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## STIGMA? WHAT STIGMA? A Contribution to the Debate on the Effectiveness of IMF Lending

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# STIGMA? WHAT STIGMA?

## A Contribution to the Debate on the Effectiveness of IMF Lending

### Abstract

There is a perception that IMF programmes are not catalytic and instead associated with large capital outflows, higher refinancing costs for sovereigns and adverse movements in stock markets. This has led to concerns that an expectation 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 address these questions 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 a propensity score matching approach, indicate no role for ‘IMF stigma’ stemming from the fear of adverse market movements.

JEL-Codes: E020, F320, F330, F340.

Keywords: capital flows, IMF conditionality, IMF recidivism, global financial safety net, Asian crisis, treasury bill rates.

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## **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 impairs the effectiveness of IMF lending as part of the GFSN.

The underlying reasons for the perception of stigma can broadly be divided in two categories. In case of political stigma, a government might refrain from requesting an IMF programme in view of the (expected) political backlash if politicians, media or the public perceive an infringement of the country's sovereignty or a loss of face, or if the conditionality is perceived as too harsh. In case of financial market stigma, the authorities might fear an adverse financial market reaction. In this paper we will focus on the latter aspect.

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 past market reactions determine the future likelihood of governments asking for an IMF programme by comparing similar countries with different experiences 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 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. Given that 'IMF stigma' has been mentioned in the international policy debate also as a regional phenomenon, we also 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. Finally, we do find some evidence of a so-called 'revolving door' effect of IMF programmes, i.e. that past users of IMF loans are more likely to request one again, but this effect vanishes if we control for the type of conditions in those past programmes. In particular, if we control for the number of conditions which are essential for disbursements of loan tranches and for the number of structural conditions, the 'revolving door' effect does not persist. We interpret this result as an indication that those countries are more likely to be persistent users of IMF resources, which face particularly difficult economic conditions or which face difficulties in implementing IMF conditionality. However, we cannot say whether such difficulties in implementation would be driven by country-specific conditions or by the design of IMF conditionality, both of which are considered possible explanations in the literature.

Overall, our results imply that the notion of a generalised (perception of) financial market stigma is overstated. Instead, revoking the notion of IMF financial market stigma 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.

# 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 a programme or the successful continuation of it. The idea behind such a notion of 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 and adverse financial market reaction instead of fostering confidence in the future growth path.

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 its economic and political fundamentals.<sup>1</sup>

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)

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<sup>1</sup>For a recent debate of conditionality stigma refer to Andone and Scheubel (2017).

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 global factors to estimate the reaction to every IMF programme-related announcement (event) for every country in the sample.<sup>2</sup>

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

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<sup>2</sup>To handle the possible endogeneity of IMF programmes determined by anticipation effects and market rumours, we define a window of 6 months around each "IMF event". Moreover, as the decision for an IMF programme in  $t$  may be related to past country fundamentals which are also related to past market reactions, and past market reactions may have affected current fundamentals, we also control for these confounding factors. Our set of control variables includes global risk and market variables, some key country fundamentals, as well as lags of these variables.



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. In view of the limited observations for individual countries, identifying whether observed past reactions determine the future likelihood of an IMF programme is only possible in a cross-country setting.

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<sup>3</sup> 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 panel 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

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<sup>3</sup>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.

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 country decision. Overall, our results, after controlling for fundamentals and selection bias, do not give any evidence for financial market stigma.

Section 2 provides an 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**

There are only few studies which try to assess the impact of IMF programmes on financial markets for a large group of countries. Chapman et al. (2015) argue that multilateral crisis lending affects markets not in one but in various ways. On the one hand, it can reassure bond markets by providing liquidity to the sovereign. On the other hand, a programme announcement can reveal private information. Moreover, the market reaction also depends on the expectations about compliance with conditionality. Using a sample of 66 non-OECD countries from 1992 to 2002, they find that increasing the scope of conditionality reduces the yield on short-term government bonds and that larger IMF loans are more effective at stemming capital flight. They also find that the net effect of announcing a new programme is to raise borrowing costs.

Mody and Saravia (2006) investigate whether IMF programmes improve spreads paid on the bonds with an average maturity of seven years issued from 1990 to 1999 by a sample of emerging market and developing countries. They find that IMF programmes do not provide a uniformly favourable signalling effect. Instead, their evidence is most consistent with a positive effect of IMF programmes when they are viewed as likely to lead

to policy reform and when undertaken before economic fundamentals have deteriorated significantly. The size of the IMF's programme matters but the credibility of a joint commitment by the country and the IMF appears to be critical.

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.<sup>4</sup> However, the positive effect seems to reverse if governments do not implement the conditions (Kutan et al., 2012). Evidence on an impact on 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

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<sup>4</sup>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-à-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.

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.

While the literature is thus not clear on the 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

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. <sup>a</sup>	%	Haver/IFS	January 1992 - Dec 2015, 109 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 Forecast	Monthly weighted average of the semestral 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 2010, 140 Countries
Eco	Level of economic risk	Index	ICRG	January 1992 - Dec 2010, 140 Countries
Fin	Level of financial risk	Index	ICRG	January 1992 - Dec 2010, 140 Countries
Comp	Weighted average of economic, political, and financial risks	Index	ICRG	January 1992 - Dec 2010, 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
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.

<sup>a</sup>The bond maturity ranges from 12 weeks to 1 year depending on the country's definition.

yields. In particular, we use the monthly treasury bill rates reported in the International Financial Statistics (IFS). 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 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.<sup>5</sup> 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.

These treasury bill rate data we use are not harmonised across countries as countries report different instruments with maturities between 3 and 12 months.<sup>6</sup> 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.<sup>7</sup> 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.

