

Finally, in column 12 we control for the presence of a deposit insurance scheme by including a dummy for country-year observations in which an explicit deposit insurance scheme is in place ($DEPOSIT\ INSURANCE$). We find that the presence of a deposit insurance induces moral hazard and increases bank risk taking, since it is positively correlated with the likelihood of a future banking crisis, as in Demirguc-Kunt and Detragiache (1998, 2002).

6.3 The role of policies and institutions

In Table 9 we assess whether the effect of the IMF intervention differs according to the macroeconomic and institutional characteristics of borrowing countries, splitting the sample along the four dimensions of the institutional framework introduced in section 5.1. We start by splitting the sample according to two measures of the quality of institutions and macroeconomic policies. We observe that the effect of IMF intervention is significant only in countries with a sound institutional framework (defined as countries where the ICRG score is above the sample median, see Table 9, columns 1 and 2) and where the index of access to sound money is above the median (columns 3 and 4). This evidence is consistent with the hypothesis that the catalytic effect of the Fund is stronger and the risks of moral hazard are lower in countries with sound institutions and monetary environment.

Then, we investigate the possibility that the effectiveness of IMF lending would depend on the presence of democratic institutions. We split the sample around the value of zero for the polity index from the Polity IV data set ($POLITY$) and using the dummy identifying democracies and dictatorships ($DEMOCRACY$). In this case, we find less robust results. The coefficient on the dummy for the IMF arrangement is negative and statistically significant only when associated with a high polity score, consistent with the idea that consolidated democratic environments enhance the effectiveness of IMF interventions. However, the coefficient for $IMF\ ARRANGEMENT$ is never significant when splitting the sample between democracies and autocracies (although in the democracies sub-sample its p-value is equal to 0.12).

In interpreting all these findings it is interesting to note that the magnitude of the coefficients across sub-sample is generally quite similar, apart when using $DEMOCRACY$ (columns 7 and 8), even if many regressions are weakly identified (the LIML estimates, however, support the main results, see Table A2 in the Appendix).

and one. This is the index used in the regressions and it has the advantage of being a continuous measure, rather than a 0/1 dummy for financially liberalized countries.

6.4 Instruments and sub-samples

The IV estimates rely on the key untestable assumption of exogeneity of the instruments. To further confirm the soundness of our strategy, we consider the share of US aid in total aid inflows as an additional instrument. As discussed in section 4.2, this instrument is unlikely to direct affect financial stability, also because it is not a direct measure of financial support to a country, but only a measure of the relative importance of the US among donors. Results show that the instrument is relevant and confirm the negative and significant coefficient on the IMF participation binary indicator (Table 10, columns 1 and 2).

Given that our model does not have country fixed-effects, one could conjecture that results may be driven by the difference between countries which had banking crises and the ones which have never gone through a systemic crisis. If countries that never experienced banking crises differs substantially from countries that went through at least one systemic crisis along some unobservable characteristic, the significance of IMF participation effect could not be interpreted as a causal impact of IMF on financial stability. To rule out this possibility we re-estimate our baseline model on the sub-set of 83 countries which had at least one systemic banking crisis over the sample period. We find that, if anything, the IMF participation effect is even stronger in the crisis-country sample (Table 10, column 3).

A third concern is that pooling all countries and IMF arrangements in the sample may mask some sort of heterogeneity across countries and loan type, as they differ in length, costs and conditionalities. We start excluding low-income countries as they generally have less developed financial markets and we still find that IMF programs are associated with subsequent lower incidence of banking crises in the 75 countries classified by the World Bank as middle-income (column 4).[23] In columns 5-6 we split the sample between non-concessional loans, financed via the General Resources Account (GRA), and concessional loans, extended from the Poverty Reduction and Growth Trust (PRGT, which recently replaced the Poverty Reduction and Growth Facility - PRGF). We do not find any significant difference in the effect of Fund interventions on banking stability across arrangement type. Our IV strategy proves valid in the two sub-samples and the estimates show that concessional and non-concessional IMF-supported programs are associated with similar lower probabilities of banking crises.

[23] The sub-sample of low-income countries is too small and the estimates loose power and precision, but the sign on the IMF presence variable is still negative.

6.5 Propensity score matching results

To the extent that one could doubt the true exogeneity of our instrumental variables and the reliability of our IV strategy, we also use a propensity score matching (PSM) methodology to deal with the selection bias of IMF-supported programs. Our approach is similar to the one adopted by Mumssen *et al.* (2013) and Bal-Gunduz and Crystallin (2014) to estimate the impact of IMF lending arrangements on GDP growth and donor assistance, and build on a larger macroeconomic literature that uses matching techniques (Glick *et al.*, 2006; Forbes and Klein, 2013; Bussière *et al.*, 2014).

In our setting, a sample selection bias can arise if countries which borrow from the IMF are fundamentally different from the others along a number of characteristics. In this case, the difference in the likelihood of a banking crisis observed comparing two group of countries, the one that borrowed from the Fund (the "treated") and the other that did not (the "control group") could be biased.

The propensity score matching addresses this problem by building two samples of countries, similar on a number of *observable* characteristics, but different only in the presence of the "treatment" (the IMF lending arrangement). The "average treatment effect" on the treated (ATT) – which is our measure of the IMF participation effect – is simply the difference in the frequency of banking crises between these two groups. Without entering in a detailed methodological discussion, for our purposes it is enough to say that the PSM reduces the multidimensional problem by matching treated and control observations on the basis of the "propensity score" (PS), which is the estimated probability that each unit of observation in the panel receives the treatment (Rosenbaum and Rubin, 1985). The PSM relies on two critical assumptions: (1) conditional independence, and (2) the presence of a common support. The former, also known as cofoundedness, implies that the selection into treatment is solely based on observed characteristics. The second one requires that observations with similar observable characteristics have a positive probability of being both treated and non-treated: this happens when the PS of the treated observations are bounded between the minimum and maximum PS of the control observations. Both assumptions require the specification of a comprehensive selection model, which should include a large set of observable characteristics.[24]

Consistently with the IV analysis, in our context the propensity score is the estimated probability that a country agrees to sign at least an IMF arrangement in a five-year window, conditional on a number of pre-treatment characteristics, which include: (i) the dummies for the occurrence of banking, currency or sovereign crises in the previous three years; (ii) almost all the explanatory variables we have used in the baseline regression and in the robustness exercises measured in t-1;

[24]For a more detailed discussion of many methodological aspects of PSM, see Caliendo and Kopeinig (2008) and Mumssen *et al.* (2013, Annex 1) for an application to IMF-supported programs.

and (iii) the three instruments used in the analysis averaged over the previous five years:

$$Pr(IMF\ ARRANGEMENT_{i,(t,t+4)} = \sum_{j=1}^{3} \beta^j \mathbf{PAST\ \ CRISES}_{i,(t-1,t-3)} + \\ + \sum_{j=1}^{n} \delta^j \mathbf{CONTROLS}^j_{i,t-1} + \sum_{j=1}^{3} \gamma^j \mathbf{INSTRUMENTS}^j_{i,(t-1,t-5)} + \epsilon_{i,t} \tag{2}$$

Once the PS is estimated[25], there are different matching algorithms that can be used to pair observations on the ground of observable characteristics, each having their own advantages and drawbacks (see Caliendo and Kopeinig, 2008). We use three different algorithms: the “five nearest neighbors” with replacement, the “radius with caliper”, and the “kernel”. The former method pairs each observation in the treated group with the five closest observations (measured using the PS) in the control group; we use this algorithm with replacement, so that observations in the control group could be taken more than once as a match. The average treatment effect is computed as a simple average of the differences in outcomes across the paired matches. The “radius with caliper" uses a similar approach but rather than taking the five closest neighbors, considers all those that fall within a maximum radius (i.e. the caliper, set to 0.1) from the propensity score of the treated observation. The “kernel", instead, is a non-parametric matching estimator which compares the treated observations with a weighted average of the control observations, where the weight are a negative function of the difference in the PS between the treated and the control observations.

The results of the PSM are reported in Table 11. The three alternative algorithms consistently show that the frequency of banking crises is significantly lower in countries that had at least one IMF-supported programs in the previous five years than in the matched sample of countries without Fund programs. The magnitude of the effect is lower than the one estimated by IV, but it consistently supports the hypothesis that IMF programs are associated with a lower incidence of future systemic banking crises across countries. Table 11 also reports some diagnostic tests to assess the quality of the matching. In particular, we see that both the mean and median absolute standard percentage biases are dramatically reduced by each of the matching algorithms. In all cases, the mean absolute bias is below the critical value of 5, suggesting that, on average, the matching has been able to remove any significant differences in observable variables between the treatment and the control group. Table A4 in the Appendix shows the means of all the covariates for the two groups and the value of the t-test to check if differences between groups before and after matching are statistically

[25]The estimates of equation 2 are reported in Table A3 in the Appendix; results are consistent with expectations and show that IMF programs are more likely in countries with lower per capita GDP and less financially and trade integrated, after episodes of sovereign debt crises, and where debt liabilities are higher and portfolio and FDI liabilities lower, the currency is depreciating, and real interest rates are higher. The degree of political similarity between the borrower country and the US/G7 and past elections are also significant predictors of IMF programs.

significant. We can see that, irrespective of the algorithm, the matching leads to a large reduction in the bias for all covariates, and significant differences persist exclusively in terms of per capita GDP, inflation and the ratio of M2 over reserves. Finally, the pseudo-R^2 is the one from the probit estimation of the selection equation estimated before and after matching. As expected, given that after matching there should not be systematic differences of the covariates across treated and non-treated, we observe that the pseudo-R^2 is very low in the matched group.

