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Stigma? What stigma?
A contribution to the debate on
financial market effects of IMF lending

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Abstract

In the policy debate on the effectiveness of the Global Financial Safety Net, concerns have been raised that expectations of adverse effects of IMF programmes may deter countries from asking for an IMF programme when they need one, a form of 'IMF stigma'. We explore the existence of IMF financial market stigma using monthly data by estimating how and to which extent adverse market reactions to a programme materialise and how past experience with adverse market reactions affects subsequent IMF programme participation. Our results, derived with event history techniques and propensity score matching, indicate no role for 'IMF stigma' stemming from the fear of adverse market movements. Instead, we find evidence of 'IMF recidivism' driven by adverse selection and IMF conditionality.

Non-technical summary

During the global financial crisis, the relevance of the Global Financial Safety Net (GFSN) with the IMF at its centre has strongly increased. The IMF, on its own and working with other stakeholders, has played a key role in addressing the fallout from the crisis. Notwithstanding these interventions, a recurrent argument in the debate on the international financial architecture is that in some cases the perception of stigma related to IMF lending still weighs on governments' decisions to approach the IMF in case of balance of payments needs. Any hesitation in turning to the IMF in case of crisis would impair the effectiveness of IMF lending as part of the GFSN.

IMF stigma refers to the discredit or taint that some countries feel they will attract by seeking IMF assistance. Countries fear that this taint will bring a backlash, either from the electorate (political stigma) or financial markets (financial market stigma). Several factors can be driving the perception of stigma. Stigma might be perceived by countries that attach a lot of importance on safeguarding their sovereignty and do not want to be seen as having to ask others for financing or do not want to accept conditions that they feel are imposed by others. Harsh or even excessive conditionality has also been cited as one reason for the perception of stigma, e.g. during the Asian crisis. The perception of stigma can also be linked to the perceived lack of evenhandedness in surveillance and lending and the underrepresentation of emerging markets in the IMF's governing bodies. Financial market backlash can occur in two cases: (i) if a country has to approach the IMF in view of vulnerabilities which up to that point were not fully publicly known; and (ii) if the design of a programme or its implementation does not convince financial markets.

The focus of this paper is on pinning down evidence of a possible financial market stigma. Our analysis proceeds in two steps. First, since previous studies are ambiguous on whether IMF programmes are catalytic, we investigate whether there is a negative

financial market reaction to IMF programmes which would constitute a reason for IMF financial market stigma to emerge. As we would like to capture those developments which might deter policy makers from approaching the IMF, we focus on sovereign refinancing costs. Second, we investigate whether negative past market reactions determine the future likelihood of governments asking for an IMF programme by comparing otherwise similar countries which experienced a different market reaction to IMF programmes in the past.

Our first step results suggest that in some cases there is a negative effect on short-term sovereign bonds, while in other cases there is a positive, catalytic effect of IMF programmes at least in the short run, i.e. for the duration of a programme. Our second step results indicate that neither a positive nor a negative financial market reaction have a significant impact on governments' decision to approach the IMF for a programme once we control for all other possible drivers of this decision. Among the most relevant drivers of agreeing on IMF programmes, we find that for repeated users of IMF resources particularly adverse selection and extensive (structural) conditionality play a role.

Given that IMF stigma has been mentioned in the international policy debate also as a regional phenomenon, we test whether a country's neighbours' past movements in sovereign rates in response to an IMF programme have a significant impact on the likelihood of approaching the IMF. We do not find evidence for such a regional phenomenon.

Overall, our results imply that the notion of a generalised (perception of) financial market stigma is overstated. Instead, invoking the notion of IMF financial market stigma might be used in individual cases by authorities struggling with transparency (i.e. concerns about revealing the country's economic situation to markets) or with a lack of ownership for unpopular reforms.

1 Introduction

It seems to be a generally accepted fact that announcements related to IMF programmes can trigger financial market reactions and possibly capital flows. While the overall effect and even sign of such reactions remain debated in the literature to this date, potential financial market reactions have recently been identified in the debate of international policy fora as a possible reason inhibiting the agreement on an IMF programme or the successful continuation of it. The idea behind such a notion of what in the following we will call financial market IMF stigma is that an IMF programme may send a negative signal to markets in that it reveals macroeconomic problems and thereby triggers an adverse financial market reaction instead of fostering confidence in the future growth path. Another reason for financial market reactions might be that market participants do not find the programme and its conditionality convincing.

Typically, the experience of the Asian economies during the Asian crisis is named as the longest-lasting example of such a deterring effect (e.g. Ito, 2012). The crisis started in Indonesia in 1997 and spread across South-East Asia. When it became clear that the recommended policies were not as successful as they were expected to be, countries experiencing the crisis and those linked to them financially or commercially experienced large capital outflows (Radelet and Sachs, 1998). No South-East Asian country has requested the IMF's assistance since this crisis. The fact that IMF programmes, particularly if they last long, may lead to a government crisis (Dreher and Gassebner, 2012), may have contributed to this. More recently, some authors (e.g. Alexiadou et al., 2015; Reinhart and Trebesch, 2015) have even suggested that the negative experience of some IMF debtors, particularly Greece, may lead to a new financial market stigma in that IMF programmes are considered as a generally negative signal not only about a country's growth path, but also about it's economic and political fundamentals.[1]

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[1] For a recent debate of IMF stigma related in particular to conditionality refer to Andone and Scheubel (2017).

However, there is no conclusive evidence supporting the notion of a general financial market stigma. As we discuss in the literature review in section 2, it is not even clear whether the financial market reaction to an IMF programme is positive (i.e. catalytic) or negative. Many authors argue in favour of a catalytic effect of IMF programmes on private capital flows since the IMF offers temporary protection from default and it asks the country to implement reforms that help macroeconomic adjustment (e.g. Corsetti et al., 2006). However, an IMF programme can also signal to creditors that country fundamentals are worse and the default risk is higher than assessed by market participants (e.g. Reinhart and Trebesch, 2015). A causal link between previous financial market reactions to IMF programmes and the inclination to approach the IMF again has not been established so far. Yet, the policy debate seems to take the existence of financial market stigma for granted. This paper intends to provide a contribution to the debate on whether this assumption is justified.

Our analysis proceeds in two steps. First, to provide evidence on whether financial market stigma exists, we need to identify whether there is a clear financial market reaction to IMF programmes. In a second step, we investigate whether these reactions determine the future likelihood of an IMF programme.

The financial market reaction to IMF programmes has been quantified by a number of authors, as we detail in section 2, and we build on the approaches used in previous research, particularly on those papers using higher frequency data. We use monthly data on treasury bill rates and on stock market indices as the main dependent variables to capture a financial market reaction. Similar to the studies using very high frequency data, we define a window around an IMF-related event which helps controlling for anticipation effects to estimate the reaction to every IMF programme-related announcement (event) for every country in the sample.

In the second step, we try to identify whether these financial market reactions determine the future likelihood of an IMF programme. There are two channels through

which the potential financial market reaction might affect a country's decision to ask the IMF for a programme. The first channel would be learning from experience, similarly to the learning from the conditionality experienced in the past as in Andone and Scheubel (2017). The second channel would be learning from regional or peers' experience. To estimate these channels is the key innovation provided by this paper.

To estimate the first channel we use the predicted values of market reactions to IMF events (country/month observations) from the first stage estimation to find similar countries in a propensity score matching approach. We compare country/month observations which are characterised by a previous negative market reaction to similar countries with a previous positive reaction and to similar countries with no previous programme. Therefore, inference is based on between-country differences in market reactions under otherwise similar conditions, which helps us avoid a circular argument. In addition, we also control for both a country's history with the IMF and its macroeconomic, political and financial fundamentals to avoid endogeneity.

To estimate the second channel we include the past average market reaction among a country's neighbours[2] to past IMF programmes. This allows us to also control for learning from the market's reactions to peers' programmes. In the latter aspect, we closely follow the approach by Andone and Scheubel (2017).

Our approach has two main advantages. First, we use corrections from time series techniques for the first step to account for the time series nature of the data and the related challenges for identification. Second, by using the results from the first step analysis in a treatment/control set-up defined by propensity score matching to estimate the impact of previous movements on the likelihood of having an IMF programme, we are able to provide a quantification of IMF financial market stigma. In addition, we can verify to what extent the neighbours' past experiences with the IMF drive a

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[2]We define neighbours in three ways. First, we look at a country's trade partners. Second we look at a country's geographical neighbours. Third, we consider all countries at a similar stage of economic development as defined in the IMF's World Economic Outlook country groups.

country decision. Overall, our results, after controlling for fundamentals and selection bias, do not give any evidence for financial market stigma. Instead, we find evidence for IMF recidivism which is driven by adverse selection and extensive (structural) IMF conditionality.

Section 2 provides on overview of the literature on market effects of IMF programmes. Section 3 lists the data sources and provides a data description. To illustrate common econometric challenges associated with the policy question, we dedicate section 4 to discussing our identification strategy and econometric approach. In section 5 we discuss our results as well as econometric tests and robustness. Section 6 concludes.

2 Literature review

Many studies of financial market reactions to announcements related to IMF programmes are case studies, which may be related to the difficulties of gathering detailed and high-frequency market information on many IMF programmes and matching them with available financial data. These studies generally find that stock markets react positively to the official news that an IMF arrangement is agreed (e.g. Kaminsky and Schmukler, 1999), but news from other international organisations, credit rating agencies, and neighbouring countries also have an effect.[3] However, the positive effect seems to reverse if governments do not implement the conditions (Kutan et al., 2012). Evidence on an impact on

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[3]In particular, Evrensel and Kutan (2008a) estimate the response of forward exchange markets to IMF-related announcements for Thailand and Indonesia, using data on the 3-, 9-, and 12-month forward exchange rates. Their results indicate that financial markets respond favorably to IMF-related news, especially to the announcement of negotiations, with a premium on the baht and the rupiah. Evrensel and Kutan (2008b) estimate the changes in daily bond spreads (vis-á-vis US bonds) in Indonesia and Korea due to IMF-related news during the Asian crisis. Both in Indonesia and Korea the announcements associated with program negotiations and approval lead to declining spreads. Kutan et al. (2012) examine the impact of IMF-related news on both financial and real stock sector returns in Indonesia during the Asian crisis. Gogstad et al. (2014) investigate the effects on the stock market of the policy announcements from the IMF and European authorities during the recent Greek Sovereign Debt Crisis. Both studies find that financial sectors have stronger reactions to international institutions (and Greek government policy action) announcements than the real sectors. Banking and financial sectors react predominantly negatively to unfavorable announcements, while real sector responses are mixed.

asset values is limited (Brealey and Kaplanis, 2004). In addition, Saravia (2013) finds evidence that IMF lending programmes on average reduce the maturity of sovereign bond issues.

A significant number of papers also look at the potential catalytic effect of IMF programmes using annual data. However, the use of annual observations also reduces the possibility to properly control for the endogeneity of IMF programmes in these studies. In terms of results, the studies using annual data find mixed results which do not point to a universally catalytic effect of IMF programmes. On the contrary, the effect on bond yields seems to be negative unless policy reform in the country can be considered as highly likely (e.g. Bird and Rowlands, 2002; Mody and Saravia, 2006) and a positive effect on private capital flows only emerges for countries which do not restructure their debt (van der Veer and de Jong, 2014). Edwards (2006) does not find any catalytic effect on portfolio flows as the austerity often imposed as part of IMF conditionality in effect results in capital flight because it reduces future returns. The limited effect on private capital flows is somewhat confirmed by Erce and Riera-Crichton (2015) who find that an IMF programme does not catalyse foreign capital, albeit encouraging domestic investors to repatriate their foreign assets.