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<sup>5</sup>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.

<sup>6</sup>Since bonds with a longer maturity have a more stable behavior and, consequently, present a smoother reaction to shocks, this heterogeneity can affect our estimates. Therefore, we also use other dependent variables and different samples as robustness checks.

<sup>7</sup>Also refer to Table 1. The treasury bill rate is denoted by *T-Bill*.

## 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 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. 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 particularly 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. Only if we are able to distinguish the market movement which is purely related to an IMF event as opposed to market movements, e.g. related to expectations about country fundamentals, we can



make sure that the past market reaction to the IMF programme is exogenous to the current economic situation and hence exogenous to other determinants of a current IMF programme.

To derive an unbiased estimate of a market reaction to an IMF programme, i.e. without confounding it with a reaction to e.g. worsened or improved country fundamentals, we follow the event study literature in using higher frequency data<sup>8</sup> and defining a window around any IMF event in the sample.<sup>9</sup> 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. This allows us to estimate an event-specific market reaction within this window.

To thereafter adequately identify the effect of a potential financial market reaction on the likelihood of agreeing on a programme we need to be careful to avoid circularity in the argument since we proxy the potential financial market reaction with the past financial market reaction to a programme and then would like to relate this to the future likelihood of the programme. Therefore, we do not estimate the effect of a potential financial market reaction to an IMF event on the likelihood of agreeing on a programme

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<sup>8</sup>Studies using daily data (e.g. Kaminsky and Schmukler, 1999; Brealey and Kaplanis, 2004; Evrensel and Kutan, 2008a,b; Glennester and Shin, 2008; 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.

<sup>9</sup>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, 2016). However, annual data often only reports the average of higher frequency observations, and this raises two concerns. First, annual data 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 an 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 makes it more difficult to address the potential reverse causality between macro-financial outcomes and IMF programmes. Worsening macroeconomic and financial fundamentals may be the reason for a country to approach the IMF for help, but approaching the IMF for help may lead to worse macro-financial outcomes. Therefore, it is important to control for the macro-financial situation before an IMF event; yet doing so is difficult with annual data whenever an IMF event does not take place at the beginning or the end of a year. As a consequence, estimates based on annual observations may be biased (Cavallo and Rigobon, 2016).

for each country in the sample, but instead we use a propensity score matching approach 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.

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

We estimate the following panel model to derive an unbiased estimate of the market reaction to an IMF event:

$$\begin{aligned}
 y_{i,t} = & \alpha_i + f_t + \sum_{j=1}^n \delta^j y_{i,t-j} + \sum_{j=0}^n \gamma^j \mathbf{x}_{i,t-j} \\
 & + \beta^e IMF_{i,t,e} + \sum_{j=0}^n \phi^j \bar{\mathbf{z}}_{i,t-j} + \epsilon_{i,t}
 \end{aligned} \tag{1}$$

The dependent variable  $y_{i,t}$  is the logarithm of either a country's t-bill rate or the stock market index.  $(\alpha_i)$  denote country fixed effects and  $(f_t)$  denote time fixed effects to account for structural differences and global/regional factors.  $y_{i,t-j}$  denotes the  $t-j$  lag of the dependent variable, with  $n \in (1, 3)$ .  $\mathbf{x}_{i,t-j}$  is the vector of the  $t-j$  lag of the exogenous variables (country fundamentals), with  $j \in (0, 3)$ . Lag lengths have been chosen in line with the AIC criterion. More specifically  $\mathbf{x}$  includes: CPI, reserves, the exchange rate, as well as in some specifications the political risk, financial risk, and economic risk measures prepared by the PRS group, and IMF WEO bi-annual growth forecasts.<sup>10</sup> To avoid endogeneity, we restrict the contemporaneous effect of IMF programmes, reserves, exchange rate, and political and financial risk to be zero. This is necessary as contemporaneous changes in these variables may be related to the IMF event.

To be able to estimate market reactions to specific IMF events in a panel set-up, we

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<sup>10</sup>Also refer to Table 1).

construct dummy variables, one for each IMF event in the sample, which are denoted by  $IMF_{i,t,e}$ . These dummy variables take the value 1 if there is a programme  $e$  in country  $i$  at time  $t$  and can therefore be interpreted like country-time fixed effects. As a consequence,  $\beta^e$  represents the estimated marginal effect of IMF event  $e$ .

The vector  $z_{i,t-j}$  includes controls for global and regional factors which cannot be adequately captured by either the country or time fixed effects. 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 units (Phillips and Sul, 2003; Phillips and Su, 2007; Sarafidis and Robertson, 2009; Chudik and Pesaran, 2015).<sup>11</sup> 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, which is similar to the estimators proposed in Pesaran (2006) and Bai (2009), in that we deal with global unobserved factors and strong cross-sectional dependency by adding regional averages. In particular, we add weighted averages for trade partners of variables in  $\mathbf{x}$  with the weights calculated as the percentage of imports of country  $i$  from country  $k$ , trade partners' averages of the dependent variable  $y$ , the VIX to control for global financial conditions, and a regional financial index. The regional financial index is calculated as the average of financial indexes for countries belonging to the same regions of country  $i$ . All control variables are included with lag  $j \in (0, 3)$ , with the exception of the reserves, trade partners' average of the dependent variable, and the average of the regional financial index, which enter with a lag  $j \in (1, 3)$ . To further account for cointegrating relations, we express the model in

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<sup>11</sup>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.

differences (Granger and Newbold, 1974).

### 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, we then apply a propensity score matching approach to compare countries with past positive or negative movements to countries without a movement or no programme.