6.6 Alternative lag structures

As a final check, we test the robustness of our results to alternative lag structure used to define the IMF participation. IMF programs may strengthen the stability of the banking sector in the short-term, or their effects could materializes over a longer span of time, especially when they work through the reform channel. Table 12 allows for a more flexible definition of the dummy variable $IMF\ ARRANGEMENT$, which is equal to one if the country signs at least one Fund-supported program in the previous 3, 4, 6 or 7 years. Column 1 reports the baseline specification, when the IMF participation is assessed over the 5-year window. The results indicate that the positive effect of Fund-supported program on banking stability materialize in the short- and medium-run, as the IMF participation effect is statistically significant even when the Fund presence in a country is evaluated over shorter horizons. By contrast, when we expand the time window, we see that the coefficient, although negative, becomes smaller in absolute value and it is less precisely estimated. These findings suggest the effects of the programs tend to vanish over time, when many other macroeconomic and institutional developments could offset the positive effect of the IMF-supported program.

7 Conclusions

The role of the Fund in past systemic banking crises has been extensively debated and the global financial crisis has further stimulated the discussion about the on-going IMF reform process, making the development of an enhanced crisis prevention toolkit a priority. In theory, IMF lending may influence the likelihood of systemic banking crises through a plethora of contrasting effects, related to liquidity support, moral hazard, and bad and good signaling. Hence, whether and how IMF involvement affects the probability of a systemic banking crisis remains an empirical question.

In this paper we contribute to this debate, estimating the effect of IMF-supported programs on the probability of banking crises in a large sample of developing countries. Our empirical results indicate that the standard OLS estimates are biased by a reverse causality going from a higher probability of a banking crisis to a more likely intervention by the Fund. In other words, it is financial fragility

which may lead to the Fund's intervention in the country, rather than the other way round. When we take into account the endogeneity of the IMF's presence in a country, our results consistently show that countries which signed IMF lending programs are actually less likely to experience a banking crisis in future years than other comparable countries. This finding suggests that the bad signals and moral hazard effects, if present, are offset by the credit availability provided by the IMF-supported program.

We also find that the crisis prevention role of the Fund is connected to the macroeconomic policies and financial reforms which come with the loan and to a direct liquidity support effect. In fact, other things being equal, the likelihood of a banking crisis is lower for borrowers which are compliant with IMF conditionalities than for the non-compliant ones. In addition, we find that the negative association between IMF-supported programs and the probability of future banking crises is significant only when the loan is sufficiently large. This result is consistent with the benefit of the IMF intervention acting through liquidity support.

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Table 1: Determinants of banking crises: selected review

Paper	Sample	Period	Methodology	Main results
Demirguc-Kunt and Detragiache (1998)	65 market economies	1980-1994	Pooled logit; X_t	Growth (-), RIR (+), Inflation (+), M2/reserve (+), Private credit (+), Deposit insurance (+), Law & order (-), pc GDP (-)
Kaminsky and Reinhart (1999)	20 countries	1970-1995	Early warning indicators	Signals are strongly correlated with output and stock prices, interest rates, terms of trade, and reserves
Hardy and Pazarbasioglu (1999)	50 countries	1980-1997	Multinomial logit with FE; $X_{t;t-1;t-2}$	Growth (-), Inflation (+), RER depreciation (+), interest rates (+), credit expansion (+), capital inflows (+)
Noy (2004)	61 non-OECD countries	1975-1997	Pooled probit; X_{t-1}	Growth (-), Inflation (+), M2/reserve (+), RER depreciation (+), Financial liberalization (+)
Demirguc-Kunt and Detragiache (2005)	94 countries	180-2002	Pooled logit; X_t	Growth (-), RIR (+), Inflation (+), M2/reserve (+), Private credit (+), Deposit insurance (+), Credit growth$_{t-2}$ (+), pc GDP (-)
Beck *et al.* (2006)	69 countries	1980-1997	Pooled logit; X_t	Growth (-), RIR (+), pc GDP (-), Banking sector concentration (-), Banking restrictions (+)
Von Hagen and Ho (2007)	47 countries	1980-2001	Conditional logit; X_{t-1}	Growth (-), RIR (+), Overvalued ER (-), Deposit insurance (+)
De Haan and Shehzad (2009)	85 countries	1973-2002	Probit model with RE; X_t	Growth (-), RIR (+), pc GDP (-), RER depreciation (+), Financial liberalization (-). Financial reforms (-, conditioned on banking supervision)
Klomp (2010)	110 countries	1970-2007	Random coefficient logit model; X_t	Growth (-), RIR (+), Credit growth (+), M2/reserve (+), Globalization (+)
Joyce (2011)	20 emerging markets	1976-2002	Conditional logit; X_{t-1}	Growth (-), Inflation (+), FDI & portfolio liabilities (-), Debt liabilities (+), financial openness (-), fixed exchange rate (+)
Duttagupta and Cashin (2011)	50 countries	1990-2005	Binary classification tree	Banking crisis increases with: high inflation, liquidity dollarization combined with nominal depreciation or bank illiquidity, low bank profitability
Aizenman and Noy (2013)	102 countries	1980-2010	Pooled logit with and w/out FE; X_{t-1}	Growth (-), Credit growth (+), pc GDP (-), Hyperinflation (+), Banking and currency crisis history (+)
Gourinchas and Obstfeld (2012)	79 countries	1973-2010	Panel logit with FE; X_{t-1}	Public debt (+), Credit growth (+), Output gap (+), RER depreciation (-), Reserves (-)

Notes: RIR = real interest rate; RER = real exchange rate; FE = Fixed effects; RE = Random effects. X_t and X_{t-n} indicates that the main set of covariates is contemporaneous or lagged by n year(s), respectively.