Chapman et al. (2015) and Gehring and Lang (2018) are exceptions in that they (at least partly) also look at monthly data. While Chapman et al. (2015) find an overall negative effect of IMF programmes on countries' short-term borrowing costs, Gehring and Lang (2018) suggest that IMF programmes provide a positive signal and affect sovereign ratings positively. Our work is closest to Chapman et al. (2015) in design in that we do not build on annual data at all and look at short-term treasury bill rates. However, both Chapman et al. (2015) and Gehring and Lang (2018) rely on standard instruments for addressing potential endogeneity while we exploit the higher frequency nature of the data to do so. Our results resonate with the claim by Chapman et al. (2015) that market effects of IMF programmes are heterogenous across countries, but in

contrast to both studies we also estimate the heterogeneity across countries and show that the financial market effects of IMF programmes are neither universally bad nor universally good.

While the literature has so far not been clear on a possible catalytic role of the IMF, existing studies point more unambiguously to IMF programmes lowering the likelihood of crises, including capital flow reversals. A country is less likely to experience a sudden stop during a programme (Eichengreen et al., 2008), with the effect operating more powerfully in countries with strong fundamentals. Dreher and Walter (2010) find that an IMF programme during the previous 5 years reduces the likelihood of a currency crisis, while Papi et al. (2015) show that programme countries are also less likely to experience a future banking crisis. However, IMF programmes seem to raise the likelihood of sovereign debt crises (Jorra, 2012).

3 The data set

3.1 IMF MONA data

Our analysis is based on IMF programme data from the IMF's MONitoring of Fund Arrangements (MONA) data set, which we have cleaned and harmonised based on the algorithm by Andone and Scheubel (2017). The IMF's MONA data set starts in 1992 and covers IMF programme reviews by date. This is important as not only a programme approval may trigger market reaction, but also unsuccessful or successful reviews or a waiving of conditions may do so. In particular, the MONA data set provides all dates with a programme "action", such as an agreement on a programme, a disbursement, a change in the conditions, a change in the review or disbursement dates or an IMF Board agreement on one of these "actions". As we are interested in the reactions of markets to specific "actions", we look at these "actions" by date, and not summarised by year as in Andone and Scheubel (2017). This means that our data set of IMF programme-related

"actions" has a monthly frequency. To that end the IMF data we use also differs from Kentikelenis et al. (2016) who mainly focus on conditionality in an annualised data set while we focus on the timing of programme-related "actions". In the following, we refer to any such IMF programme-related "action" as IMF event. The MONA database allows us to identify these events, which we have listed in Table A.3 in Appendix A. Appendix A also provides an overview of the distribution of IMF events across time and across regions.

3.2 Dependent variable and controls

Our choice of the appropriate dependent variables is motivated by their significance for a country's financial position and by their availability for a large set of IMF members. In particular, we require a variable for detecting a reaction from market participants to the announcement of or change to an IMF programme which will also play an immediate role for the country's government. Only if movements in this variable are relevant for the country authorities, these movements could potentially trigger a reluctance by the country authorities to ask for an IMF programme in the future.

Therefore, sovereign yields are our first variable of choice. However, the most indicative measure, the 10-year sovereign yield, is available only for a comparatively small set of countries. In total, we have monthly 10-year sovereign yield data for 43 countries, of which however only 9 series go back to 2000 and only 1 series (Vietnam) goes back to the first year for which also full MONA data is available, i.e. to 1992.

Therefore, we follow Chapman et al. (2015) in using short term sovereign bond yields. In particular, we use the monthly treasury bill rates reported in the International Financial Statistics (IFS) that we complemented with data gathered from the Haver Dataset. Treasury bill rates are mainly a barometer for the short-term interest rate and may therefore be affected by investors' perception of short versus long-term country risk. For example, should an investor perceive their investment as safe only as long as the IMF

Table 1: List of Variables and Coverage

Variable	Definition	Type	Source	Coverage
T-Bill	Interest rate paid by short-term government bonds on the secondary market.[a]	%	Haver/IFS	January 1992 - Dec 2015, 106 Countries
Stock	Value of country stock market	Index	WB	January 1992 - Dec 2015, 73 Countries
Res	Amount of country reserves, including gold ($)	Value	WB	January 1992 - Dec 2015, 200 Countries
Growth Forecasts	Monthly weighted average of the semester WEO growth forecast	Index	WB	January 1992 - Dec 2015, All Countries
CPI	Level of consumption prices, seasonally adjusted	Index	WB	January 1992 - Dec 2015, 212 Countries
Exc	Nominal exchange rate, LCU per USD (period avg.)	%	WB	January 1992 - Dec 2015, 232 Countries
Pol	Level of political risk	Index	ICRG	January 1992 - Dec 2015, 140 Countries
Eco	Level of economic risk	Index	ICRG	January 1992 - Dec 2015, 140 Countries
Fin	Level of financial risk	Index	ICRG	January 1992 - Dec 2010, 145 Countries
Comp	Weighted averageof economic, political, and financial risks	Index	ICRG	January 1992 - Dec 2015, 140 Countries
VIX	Volatility of US S&P500	Index	FRED	January 1992 - Dec 2015, All Countries
Spread	Returns spread between US "safe" and "junk" assets	Index	FRED	January 1992 - Dec 2015, All Countries
G7	Voting in line with the G7 group at the UN General Assembly (UNGA)	Share	Bailey et al. (2017)	January 1992 - Dec 2014, All Countries
Debt Res	Official debt restructuring provided by Paris Club of official creditors, amount treated	USD mn	Cheng et al. (2016)	May 1956 - Jun 2015, All Countries
Reg T-Bill	Regional average of T-Bill Rate		Authors' comp.	January 1992 - Dec 2015, All Countries
Reg Stock	Regional average of Stock Ind		Authors' comp.	January 1992 - Dec 2015, All Countries
Reg Res	Regional average of Res		Authors' comp.	January 1992 - Dec 2015, All Countries
Reg CPI	Regional average of CPI		Authors' comp.	January 1992 - Dec 2015, All Countries

Notes: List of variables with the corresponding coverage. Country coverage as in December 2015. WB = World Bank, IFS= International Financial Statistics, FRED = Federal Reserves Economic Data; ICRG = International Country Risk Guide, WEO = World Economic Outlook. All variables are used in logarithmic form.

[a]The bond maturity ranges from 12 weeks to 1 year depending on the country's definition.

is present in a country, they should prefer treasury bill rates with a maturity of up to 2 years to sovereign bonds with a longer maturity. Consequently, should the IMF have a catalytic effect at least for the duration of the programme (which, for its workhorse programme, the Stand-By Arrangement or SBA, is 2 years), we should expect treasury bill rates to fall upon the agreement of a programme or upon the successful conclusion of a review.[4] Therefore, in the absence of data available at the longer end of the yield curve, movements in the treasury bill rate also represent a good approximation of market reactions to the programme.

The treasury bill rate data we use are not harmonised across countries as countries report different instruments with maturities between 3 and 12 months.[5] The monthly observations constitute the averages of daily observations. Treasury bill data are available for 66 countries that have had at least one IMF programme, from a minimum of 58 to a maximum of 288 months.[6] This coverage allows us to estimate the effect of 149 IMF programmes, composed of 1782 IMF related events. Table (1) provides a brief description of additional dependent and control variables.

4 Identification and Econometric Model

4.1 Identification

In this paper we would like to estimate whether a potential adverse financial market reaction may deter a country in need of financial assistance to approach the IMF for help. As we cannot measure a country authority's expectations about potential market reactions to an IMF event, our main identifying assumption is that a country's authority

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[4]Saravia (2013) finds that governments tend to lower the maturity of their issuance during an IMF programme, which renders some support to the hypothesis that an IMF programme lowers a government's short-term refinancing costs.

[5]Since instruments with a longer maturity have a more stable behavior and, consequently, present a less pronounced reaction to shocks, this heterogeneity can affect our estimates. Therefore, we also use other dependent variables and different samples as robustness checks.

[6]Also refer to Table 1. The treasury bill rate is denoted by *T-Bill*.

will make conjectures based on past experience. This key assumption is in line with our basic understanding on how financial market stigma emerges. If an IMF programme has resulted e.g. in capital outflows in the past, a country's authorities will be more wary about such effects in any future programme. The literature is unambiguous on previous relationships with the IMF having an impact on the future relationship (e.g. Bird and Rowlands, 2002; Hutchinson and Noy, 2003; Conway, 2007; Marchesi and Sabani, 2007).

A further identifying assumption is that conjectures about past experience are also formed based on neighbours' experience.[7] This is a reasonable assumption considering that there are several contagion channels through which crises are transmitted. Investor reactions to economic policies in one country may be related to expectations about future policies in another country (e.g. Forbes et al., 2016, for the case of capital controls). Moreover, the financial and trade integration channel is typically important (e.g. Fratzscher, 1998; Haile and Pozo, 2008), pointing to the potential of contagion particularly between trade partners. Some evidence also confirms the importance of contagion between neighbours during financial crises (Fang and Qin, 2013).

The identification of a causal relationship between a country's (or its neighbours') past experience with the IMF and the likelihood of a future programme requires an unbiased estimate of past market reactions to IMF programmes. This requires ruling out an endogeneity of the IMF programme to the market reaction, i.e. we need to make sure that the IMF programme triggered the market reaction and not the other way round. As we will argue below, instrumenting the IMF programme would not work in our setting since we estimate the specific market reaction for *each* IMF event in the sample. Therefore, we build on the notion that reverse causality – a market movement triggering the need for an IMF programme rather than macroeconomic fundamentals – can be ruled out if we can control for all other factors which might cause such a market movement.

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[7]We define neighbours both based on geographical proximity and based on the intensity of trade relations.

To address endogeneity concerns, i.e. to derive an unbiased estimate of a market reaction to an IMF programme, we do not use the standard approach with annual data and instrumental variables for several reasons.[8] First, annual data often only report the average of higher frequency observations, and this raises two concerns. Using the average of higher frequency observations makes it more difficult to quantify an exact financial market reaction. Financial market movements may reverse within a year such that annual observations may mask such movements which would make it difficult to identify any effect in a regression analysis. Similarly, if an event happens at the end of a year, the estimated coefficient of a regression would be based on developments effectively preceding the event, thereby leading to false conclusions about the estimated effect.

Second, annual data also make it more difficult to disentangle the potential reverse causality between macroeconomic developments and/or market movements and IMF programmes. Worsening macroeconomic fundamentals and financial market movements may be the reason for a country to approach the IMF for help, but approaching the IMF for help may also lead to worse macroeconomic outcomes and trigger market movements. Controlling for macroeconomic fundamentals and market movements *before* an IMF event is particularly difficult with annual data whenever an IMF event does not take place at the beginning of a year given that above all market movements may be of a short-term nature. Therefore, with annual data the main solution is finding a valid instrument, which however may still give biased estimates if data is averaged from events which do not take place at the beginning of a year (Cavallo and Rigobon, 2016).

Therefore, our approach to address potential reverse causality is to use higher frequency data. To address other endogeneity concerns which may exist with higher frequency data, we follow the event study literature in controlling for both macroeconomic

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[8] Annual data has been used extensively in studies on the effects of IMF programmes (e.g. Mody and Saravia, 2006; van der Veer and de Jong, 2014; Bas and Stone, 2014; Gehring and Lang, 2018).

fundamentals and market movements before an IMF event.[9] In contrast to studies using annual data, which often have to rely on instrumenting to rule out endogeneity, studies using high-frequency data instead define a narrow-enough window around the event they study and then within this window control for any other effects which might drive market movements. If these other factors are sufficiently controlled for, the residual market movement can be attributed to the event in question.

In particular we follow Gray (2009); Chapman et al. (2015); Fuchs and Gehring (2017) in using monthly data for a wide set of macroeconomic and financial variables. Within a window of 2 months before the event and 3 months after the event we then control for developments which might cause reverse causality.[10] With this approach, we estimate an event-specific market reaction for *each* event in the sample. This approach is detailed in Section 4.2.