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  $t$ :

$$IMF_{i,t} = f(m_{i,t}|\Upsilon) + \eta_{i,t} \quad (2)$$

where  $IMF_{i,t}$  is an indicator variable which is equal to 1 if an IMF programme is approved in month  $t$ ,  $m_{i,e-1}$  is a dummy variable taking value equal one if the estimated effect of the previous IMF programme in step 1 has been positive, i.e.  $m_{i,e-1} = 1$  iff  $\hat{\beta}_{e-1} > 0$ ,  $\Upsilon = \sum_{j=0}^n \gamma^j \mathbf{x}_{i,t-j}$  and  $\mathbf{x}$  is the same vector of control variables described above.

What we propose here is that a country authority agreeing on an IMF programme in month  $t$  is a non-parametric function of having experienced an increase in t-bills prior to  $t$ , conditional on the fundamentals  $\Upsilon$ . ‘Treatment’ in our setting can hence 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  $t$ , or, in our robustness checks, as having experienced a *decrease* in t-bill rates prior to  $t$ . This ensures that we test our hypothesis against two counterfactuals. Therefore, step 2 provides an estimate of the treatment on the treated by using a propensity score matching where

countries are matched on fundamentals.

Arguably, this approach does not account for cases which have never experienced an IMF event in the past not because it did not need IMF support, but because financial market stigma has become a regional issue resulting in some countries having never had an IMF programme *because of* IMF stigma, our results would be biased downwards. To work around this issue we build on the fact that this would be mostly country authorities who see adverse financial market reactions to IMF programmes among their peers may be more reluctant to approach the IMF for help. Instead of conditioning on own past experience, we condition on neighbours' past experience, similar to Andone and Scheubel (2017), estimating equation (2) as:

$$IMF_{i,t} = f(m_{i,t}^n | \mathbf{\Upsilon}) + \eta_{i,t} \quad (3)$$

with  $m_{i,t}^n$  denoting the average past reaction among neighbours.

## 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.<sup>12</sup> 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

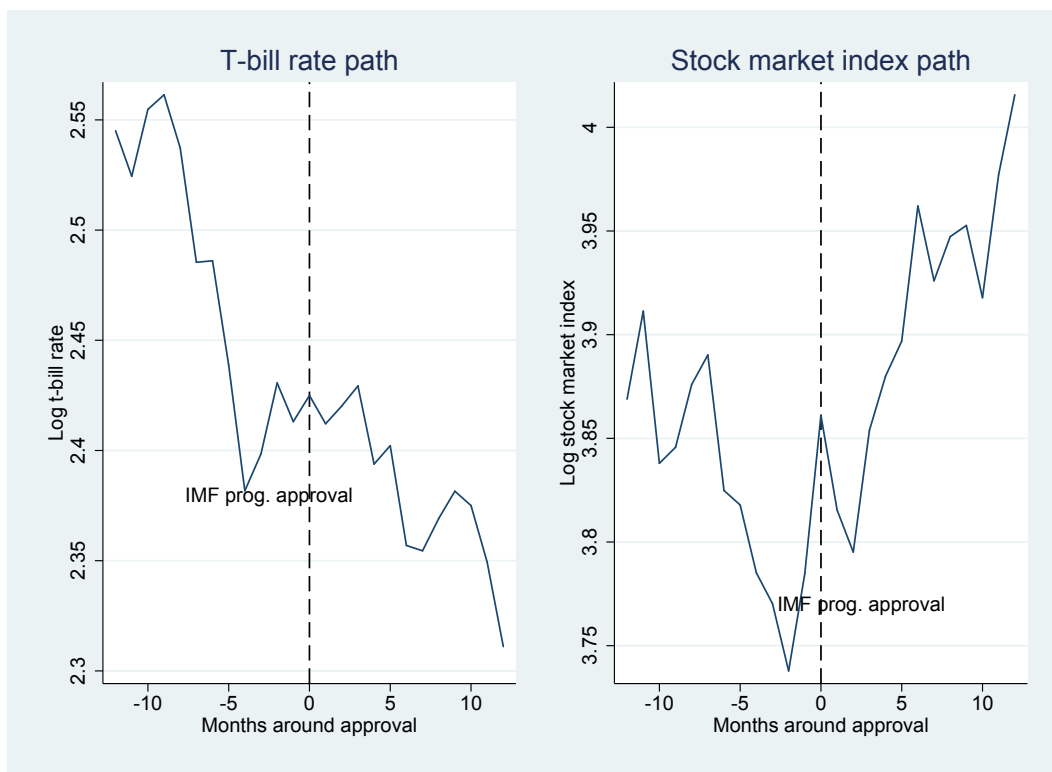
Variable	Cumulated	Impact	Concessional	Non-concessional	Advanced	Emerging
T-bill Rate	0.0363	0.0346	0.0204	0.0406	-0.0117	0.0383
Stock Index	-0.044	-0.0302	-0.0184	-0.066	-0.0097	-0.0287

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, 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 for the stock market index 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.

<sup>12</sup>Data availability restricts the sample to 147 programmes when considering the t-bill rate and 84 programmes when considering the stock market index.