Table 2: Variables: definition, sources and summary statistics

Variable label	Definition	Source	Mean	s.d.	Obs.
BANKING CRISIS	Dummy equal to one for country-year observations in which there is a systemic banking crisis, and zero otherwise	Laeven and Valencia (2013)	0.03	0.17	2,250
CURRENCY CRISIS	Dummy equal to one for country-year observations in which there is a currency crisis, and zero otherwise.	Laeven and Valencia (2013)	0.03	0.18	2,250
SOVEREIGN CRISIS	Dummy equal to one for country-year observations in which there is a sovereign debt crisis, and zero otherwise.	Laeven and Valencia (2013)	0.01	0.10	2,250
FINANCIAL CRISES	Dummy equal to one for country-year observations in which there is a banking, currency and/or sovereign debt crisis, and zero otherwise.	Laeven and Valencia (2013)	0.07	0.29	2,250
IMF ARRANGEMENT$_{t-1,t-5}$	Dummy equal to one for countries which signed an IMF loan agreement in the previous five-year period, and zero otherwise.	IMF historical data set	0.60	0.49	2,250
IMF LOAN/GDP$_{t-1,t-5}$	Logarithm of 1 + the amount approved, as a share of GDP, by the IMF arrangement signed in the previous five-year period. The variable is set to zero if the country has not signed any agreement in the previous five-year period. In case of multiple agreements, we take the sum of the loan-to-GDP ratios	IMF historical data set and World Development indicators	0.84	0.85	2,250
NON COMPLIANCE$_{t-1,t-5}$	Dummy equal to 1 if the country was compliant with its IMF program in the previous 5 years. A country is coded as compliant when at most 25% of the amount agreed under an IMF arrangement remained undrawn at program expiration and as zero otherwise	Dreher and Walter (2010)	0.50	0.50	2,250
PROXIMITY$_{t-6,t-10}$	Average alignment score, measured as the share of alignment votes with G7 countries on regular UNGA votes between $t-6$ and $t-10$	Kilby (2009a)	0.62	0.07	2,250
ELECTION$_{t-6,t-10}$	Dummy equal to 1 if the country had at least one executive election between $t-6$ and $t-10$	Beck *et al.* (2001)	0.48	0.50	2,250
US AID$_{t-6,t-10}$	Net ODA inflows from the US as a share of total ODA inflows	OECD, DAC Aid statistics	0.21	0.21	1,907
MOVEMENT$_{t-6,t-10}$	Change in the average alignment score with G7 countries between $t-6$ and $t-10$	Kilby (2009a)	0.00	0.02	2,246
GDP PC	Logarithm of real GDP per capita, in constant USD	World Development Indicators	6.79	1.08	2,248
GDP GROWTH	Annual growth rate of real GDP	World Development Indicators	0.02	0.04	2,239
SHORT TERM DEBT	Short-term debt (% of total external debt)	World Development Indicators	0.11	0.10	2,246
CREDIT GROWTH	Change in domestic credit to private sector (% of GDP) between t and $t-1$	World Development Indicators	0.04	0.16	2,225
PRIVATE CREDIT	Domestic credit to private sector (% of GDP)	World Development Indicators	0.26	0.20	2,181
GDP DEFLATOR	GDP deflator	World Development Indicators	0.14	0.31	2,226
DEPRECIATION	Rate of change of the nominal official exchange rate (for the US this is the rate of change of the nominal effective exchange rate)	World Development Indicators	0.12	0.32	2,207
M2/RESERVE	Money and quasi money (M2) to total reserves ratio	World Development Indicators	0.07	0.17	2,236
OPENNESS	Merchandise trade (% GDP)	World Development Indicators	0.56	0.28	2,243
PORTFOLIO LIABILITIES	Portfolio equity liabilities (% of GDP)	Lane and Milesi-Ferretti (2007, updated data set)	0.01	0.03	2,233
DEBT LIABILITIES	Debt liabilities (% of GDP)	Lane and Milesi-Ferretti (2007, updated data set)	0.65	0.47	2,246
FDI LIABILITIES	Foreign direct investment liabilities (% of GDP)	Lane and Milesi-Ferretti (2007, updated data set)	0.25	0.26	2,247
COUNTRY RISK	The Composite Index published by the International Country Risk Guide (ICRG)	The PRS Group	61.54	10.11	1,580
SOUND MONEY	An index consisting of the following indicators: (i) Average annual growth of the money supply in the last five years minus average, (ii) annual growth of real GDP in the last ten years, (iii) Standard inflation variability in the last five years, (iv) Recent inflation rate, and (v) Freedom to own foreign currency bank accounts domestically and abroad	Fraser Institute, Gwartney *et al.* (2012), and Teorell *et al.* (2011)	6.29	1.99	1,287
POLITY	Polity score, based on six measures that record key qualities of executive recruitment, constraints on executive authority, and political competition. The index is part of the Polity IV project and ranges from -10 (hereditary monarchy) to +10 (consolidated democracy).	Marshall *et al.* (2010) and Teorell *et al.* (2011)	1.30	6.56	1,928
DEMOCRACY	Dummy variable coded 1 if the regime qualifies as democratic	Cheibub *et al.* (2010)	0.47	0.50	2,070
DEPOSIT INSURANCE	Dummy equal to one for countries that adopted explicit deposit insurance systems in year t and zero otherwise	Demirguc-Kunt *et al.* (2008)	0.21	0.41	1,600
FINANCIAL OPENNESS	The Chinn-Ito *de jure* measure of financial openness	Chinn and Ito (2010)	-0.29	1.37	2,225
FINANCIAL REFORMS	An index of financial liberalization, based on eight dimensions and scaled between 0 (fully repressed) and 1 (fully liberalized)	Abiad *et al.* (2008)	0.50	0.25	947
REAL INTEREST RATE	Real interest rate as the nominal interest rate (according to data availability, Treasury Bill rate, discount rate, or deposit rate) minus the GDP deflator.	International Financial Statistics and World Development Indicators	0.01	0.15	2,104
DEBT SERVICE	Total debt service (% of exports of goods, services and primary income)	World Development Indicators	17.16	13.80	2,109
CURRENT ACCOUNT	Current account balance % GDP)	World Development Indicators	2.27	6.98	1,390

Notes: Summary statistics are calculated on the sample of 113 countries used in the empirical analysis (see Table 4).

Table 3: IMF lending arrangements and banking crises

Panel A: Banking crises and and previous financial crises

	Conditional on:			
Probability of a banking crisis:	Banking crises	Sovereign crises	Currency crises	Any crises
conditional on no past crisis	3.25	2.76	2.61	2.48
conditional on past crises	1.82	4.82	3.84	3.7
T-test	0.060*	0.021**	0.047**	0.037**

Panel B: Banking crises and IMF-supported programs

	No program		IMF program		Whole sample		T-test
	Obs.	%	Obs.	%	Obs.	%	on means
Whole sample							
Tranquil year	3,673	98.03	652	95.04	4,325	97.56	
Crisis year	74	1.97	34	4.96	108	2.44	0.000***
Total	3,747	100	686	100	4,433	100	
Middle-income countries							
Tranquil year	2,482	98.03	388	94.63	2,870	97.55	
Crisis year	50	1.97	22	5.37	72	2.45	0.000***
Total	2,532	100	410	100	2,942	100	
Low-income countries							
Tranquil year	1,191	98.02	264	95.65	1,455	97.59	
Crisis year	24	1.98	12	4.35	36	2.41	0.090*
Total	1,215	100	276	100	1,491	100	

Notes: Statistics based on the sample of 113 countries included in the model, over 1970-2010. The last column reports the p-values of the two-tail mean comparison test on the probability of systemic banking crisis in country-year observations in which an IMF program has been agreed or not.

Table 4: Baseline regressions

Dep Var: Prob(BANKING CRISIS$_t$)	(1) Baseline	(2)	(3)	(6) pre-GFC	(7)	(8) IMF loan size	(9)	(8) Bootstrapped s.e.	(9)
	OLS	1^{st}-stage	2SLS	1^{st}-stage	2SLS	1^{st}-stage	2SLS	1^{st}-stage	2SLS
IMF ARRANGEMENT$_{t-1,t-5}$	0.010		-0.076**		-0.110***				-0.076**
	[0.009]		[0.035]		[0.041]				[0.034]
IMF LOAN/GDP$_{t-1,t-5}$							-0.064**		
							[0.028]		
GDP PC$_{t-1}$	0.002	-0.077***	-0.003	-0.063**	-0.005	-0.160***	-0.007	-0.067***	-0.003
	[0.004]	[0.023]	[0.005]	[0.025]	[0.006]	[0.041]	[0.007]	[0.010]	[0.005]
GDP GROWTH$_{t-1}$	-0.153*	0.070	-0.151*	0.333	-0.201*	-0.476	-0.186**	0.312	-0.151*
	[0.086]	[0.305]	[0.091]	[0.309]	[0.104]	[0.478]	[0.094]	[0.225]	[0.086]
SHORT TERM DEBT$_{t-1}$	0.071*	-0.679***	0.029	-0.791***	-0.003	-1.489***	-0.014	-0.643***	0.029
	[0.041]	[0.225]	[0.052]	[0.247]	[0.063]	[0.342]	[0.060]	[0.104]	[0.045]
CREDIT GROWTH$_{t-1}$	0.034*	0.013	0.038**	-0.013	0.032	-0.023	0.037*	0.012	0.038*
	[0.018]	[0.057]	[0.018]	[0.056]	[0.020]	[0.101]	[0.019]	[0.058]	[0.020]
DEPRECIATION$_{t-1}$	0.009	-0.006	0.006	-0.006	0.003	-0.027	0.005	0.024	0.006
	[0.008]	[0.025]	[0.009]	[0.025]	[0.009]	[0.055]	[0.009]	[0.027]	[0.013]
M2/RESERVE$_{t-1}$	0.010	-0.144	-0.005	-0.129	-0.010	-0.261*	-0.012	-0.162***	-0.005
	[0.023]	[0.101]	[0.029]	[0.103]	[0.032]	[0.136]	[0.029]	[0.056]	[0.025]
OPENNESS$_{t-1}$	-0.033***	-0.130	-0.046***	-0.118	-0.055***	0.029	-0.032**	-0.090**	-0.046***
	[0.013]	[0.083]	[0.015]	[0.086]	[0.018]	[0.149]	[0.015]	[0.037]	[0.013]
PORTFOLIO LIABILITIES$_{t-1}$	0.032	-0.103	0.017	0.169	0.061	0.679	0.071	-0.219	0.017
	[0.096]	[0.725]	[0.109]	[0.835]	[0.149]	[1.080]	[0.117]	[0.344]	[0.125]
DEBT LIABILITIES$_{t-1}$	0.007	0.136***	0.016*	0.160***	0.017	0.436***	0.033**	0.117***	0.016*
	[0.007]	[0.043]	[0.010]	[0.045]	[0.012]	[0.075]	[0.016]	[0.023]	[0.009]
FDI LIABILITIES$_{t-1}$	-0.035***	-0.060	-0.036**	-0.116	-0.041**	0.019	-0.031*	-0.086**	-0.036***
	[0.010]	[0.082]	[0.014]	[0.088]	[0.019]	[0.157]	[0.019]	[0.042]	[0.011]
BANKING CRISIS$_{t-1,t-8}$	-0.025***	0.185***	-0.010	0.181***	-0.005	0.288***	-0.006	0.185***	-0.01
	[0.007]	[0.041]	[0.009]	[0.041]	[0.010]	[0.067]	[0.010]	[0.023]	[0.009]
SOVEREIGN CRISIS$_{t-1,t-8}$	0.006	0.180***	0.024*	0.171***	0.031**	0.478***	0.043**	0.178***	0.024*
	[0.012]	[0.049]	[0.014]	[0.049]	[0.015]	[0.095]	[0.018]	[0.031]	[0.014]
CURRENCY CRISIS$_{t-1,t-8}$	-0.003	0.137***	0.012	0.124***	0.019	0.353***	0.024	0.166***	0.012
	[0.008]	[0.035]	[0.011]	[0.036]	[0.012]	[0.068]	[0.015]	[0.022]	[0.011]
1^{st}-stage coefficients on instruments									
PROXIMITY$_{t-6,t-10}$		1.113***		1.092***		1.764***		0.791***	
		[0.295]		[0.303]		[0.421]		[0.150]	
ELECTION$_{t-6,t-10}$		0.155***		0.146***		0.117**		0.139***	
		[0.038]		[0.040]		[0.057]		[0.019]	
Observations	2,250	2,250	2,250	1,980	1,980	2,250	2,250	2,250	2,250
K-P weak identification (F-test)			18.552		15.072		12.859		
C-D weak identification (F-test)									72.449
K-P underidentification (p-value)			0.000		0.000		0.000		0.000
Overidentification test (p-value)			0.650		0.284		0.852		0.622