Once we have estimated this (unbiased) market-reaction to each event in the sample, we can relate these event-specific reactions to the likelihood of later IMF programmes. However, regressing the likelihood of country i starting an IMF programme in month t on the market reaction to an IMF event in month $t - x$ for each country i would be problematic for two reasons. First, this would allow us to only estimate how past market reactions shape future relations with the IMF for countries which have used the IMF in the past. Relying on time variation only would give an estimate how the average past market reaction experienced by country i affects its likelihood of approaching the IMF again. It would not allow us to e.g. compare countries which experienced a favourable and an unfavourable market reaction. Second, country-specific estimates could easily create another source of endogeneity. If a country turns out to be less likely to have an

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[9]Studies using daily data (e.g. Kaminsky and Schmukler, 1999; Brealey and Kaplanis, 2004; Evrensel and Kutan, 2008a,b; Glennester and Shin, 2008; Lo Duca and Stracca, 2015) cannot measure longer-term effects and rather provide a precise estimate of immediate financial market reactions. We follow Erce and Riera-Crichton (2015) who use quarterly data in choosing a slightly longer horizon to allow for measuring distinguishable effects. Since the frequency of programme reviews may be shorter than quarterly, we choose monthly data.

[10] We use a window of 2 months before the event and 3 months after the event for our baseline estimation, but results do not differ when using a symmetric window.

IMF programme at month t when it faced an adverse market reaction at month $t - x$, this would not necessarily have to be related to the market reaction. Instead, it could also be related to the past programme having been successful.

Therefore, we do not use the event-specific estimates to estimate a country-specific likelihood of agreeing on a programme, but instead we pool the event-specific estimates to use a propensity score matching approach on this pooled sample. Effectively, this allows us to compare countries which experienced a positive market movement with countries that experienced a negative market movement and to compare countries which have experienced a past negative or a past positive reaction to countries which did not experience an IMF programme in the past. It also helps us to rule out the possible endogeneity mentioned above. This approach is detailed in Section 4.3.

4.2 Econometric model for the quantification of a financial market reaction to IMF events

To derive an unbiased estimate of the market reaction to an IMF event, we follow the event-study literature in defining a narrow-enough window around each event e happening at time t which also includes possible anticipation effects in this window. In our baseline model, this window is $l_e - 2; l_e + 3$ with l_e denoting the month of event e.[11]

To be able to estimate market reactions for each IMF event in the sample while making sure that standard errors are drawn from the same distribution, we estimate a panel model in which we include dummy variables, one for each IMF event in the sample, which is similar to explicitly including dummies for country-time fixed effects in a panel model. The set of dummy variables is denoted by $\boldsymbol{IMF}_e$.[12] These dummy variables take the value 1 if there is an IMF event e in country i at time t. Note that $t \in (-2; +3)$ and $t_e = 0$ in this set-up since we restrict the window we consider for the estimation for

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[11] We also tested narrower windows, wider windows and symmetric windows, neither of which affect the results.

[12] We do not add subscripts here since each event e is specific to a country i and point in time t.

each event to be 2 months before and 3 months after the event. This approach gives us a vector of country-time-specific market reactions to each IMF event in the sample, which in the following we denote by the vector $\boldsymbol{\beta}_e$.

The following equation illustrates this panel model, expressed in differences to rule out possible spurious correlations (Granger and Newbold, 1974):

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\begin{aligned}
Dy_{i,t} =& \ \boldsymbol{\alpha}_i + D\boldsymbol{f}_t + \sum_{j=1}^{n} \delta^j Dy_{i,t-j} + \sum_{j=0}^{n} \boldsymbol{\gamma}^j D\boldsymbol{x}_{i,t-j} \\
& + \ \boldsymbol{\beta}_e DIMF_e + \sum_{j=0}^{n} \boldsymbol{\phi}^j D\bar{\boldsymbol{z}}_{i,t-j} + \epsilon_{i,t}
\end{aligned}
\tag{1}
$$

The dependent variable $y_{i,t}$ is the logarithm of either a country's t-bill rate or the stock market index. The vector $\boldsymbol{\alpha}_i$ denotes country fixed effects and the vector $\boldsymbol{f}_t$ denotes time fixed effects to account for structural differences over time. $y_{i,t-j}$ denotes the $t-j^{th}$ lag of the dependent variable, with $n \in (1,3)$. $\boldsymbol{x}_{i,t-j}$ is the vector of the $t-j$ lag of the exogenous variables $\boldsymbol{x}$ (country fundamentals), with $j \in (0,3)$. Similarly, $\bar{\boldsymbol{z}}_{i,t-j}$ is the vector of the $t-j^{th}$ lag of the weighted cross-country average of the exogenous variables excluding country i, i.e. $\bar{\boldsymbol{z}}_{i,t-j} = \sum_{1}^{b \neq i} (\boldsymbol{x}_b)$ with $j \in (0,3)$ except for reserves, trade partners' average of the dependent variable, and the average of the regional financial index, which enter only with a lag, i.e. $j \in (1,3)$. Lag lengths have been chosen in line with the AIC criterion.

The control variables $\boldsymbol{x}$ are the essential element of our identification strategy in this step. The main source of endogeneity would be movements in t-bill rates which already follow a trend. In that case, we would falsely attribute this trend to the IMF event, even though it had been triggered by different developments. Therefore, we add the variables in $\boldsymbol{x}$ to control for several sources of market movements which could confound the movement of the t-bill rates.

We proceed particularly carefully to control for possible changes in financial and macroeconomic developments around the IMF event. We include in all specifications the CPI since t-bill rates typically mirror short term interest rates, as well as the flow of a country's reserves and exchange rate movements. To control for expectations of a country's financial risk, we include the ICRG financial risk rating measured by the PRS group.

Since markets could also react to developments in macroeconomic fundamentals, we include in some specifications not only IMF WEO bi-annual growth forecasts, but also the ICRG economic risk measure. The ICRG economic risk measure represents a forward-looking assessment of the market since it contains *expectations* about macroeconomic developments. Therefore, we believe that particularly by including an economic forecast as well as a measure of market expectations of economic developments we cannot only capture anticipation effects for the period prior to the IMF event, but also sufficiently control for a possible change in expectations on macroeconomic outcomes in case the IMF event affects those, e.g. because of programme conditionality, for the period after the IMF event.

Finally, one may argue that also political factors affect market movements and that an IMF event could have different effects, e.g. depending on the political ownership of the country authorities. Therefore, we also include the ICRG political risk rating, which should capture these political factors well. Finally, we try to avoid missing out on other sources of support similar to the IMF by including measures of the use of the global financial safety net (Scheubel and Stracca, 2016)and a measure of whether a country is supported through debt restructuring from the Paris Club taken from Cheng et al. (2016) in a robustness check.

To isolate the effect of the IMF event we restrict the contemporaneous effect of reserves, the exchange rate, and the political and financial risk to be zero. This is necessary as contemporaneous changes in these variables may be related to the IMF

event. All variables which we use in the model, as well as the sources, are listed in Table (1).

Another source of endogeneity which we need to address is global components affecting all countries at the same time. As shown in the time series literature on common correlated effects, fixed effects cannot fully control for global components affecting all countries at the same time. This can lead to cross-correlation between countries. (Phillips and Sul, 2003; Phillips and Su, 2007; Sarafidis and Robertson, 2009; Chudik and Pesaran, 2015).[13] These unobserved factors can contemporaneously affect a large set of countries and thereby bias the estimates (Pesaran, 2006). Bai (2009, 2013), demonstrate for panel data that an interacted fixed-effects model, i.e. a model which adds interaction terms obtained from multiplying the units and time-series dummies, usually performs well in terms of reducing the cross-sectional dependency.

We can emulate the basic idea of this estimator in that we deal with global unobserved factors and strong cross-sectional dependency by adding the VIX to the set of controls x to capture global developments and by adding regional averages. In particular, we add weighted averages of the variables in x for a country's trade partners with the weights calculated as the percentage of imports of country i from country k, trade partners' averages of the dependent variable y, and a regional financial index. The regional financial index is calculated as the average of financial indexes for countries belonging to the same region as country i.

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[13]For instance, financial market liquidity and confidence are lower during periods of global slack, and reduced liquidity and confidence may magnify signals about country fundamentals. Therefore, an IMF programme in the aftermath of the global financial crisis could have had different effects from a programme during a period of strong global growth.

4.3 Econometric model for the estimation of the impact of the estimated financial market reaction on the likelihood of agreeing on an IMF programme

To establish whether the past t-bill rate movement has an impact on the likelihood of asking for an IMF programme in the future, our identification strategy relies on comparing otherwise similar countries which only differ with respect to the past market reaction to an IMF event. Note that based on our strategy for dealing with endogeneity in the first step, the reactions we compare between countries should be fully exogenous to past economic conditions and purely related to the IMF itself. Therefore, our main concern is to rule out that the likelihood of a country agreeing on a current programme differs because the countries differ regarding current macroeconomic and financial market developments rather than just the past reaction to an IMF event.

We thus follow the literature on natural experiments by looking at distinct groups in terms of the past market reaction to an IMF event while ensuring that we control for any differences that may currently exist between these countries by choosing an adequate set of control variables. Our treatment group is those countries which experienced a past negative market reaction, i.e. an increased t-bill rate after an IMF programme. Specifically, we group all countries according to the estimate $\hat{\boldsymbol{\beta}}_e$. The treatment group is defined by $\hat{\boldsymbol{\beta}}_e > 0$. A second treatment group is defined by $\hat{\boldsymbol{\beta}}_e < 0$ and the control group is defined by $\hat{\boldsymbol{\beta}}_e = 0$. In addition, we always look at the sub-groups of recipients of non-concessional programmes and concessional programmes separately. The country groups eligible for the respective type of programme are very distinct such that it is advisable to compare developments only *within* these groups.

In comparing these groups, we apply a propensity score matching approach which gives us the opportunity to explore different counterfactuals. As we would like to understand whether $\hat{\boldsymbol{\beta}}_e > 0$ reduces the probability of agreeing on a new programme, we can explore two counterfactuals. First we can explore the probability of agreeing on a

new programme if the country would have experienced a fall in the t-bill rate instead of an increase, and the probability of agreeing on a new programme if the country would have not experienced an IMF event in the past.

Propensity score matching is a standard non-parametric approach to estimating causal treatment effects by estimating the likelihood of being treated, the propensity score, (in our case, of experiencing $\hat{\boldsymbol{\beta}}_e > 0$ in the past) given a set of observed characteristics $\boldsymbol{x}$ and then comparing the outcome (in our case whether a country agrees on an IMF programme or not) with those observations which have a very similar likelihood of being treated, but are not treated. As in all models for estimating treatment effects, to identify a causal treatment effect it is essential that systematic differences in outcome are related only to the set of observed characteristics $\boldsymbol{x}$. Caliendo and Kopeinig (2008) provide an overview of the framework and required identifying assumptions with a particular emphasis on propensity score matching. 'Treatment' in our setting can be understood as having experienced an increase in t-bill rates prior to t while 'controls' can be understood as observations without a change in t-bill rates prior to t. In our robustness checks we also compare the former two groups to a group which experienced a *decrease* in t-bill rates prior to t. This ensures that we test our hypothesis against two counterfactuals.

In econometric terms, we apply a discrete choice model to estimate the effect of past market reactions on the likelihood of agreeing on an IMF programme during month t:

$$IMF_{i,t} = f(m_{i,e_{t-u}}|\boldsymbol{\Upsilon}) + \eta_{i,t} \tag{2}$$

where $IMF_{i,t}$ is an indicator variable which is equal to 1 if an IMF programme is approved in month t for country i, $m_{i,e_{t-u}}$ is a dummy variable taking the value one if the estimated market reaction to the previous IMF event taking place at month $t-u$, $\hat{\boldsymbol{\beta}}_{e_{t-u}} > 0$, was larger than zero. Put differently, $m_{i,e_{t-u}} = 1 \ iff \ \hat{\beta}_{e_{t-u}} > 0$. Note that

the time of the previous IMF event may differ across countries and also depending on the type of event that we look at, so the value u is country and event specific. Our key identifying assumption for this second step is then illustrated by the set of matching variables $\boldsymbol{\Upsilon} = \sum_{j=0}^{n} \boldsymbol{\gamma}^j \boldsymbol{x}_{i,t-j}$. $\boldsymbol{x}$ is the same vector of control variables we use in the first step, but we add a country's alignment with the G7, i.e. the main IMF shareholders, at the UN General Assembly to capture the political determinants of agreeing on an IMF programme (Dreher et al., 2009; Dreher and Sturm, 2012) and regional averages of the control variables to sufficiently capture possible economic spillover effects.