Figure 1: Average T-bill rates and stock market indices around IMF programme approvals

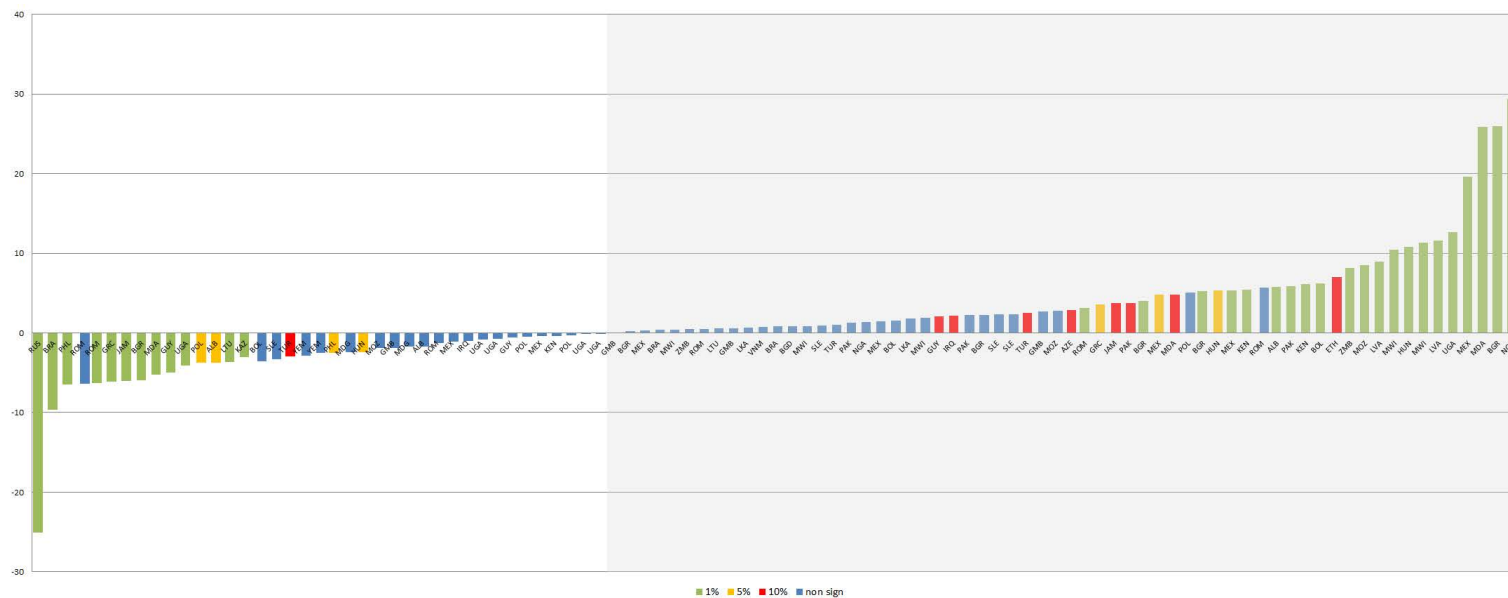


Notes: The left panel depicts the average behaviour of t-bill rates from 12 months before to 12 months after the approval of an IMF programme. The right panel depicts the average behaviour of stock market indices from 12 months before to 12 months after the approval of an IMF programme.

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



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Notes: The figure illustrates the estimated coefficients  $\beta_{i,t}^e$  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% (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 the impact of three coefficients of interest: (i) the impact of the past movement in t-bill rates ( $m_{i,e-1}$ ), (ii) the past movement in t-bill rates only for concessional programmes ( $m_{i,e-1}^{conc}$ ), and (iii) the past movement in t-bill rates only for non-concessional programmes ( $m_{i,e-1}^{nonconc}$ ). Table 3 compares country/year observations which are characterised by a previous increase in t-bill rates in response to an IMF programme to country/year observations which are characterised by no previous IMF programme, as well as country/year observations which are characterised by a previous decrease in t-bill rates in response to an IMF programme to country/year observations which are characterised by no previous IMF programme.

Table 3 suggests that both for concessional and non-concessional programmes, a country which has experienced a previous increase in t-bills is slightly *more* likely and

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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,t-1}^+$	0.005					
$m_{i,t-1}^{conc,+}$		0.005*				
$m_{i,t-1}^{nonconc,+}$			0.011**			
$m_{i,t-1}^-$				0.002		
$m_{i,t-1}^{conc,-}$					0.005	
$m_{i,t-1}^{nonconc,-}$						0.006
Neighbours	10	10	10	10	10	10
Treated obs.	2880	1643	1166	2067	676	1195
Total obs.	4407	3170	2693	3594	2203	2722

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1,4), approval of a concessional programme (2,5), approval of a non-concessional programme (3,6). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes, columns (1)-(3); or a negative change in the t-bill rate, columns (4)-(6). 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 (1-6): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

not less likely to approach the IMF again. Lower t-bill rates after an IMF programme do not lower the future likelihood significantly of approaching the IMF again. These results do not provide evidence for the existence of IMF stigma. However, they could be considered as evidence of a revolving-door effect of IMF programmes (Conway, 2007), i.e. a repeated use of IMF support which may be due to a failure of fully reaching the programme's objectives because of a flaw in its design or a lack of implementation or - possibly - moral hazard. This is particularly noteworthy since we do not only control for a country's growth prospects in the first stage when estimating the market reaction, but also because we match the growth prospects. In other words, when comparing countries with a similar growth outlook, those which in the past experienced an increase in t-bill rates are still more likely to ask for a programme. We show in our sensitivity analyses in the next sections that this 'revolving door effect' is largely driven by tough and possibly ineffective IMF programmes.

### **5.3.2 Robustness: control group**

To see whether the effect persists when using a different control group, we compare countries with a previous negative experience with countries with a previous positive experience. Table 4 shows the same baseline regression as in Table 3, except for changing the control group to countries which experienced lower t-bill rates after an IMF programme. Here we still find a *positive* effect of previous increases in t-bill rates on the likelihood of asking for an IMF programme, which is however only marginally significant and only in the full sample.