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. Columns 8 and 9 report bootstrapped (2000 repetitions) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification (this is replaced by the Cragg-Donald statistic for estimates with bootstrapped standard errors); 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Table 5: The credit channel

Dep. Var.:	**Loan size (as a share of GDP)**					
Prob(BANKING CRISIS$_t$)	(1) < 1%	(2) >= 1%	(3) < 2%	(4) >= 2%	(5) < 3%	(6) >= 3%
IMF ARRANGEMENT$_{t-1,t-5}$	-0.027 [0.045]	-0.118*** [0.042]	-0.045 [0.032]	-0.134** [0.059]	-0.063* [0.037]	-0.110* [0.056]
1^{st}-stage coefficients on instruments						
PROXIMITY$_{t-6,t-10}$	1.227*** [0.359]	0.919*** [0.315]	1.232*** [0.385]	0.824** [0.340]	1.289*** [0.367]	0.721** [0.354]
ELECTION$_{t-6,t-10}$	0.168*** [0.052]	0.156*** [0.039]	0.228*** [0.051]	0.112*** [0.041]	0.196*** [0.048]	0.120*** [0.042]
Observations	1,093	1,911	1,374	1,630	1,612	1,392
K-P weak identification (F-test)	16.836	13.739	19.076	7.615	16.952	7.311
K-P underidentification (p-value)	0.000	0.000	0.000	0.003	0.000	0.003
Overidentification test (p-value)	0.994	0.272	0.759	0.294	0.991	0.320

	(7) < 4%	(8) >= 4%	(9) < 5%	(10) >= 5%	(11) < 6%	(12) >= 6%
IMF ARRANGEMENT$_{t-1,t-5}$	-0.068* [0.038]	-0.102* [0.058]	-0.067* [0.037]	-0.124* [0.075]	-0.067* [0.037]	-0.120 [0.081]
1^{st}-stage coefficients on instruments						
PROXIMITY$_{t-6,t-10}$	1.078*** [0.368]	1.003*** [0.360]	0.999*** [0.345]	1.149*** [0.352]	1.047*** [0.334]	1.079*** [0.364]
ELECTION$_{t-6,t-10}$	0.193*** [0.045]	0.093** [0.044]	0.187*** [0.043]	0.047 [0.042]	0.181*** [0.041]	0.033 [0.043]
Observations	1,765	1,239	1,911	1,093	1,984	1,020
K-P weak identification (F-test)	16.635	6.999	16.469	6.879	17.085	5.426
K-P underidentification (p-value)	0.000	0.007	0.000	0.005	0.000	0.014
Overidentification test (p-value)	0.510	0.980	0.401	0.555	0.363	0.332

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include the standard set of control variables, as in the baseline specification of Table 4 excluding 2008-2010 (columns 4-5). The coefficients on those variables are not reported for reasons of space. We report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments).

Table 6: The reform channel

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Compliance with conditionality				World Bank adjustment loans			
	2SLS	LIML	2SLS	LIML	2SLS	LIML	2SLS	LIML
IMF ARRANGEMENT$_{t-1,t-5}$	-0.206**	-0.206***			-0.117*	-0.121*		
	[0.094]	[0.079]			[0.066]	[0.067]		
IMF LOAN/GDP$_{t-1,t-5}$			-0.112*	-0.121**				
			[0.057]	[0.055]				
WB LENDING$_{t-1,t-5}$							-0.152**	-0.154**
							[0.075]	[0.077]
NON COMPLIANCE$_{t-1,t-5}$	0.132**	0.132***	0.086**	0.092**				
	[0.058]	[0.047]	[0.041]	[0.037]				
GDP PC$_{t-1}$	-0.009	-0.009	-0.013	-0.014*	-0.005	-0.005	-0.004	-0.005
	[0.007]	[0.006]	[0.010]	[0.008]	[0.007]	[0.006]	[0.007]	[0.006]
GDP GROWTH$_{t-1}$	-0.092	-0.092	-0.174*	-0.176*	-0.205*	-0.204*	-0.083	-0.081
	[0.099]	[0.094]	[0.095]	[0.093]	[0.118]	[0.114]	[0.138]	[0.137]
SHORT TERM DEBT$_{t-1}$	-0.003	-0.003	-0.052	-0.062	-0.018	-0.022	-0.065	-0.067
	[0.063]	[0.050]	[0.080]	[0.073]	[0.083]	[0.079]	[0.094]	[0.092]
CREDIT GROWTH$_{t-1}$	0.050**	0.050**	0.041**	0.042*	0.036	0.036	0.045*	0.045
	[0.021]	[0.025]	[0.021]	[0.024]	[0.023]	[0.029]	[0.023]	[0.030]
DEPRECIATION$_{t-1}$	0.007	0.007	0.007	0.007	0.001	0.000	-0.003	-0.003
	[0.010]	[0.012]	[0.010]	[0.012]	[0.009]	[0.013]	[0.009]	[0.014]
M2/RESERVE$_{t-1}$	-0.023	-0.023	-0.023	-0.026	-0.009	-0.010	-0.016	-0.017
	[0.033]	[0.026]	[0.031]	[0.028]	[0.034]	[0.027]	[0.036]	[0.029]
OPENNESS$_{t-1}$	-0.066***	-0.066***	-0.031*	-0.031**	-0.061***	-0.062***	-0.082***	-0.082***
	[0.023]	[0.019]	[0.018]	[0.015]	[0.022]	[0.022]	[0.030]	[0.028]
PORTFOLIO LIABILITIES$_{t-1}$	0.001	0.001	0.104	0.110	0.349	0.354	0.710*	0.718**
	[0.131]	[0.147]	[0.135]	[0.153]	[0.271]	[0.245]	[0.398]	[0.350]
DEBT LIABILITIES$_{t-1}$	0.027**	0.027**	0.050**	0.053**	0.019	0.019	0.039*	0.039*
	[0.013]	[0.012]	[0.025]	[0.022]	[0.014]	[0.014]	[0.021]	[0.022]
FDI LIABILITIES$_{t-1}$	-0.026	-0.026	-0.019	-0.018	-0.043*	-0.044*	-0.041*	-0.041*
	[0.021]	[0.017]	[0.028]	[0.018]	[0.022]	[0.024]	[0.022]	[0.024]
BANKING CRISIS$_{t-1,t-8}$	-0.010	-0.010	-0.008	-0.006	-0.008	-0.007	0.008	0.008
	[0.011]	[0.012]	[0.013]	[0.013]	[0.014]	[0.017]	[0.021]	[0.022]
SOVEREIGN CRISIS$_{t-1,t-8}$	0.019	0.019	0.046*	0.050**	0.034*	0.035*	0.035*	0.035*
	[0.014]	[0.014]	[0.024]	[0.024]	[0.020]	[0.020]	[0.021]	[0.020]
CURRENCY CRISIS$_{t-1,t-8}$	0.013	0.013	0.028	0.031*	0.019	0.020	0.017	0.018
	[0.013]	[0.011]	[0.019]	[0.018]	[0.017]	[0.016]	[0.017]	[0.014]
1^{st}-stage coefficients on instruments								
PROXIMITY$_{t-6,t-10}$	0.614**	0.614***	1.191***	1.191***				
	[0.250]	[0.119]	[0.410]	[0.219]				
ELECTION$_{t-6,t-10}$	0.055*	0.055***	0.002	0.002	0.129***	0.129***	0.089**	0.089***
	[0.031]	[0.015]	[0.056]	[0.028]	[0.043]	[0.022]	[0.045]	[0.023]
MOVEMENT$_{t-6,t-10}$					1.771***	1.771***	2.255***	2.255***
					[0.667]	[0.516]	[0.667]	[0.522]
Observations	2,250	2,250	2,250	2,250	1,678	1,678	1,678	1,678
K-P weak identification (F-test)	6.167		4.278		8.343		8.803	
C-D weak identification (F-test)		22.044		15.000		23.836		18.287
K-P underidentification (p-value)	0.006	0.000	0.026	0.000	0.002	0.000	0.006	0.000
Overidentification test (p-value)	0.933	0.910	0.260	0.145	0.175	0.218	0.526	0.495

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification (this is replaced by the Cragg-Donald statistic for LIML estimates); 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions (for the LIML estimates we report the Anderson-Rubin overidentification test), testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Table 7: IMF programs which do not follow financial crises