Arguably, this approach does not account for cases which never experienced an IMF event in the past, not because they did not need IMF support, but because of financial market stigma. If this were the case, our estimates would be biased downwards. To work around this issue we build on the second channel we have discussed as a possible source of IMF financial market stigma. If a country has never had an IMF programme before, its own negative experience cannot be a source of stigma. The other possible channel we consider is observing adverse effects among peers. Therefore, in a next step, instead of conditioning on own past experience, we condition on neighbours' past experience, similar to Andone and Scheubel (2017), and estimate equation (2) as:

$$IMF_{i,t} = f(m^n_{i,e_{t-u}} | \boldsymbol{\Upsilon}) + \eta_{i,t} \tag{3}$$

with $m^n_{i,e_{t-u}}$ denoting the average past reaction among neighbours. Again, as $\bar{\boldsymbol{z}}$ includes regional averages of the control variables, it should sufficiently capture possible economic spillover effects. This implies that if we still find an effect, we do not measure pure economic contagion.

5 Results

5.1 First stage descriptive results

This section sheds some light on the relation between IMF events and our main dependent variables. In presenting descriptive evidence we focus on the approval of an IMF programme since the approval often contains significant information for market participants, while not being preceded by any other event related to the same country and programme. This is different from e.g. programme reviews which have information content related to previous reviews, for instance if a condition is carried over to the next review or if a review is delayed because of non-observance of a condition from the previous review.

Table 2 reports the correlation of the t-bill rate and the stock market index with the approval of an IMF programme.[14] A positive value in Table 2 indicates that in the period after the agreement on a programme the value of the variable on average is higher, and vice versa.

Table 2: Correlation between programme approval and key dependent variables

Variable	Cumulated	Impact	Concessional	Non-concessional	Advanced	Emerging
T-bill Rate	0.1309	0.0320	0.0320	0.0337	0.0005	0.0524
Stock Index	-0.0963	-0.0214	0.0120	-0.0435	-0.0081	-0.0376

Notes: The table reports the correlation between the approval of an IMF programme and the t-bill rate/the stock market index. The dummy variable indicating programme approval takes the value one in the month in which an IMF programme is agreed and the news is released publicly with the exception of the column 'Cumulated', where the dummy takes the value one from two months before programme agreement to three months after programme agreement. There are 5 countries with IMF programmes in the sample for which we have data on stock market indices available which are classified by the IMF as advanced: Greece, Iceland, Ireland, Latvia, and Lithuania.

Figure 1 reports the sample average path for the t-bill rate and stock market indices (expressed in logarithmic form) in a window from 12 months before the approval to 12 months after the approval. Three stylised facts stand out. First, on average, treasury bill rates seem to decline around the approval of an IMF programme. This is not yet a confirmation of a catalytic effect of the IMF and could indicate, inter alia,

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[14]Data availability restricts the sample to 147 programmes when considering the t-bill rate and 84 programmes when considering the stock market index.

expectations about interest rates or a portfolio shift from longer-term sovereign bonds to short-maturity bills. However, it suggests lower refinancing costs for the sovereign and hence an effect that should be perceived positively by the country authorities. Second, the average of the stock market indices shows a positive trend around the approval of an IMF programme. This movement also suggests an overall positive market reaction to an IMF programme. Third, Figure 1 seems to point to anticipation effects since the effects are visible already approximately 2-3 months ahead of the official programme approval. While we should refrain from drawing too early conclusions from this, these movements may suggest that the approval of an IMF programme could contain some additional information which is overall considered positive by market participants.

5.2 First stage multivariate results

This section reports the results of our benchmark model in Equation (1). We first report results only for IMF programme approvals. Since Equation (1) estimates coefficients for each event, we present the estimated coefficients in a figure. Figure 2 shows the distribution of the estimated effects of the approval of an IMF programme on the t-bill rate for the 101 programmes for which the estimated effect is different from zero.

Figure 1: Average T-bill rates and stock market indices around IMF programme approvals
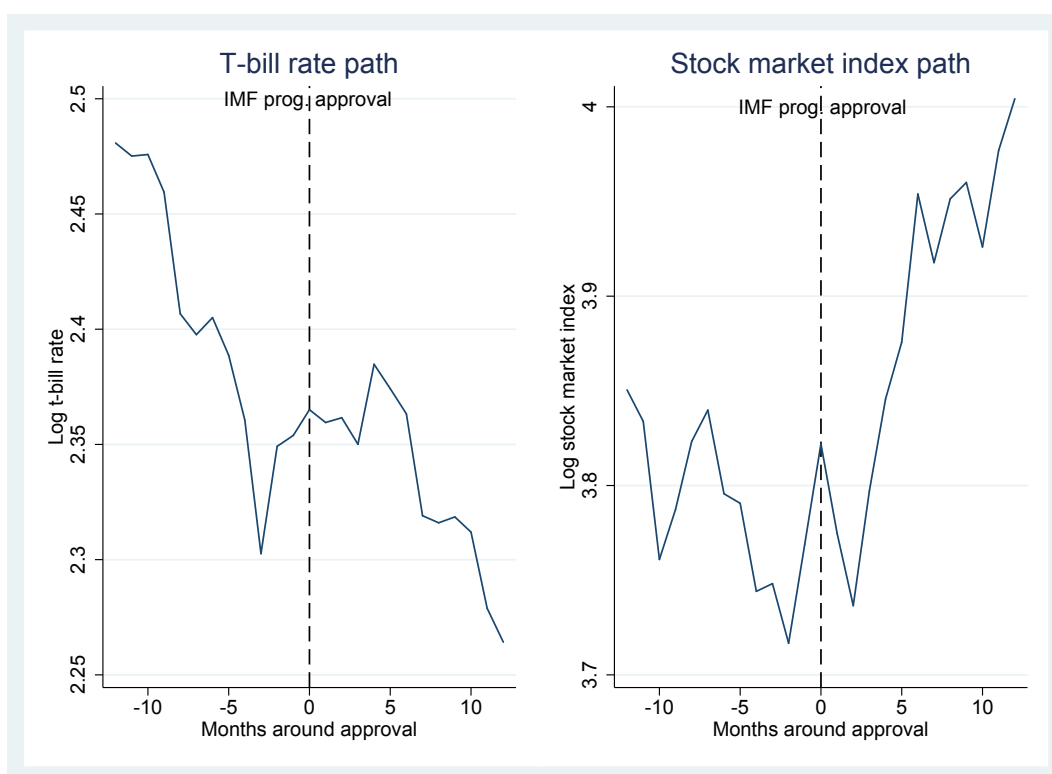
Notes: The left panel depicts the average behaviour of t-bill rates from 12 months before to 12 months after the approvement of an IMF programme. The right panel depicts the average behaviour of stock market indices from 12 months before to 12 months after the approvement of an IMF programme.

Figure 2: Estimated effect of the approval of an IMF programme on the t-bill rate

■ 1%   ■ 5%   ■ 10%   ■ non sign
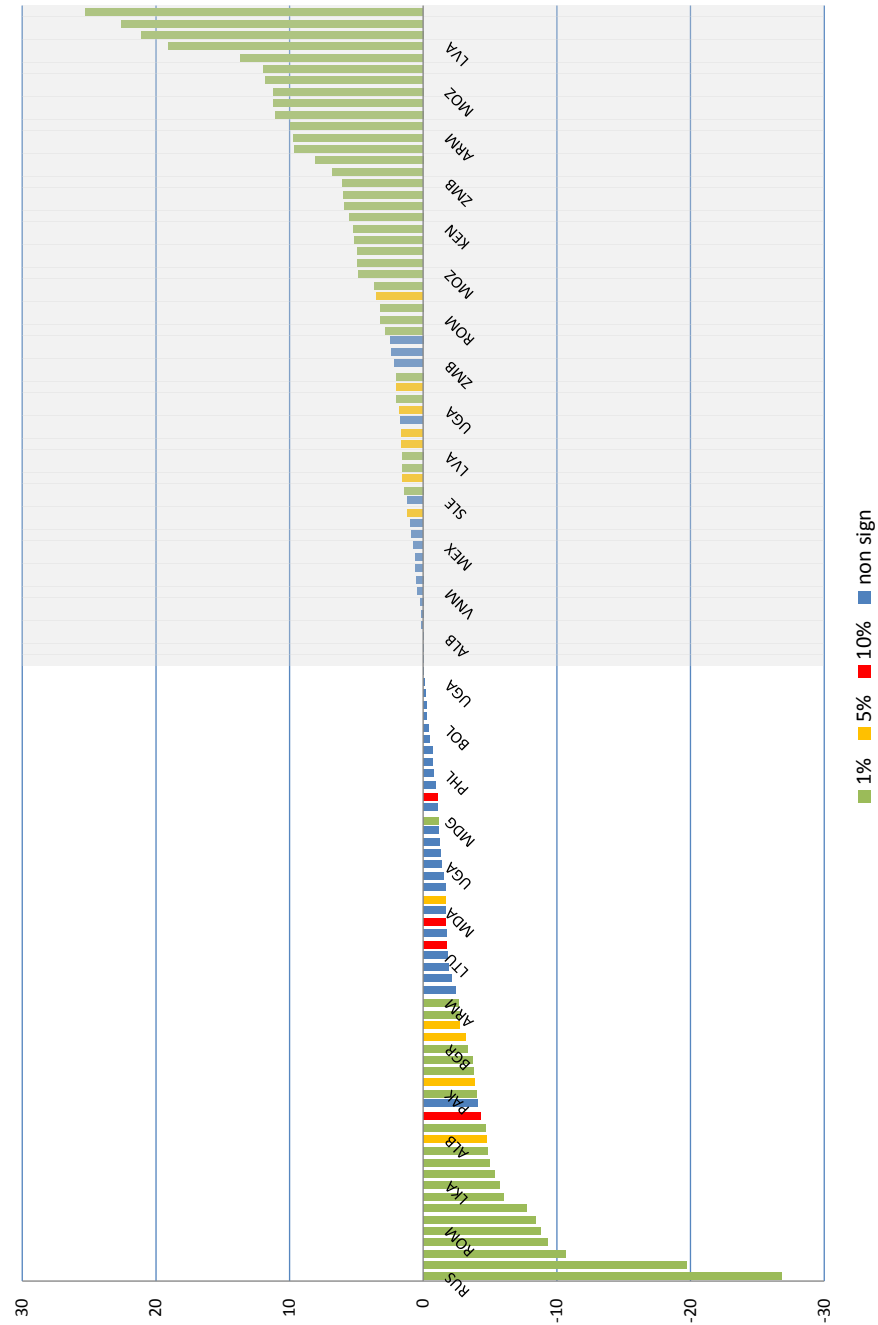
Notes: The figure illustrates the estimated coefficients β^e from Equation (1). The IMF dummy variable takes the value 1 for 2 months before the approval of a programme and 3 months after the approval of a programme, hence the depicted coefficients represent cumulative effects. Coefficients which are significant at the 1% level are coloured green, coefficients which are significant at the 5% level are coloured orange and coefficients which are significant at the 10% level are coloured red. Blue-coloured bars are insignificant.