### **5.3.3 Robustness: programme design**

To further investigate the nature of the 'revolving door effect', we further account for programme design. We already control for the possibility that programmes had simply not been successful in the past by including WEO growth outlook and a country's

Table 4: 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) All Programmes	(2) Concessional	(3) Non Concessional
$m_{i,e-1}$	0.005*		
$m_{i,e-1}^{conc}$		-0.001	
$m_{i,e-1}^{nonconc}$			0.004
Neighbours	10	10	10
Treated obs.	2880	1643	1166
Total obs.	4947	2319	2361

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 (3). 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 were approved (all columns 1, concessional columns 2, non concessional columns 3). 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

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 to the specification from Table 4 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). Table 5 presents these results.

If hard conditionality is included in the matching variables, the positive effect of past increases in t-bill rates on the likelihood of agreeing on an IMF programme vanishes, i.e. previous experience doesn't matter, neither positive nor negative experience. We interpret this as some evidence of the 'revolving door effect'. Since we are confident that we already control for economic conditions in both stages, we conjecture that the

Table 5: Controlling for conditionality (control group: previous negative changes in t-bill rates)

	(1) All Programmes	(2) Concessional	(3) Non Concessional
$m_{i,e-1}$	0.004		
$m_{i,e-1}^{conc}$		0.000	
$m_{i,e-1}^{nonconc}$			0.006
Neighbours	10	10	10
Treated obs.	2388	1332	985
Total obs.	4365	1971	2127

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 (3). 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 were 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, financial risk, lag 1-3, number of hard conditions based on Andone and Scheubel (2017) to account for programme design.\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

revolving door effect is related to implementation problems associated with particularly tough conditionality.

### 5.3.4 Robustness: estimates based on neighbours' experience

Our results do not change if we instead only consider treatment based on *neighbours'* experience with the IMF. These results can be found in Appendix Section B.2. In particular, when comparing countries with neighbours that experienced an increase in t-bill rates to countries with neighbours that experienced a decrease in t-bills we do not find any significant results. When comparing countries with neighbours that experienced an increase in t-bill rates to countries which did not have a previous programme, we find the same results as when we define treatment based on own experience. Specifically, we again find a *positive* likelihood of approaching the IMF. Again, this effect vanishes if we control for neighbours' hard conditions and hence we conclude that the positive effect we find is related to implementation issues.

### 5.3.5 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.3 we detail that we do not find any evidence of experiences of past increases in t-bill rates on the envelope of a possible future programme. In Appendix Section B.4 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.5 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 not 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 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 experienced a positive reaction as well as 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 (with financial market stigma being defined as a previous negative market reaction lowering the likelihood of a later programme), even after controlling for past and present economic conditions. 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. Second, by contrast, we find some evidence for a revolving door effect, i.e. past negative market reactions being associated with a higher subsequent likelihood of agreeing on an IMF programme. However, also this effect vanishes if we control for programme design in the form of an index of ‘hard conditionality’ as *inter alia* used in Andone and Scheubel (2017), a measure of how many prior actions and structural conditions were included in a programme.

This paper also provides several robustness checks which underpin the results. In addition to using high-frequency data, we also include controls for economic prospects and political and economic fundamentals to show that our results are not driven by improved fundamentals due to previous successful programmes. Our results are also

robust to using country fixed effects. Most importantly, however, 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. 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 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 overall are received as tools to improve a country's economic outlook particularly 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 evidence on the stigma associated with IMF conditionality, which is *inter alia* discussed in Andone and Scheubel (2017), nor do we provide evidence on the stigma 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.<sup>13</sup>

Table A.1: Distribution of IMF Programmes across facilities

Arrangement Type	N. of Programmes	Arrangement Type	N. of Programmes
ECF	67	PLL	4
EFF	70	PRGF	163
ESAF	102	PSI	25
ESF	6	SAF	0
FCL	24	SBA	329
PCL	2	SCF	13

Number of IMF programmes used as distributed across the different facilities. Note that tools do not sum to 460 since a number of programmes exploited more than one tool.

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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<sup>13</sup>24 programmes included both a concessional and a non concessional type of facilities. They 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	236	54.63
Concessional Facilities	200	46.36

Number of IMF programmes contained in the MONA database as distributed according to their characteristics. Note that are missing 20 programmes where twofold programmes with both a concessional and a non concessional component have been adopted.

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	15	10	8	17	50	22	25	147	8
Start	1	1	1	2	3	3	1	12	1
Actual End	20	11	10	16	48	23	21	149	8
Original Review	92	47	53	82	213	122	91	700	38
Revised Review	1	9	2	13	37	16	4	82	4
Completed Review	48	20	19	31	113	49	42	322	17
Original Disb.	51	24	6	35	70	52	46	284	15
Actual Disb.	16	4	12	19	52	26	27	156	8
Total	244	126	111	215	586	313	257	1852	

*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	1	0	0	0	0	0	1	1	3	0.16
1993	5	0	0	3	0	1	7	2	18	0.97
1994	11	4	6	15	0	8	23	6	73	3.94
1995	6	0	10	18	0	11	30	4	79	4.27
1996	7	1	2	32	0	9	45	7	103	5.56
1997	7	0	10	28	0	16	45	4	110	5.94
1998	8	1	10	24	0	11	22	8	84	4.54
1999	4	1	9	29	0	16	29	19	107	5.78
2000	6	4	11	28	0	12	32	9	102	5.51
2001	12	1	13	38	0	14	25	8	111	5.99
2002	8	0	6	45	1	24	15	12	111	5.99
2003	3	0	4	49	6	29	5	11	107	5.78
2004	5	0	8	44	5	27	1	12	102	5.51
2005	4	0	9	29	9	19	0	14	84	4.54
2006	10	0	7	30	5	15	2	6	75	4.05
2007	3	0	6	32	6	15	0	4	66	3.56
2008	7	0	2	27	4	18	0	4	62	3.35
2009	5	0	6	39	8	20	1	3	82	4.43
2010	17	0	9	53	14	16	1	8	118	6.37
2011	5	0	7	54	11	19	0	3	99	5.35
2012	6	0	5	46	6	12	0	6	81	4.37
2013	7	0	9	35	6	10	0	5	72	3.89
2014	0	0	0	2	1	0	0	0	3	0.16

*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  $\beta_{i,t}^e$  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.<sup>14</sup> 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.