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2)	(3)	(4)
	IMF lending arrangements conditional on no past:			
	Financial crises	Banking crises	Currency crises	Sovereign crises
IMF ARRANGEMENT$_{t-1,t-5}$	-0.080**	-0.086**	-0.082**	-0.069**
	[0.037]	[0.038]	[0.038]	[0.032]
GDP PC$_{t-1}$	-0.004	-0.006	-0.003	-0.004
	[0.005]	[0.005]	[0.005]	[0.005]
GDP GROWTH$_{t-1}$	-0.139	-0.125	-0.150*	-0.172*
	[0.085]	[0.089]	[0.091]	[0.089]
SHORT TERM DEBT$_{t-1}$	0.060	0.040	0.048	0.063
	[0.051]	[0.056]	[0.050]	[0.048]
CREDIT GROWTH$_{t-1}$	0.049**	0.038*	0.042**	0.041**
	[0.020]	[0.020]	[0.020]	[0.019]
DEPRECIATION$_{t-1}$	0.007	0.007	0.003	-0.005
	[0.010]	[0.012]	[0.009]	[0.008]
M2/RESERVE$_{t-1}$	-0.013	-0.010	-0.016	-0.012
	[0.030]	[0.031]	[0.028]	[0.034]
OPENNESS$_{t-1}$	-0.041***	-0.044***	-0.045***	-0.039***
	[0.014]	[0.014]	[0.016]	[0.014]
PORTFOLIO LIABILITIES$_{t-1}$	0.072	0.030	0.037	0.020
	[0.114]	[0.117]	[0.114]	[0.104]
DEBT LIABILITIES$_{t-1}$	0.017*	0.016	0.020**	0.018*
	[0.010]	[0.011]	[0.010]	[0.010]
FDI LIABILITIES$_{t-1}$	-0.031**	-0.033**	-0.038***	-0.037***
	[0.014]	[0.015]	[0.015]	[0.014]
BANKING CRISIS$_{t-1,t-8}$	-0.035***	-0.055***	-0.017**	-0.009
	[0.010]	[0.016]	[0.009]	[0.009]
SOVEREIGN CRISIS$_{t-1,t-8}$	-0.017	0.010	-0.031**	0.012
	[0.011]	[0.010]	[0.015]	[0.011]
CURRENCY CRISIS$_{t-1,t-8}$	-0.005	0.030*	0.019	-0.034
	[0.016]	[0.016]	[0.016]	[0.022]
1^{st}-stage coefficients on instruments				
PROXIMITY$_{t-6,t-10}$	1.325***	1.164***	1.062***	1.266***
	[0.255]	[0.320]	[0.272]	[0.304]
ELECTION$_{t-6,t-10}$	0.139***	0.149***	0.168***	0.163***
	[0.036]	[0.041]	[0.038]	[0.039]
Observations	2,061	2,140	2,098	2,147
K-P weak identification (F-test)	24.539	16.622	21.896	20.680
K-P underidentification (p-value)	0.000	0.000	0.000	0.000
Overidentification test (p-value)	0.969	0.752	0.792	0.946

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Table 8: Additional control variables

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IMF ARRANGEMENT$_{t-1,t-5}$	-0.133*	-0.128*	-0.160*	-0.124*	-0.082**	-0.076**	-0.079*	-0.099**	-0.047*	-0.074**	-0.285*	-0.144**
	[0.076]	[0.069]	[0.089]	[0.068]	[0.041]	[0.035]	[0.044]	[0.039]	[0.027]	[0.037]	[0.156]	[0.070]
ECA	0.069*											
	[0.041]											
LAC	0.029											
	[0.027]											
MENA	-0.030											
	[0.019]											
SSA	0.017											
	[0.018]											
COUNTRY RISK$_{t-1}$		-0.001										
		[0.001]										
SOUND MONEY$_{t-1}$			-0.013***									
			[0.005]									
POLITY$_{t-1}$				0.001								
				[0.001]								
DEMOCRACY$_{t-1}$					0.008							
					[0.010]							
GDP DEFLATOR$_{t-1}$						0.021						
						[0.025]						
PRIVATE CREDIT$_{t-1}$							0.002					
							[0.033]					
DEBT SERVICE$_{t-1}$								0.135***				
								[0.048]				
CURRENT ACCOUNT$_{t-1}$									0.001			
									[0.035]			
FINANCIAL OPENNESS$_{t-1}$										-0.001		
										[0.003]		
FINANCIAL REFORMS$_{t-1}$											0.026	
											[0.068]	
DEPOSIT INSURANCE$_{t-1}$												0.031*
												[0.018]
1^{st}-stage coefficients on instruments												
PROXIMITY$_{t-6,t-10}$	0.305	1.081***	0.795**	0.786**	1.019***	1.134***	1.054***	1.004***	1.441***	1.145***	0.785**	0.897***
	[0.305]	[0.326]	[0.369]	[0.305]	[0.288]	[0.291]	[0.306]	[0.285]	[0.278]	[0.296]	[0.329]	[0.334]
ELECTION$_{t-6,t-10}$	0.123***	0.086*	0.117**	0.115***	0.139***	0.157***	0.135***	0.149***	0.150***	0.153***	0.065	0.115***
	[0.034]	[0.048]	[0.045]	[0.042]	[0.038]	[0.038]	[0.040]	[0.039]	[0.047]	[0.041]	[0.052]	[0.043]
Observations	2,250	1,530	1,355	2,013	2,162	2,236	2,197	2,113	1,358	2,222	997	1,682
K-P weak identification (F-test)	7.075	7.732	5.538	7.199	15.000	19.682	11.862	16.278	26.187	17.135	3.536	6.756
K-P underidentification (p-value)	0.003	0.005	0.013	0.004	0.000	0.000	0.000	0.000	0.000	0.000	0.057	0.004
Overidentification test (p-value)	0.044	0.837	0.254	0.640	0.638	0.614	0.611	0.234	0.738	0.630	0.824	0.385

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include the standard set of control variables, as in the baseline specification of Table 4 (columns 2-3). In column (1) the excluded geographical region is "East Asia & Pacific". The other regions are: "Europe & Central Asia" (ECA), "Latin America & Caribbean" (LAC), "Middle East & North Africa" (MENA), and "Sub-Saharan Africa" (SSA). At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Table 9: Sample splits according to the institutional setting

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	COUNTRY RISK		SOUND MONEY		POLITY		DEMOCRACY	
	Low	High	Low	High	Low	High	No	Yes
IMF ARRANGEMENT$_{t-1,t-5}$	-0.146	-0.135**	-1.091	-0.124*	-0.142	-0.123**	-0.240	-0.062
	[0.258]	[0.061]	[1.793]	[0.074]	[0.199]	[0.060]	[0.205]	[0.039]
GDP PC$_{t-1}$	0.012	-0.000	-0.005	0.007	-0.013	0.003	-0.027	0.010
	[0.014]	[0.006]	[0.036]	[0.014]	[0.015]	[0.007]	[0.019]	[0.006]
GDP GROWTH$_{t-1}$	-0.255	0.005	-0.203	-0.200	-0.300	-0.051	-0.396**	-0.037
	[0.234]	[0.110]	[0.531]	[0.218]	[0.194]	[0.130]	[0.192]	[0.121]
SHORT TERM DEBT$_{t-1}$	-0.235	-0.042	-0.949	-0.025	-0.112	0.090	-0.138	0.063
	[0.529]	[0.056]	[1.833]	[0.112]	[0.224]	[0.077]	[0.205]	[0.070]
CREDIT GROWTH$_{t-1}$	0.066	0.070**	-0.020	0.103**	-0.006	0.071**	0.002	0.074**
	[0.042]	[0.034]	[0.112]	[0.052]	[0.034]	[0.028]	[0.035]	[0.031]
DEPRECIATION$_{t-1}$	0.014	-0.005	-0.018	-0.055	0.003	0.014	0.005	0.013
	[0.018]	[0.023]	[0.049]	[0.052]	[0.016]	[0.009]	[0.017]	[0.009]
M2/RESERVE$_{t-1}$	0.049	-0.082**	-0.091	0.009	0.009	0.032	0.006	0.010
	[0.050]	[0.036]	[0.174]	[0.096]	[0.039]	[0.077]	[0.046]	[0.076]
OPENNESS$_{t-1}$	-0.024	-0.058*	-0.187	-0.083*	-0.066	-0.044*	-0.080	-0.043*
	[0.050]	[0.033]	[0.214]	[0.050]	[0.061]	[0.026]	[0.052]	[0.023]
PORTFOLIO LIABILITIES$_{t-1}$	-0.146	-0.055	4.231	0.206	0.274	-0.162	0.208	-0.036
	[0.585]	[0.162]	[6.119]	[0.293]	[0.439]	[0.183]	[0.380]	[0.163]
DEBT LIABILITIES$_{t-1}$	0.009	0.052*	0.106	0.061	0.003	0.044**	0.017	0.035**
	[0.015]	[0.027]	[0.178]	[0.039]	[0.027]	[0.018]	[0.027]	[0.017]
FDI LIABILITIES$_{t-1}$	-0.009	-0.026	-0.243	-0.063*	-0.007	-0.030	0.031	-0.090***
	[0.032]	[0.029]	[0.490]	[0.035]	[0.060]	[0.030]	[0.052]	[0.025]
FINANCIAL CRISES$_{t-1,t-8}$	0.002	0.033*	0.314	0.022	0.031	0.014	0.058	-0.010
	[0.062]	[0.019]	[0.564]	[0.024]	[0.055]	[0.020]	[0.053]	[0.019]
1^{st}-stage coefficients on instruments								
PROXIMITY$_{t-6,t-10}$	0.708	1.187***	0.284	1.120**	0.207	1.038***	0.202	1.353***
	[0.503]	[0.366]	[0.447]	[0.499]	[0.602]	[0.293]	[0.519]	[0.263]
ELECTION$_{t-6,t-10}$	0.041	0.096	0.007	0.165**	0.070	0.146***	0.077	0.166***
	[0.048]	[0.060]	[0.040]	[0.075]	[0.057]	[0.047]	[0.055]	[0.045]
Observations	743	837	624	663	830	1,098	1,098	972
K-P weak identification (F-test)	1.183	8.319	0.209	5.901	0.887	10.080	1.179	21.179
K-P underidentification (p-value)	0.324	0.007	0.811	0.010	0.408	0.001	0.306	0.000
Overidentification test (p-value)	0.332	0.824	0.983	0.824	0.601	0.662	0.521	0.515

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed. The sample is split according to country year observations for which the institutional indicator is below or above the sample median (columns 1-6) and according to the dummy identifying democracies and autocracies (column 7-8). LIML estimates confirm these findings and are available upon request.