Figure 2 illustrates that the cumulative effect of the approval of an IMF programme on the change in t-bill rates ranges from -25% (Russia, July 1999) to 29.5% (Nigeria, October 2005). In total, 62 of the IMF programmes raised the t-bill rate (shadowed area), while 39 programmes lowered it. This distribution highlights that an IMF programme can have both positive and negative short-term effects on the t-bill rate. Such differences would be masked by estimating only the sample average and they would not show up either in the chart depicting the average movement of the t-bill rate. Not surprisingly, programmes with the largest effects tend also to be significant: according to our estimates, 49 out of 101 programmes had an effect significant at least at the 10%, and 33 were significant at the 1% level (green bars).

Section B.1 in Appendix B provides further details on the distribution of estimated coefficients by country group and over time.

5.3 Second stage multivariate results

5.3.1 Benchmark model for IMF programme approvals

We first report in Table 3 the main results for the second stage based on a propensity score matching. We report two coefficients of interest: (i) the impact of the past movement in t-bill rates only for concessional programmes ($m_{i,e_{t-u}}^{conc}$), and (ii) the past movement in t-bill rates only for non-concessional programmes ($m_{i,e_{t-u}}^{nonconc}$). Table 3 compares country/month observations which are characterised by a previous increase in t-bill rates in response to an IMF programme to country/month observations which are characterised by no previous IMF programme, as well as country/month observations which are characterised by a previous decrease in t-bill rates in response to an IMF programme to country/month observations which are characterised by no previous IMF programme.

Tables 3 and 4 suggest that both for concessional and non-concessional programmes *both* a previous increase in t-bills and a previous decrease in t-bills slightly *raise* the likelihood of approaching the IMF again.

Table 3: The impact of previous positive and negative changes in t-bill rates on the likelihood of agreeing on an IMF programme (control group: no previous programme)

| | Increase in t-bills vs. no previous progr | | Decrease in t-bills vs. no previous progr | |
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.006***			
$m_{i,e-1}^{nonconc}$		0.010***		
$m_{i,e-1}^{conc,-}$			0.006**	
$m_{i,e-1}^{nonconc,-}$				0.007***
Neighbours	10	10	10	10
Treated obs.	1553	1122	1028	1526
Total obs.	8912	8481	8387	9952

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (columns 1,2); or a negative change in the t-bill rate (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3, voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: The impact of previous positive and negative changes in t-bill rates of programmes with Hard Conditions on the likelihood of agreeing on an IMF programme (control group: no previous programme)

	Increase in t-bills vs. no previous progr		Decrease in t-bills vs. no previous progr	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.007***			
$m_{i,e-1}^{nonconc}$		0.006		
$m_{i,e-1}^{conc,-}$			0.006**	
$m_{i,e-1}^{nonconc,-}$				0.007**
Neighbours	10	10	10	10
Treated obs.	1066	681	766	1107
Total obs.	10751	10366	10451	12042

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (columns 1,2); or a negative change in the t-bill rate (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns):CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

These results suggest that there is no evidence for the existence of IMF financial market stigma as captured by previous movements in t-bill rates. However, they suggest that those countries which experienced a previous programme are *more* likely to access the IMF compared to countries with no previous IMF experience. One possible explanation for a positive coefficient is the so-called revolving-door effect of IMF programmes (Conway, 2007), i.e. a repeated use of IMF support. While the phenomenon is found frequently in the data, there are many possible sources of IMF recidivism, which range from adverse selection (Bas and Stone, 2014) with structurally weaker countries asking for programmes more often (Bird et al., 2004), which is possibly related to a lack of implementation of programme goals or to moral hazard or ineffective programmes (Bird, 2001; Hutchinson and Noy, 2003). A more recent theory is that a country under an initial IMF programme also receives support from other international donors, such as the Paris Club, often conditional on continuing an IMF programme or conditional on starting a new IMF programme (Cheng et al., 2016). We further investigate the possible sources of the revolving door effect we see in the data in several robustness checks.

5.3.2 Robustness: moral hazard, political factors

Moral hazard and political determinants of programme participation are among the more frequent explanations for a revolving door effect of IMF programmes. While the moral hazard explanation suggests that countries under a programme may be less likely to implement needed reforms since they receive support from the IMF, other political determinants are inter alia related to negative growth effects and the fallout these may have on electoral cycles, resulting in imperfect implementation and hence the requirement for a new programme (e.g. Dreher, 2003, 2004).

Many of these factors are already controlled for in our baseline specification. In particular, we do not only control for a country's growth prospects in the first stage when estimating the market reaction, but also on its political risk outlook as measured

by the PRS group. In addition, we match on these variables. In other words, when comparing countries with a similar economic and political outlook, those which in the past experienced a programme in the past are still more likely to ask for a programme.

Another political element which has been found to drive the likelihood of entering into an IMF arrangement is a country's political closeness to the IMF's main shareholders (Dreher et al., 2009; Dreher and Sturm, 2012), which is one of the reasons why a country's alignment with the IMF's main shareholders in the UN General Assembly is often used as an instrument for IMF programmes. Therefore, we also control for a country's alignment with the G7 in the UN General Assembly in all specifications for stage 2.

Finally, we also check the possibility that other donors may require continued IMF programme participation for providing their support. To that end, we include both in the first step estimation as well as in the matching the information whether a country has received a Paris Club debt restructuring in the window around agreeing on the IMF programme. These results are presented in Appendix Section B.3. The results are not affected. Therefore, our result cannot be driven by the existence of Paris Club debt restructurings and attached conditions either.

In sum, we rule out that this may drive the revolving door effect we see in the data. We therefore rule out that the recidivism in our results is driven by effects of a previous IMF programme on the economic and political landscape.

5.3.3 Robustness: adverse selection/control group

Next we investigate whether adverse selection could drive the revolving door effect we see in the data. Indeed, so far we have only compared the countries which have experienced a movement in t-bill rates to countries which have never experienced an IMF event in the past. Therefore, we might just see the effect of a selection into using the IMF. To test this hypothesis, we also compare those countries which have experienced a positive movement in t-bill rates to countries with a previous positive experience.

Table 5 shows the same baseline regression as in Table 3, except for changing the control group to countries which experienced *lower* t-bill rates after the last IMF event. Here we don't find a significant positive effect of previous increases in t-bill rates on the likelihood of asking for an IMF programme, which may render some support to adverse selection driving the result. Note however that we still find a marginally significant positive effect when pooling concessional and non-concessional programmes.

Table 5: The impact of previous positive changes in t-bill rates on the likelihood of agreeing on an IMF programme (control group: previous negative changes in t-bill rates)

| | Baseline | |
	(1) Concessional	(2) Non- concessional
$m_{i,e-1}^{conc}$	0.005	
$m_{i,e-1}^{nonconc}$		0.002
Neighbours	10	10
Treated obs.	1553	1122
Total obs.	2581	2648

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1), approval of a concessional programme (2), approval of a non-concessional programme. Treatment group: having had a positive change in the t-bill rate during previous IMF programmes. Control group: observations for countries that experienced a decline in the t-bill rates when programmes where approved. Matching variables: CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

5.3.4 Robustness: programme design

To further investigate the nature of the revolving door effect in our data, we therefore further account for programme design. We already control for the possibility that programmes had simply not been successful in the past by including the WEO growth outlook and a country's ICRG economic risk not only in the first stage (i.e. ensuring that the market reaction is already cleaned of expectations about growth), but also in the second stage to make sure that we compare countries with a similar growth outlook.

However, the growth outlook may not fully account for difficulties in implementation or programmes which are more far-reaching and hence more difficult to implement. To account for these features of a programme, we include in the specification from Table 5 an index of hard conditionality, i.e. conditions which were essential for a programme to continue and disbursements to be made, as calculated by Andone and Scheubel (2017). For programmes prior to 2009, this index captures particularly the degree of how structural a programme design was while the index also more generally gives an indication how 'tough' it is for a country to implement a programme. Table 6 presents these results, comparing those countries which experienced a past negative reaction with countries which experienced a past positive reaction.[15]

Table 6: Controlling for conditionality (control group: previous negative changes in t-bill rates)

	Hard Conditions	
	(1) Concessional	(2) Non- concessional
$m_{i,e-1}^{conc}$	0.010	
$m_{i,e-1}^{nonconc}$		-0.005
Neighbours	10	10
Treated obs.	1066	681
Total obs.	1832	1788

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1), approval of a concessional programme (2), approval of a non-concessional programme. Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (1). Control group: observations for countries that experienced a decline in the t-bill rates when programmes where approved (all columns 1, concessional columns 2, non-concessional columns 3). Matching variables: CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year; number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

If hard conditionality is included among the matching variables, the positive effect of past increases in t-bill rates on the likelihood of agreeing on an IMF programme vanishes,

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[15]Of course, it does not make sense in this set-up to compare to countries which never had a programme since in this case the coefficient would also capture the effect of having had a programme in the past.

i.e. previous experience with IMF events does not affect the likelihood of agreeing on an IMF programme. We interpret this as some evidence that the revolving door effect may in fact also be related to ineffective IMF conditionality. Since we are confident that we already control for economic conditions in both stages, we conjecture that this limited effectiveness is related to implementation problems associated with particularly tough conditionality rather than with conditionality being ineffective in achieving the desired macroeconomic outcomes.

5.3.5 Robustness: estimates based on neighbours' experience

Since we do not find IMF financial market stigma based on past experience with IMF events, we also investigate another potential channel related to the claim often mentioned in policy circles that IMF stigma is in fact a regional phenomenon. While we include regional averages of control variables in all specifications to control for economic spillovers, we also look at a specification in which we only consider treatment based on *neighbours'* experience with the IMF. These results can be found in Appendix B Section B.4.

When comparing countries with neighbours that experienced an increase in t-bill rates to countries with neighbours that experienced no previous programme we find a significantly higher likelihood of agreeing on an IMF programme among the countries with neighbours that experienced an increase in t-bill rates. Interestingly, if we compare countries with neighbours that experienced a decrease in t-bill rates to countries with neighbours that experienced no previous programme, we still find a significantly higher likelihood of agreeing on an IMF programme among the countries whose neighbours with a non-concessional programme experienced a decrease in t-bill rates, but we do not find such an effect for those countries whose neighbours with a concessional programme experienced a decrease in t-bill rates. This would suggest that if at all, IMF financial market stigma would be an issue related to concessional programmes.

When comparing countries whose neighbours experienced an increase in t-bill rates to countries whose neighbours experienced an decrease in t-bill rates, again we find a significantly higher likelihood of agreeing on an IMF programme for those countries whose neighbours experienced an increase in t-bill rates during a concessional programme, but not during a non-concessional programme.

However, even this effect vanishes when we control for neighbours' hard conditions. This leads us to conclude that our results confirm the main finding of Andone and Scheubel (2017) that the notion of IMF stigma can be traced back to extensive conditionality. There are two possible explanations for this result. First, interpreting conditionality as a form of costs attached to an IMF programme, measuring IMF stigma would be equivalent to measuring the cost governments attach to IMF conditionality. Second, conditionality may be difficult to implement.

5.3.6 Further robustness checks

Our additional robustness checks include checking for effects on the programme envelope instead of on the likelihood of entering a programme, checking for the likelihood of other IMF events, such as programme reviews, and using a different dependent variable.

In Appendix Section B.5 we show evidence that experiencing both positive and negative previous changes in the t-bill rate lower the total programme envelope of a new programme compared to countries with no previous programme.[16] When comparing countries with previous positive changes in t-bill rates to positive negative changes in t-bill rates we find that those with positive changes in t-bill rates have a smaller subsequent programme envelope, but only for concessional programmes. This effect persists even when controlling for programme conditionality.

In Appendix Section B.6 we restrict the sample to those programmes which are not follow-up programmes. Often, one type of IMF programme is merged into another one,

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[16]This effect is slightly less robust for concessional programmes.

and MONA data helps us to filter out those cases. Among these, we find a higher likelihood of asking for a programme among the non-concessional users, but again this holds for *both* those with previous increases and previous decreases in the t-bill rate. As before, these effects vanish when comparing those countries with previous increases with those countries with previous decreases, pointing again to recidivism as a consequence of adverse selection into programmes.