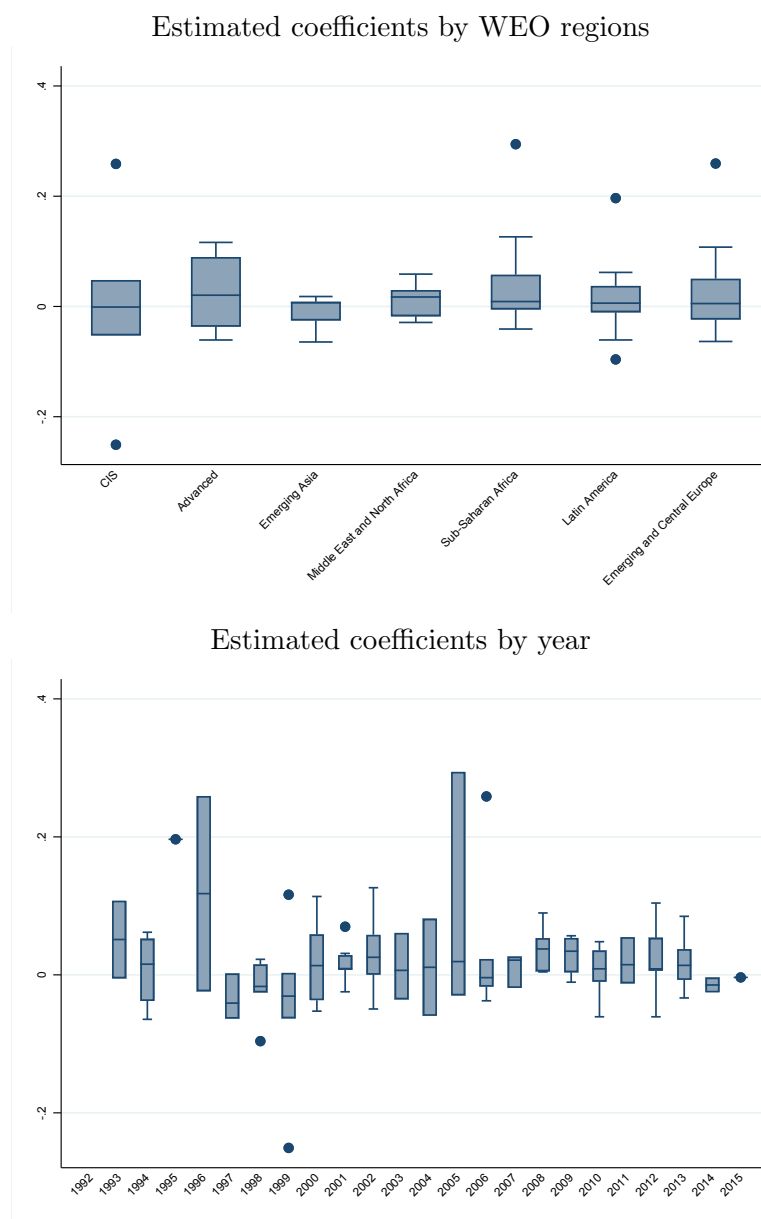
When looking at the distribution of coefficients over time, shown in the bottom panel of Figure B.1, the results suggests that with the exception of 1997-1999 and 2006 the estimated coefficients are more positive than negative. Only during the Asian crisis (and in 2006/2014) they were clearly negative. 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.

### B.2 Further robustness checks: neighbours

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<sup>14</sup>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



Notes: The figure illustrates the estimated coefficient  $\beta_e$  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.

Table B.1: The impact of previous neg. changes in t-bill rates in trade partners on the likelihood of agreeing on an IMF programme (control group: previous pos. changes)

	Baseline			Hard Conditions		
	(1) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}$	-0.002			-0.002		
$m_{i,e-1}^{conc}$		0.002			-0.001	
$m_{i,e-1}^{nonconc}$			0.003			0.002
Neighbours	10	10	10	10	10	10
Treated obs.	3655	2452	2110	2782	2199	1432
Total obs.	5430	3567	3467	4280	3145	2662

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1,4), approval of a concessional programme (2,5), approval of a non concessional programme (3,6). Treatment group: trade partners having had a positive change in the tbill rate during previous IMF programmes (1). Control group: observations for countries that experienced a decline in the tbill rates when programmes were approved (all columns 1,4, concessional columns 2,5, non concessional columns 3,6). 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. Columns (4)-(6) 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 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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}^+$	0.005					
$m_{i,e-1}^{conc,+}$		0.005**				
$m_{i,e-1}^{nonconc,+}$			0.006**			
$m_{i,e-1}^-$				0.007		
$m_{i,e-1}^{conc,-}$					0.003	
$m_{i,e-1}^{nonconc,-}$						0.006
Neighbours	10	10	10	10	10	10
Treated obs.	3655	2452	2110	1775	1115	1357
Total obs.	4801	3924	3267	3218	2604	2838