Table 10: The *US AID* instrument and sub-samples

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2) Additional instrument	(3) Crisis	(4) Middle-income	(5) Arrangement type	(6)
	1^{st}-stage	2SLS	2SLS	2SLS	GRA	PRGT & PRGF
IMF ARRANGEMENT$_{t-1,t-5}$		-0.066**	-0.187**	-0.058*	-0.075**	-0.073**
		[0.033]	[0.077]	[0.035]	[0.035]	[0.034]
GDP PC$_{t-1}$	-0.081***	-0.004	-0.002	0.005	-0.003	-0.004
	[0.028]	[0.005]	[0.008]	[0.006]	[0.005]	[0.005]
GDP GROWTH$_{t-1}$	-0.041	-0.228**	-0.163	-0.026	-0.154*	-0.158*
	[0.322]	[0.106]	[0.153]	[0.083]	[0.091]	[0.086]
SHORT TERM DEBT$_{t-1}$	-0.968***	0.039	-0.039	0.018	0.037	-0.016
	[0.265]	[0.059]	[0.090]	[0.052]	[0.053]	[0.038]
CREDIT GROWTH$_{t-1}$	0.000	0.045**	0.070**	0.042**	0.028	0.040**
	[0.060]	[0.020]	[0.029]	[0.021]	[0.019]	[0.019]
DEPRECIATION$_{t-1}$	-0.040	-0.002	0.002	0.005	0.006	0.001
	[0.031]	[0.010]	[0.010]	[0.009]	[0.009]	[0.009]
M2/RESERVE$_{t-1}$	-0.096	0.021	0.031	-0.007	-0.009	-0.012
	[0.107]	[0.031]	[0.048]	[0.031]	[0.030]	[0.025]
OPENNESS$_{t-1}$	-0.136	-0.042***	-0.078**	-0.038**	-0.042***	-0.043***
	[0.086]	[0.014]	[0.033]	[0.017]	[0.015]	[0.015]
PORTFOLIO LIABILITIES$_{t-1}$	0.463	0.035	-0.304	0.056	0.023	0.05
	[0.747]	[0.115]	[0.189]	[0.107]	[0.110]	[0.119]
DEBT LIABILITIES$_{t-1}$	0.123***	0.013	0.031*	0.019	0.012	0.013
	[0.044]	[0.010]	[0.017]	[0.013]	[0.010]	[0.009]
FDI LIABILITIES$_{t-1}$	0.011	-0.030**	-0.021	-0.044***	-0.040***	-0.024*
	[0.079]	[0.015]	[0.031]	[0.015]	[0.013]	[0.013]
BANKING CRISIS$_{t-1,t-8}$	0.168***	-0.010	-0.013	-0.015	-0.01	0.001
	[0.041]	[0.008]	[0.014]	[0.011]	[0.009]	[0.010]
SOVEREIGN CRISIS$_{t-1,t-8}$	0.190***	0.016	0.027	0.021	0.025*	0.005
	[0.054]	[0.013]	[0.018]	[0.015]	[0.014]	[0.014]
CURRENCY CRISIS$_{t-1,t-8}$	0.116***	0.008	0.027	0.006	0.012	0.018
	[0.035]	[0.010]	[0.018]	[0.012]	[0.011]	[0.011]
1^{st}-stage coefficients on instruments						
PROXIMITY$_{t-6,t-10}$	0.902**		0.890***	1.311***	1.120***	1.107***
	[0.388]		[0.316]	[0.340]	[0.300]	[0.304]
ELECTION$_{t-6,t-10}$	0.181***		0.128***	0.159***	0.158***	0.162***
	[0.041]		[0.043]	[0.052]	[0.040]	[0.040]
US AID$_{t-6,t-10}$	0.262**					
	[0.102]					
Observations	1,907	1,907	1,626	1,463	2,104	2,028
K-P weak identification (F-test)		12.270	8.615	18.086	18.440	18.635
K-P underidentification (p-value)		0.000	0.003	0.000	0.000	0.000
Overidentification test (p-value)		0.680	0.526	0.870	0.821	0.584

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Table 11: Propensity score matching

		Matching algorithm		
		5-nearest neighbor	Radius	Kernel
IMF participation effect	ATT	-0.032	-0.039	-0.035
	s.e.	0.016	0.015	0.014
	t-Statistic	-2.030**	-2.570***	-2.490***
Matched group	Mean bias	4.400	4.100	4.200
	Median bias	4.400	3.700	3.800
	Pseudo-R^2	0.024	0.024	0.022
Unmatched group	Mean bias	19.500	19.500	19.500
	Median bias	16.700	16.700	16.700
	Pseudo-R^2	0.170	0.170	0.170
Observations	Treatment	994	994	969
	Control	469	469	469

Notes: The table reports the average treatment (IMF participation) effect on the treated (ATT) and the associated standard errors for three different matching algorithms: the "five nearest neighbors" with replacement, the "radius with caliper" (set at 0.1), and the "kernel". * significant at 10%; ** significant at 5%; *** significant at 1%. The following rows report: (1) the mean and the median of the (absolute) standardised percentage bias, and (2) the pseudo-R^2 of the probit estimation of the selection equation used to calculate the propensity score (see Table A3), for the matched and unmatched observations. The standardised percentage bias is calculated as the percentage difference of the sample means in the treated and control (matched or unmatched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and control observations; see Table A4 for a list of the standardised percentage bias for each variable.

Table 12: Different lag structure

Dep Var: Prob(BANKING CRISIS$_t$)	(1) 5-year	(2) 3-year	(3) 4-year	(4) 6-year	(5) 7-year
IMF ARRANGEMENT$_{t-1,t-5}$	-0.076**				
	[0.035]				
IMF ARRANGEMENT$_{t-1,t-3}$		-0.062*			
		[0.037]			
IMF ARRANGEMENT$_{t-1,t-4}$			-0.072*		
			[0.037]		
IMF ARRANGEMENT$_{t-1,t-6}$				-0.049	
				[0.033]	
IMF ARRANGEMENT$_{t-1,t-7}$					-0.027
					[0.035]
GDP PC$_{t-1}$	-0.003	0.004	0.001	-0.002	0.002
	[0.005]	[0.005]	[0.005]	[0.005]	[0.005]
GDP GROWTH$_{t-1}$	-0.151*	-0.086	-0.105	-0.138	-0.143*
	[0.091]	[0.079]	[0.084]	[0.088]	[0.087]
SHORT TERM DEBT$_{t-1}$	0.029	0.028	0.020	0.039	0.022
	[0.052]	[0.050]	[0.052]	[0.048]	[0.040]
CREDIT GROWTH$_{t-1}$	0.038**	0.042**	0.047**	0.038**	0.047***
	[0.018]	[0.019]	[0.019]	[0.017]	[0.017]
DEPRECIATION$_{t-1}$	0.006	0.020*	0.015	0.010	0.013
	[0.009]	[0.012]	[0.012]	[0.008]	[0.010]
M2/RESERVE$_{t-1}$	-0.005	0.007	0.001	0.000	0.010
	[0.029]	[0.027]	[0.028]	[0.028]	[0.029]
OPENNESS$_{t-1}$	-0.046***	-0.048***	-0.048***	-0.038***	-0.031**
	[0.015]	[0.014]	[0.014]	[0.013]	[0.012]
PORTFOLIO LIABILITIES$_{t-1}$	0.017	-0.018	-0.007	0.041	0.074
	[0.109]	[0.104]	[0.107]	[0.101]	[0.101]
DEBT LIABILITIES$_{t-1}$	0.016*	0.016*	0.016*	0.016*	0.016**
	[0.010]	[0.009]	[0.010]	[0.009]	[0.008]
FDI LIABILITIES$_{t-1}$	-0.036**	-0.042***	-0.039***	-0.036***	-0.036***
	[0.014]	[0.013]	[0.014]	[0.013]	[0.011]
BANKING CRISIS$_{t-1,t-8}$	-0.010	-0.016*	-0.011	-0.013	-0.015*
	[0.009]	[0.009]	[0.009]	[0.009]	[0.009]
SOVEREIGN CRISIS$_{t-1,t-8}$	0.024*	0.015	0.020	0.017	0.013
	[0.014]	[0.015]	[0.015]	[0.013]	[0.014]
CURRENCY CRISIS$_{t-1,t-8}$	0.012	0.008	0.010	0.003	-0.000
	[0.011]	[0.010]	[0.011]	[0.009]	[0.009]
1^{st}-stage coefficients on instruments					
PROXIMITY$_{t-6,t-10}$	1.113***	1.062***	1.109***	1.065***	0.978***
	[0.295]	[0.228]	[0.269]	[0.296]	[0.294]
ELECTION$_{t-6,t-10}$	0.155***	0.299**	0.409***	0.603***	0.613***
	[0.038]	[0.134]	[0.144]	[0.149]	[0.156]
Observations	2,250	2,407	2,330	2,172	2,091
K-P weak identification (F-test)	18.552	14.896	14.785	17.349	15.185
K-P underidentification (p-value)	0.000	0.000	0.000	0.000	0.000
Overidentification test (p-value)	0.650	0.277	0.408	0.349	0.130

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Kleibergen-Paap rk Wald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Sargan test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed.