Appendix Section B.7 presents results on how the treatment group differs if we control for programme duration in the first step. In this case, we also find a higher likelihood of asking for a programme except for the users of a previous concessional programme which experienced a decrease in t-bill rates. This difference persists when comparing countries with previous positive changes in t-bill rates with countries with previous negative changes in t-bill rates, but it does not persist when controlling for programme conditionality.

In Appendix Section B.8 we present evidence that there is no IMF financial market stigma either when we look at more granular IMF-related events.

Finally, in Appendix Section B.9 we present the full set of results when changing the dependent variable in the first stage from t-bill rates to stock market indices. These results point to an encouraging effect of previous increases in stock markets after an IMF programme, but contain no indication of financial market stigma.

6 Conclusion

An argument brought forward in the policy debate on the adequacy of the coverage of the Global Financial Safety Net (GFSN) suggests that some countries may not approach the IMF – the element of the GFSN with the most global coverage – because of a fear of adverse financial market reactions, such as excessive capital outflows. Given that a potential catalytic function is one of the IMF's goals in providing crisis funding to its

members while evidence of this effect of IMF programmes is mixed, we look into whether and how potential financial market reactions to an IMF programme may deter a country authority to seek IMF financial support. In other words, we investigate whether there is a form of IMF 'stigma' that is associated with movements in financial market variables that matter to governments. Therefore, we focus on 2-12 month treasury bill rates as well as on broad stock market indices.

Our approach has two steps. First, we ask a question which has been standard in the literature: what is the financial market reaction to an IMF programme? Since the literature is not clear on this question, also because of different approaches in terms of data frequency and estimation technique, we re-estimate the impact of IMF programmes on the t-bill rate and the stock market index with a monthly frequency which which we believe addresses many endogeneity concerns otherwise present in studies using annual data. This also makes our study more representative than country-specific case studies with higher frequency data.

Thereafter, we use a propensity score matching approach to compare countries which in the past experienced a negative market reaction with countries which never had a programme. We differentiate between concessional and non-concessional programmes.

Our study conveys three important results. First, we do not find any evidence of financial market stigma playing a role in agreeing on an IMF programme programme (with financial market stigma being defined as a previous increase in t-bill rates in response to a programme lowering the likelihood of a later programme), even after controlling for past and present economic conditions. Importantly, we also check for other channels of IMF financial market stigma, such as learning from peers, for which we find do not find any evidence either. While the data show that in some cases there are strong and negative financial market reactions, our analysis suggests that there is no general (perception of) financial market stigma since previous increases (as well as decreases) in t-bill rates if at all *raise* the likelihood of agreeing on a subsequent IMF

programme. Taken by itself, this result confirms the incidence of IMF recidivism.

Second, we find that an association between past market reactions and a *higher* subsequent likelihood of agreeing on an IMF programme in some specifications vanishes if we control for adverse selection and particularly when we control for programme design in the form of an index of 'hard conditionality' as inter alia used in Andone and Scheubel (2017). Since our specifications include several controls for how a previous programme may have shaped economic prospects and for countries' current economic prospects, our results do not suggest that repeated use of IMF programmes is associated with its effectiveness (or non-effectiveness). Instead, they suggest that either the index of conditionality measures adverse selection in that more vulnerable countries or that it points to difficulties in implementation which leads to more binding conditions. Therefore our results can be interpreted as further support for the 'adverse selection' explanation of a repeated use of IMF programmes as well as for the claim that implementation issues may be behind IMF recidivism.

Third, we extend the analysis beyond the common analyses of IMF programme approvals to other points in time with important news regarding a programme, such as the disbursement of a loan or a conclusion of a programme review and to the programme envelope. Particularly the analyses of the programme envelope suggest that at least previous users of IMF programmes need smaller programme envelopes in subsequent programmes, even though they might ask for them more frequently. Finally, we also look at the impact of past stock market movements in response to an IMF programme on the likelihood of agreeing on an IMF programme in the future. Also for such other IMF events and for different dependent variables we do not find evidence of financial market stigma.

Overall, our results suggest that there is no general IMF financial market stigma; on the contrary, our first stage results confirm that at least for some countries there is a certain catalytic effect of IMF programmes at least in the short run, i.e. for the

duration of a programme. Therefore, the results from this paper are important for the on-going policy debate, for example in the G20 fora, regarding the effectiveness of the GFSN. In particular, they suggest that the IMF's programmes are received as tools to improve a country's economic outlook particularly by stock markets. This implies that the notion of a more global problem of stigma is overstated. Instead, it might be used in individual cases by authorities struggling with transparency (e.g. concerns about revealing the country's economic situation to markets) or with a lack of ownership for unpopular reforms. In other words, our results suggest that IMF (financial market) stigma is less of an issue than e.g. problems associated with the implementation of IMF conditionality which may lead to a 'revolving door' use of IMF loans.

We do not provide additional evidence on the stigma associated with IMF conditionality, specifically that IMF conditionality which is considered as inadequate may lead to IMF stigma and that this effect may differ between IMF instruments and the type of conditionality attached. These issues are, inter alia, analysed discussed in Andone and Scheubel (2017). Neither do we provide evidence on the politics of stigma discussions associated with a perceived loss of sovereignty. Particularly the latter phenomenon is a case for future research.

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Appendix A: Data

A.1   MONA data

For this paper, we use IMF MONA data which is publicly available, for years 1992-2016. This data set includes 460 programmes in 118 countries. Table A.1 provides an overview of the number of programmes by facility. Among these, the most relevant is the SBA (Stand-by-Arrangement, first adoption in 1952) which can be considered the IMF workhorse and represents about the 71% of programmes (329 of in total 460). Other important facilities are the PRGF (Poverty Reduction and Growth Facility, 35%, first adoption in 1999) and the ESAF (Enhanced Structural Adjustment Facility, 22%, first adoption in 1975). Concessional programmes represent 54% of total number, as indicated in Table A.2.[17]

Table A.1: Distribution of IMF Programmes across facilities

Arrangement Type	N. of Programmes	Arrangement Type	N. of Programmes
ECF	38	PLL	2
EFF	51	PRGF	107
ESAF	75	PSI	19
ESF	4	SAF	2
FCL	15	SBA	196
PCL	1	SCF	9

Number of IMF programmes used, as distributed across facilities. Note that these do not sum to 460 since a number of programmes exploited more than one facility. Note that subsequent programmes are counted as new programmes, e.g. every renewal of an FCL is counted as a new FCL.

There are several types of IMF events which could potentially trigger a financial market reaction. These include the programme start dates and end dates, the date of approval by the IMF Executive Board, the scheduled, revised and completed date of each review, and the scheduled and actual dates for disbursements of a loan tranche.

As Table A.4 illustrates, most of the events are concentrated in regions other than

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[17]24 programmes included both a concessional and a non-concessional type of facilities. The are not reported in Table A.2.

Table A.2: Distribution of IMF Programmes according to their requirements

Type of Programme	Number	Percent
Non-Concessional Facilities	237	53.37
Concessional Facilities	207	46.62

Number of IMF programmes contained in the MONA database as distributed between concessional and non-concessional programmes. Note that 20 programmes are missing as these were programmes with both a concessional and a non-concessional component.

Table A.3: IMF programme-related events

Approval	Date of programme approval at the IMF Executive Board. On this date information on the key parameters of the arrangement is disclosed.
Start	Programme start date. Often coincides with Approval.
Original Disbursement	Date when a tranche of the loan is scheduled to be disbursed by the IMF.
Actual Disbursement	Actual date of disbursement, as modified by the IMF.
Original Review	Scheduled review dates by which certain conditions have to be met.
Revised Review	Revised date of a review
Completed Review	Actual date of review completion marked by an IMF board decision.
End	Actual date of arrangement conclusion, if the original end date had been modified.

Notes: Simplified classification of IMF events contained in the MONA database.

Advanced Economies and South-East Asia. Most events are recorded for Sub-Saharan Africa and Latin America followed by Asia (excluding South-East Asia), Commonwealth of Independent States (CIS) and Russia, and North Africa and the Middle East. The number of events is affected by the number of countries having a programme, the intensity of the programme which affects the number of reviews, and the progress in implementing the programme which affects the number of revisions to originally scheduled events. Table A.4 also shows how the events are distributed across the different regions. The distribution of IMF events closely follow the distribution of Approvals. Approximately a third of the programmes in Latin America and Sub-Saharan Africa were delayed. At the same time disbursements were rescheduled more often in Latin America and Advanced Economies than e.g. in Sub-Saharan Africa. At the same time, reviews were more often delayed in Sub-Saharan Africa.

The number of events per year varies between 3 and 118, as illustrated by Table A.5. Not surprisingly, the highest number of events is recorded during years of a regional or global crisis.

Table A.4: IMF events by region

Region	CIS and Russia	Advanced	South-East Asia	North-Africa and Middle East	Sub-Saharan Africa	Latin America and Caribbean	Asia ex. SE Asia	Total	%
Approval	54	25	30	39	166	94	56	464	8.90
Start	0	1	1	1	3	1	0	7	0.13
Actual End	50	20	26	33	126	77	48	380	7.29
Original Review	249	116	153	188	724	376	236	2,042	39.16
Revised Review	9	12	7	27	113	36	24	228	4.37
Completed Review	110	42	68	79	375	152	94	920	17.64
Original Disb.	109	69	47	75	292	190	75	857	16.43
Actual Disb.	46	12	21	30	131	46	31	317	6.08
Total	627	297	353	472	1,930	972	564	5,215	

Notes: Type of IMF events by regions as defined in the IMF WEO. *Start* is only counted if the start date does not coincide with the approval date. *Revised Review* and *Actual Disbursement* are only counted if they differ from *Original Review* and *Original Disbursement*. The number of specific events is reported together with the share of total IMF related events in parentheses.

Table A.5: IMF events by year

Year	Approval	Start	Actual End	Original Review	Revised Review	Completed Review	Original Disb.	Actual Disb.	Total	%
1992	7	0	0	3	0	1	3	0	14	0.27
1993	23	1	0	23	0	10	27	7	91	1.74
1994	34	3	15	52	0	26	81	12	223	4.28
1995	29	2	22	74	0	35	95	19	276	5.29
1996	31	1	21	85	0	36	118	18	310	5.94
1997	19	0	24	71	0	42	105	12	273	5.23
1998	21	0	18	76	0	33	95	12	255	4.89
1999	20	0	19	79	0	43	85	15	261	5.00
2000	22	0	20	93	0	45	81	11	272	5.22
2001	21	0	23	100	0	50	61	17	272	5.22
2002	21	0	21	94	1	55	42	13	247	4.74
2003	20	0	17	105	8	53	29	21	253	4.85
2004	13	0	21	102	10	46	20	16	228	4.37
2005	15	0	16	92	21	39	4	25	212	4.07
2006	14	0	15	88	26	37	2	14	196	3.76
2007	10	0	11	72	21	41	0	10	165	3.16
2008	19	0	11	64	15	40	1	12	162	3.11
2009	24	0	13	110	23	51	1	16	238	4.56
2010	31	0	19	139	26	52	2	20	289	5.54
2011	14	0	15	132	26	48	2	9	246	4.72
2012	18	0	17	113	15	35	1	15	214	4.10
2013	13	0	22	97	19	38	1	7	197	3.78
2014	12	0	13	91	11	29	1	12	169	3.24
2015	13	0	7	87	6	35	0	4	152	2.91

Notes: Type of IMF events by year as defined in the IMF WEO. *Start* is only counted if the start date does not coincide with the
approval date. *Revised Review* and *Actual Disbursement* are only counted if they differ from *Original Review* and *Original Disbursement*.