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1,4), approval of a concessional programme (2,5), approval of a non-concessional programme (3,6). Treatment group: trade partners having had a positive change in the t-bill rate during previous IMF programmes, columns (1)-(3); or a negative change in the t-bill rate, columns (4)-(6). 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 (1-6): CPI, economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

### B.3 Further robustness: programme envelope

Table B.3: 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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}$	-0.011			-0.011		
$m_{i,e-1}^{conc}$		-0.006			-0.008	
$m_{i,e-1}^{nonconc}$			-0.004			-0.010
Neighbours	10	10	10	10	10	10
Treated obs.	2880	1643	1166	2388	1332	985
Total obs.	4947	2319	2361	4365	1971	2127

Notes: Results from a propensity score matching design. Dependent variable: original amount agreed for an IMF programme (1,4), concessional programme amount (2,5), non concessional programme amount (3,6). 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 were approved (all columns 1,4, concessional columns 2,5, non concessional columns 3,6). 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. Columns (4)-(6) 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.4: 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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}^+$	-0.012					
$m_{i,e-1}^{conc,+}$		-0.012				
$m_{i,e-1}^{nonconc,+}$			-0.020			
$m_{i,e-1}^-$				-0.007		
$m_{i,e-1}^{conc,-}$					-0.004	
$m_{i,e-1}^{nonconc,-}$						-0.021
Neighbours	10	10	10	10	10	10
Treated obs.	2880	1643	1166	2067	676	1195
Total obs.	4407	3170	2693	3594	2203	2722

Notes: Results from a propensity score matching design. Dependent variable: original amount agreed for an IMF programme (1,4), concessional programme amount (2,5), non-concessional programme amount (3,6). Treatment group: having had a positive change in the t-bill rate during previous IMF programmes, columns (1)-(3); or a negative change in the t-bill rate, columns (4)-(6). 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## B.4 Further robustness: other IMF events

Table B.5 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.5: 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)

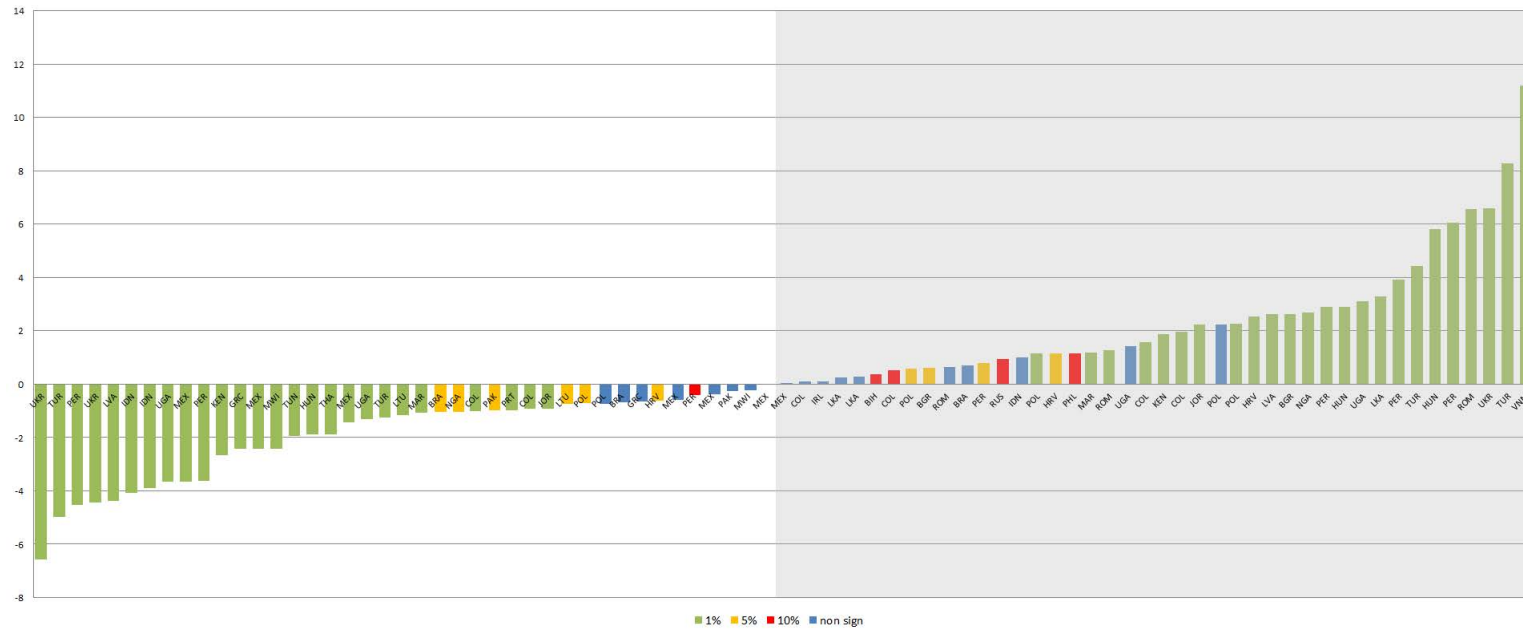
	Major Events			Reviews		
	(1) Approval	(2) Start	(3) End	(4) Original Rev	(5) Revised Rev	(6) Completed Rev
	0.006*					
		-0.126				
			-0.006			
				0.005		
					0.015	
compl						-0.006
Neighbours	10	10	10	10	10	10
Treated obs.	2606	205	1827	2626	326	2269
Total obs.	4947	235	3132	5263	968	3941

Notes: Results from a propensity score matching design. Dependent variable: Column (1) IMF programme approvals, Column (2) of the programme start, Column (3) programme end date, Column (4) Original Review Date (may coincide with a disbursement), Column (5) Revised Review Date (as compared to the original schedule), Column (6) Completed Review (often coincides with a disbursement). Treatment: having experienced a positive 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. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

## B.5 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  $\beta_e$  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 are 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.<sup>15</sup> 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.