Appendix - intended for online publication only

Table A1: Sample

Country	Banking crises	IMF programs	LIC	Country	Banking crisis	IMF program	LIC
Albania	0	3	0	Lao PDR	0	2	1
Algeria	1	2	0	Latvia	1	3	0
Angola	0	1	0	Lebanon	0	0	0
Argentina	3	8	0	Lesotho	0	5	0
Armenia	0	4	0	Liberia	0	3	1
Azerbaijan	0	0	0	Lithuania	0	2	0
Bangladesh	1	5	1	Macedonia	0	3	0
Belarus	0	1	0	Madagascar	1	10	1
Belize	0	0	0	Malawi	0	9	1
Benin	1	5	1	Malaysia	0	0	0
Bhutan	0	0	0	Maldives	0	1	0
Bolivia	1	4	0	Mali	0	7	1
Bosnia and Herzegovina	0	2	0	Mauritania	1	6	1
Botswana	0	0	0	Mauritius	0	2	0
Brazil	0	3	0	Mexico	1	5	0
Bulgaria	1	3	0	Moldova	0	3	0
Burkina Faso	1	5	1	Mongolia	1	3	0
Burundi	1	4	1	Morocco	0	3	0
Cambodia	0	1	1	Mozambique	0	4	1
Cameroon	1	4	0	Nepal	1	4	1
Cape Verde	1	2	0	Nicaragua	1	3	0
Central African Republic	1	6	1	Niger	1	8	1
Chad	2	1	1	Nigeria	1	4	0
Chile	0	1	0	Pakistan	0	8	0
China	1	1	0	Panama	0	4	0
Colombia	2	4	0	Papua New Guinea	0	4	0
Comoros	0	2	1	Paraguay	1	2	0
Congo, Democratic Rep.	2	5	1	Peru	0	7	0
Congo, Republic Of	1	4	0	Philippines	2	4	0
Costa Rica	2	5	0	Romania	0	4	0
CÙte d'Ivoire	1	8	0	Russian Federation	0	1	0
Djibouti	0	3	0	Rwanda	0	4	1
Dominican Republic	1	7	0	Senegal	1	9	1
Egypt	0	4	0	Sierra Leone	1	5	1
El Salvador	1	8	0	Solomon Islands	0	1	0
Eritrea	0	0	1	South Africa	0	0	0
Ethiopia	0	2	1	Sri Lanka	1	5	0
Fiji	0	0	0	St. Lucia	0	0	0
Gabon	0	7	0	Sudan	0	3	0
Gambia	0	7	1	Swaziland	1	0	0
Georgia	0	3	0	Syrian Arab Republic	0	0	0
Ghana	1	8	1	Tajikistan	0	1	1
Grenada	0	2	0	Tanzania	0	5	1
Guatemala	0	6	0	Thailand	2	3	0
Guinea	1	1	1	Togo	1	7	1
Guinea-Bissau	1	3	1	Tunisia	1	2	0
Guyana	0	2	0	Turkey	2	4	0
Haiti	1	2	1	Uganda	1	4	1
Honduras	0	7	0	Ukraine	1	2	0
India	1	1	0	Uruguay	1	8	0
Indonesia	1	1	0	Vanuatu	0	0	0
Iran	0	0	0	Venezuela	1	1	0
Jamaica	1	7	0	Vietnam	1	1	1
Jordan	1	5	0	Yemen	0	2	1
Kazakhstan	1	1	0	Zambia	1	7	1
Kenya	2	8	1	Zimbabwe	0	0	1
Kyrgyz Republic	0	3	1	**Total**	**65**	**390**	38

Notes: For each country included in the sample used in the empirical analysis we list the number of banking crises and the number of IMF lending arrangements agreed between 1970 and 2010, and a binary indicator to identify low-income countries (LIC) from middle-income countries, as classified by the World Bank.

Table A2: Sample splits according to the institutional setting, LIML estimates

Dep Var: Prob(BANKING CRISIS$_t$)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	COUNTRY RISK		SOUND MONEY		POLITY		DEMOCRACY	
	Low	High	Low	High	Low	High	No	Yes
IMF ARRANGEMENT$_{t-1,t-5}$	-0.168	-0.135***	-1.092	-0.124*	-0.159	-0.123**	-0.269	-0.062
	[0.202]	[0.051]	[1.197]	[0.074]	[0.218]	[0.051]	[0.181]	[0.041]
GDP PC$_{t-1}$	0.011	-0.000	-0.005	0.007	-0.014	0.003	-0.029*	0.010
	[0.012]	[0.006]	[0.021]	[0.009]	[0.014]	[0.006]	[0.016]	[0.006]
GDP GROWTH$_{t-1}$	-0.247	0.005	-0.202	-0.200	-0.306*	-0.051	-0.409**	-0.036
	[0.215]	[0.139]	[0.421]	[0.215]	[0.182]	[0.144]	[0.168]	[0.149]
SHORT TERM DEBT$_{t-1}$	-0.277	-0.042	-0.949	-0.026	-0.129	0.090	-0.163	0.063
	[0.395]	[0.052]	[1.185]	[0.095]	[0.243]	[0.061]	[0.171]	[0.060]
CREDIT GROWTH$_{t-1}$	0.001	0.001**	-0.000	0.001*	-0.000	0.001**	-0.000	0.001*
	[0.001]	[0.000]	[0.001]	[0.001]	[0.000]	[0.000]	[0.000]	[0.000]
DEPRECIATION$_{t-1}$	0.015	-0.005	-0.018	-0.055	0.003	0.014	0.004	0.013
	[0.018]	[0.033]	[0.044]	[0.058]	[0.021]	[0.014]	[0.020]	[0.015]
M2/RESERVE$_{t-1}$	0.050	-0.082**	-0.091	0.009	0.008	0.032	0.005	0.010
	[0.043]	[0.041]	[0.140]	[0.051]	[0.033]	[0.057]	[0.031]	[0.061]
OPENNESS$_{t-1}$	-0.021	-0.058**	-0.187	-0.083**	-0.071	-0.044*	-0.087*	-0.043*
	[0.051]	[0.027]	[0.136]	[0.041]	[0.071]	[0.023]	[0.047]	[0.025]
PORTFOLIO LIABILITIES$_{t-1}$	-0.179	-0.055	4.233	0.206	0.264	-0.162	0.209	-0.036
	[0.584]	[0.152]	[4.372]	[0.272]	[0.422]	[0.184]	[0.301]	[0.199]
DEBT LIABILITIES$_{t-1}$	0.008	0.052**	0.106	0.061**	0.005	0.044***	0.019	0.035**
	[0.017]	[0.024]	[0.114]	[0.031]	[0.030]	[0.015]	[0.022]	[0.016]
FDI LIABILITIES$_{t-1}$	-0.007	-0.026	-0.243	-0.063	-0.003	-0.030	0.036	-0.090***
	[0.043]	[0.030]	[0.309]	[0.048]	[0.065]	[0.029]	[0.044]	[0.028]
FINANCIAL CRISES$_{t-1,t-8}$	0.007	0.033**	0.315	0.022	0.036	0.014	0.066	-0.010
	[0.049]	[0.017]	[0.378]	[0.022]	[0.061]	[0.017]	[0.048]	[0.017]
1^{st}-stage coefficients on instruments								
PROXIMITY$_{t-6,t-10}$	0.708**	1.187***	0.284	1.120***	0.207	1.038***	0.202	1.353***
	[0.279]	[0.211]	[0.278]	[0.313]	[0.316]	[0.190]	[0.272]	[0.186]
ELECTION$_{t-6,t-10}$	0.041	0.096***	0.007	0.165***	0.070**	0.146***	0.077***	0.166***
	[0.030]	[0.032]	[0.034]	[0.037]	[0.032]	[0.027]	[0.029]	[0.027]
Observations	743	837	624	663	830	1,098	1,098	972
C-D weak identification (F-test)	4.282	24.240	0.526	16.916	2.545	33.629	4.005	55.828
K-P underidentification (p-value)	0.013	0.000	0.584	0.000	0.076	0.000	0.018	0.000
Overidentification test (p-value)	0.319	0.788	0.980	0.834	0.503	0.663	0.355	0.504

Notes: The table reports the regression coefficients and, in brackets, the associated clustered (at country level) standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. At the bottom of the table we report the p-value of: 1) the Cragg-Donald F-statistic testing for weak identification; 2) the Kleibergen-Paap rk LM-statistic testing the null hypothesis that the excluded instruments are not correlated with the endogenous regressor; and 3) the Anderson-Rubin test of overidentifying restrictions, testing the null hypothesis that the excluded instruments are not correlated with the error term (i.e. they are valid instruments). A constant is included but not showed. The sample is split according to country year observations for which the institutional indicator is below or above the sample median (columns 1-6) and according to the dummy identifying democracies and autocracies (column 7-8). LIML estimates confirm these findings and are available upon request.

Table A3: PSM: probit selection equation

Dep Var: Prob(IMF ARRANGEMENT$_{t,t+4}$)	(1)
GDP PC$_{t-1}$	-0.124**
	[0.058]
GDP GROWTH$_{t-1}$	-1.193
	[0.877]
SHORT TERM DEBT$_{t-1}$	0.160
	[0.493]
CREDIT GROWTH$_{t-1}$	-0.005**
	[0.002]
PRIVATE CREDIT$_{t-1}$	1.333***
	[0.314]
DEPRECIATION$_{t-1}$	0.425**
	[0.170]
OPENNESS$_{t-1}$	-0.850***
	[0.149]
M2/RESERVE$_{t-1}$	0.170
	[0.240]
FINANCIAL OPENNESS$_{t-1}$	-0.095***
	[0.034]
REAL INTEREST RATE$_{t-1}$	1.071***
	[0.266]
PORTFOLIO LIABILITIES$_{t-1}$	-4.850**
	[2.145]
DEBT LIABILITIES$_{t-1}$	0.609***
	[0.095]
FDI LIABILITIES$_{t-1}$	-0.670***
	[0.205]
BANKING CRISIS$_{t-1,t-3}$	-0.001
	[0.106]
SOVEREIGN CRISIS$_{t-1,t-3}$	0.190*
	[0.106]
CURRENCY CRISIS$_{t-1,t-3}$	0.273
	[0.170]
POLITY$_{t-1}$	0.002
	[0.009]
DEMOCRACY$_{t-1}$	-0.237*
	[0.125]
DEPOSIT INSURANCE$_{t-1}$	0.021
	[0.104]
PROXIMITY$_{t-1,t-5}$	3.000***
	[0.817]
ELECTION$_{t-1,t-5}$	0.272***
	[0.082]
US AID$_{t-1,t-5}$	0.652***
	[0.235]
Observations	1,568

Notes: The table reports the regression coefficients and, in brackets, the associated standard errors. * significant at 10%; ** significant at 5%; *** significant at 1%. A constant and five regional dummies are included but not showed.

Table A4: PSM: sample characteristics of treatment and control group

	Treated	Unmatched control group				Matched control group											
						5-nearest neighbor				Radius with caliper				Kernel			
				$\mu_t = \mu_c$				$\mu_t = \mu_c N$				$\mu_t = \mu_c R$				$\mu_t = \mu_c K$	
	(μ_t)	(μ_c)	bias	t-stat	p-v	$(\mu_c N)$	bias	t-stat	p-v	$(\mu_c R)$	bias	t-stat	p-v	$(\mu_c K)$	bias	t-stat	p-v
GDP PC$_{t-1}$	6.43	6.73	-30.90	-5.44	0.00	6.33	9.20	1.96	0.05	6.33	9.70	2.08	0.04	6.35	7.70	1.66	0.10
GDP GROWTH$_{t-1}$	0.01	0.02	-27.60	-4.93	0.00	0.01	-1.90	-0.39	0.70	0.01	0.10	0.03	0.98	0.01	-3.10	-0.66	0.51
SHORT TERM DEBT$_{t-1}$	0.10	0.10	3.30	0.60	0.55	0.11	-6.30	-1.37	0.17	0.11	-6.80	-1.46	0.14	0.11	-6.60	-1.43	0.15
CREDIT GROWTH$_{t-1}$	1.89	3.84	-11.80	-2.08	0.04	1.89	1.30	0.27	0.78	2.16	-0.40	-0.08	0.94	1.94	1.00	0.21	0.83
PRIVATE CREDIT$_{t-1}$	0.20	0.23	-16.70	-3.08	0.00	0.20	-2.90	-0.69	0.49	0.20	-1.30	-0.32	0.75	0.20	1.00	0.25	0.81
DEPRECIATION$_{t-1}$	0.16	0.09	26.00	4.28	0.00	0.13	8.60	1.85	0.07	0.13	9.40	2.01	0.04	0.13	8.70	1.85	0.07
OPENNESS$_{t-1}$	0.49	0.59	-36.50	-6.78	0.00	0.47	4.30	1.05	0.30	0.47	4.60	1.12	0.26	0.47	6.50	1.61	0.11
M2/RESERVE$_{t-1}$	0.09	0.06	16.80	2.88	0.00	0.07	9.10	2.19	0.03	0.07	8.60	2.04	0.04	0.07	7.60	1.75	0.08
FINANCIAL OPENNESS$_{t-1}$	-0.54	-0.37	-14.40	-2.59	0.01	-0.62	7.00	1.66	0.10	-0.63	7.50	1.77	0.08	-0.63	7.80	1.83	0.07
REAL INTEREST RATE$_{t-1}$	0.01	0.00	2.50	0.41	0.68	0.00	0.50	0.10	0.92	0.00	0.90	0.19	0.85	0.00	-0.60	-0.12	0.90
PORTFOLIO LIABILITIES$_{t-1}$	0.00	0.01	-25.80	-5.02	0.00	0.01	-4.40	-1.21	0.23	0.01	-3.40	-0.93	0.35	0.01	-3.40	-0.94	0.35
DEBT LIABILITIES$_{t-1}$	0.77	0.55	44.80	7.80	0.00	0.74	0.30	0.07	0.95	0.75	-1.30	-0.26	0.80	0.74	0.60	0.11	0.91
FDI LIABILITIES$_{t-1}$	0.17	0.20	-14.70	-2.62	0.01	0.17	1.30	0.30	0.76	0.17	0.50	0.11	0.91	0.18	-1.30	-0.28	0.78
BANKING CRISIS$_{t-1,t-3}$	0.18	0.12	15.00	2.60	0.01	0.17	0.50	0.11	0.91	0.17	-0.70	-0.14	0.89	0.17	-1.20	-0.24	0.81
SOVEREIGN CRISIS$_{t-1,t-3}$	0.08	0.03	21.90	4.27	0.00	0.19	9.80	2.06	0.04	0.19	9.30	1.95	0.05	0.20	6.60	1.38	0.17
CURRENCY CRISIS$_{t-1,t-3}$	0.24	0.14	24.70	3.64	0.00	0.06	5.60	1.09	0.28	0.07	3.90	0.76	0.45	0.07	1.00	0.19	0.85
POLITY$_{t-1}$	0.06	0.39	-5.00	-0.91	0.37	0.29	-3.30	-0.75	0.45	0.14	-0.90	-0.21	0.83	0.11	-0.60	-0.13	0.90
DEMOCRACY$_{t-1}$	0.38	0.39	-1.80	-0.33	0.74	0.41	-5.00	-1.10	0.27	0.40	-4.00	-0.87	0.38	0.40	-4.90	-1.07	0.29
DEPOSIT INSURANCE$_{t-1}$	0.17	0.20	-6.20	-1.12	0.26	0.18	-1.60	-0.36	0.72	0.18	-1.70	-0.37	0.71	0.20	-5.20	-1.14	0.26
PROXIMITY$_{t-1,t-5}$	0.62	0.62	9.40	1.61	0.11	0.62	-0.40	-0.09	0.93	0.62	1.00	0.22	0.82	0.62	1.70	0.36	0.72
ELECTION$_{t-1,t-5}$	0.55	0.37	37.60	6.68	0.00	0.52	5.90	1.28	0.20	0.53	4.80	1.03	0.30	0.52	6.50	1.41	0.16
US AID$_{t-1,t-5}$	0.22	0.18	16.00	2.82	0.01	0.21	0.80	0.18	0.85	0.21	1.20	0.26	0.80	0.21	1.50	0.34	0.73

Notes: The table reports the mean of each variable for the treated observations (μ_t), the unmatched control group (μ_c) and three matched control groups, obtained using three different matching algorithms: the "five nearest neighbors" with replacement ($\mu_c N$), the "radius with caliper" (set at 0.1, $\mu_c R$), and the "kernel" ($\mu_c K$). For each control group, unmatched and matched ones, the table reports: (1) the standardised percentage bias, which is the percentage difference of the sample means in the treated and non-treated (unmatched or matched) sub-samples as a percentage of the square root of the average of the sample variances in the treated and control observations; and (2) the t-tests (and the associated p-values) for equality of means in the two samples.