# Appendix B: Further regression results and robustness checks

B.1 First stage multivariate results by region and over time

Figure B.1 gives a better illustration of the distribution and the significance of the estimated coefficients $\hat{\boldsymbol{\beta}}_e$ from Equation (1) by region and over time. The top panel gives the distribution across regional groups as defined in the IMF's World Economic Outlook. The bottom panel gives the distribution over time. The boxplots in figure B.1 indicate that almost 75% of all coefficients on changes in t-bill rates following an IMF programme are negative for Emerging Asia. The results are balanced for former Soviet countries (Commonwealth of Independent States – CIS) and Emerging and Central Europe, while for all other regions they are skewed to the positive side. This would suggest that an IMF programme has particularly negative effects on a country's (short-term) refinancing costs in most regions while the effect on refinancing costs positive only for Emerging Asia. Moreover, the range of coefficients is broader for advanced economies than for other regions.[18] These results might suggest that the IMF's catalytic function may be more prevalent for the Emerging Asia, CIS and Emerging and Central Europe in that a programme plays a stronger role in raising confidence in the country's prospects.

When looking at the distribution of coefficients over time, shown in the bottom panel of Figure B.1, the results suggests that with the exception of 1997-1999 and 2006 the estimated coefficients are more positive than negative. Only during the Asian crisis (and in 2006/2014) they were clearly negative. This suggests that in most years IMF programmes were associated with *higher* refinancing costs on average. In sum, the majority of the distribution seems to be unaffected by global developments.

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[18]The results are neither driven by Greece nor by the three IMF programmes for euro area Member States.

Figure B.1: ESTIMATED EFFECTS OF IMF PROGRAMME APPROVALS ON THE T-BILL RATE BY REGION AND YEAR

Estimated coefficients by WEO regions

Estimated coefficients by year
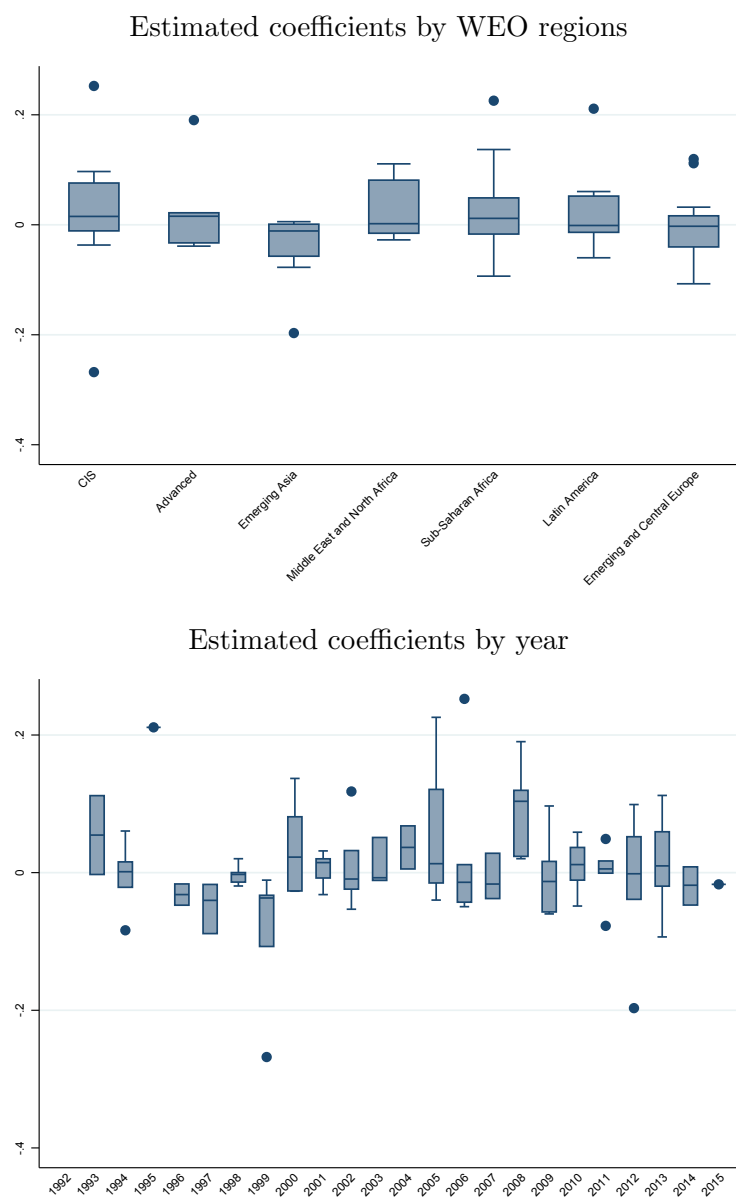
Notes: The figure illustrates the estimated coefficient β_e from Equation (1) by country and by year. The IMF dummy variable takes the value 1 for 2 months before the approval of a programme and 3 months after the approval of a programme, hence the depicted coefficients represent cumulative effects. For countries which appear more than once in the sample, only the last coefficient is reported. The regional classification is based on the IMF World Economic Outlook. The upper end of the box represents the third quartile while the lower end of the box represents the first quartile. The horizontal line in the box represents the median (second quartile). The whiskers represent the minimum and the maximum of the distribution while the dots represent the outliers.

B.2 Further robustness checks: symmetric window

Table B.1: The impact of previous positive changes in t-bill rates the likelihood of an IMF programme (control group: previous negative changes in t-bill rates)

	Baseline		Hard Conditions	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	-0.001		-0.001	
$m_{i,e-1}^{nonconc}$		-0.000		-0.007
Neighbours	10	10	10	10
Treated obs.	1483	1479	996	844
Total obs.	2581	2648	1832	1788

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes based on a symmetric window of three months (concessional columns 1,3; non-concessional columns 2,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.2: The impact of previous positive and negative changes in t-bill rates the likelihood of an IMF programme (control group: no previous programme)

| | Increase in t-bills vs. no previous progr | | Decrease in t-bills vs. no previous progr | |
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.005***			
$m_{i,e-1}^{nonconc}$		0.017**		
$m_{i,e-1}^{conc,-}$			0.006**	
$m_{i,e-1}^{nonconc,-}$				0.010***
Neighbours	10	10	10	10
Treated obs.	1483	1479	1098	1154
Total obs.	8842	8838	8457	9580

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes based on a symmetric window of three months, columns (1)-(2); or a negative change in the t-bill rate based on a symmetric window of three months, columns (3)-(4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables columns (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.3 Further robustness checks: controlling for sovereign debt restructuring

Table B.3: The impact of previous positive and negative changes in t-bill rates the likelihood of an IMF programme (control group: no previous programme)

	Increase in t-bills vs. no previous progr		Decrease in t-bills vs. no previous progr	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.002			
$m_{i,e-1}^{nonconc}$		-0.002		
$m_{i,e-1}^{conc,-}$			0.006	
$m_{i,e-1}^{nonconc,-}$				-0.010
Neighbours	10	10	10	10
Treated obs.	1277	722	993	1112
Total obs.	2475	1920	2191	2310

Notes: Results from a propensity score matching design, including the cumulative amount of previous sovereign debt restructuring in the first step. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (columns 1,2); or a negative change in the t-bill rate (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year; cumulative amount of previous sovereign debt restructuring. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.4: The impact of previous positive changes in t-bill rates the likelihood of an IMF programme (control group: previous negative changes in t-bill rates)

	Baseline		Hard Conditions	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.002		0.002	
$m_{i,e-1}^{nonconc}$		-0.001		-0.010
Neighbours	10	10	10	10
Treated obs.	1277	722	951	408
Total obs.	2270	1834	1706	1117

Notes: Results from a propensity score matching design, including the cumulative amount of previous sovereign debt restructuring in the first step. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the tbill rate during previous IMF programmes (concessional columns 1,3; non-concessional columns 2,4). Control group: observations for countries that experienced a decline in the tbill rates when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3;; voting aligned with main IMF shareholders at UN General Assembly, previous year; cumulative amount of previous sovereign debt restructuring. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.4 Further robustness checks: neighbours

Table B.5: The impact of previous positive and negative changes in t-bill rates in trade partners on the likelihood of an IMF programme (control group: no previous programme)

	Increase in t-bills vs. no previous progr		Decrease in t-bills vs. no previous progr	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.008**			
$m_{i,e-1}^{nonconc}$		0.003***		
$m_{i,e-1}^{conc,-}$			0.001	
$m_{i,e-1}^{nonconc,-}$				0.003**
Neighbours	10	10	10	10
Treated obs.	2520	4275	2763	3417
Total obs.	9257	9010	9232	9111

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: trade partners having had a positive change in the t-bill rate during previous IMF programmes, columns (1)-(2); or a negative change in the t-bill rate, columns (3)-(4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.6: The impact of previous pos. changes in t-bill rates in trade partners on the likelihood of agreeing on an IMF programme (control group: previous neg. changes)

| | Baseline | | Hard Conditions | |
	(1) Concessional	(2) Non- concessional	(3) Concessional	(4) Non- concessional
$m_{i,e-1}^{conc}$	0.006**		0.005	
$m_{i,e-1}^{nonconc}$		-0.000		0.002
Neighbours	10	10	10	10
Treated obs.	2520	4275	1226	1144
Total obs.	5283	7692	2657	2445

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: trade partners having had a positive change in the t-bill rate during previous IMF programmes (concessional columns 1,3, non-concessional columns 2,4). Control group: observations for countries that experienced a decline in the tbill rates when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.5 Further robustness: programme envelope

Table B.7: The impact of previous positive and negative changes in t-bill rates on the programme envelope (control group: no previous programme)

	Increase in t-bills vs. no previous progr		Decrease in t-bills vs. no previous progr	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	-0.08***			
$m_{i,e-1}^{nonconc}$		-0.051**		
$m_{i,e-1}^{conc,-}$			-0.055	
$m_{i,e-1}^{nonconc,-}$				-0.068***
Neighbours	10	10	10	10
Treated obs.	1553	1122	1028	1526
Total obs.	8912	8481	8387	9952

Notes: Results from a propensity score matching design. Dependent variable: original amount agreed for an concessional programme (1,3), original amount agreed for a non-concessional programme (2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes, columns (1)-(2); or a negative change in the t-bill rate, columns (3)-(4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.8: The impact of previous positive changes in t-bill rates on the programme envelope (control group: previous negative changes in t-bill rates)

	Baseline		Hard Conditions	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	-0.130**		-0.223**	
$m_{i,e-1}^{nonconc}$		0.014		0.013
Neighbours	10	10	10	10
Treated obs.	1553	1122	1066	681
Total obs.	2581	2648	1832	1788

Notes: Results from a propensity score matching design. Dependent variable: original amount agreed for an concessional programme (1,3), original amount agreed for an non-concessional programme (2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (concessional columns 1,3, non-concessional columns 2,4). Control group: observations for countries that experienced a decline in the tbill rates when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.6 Further robustness: new programmes only

Table B.9: The impact of previous positive and negative changes in t-bill rates the likelihood of an IMF programme (control group: no previous programme)

	Increase in t-bills vs. no previous progr		Decrease in t-bills vs. no previous progr	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.011			
$m_{i,e-1}^{nonconc}$		0.006***		
$m_{i,e-1}^{conc,-}$			0.003	
$m_{i,e-1}^{nonconc,-}$				0.007***
Neighbours	10	10	10	10
Treated obs.	1477	1747	981	865
Total obs.	8836	9106	8340	9291

Notes: Results from a propensity score matching design. Dependent variable: approval of a new concessional programme (columns 1,3), approval of a new non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (columns 1,2); or a negative change in the t-bill rate (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3 voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.10: The impact of previous positive changes in t-bill rates the likelihood of an IMF programme (control group: previous negative changes in t-bill rates)

	Baseline		Hard Conditions	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.000		-0.001	
$m_{i,e-1}^{nonconc}$		-0.002		-0.001
Neighbours	10	10	10	10
Treated obs.	1477	1747	1148	1196
Total obs.	2458	2612	1915	1785

Notes: Results from a propensity score matching design. Dependent variable: approval of a new concessional programme (columns 1,3), approval of a new non-concessional programme (columns 2,4). Treatment group: having had a positive change in the tbill rate during previous IMF programmes (concessional columns 1,3; non-concessional columns 2,4). Control group: observations for countries that experienced a decline in the tbill rates when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.7 Further robustness: Controlling For Duration

Table B.11: The impact of previous positive and negative changes in t-bill rates the likelihood of an IMF programme (control group: no previous programme)

| | Increase in t-bills vs. no previous progr | | Decrease in t-bills vs. no previous progr | |
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.008***			
$m_{i,e-1}^{nonconc}$		0.015***		
$m_{i,e-1}^{conc,-}$			0.005	
$m_{i,e-1}^{nonconc,-}$				0.006***
Neighbours	10	10	10	10
Treated obs.	1632	971	949	1677
Total obs.	8991	8330	8308	10103

Notes: Results from a propensity score matching design, including the cumulative duration of previous programmes in the first step. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes (columns 1,2); or a negative change in the t-bill rate (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year; cumulative duration of previous programmes. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.12: The impact of previous positive changes in t-bill rates the likelihood of an IMF programme (control group: previous negative changes in t-bill rates)

	Baseline		Hard Conditions	
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.004		0.001	
$m_{i,e-1}^{nonconc}$		0.015**		0.006
Neighbours	10	10	10	10
Treated obs.	1632	971	1101	705
Total obs.	2581	2648	1832	1788

Notes: Results from a propensity score matching design, including the cumulative duration of previous programmes in the first step. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a positive change in the tbill rate during previous IMF programmes (concessional columns 1,3; non-concessional columns 2,4). Control group: observations for countries that experienced a decline in the tbill rates when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3;; voting aligned with main IMF shareholders at UN General Assembly, previous year; cumulative duration of previous programmes. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.8 Further robustness: other IMF events

Table B.13 presents robustness checks using different types of IMF events as dependent variables. Results are shown only for the full sample without differentiating between concessional and non-concessional programmes. As we do not control for hard conditionality in the estimations for this table, we only find the previous result of a positive effect on programme approvals, but no effects for other IMF events. We consider this further evidence that the notion of financial market stigma does not show in the data.

Table B.13: The impact of previous positive and negative changes in t-bill rates on the likelihood of a specific IMF event (control group: previous negative changes in t-bill rates)

	Major Events		Reviews		
	(1) Approval	(2) End	(3) Original Rev	(4) Revised Rev	(5) Completed Rev
	0.002				
		-0.009			
			0.002		
				0.012	
					0.000
Neighbours	10	10	10	10	10
Treated obs.	2892	2238	2800	392	2375
Total obs.	5851	3505	6215	1057	4871

Notes: Results from a propensity score matching design. Dependent variable: Column (1) IMF programme approvals, Column (2) programme end date, Column (3) Original Review Date (may coincide with a disbursement), Column (4) Revised Review Date (as compared to the original schedule), Column (5) Completed Review (often coincides with a disbursement). Treatment: having experienced a negative change in the t-bill rate during previous IMF programme events. Matching variables: CPI, economic risk, growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.9 Further robustness: stock market indices as dependent variable in the first stage

The stock market index of a country is a good robustness check for our results since it is not influenced by possible changes at the long end of the yield curve or by changes in sovereign bond issuance behaviour in response to an IMF programme.

Figure B.2: Estimated effect of the approval of an IMF programme on the stock market index
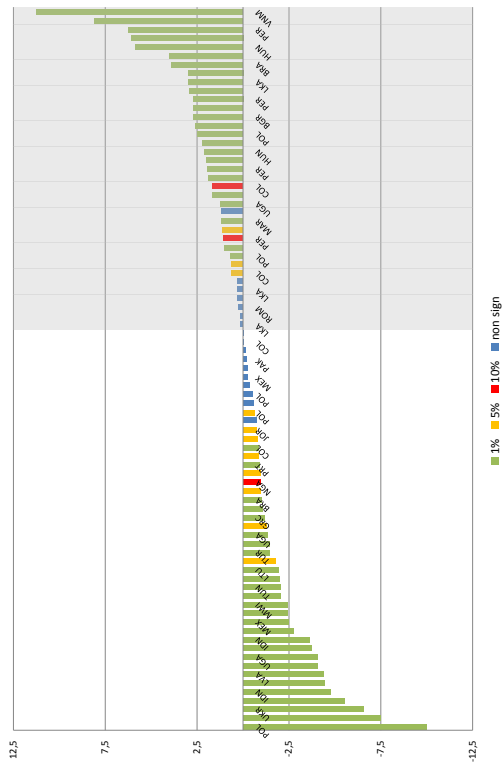
Notes: The figure illustrates the estimated coefficient β_e from Equation (1) by country when using the stock market index as dependent variable. The IMF dummy variable takes the value 1 for 2 months before the approval of a programme and 3 months after the approval of a programme, hence the depicted coefficients represent cumulative effects. For countries which appear more than once in the sample, only the last coefficient is reported. Coefficients which are significant at the 1% level are coloured green, coefficients which are significant at the 5% level are coloured orange and coefficients which are significant at the 10% level coloured red. Blue-coloured bars are insignificant.

We illustrate in Figure B.2 that we find a similar variation to t-bill rates in estimated stock market responses to an IMF programme.[19] Figure B.3 shows that the reaction in Emerging Asia is slightly positive and in Emerging and Central Europe clearly positive, while it is negative for all other regions. Therefore, while governments are arguable less reactive to stock market movements than movements in their own refinancing costs, if at all we should expect a form of financial market stigma related to reactions of the stock market in all regions except Emerging Asia and Europe. The bottom panel of figure B.3 shows negative reactions only in a limited number of years, including 1997 and 2008 as the initial years of the respective crises.

Table B.14 reports results for the control group of countries with no previous programme. We find a positive likelihood of entering an IMF programme *both* for previous decreases in stock market indices (albeit not for concessional programmes) and for previous increases in stock market indices. However, this result does not persist if we instead use as a control group countries which experienced a previous positive change in stock market indices and as a treatment group only those countries which experienced a previous negative change in stock market indices (refer to Table B.15). Therefore, we consider that the results in Table B.14 can be explained by those observations which experienced a positive stock market reaction in the past and hence were encouraged to approach the IMF again when in need.

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[19]For stock market indexes, we have 65 programmes significant out of 88, with 50 programmes significant at the 1%. 42 events increased the value of the stock index (largest event: Vietnam, April 2001, +11%), and 46 events had a negative effect (worst event: Ukraine, November 2008, -6.6%).

Figure B.3: ESTIMATED EFFECTS OF IMF PROGRAMME APPROVALS ON THE STOCK MARKET INDEX

Estimated effects of IMF programme aggregated by WEO regions.
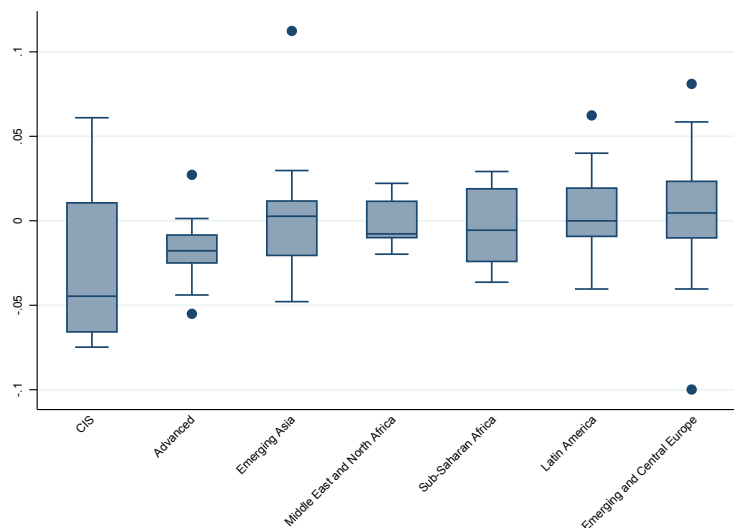
Estimated effects of IMF programme aggregated by year.
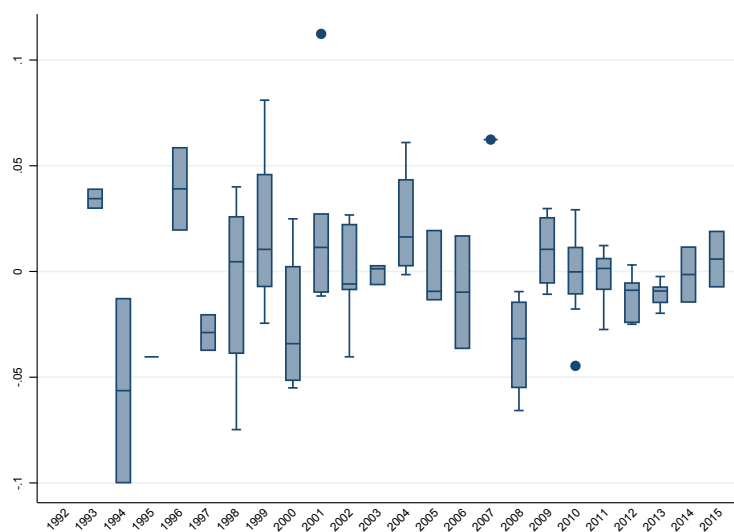
Notes: The figure illustrates the estimated coefficient β_e from Equation (1) by country. The IMF dummy variable takes the value 1 for 2 months before the approval of a programme and 3 months after the approval of a programme, hence the depicted coefficients represent cumulative effects. For countries which appear more than once in the sample, only the last coefficient is reported. The regional classification is based on the IMF World Economic Outlook. As usual with boxplots, the upper end of the box represents the third quartile while the lower end of the box represents the first quartile. The horizontal line in the box represents the median (second quartile). The whiskers represent the minimum and the maximum of the distribution while the dots represent the outliers.

Table B.14: Impact of previous negative and positive changes in stock market indexes on the likelihood of an IMF programme (control group: no previous programme)

| | Previous decrease in stock mkt index | | Previous increase in stock mkt index | |
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.007			
$m_{i,e-1}^{nonconc}$		0.026*		
$m_{i,e-1}^{conc,+}$			0.003***	
$m_{i,e-1}^{nonconc,+}$				0.004***
Neighbours	10	10	10	10
Treated obs.	223	1634	274	2379
Total obs.	8500	9948	7883	10693

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a negative change in the stock index during previous IMF programmes (columns 1,2); or a positive change in the stock index (columns 3,4). Control group: no previous IMF programme (includes countries which never had an IMF programme and observations early years when a country has not yet had any IMF programme). Matching variables (all columns): economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3; voting aligned with main IMF shareholders at UN General Assembly, previous year. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table B.15: Impact of previous negative changes in stock market indexes on the likelihood of agreeing on an IMF programme (control group: previous positive changes)

| | Baseline | | Hard Conditions | |
	(1) Concessional	(2) Non-Concessional	(3) Concessional	(4) Non-Concessional
$m_{i,e-1}^{conc}$	0.008		-0.004	
$m_{i,e-1}^{nonconc}$		-0.006		-0.003
Neighbours	10	10	10	10
Treated obs.	274	2379	117	1395
Total obs.	485	4013	291	2564

Notes: Results from a propensity score matching design. Dependent variable: approval of a concessional programme (columns 1,3), approval of a non-concessional programme (columns 2,4). Treatment group: having had a negative change in the stock index during previous IMF programmes (concessional columns 1,3; non-concessional columns 2,4). Control group: observations for countries that experienced a increase in the stock index when programmes where approved (concessional columns 1,3, non-concessional columns 2,4). Matching variables columns (1,3): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3;; voting aligned with main IMF shareholders at UN General Assembly, previous year. Columns (2,4) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

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