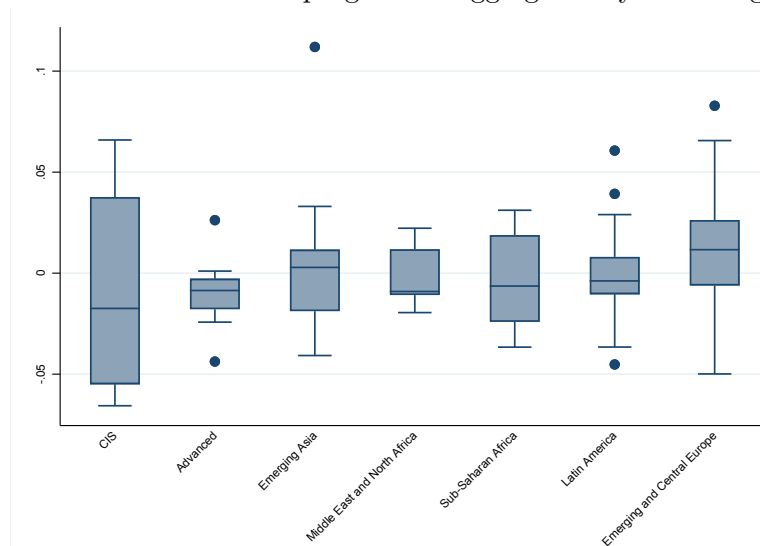
Table B.6 reports results for the control group of countries with no previous programme. We find a positive likelihood of entering an IMF programme *both* for previous decreases in stock market indices (albeit not for concessional programmes) and for previous increases in stock market indices. However, 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.7). Therefore, we consider that the results in Table B.6 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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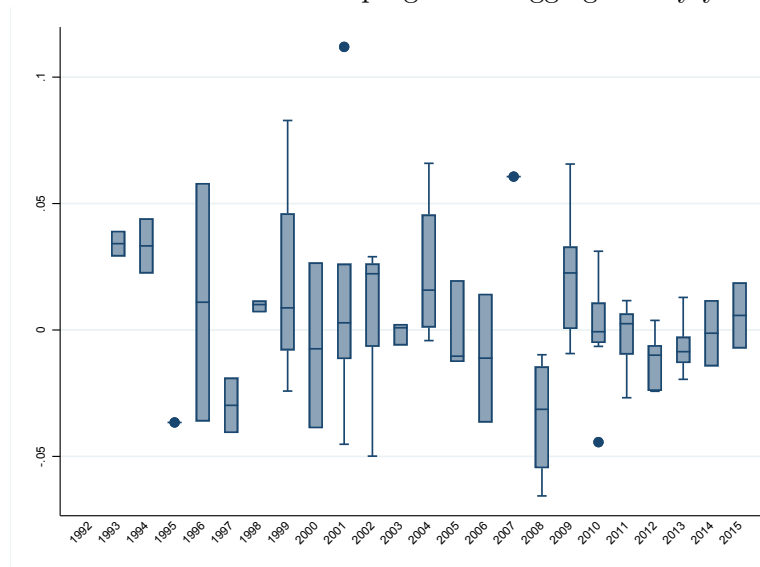
<sup>15</sup>For stock market indexes, we have 65 programmes significant out of 88, with 50 programmes significant at the 1%. 42 events increased the value of the stock index (largest event: Vietnam, April 2001, +11%), and 46 events had a negative effect (worst event: Ukraine, November 2008, -6.6%).

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  $\beta_e$  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.6: 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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}$	0.008***					
$m_{i,e-1}^{conc}$		0.004				
$m_{i,e-1}^{nonconc}$			0.008***			
$m_{i,e-1}^+$				0.009***		
$m_{i,e-1}^{conc,+}$					0.007*	
$m_{i,e-1}^{nonconc,+}$						0.010***
Neighbours	10	10	10	10	10	10
Treated obs.	2179	312	1867	2559	298	2261
Total obs.	11165	9298	10853	11545	9284	11247

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1,4), approval of a concessional programme (2,5), approval of a non concessional programme (3,6). Treatment group: having had a negative change in the stock market index during previous IMF programmes, columns (1)-(3), or a positive change in the stock market index; columns (4)-(6). 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 (1-6): economic risk, and growth forecasts, lag 0-3; reserves, exchange rate, political risk, and financial risk, lag 1-3. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

Table B.7: 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) All Programmes	(2) Concessional	(3) Non Concessional	(4) All Programmes	(5) Concessional	(6) Non Concessional
$m_{i,e-1}$	-0.002			-0.002		
$m_{i,e-1}^{conc}$		-0.000			0.008	
$m_{i,e-1}^{nonconc}$			-0.001			-0.001
Neighbours	10	10	10	10	10	10
Treated obs.	2179	312	1867	1667	141	1526
Total obs.	4738	610	4128	3728	402	3326

Notes: Results from a propensity score matching design. Dependent variable: approval of an IMF programme (1,4), approval of a concessional programme (2,5), approval of a non concessional programme (3,6). Treatment group: having had a negative change in the stock market index during previous IMF programmes (1). Control group: observations for countries that experienced an increase in the stock market index when programmes were approved (all columns 1,4, concessional columns 2,5, non concessional columns 3,6). 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. Columns (4)-(6) add the number of hard conditions based on Andone and Scheubel (2017) to account for programme design.\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .