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The Catalytic Role of the IMF financial support during the COVID-19 Pandemic

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I wanted to contribute to the analysis of the Covid-19 pandemic, because, unfortunately, this powerful virus took my grandmother Rosanna away from me.

I dedicate this work entirely to her, who supported my studies since I was a child.

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Abstract

This work provides a quantitative evaluation of the IMF financial support during the COVID-19 Pandemic by estimating the effect of Emergency Financing on private capital flows, with data on 83 EMDEs over the period 2018Q1-2022Q1 from the IMF Balance of Payments statistics.

In response to the global emergency caused by the COVID-19 pandemic, the IMF moved quickly by making its resources abundantly and quickly available. Yet, the capital flows to EMDEs reversed so violently that the Fund was unable to provide the needed amount of financing to close the financing gap of all assisted countries. Thus, the success of its interventions crucially depended on whether it also encouraged others to lend.

Applying Covariate Balancing Propensity Score, we find evidence of an average impact on the assisted economies, driven by Middle-Income Developing Countries (MIDCs), but the capital markets of Low-Income Developing Countries (LIDCs) did not benefit from the *Catalytic Effect* of the IMF financial assistance. Additionally, Emergency Financing displayed stronger effects than traditional financing facilities. These findings are robust to different identification strategies.

Keywords: COVID-19 crisis, emerging markets, capital flows, International Monetary Fund, covariate balancing propensity score

JEL Codes: F33, F34, F36, G01, G11, G15

Introduction

The COVID-19 pandemic hit the global economies severely. The spread of the virus was associated with a deterioration in growth prospects and overall radical uncertainty. The resulting pressures on the capital market were amplified by a tightening in financial conditions, and a sudden stop in the flows occurred. Emerging Markets and Developing Economies experienced large capital outflows, with more than 80% of them reporting net outflows (Martin *et al.* [2020d]). Fickle capital markets entail high risks for the aforementioned countries, plummeting currencies, external adjustments, and, in principle, defaults resulting in dramatic output losses and rising poverty.

In response to the global emergency caused by the COVID-19 pandemic, the IMF moved quickly in March 2020 by making its resources abundantly and quickly available. The Fund disbursed a record amount of financial assistance to a record number of members in record time (see IMF website, Covid financial assistance).

The aim of the financial assistance provided by the IMF is to help a country undergoing a Balance of Payments crisis to restore external viability without having to adopt disruptive economic adjustments and to avoid sovereign default.[1] In the recent widespread capital account crisis, nevertheless, the IMF was unable to provide the needed amount of financing and policy adjustments to close all the financing gaps that assisted countries have faced. As the Fund only provided a relatively small portion of the external financing needs of the assisted countries, the success of its interventions crucially depended on whether it encouraged others to lend.

IMF financing can increase the propensity of private investors to hold the financial assets of the borrowing country via two main channels (Corsetti *et al.* [2006]). First, the Fund by providing liquidity directly reduces early liquidations of domestic assets in borrowing countries. Second, the assistance of the IMF may reduce investors' willingness to withdraw. This channel, "the seal of approval effect", is a signaling effect and it is based on the assumption of

[1] See Article I of the IMF Articles of Agreements.

imperfect information in the financial market so that investors profit from the monitoring role of the IMF. Relatedly, the conditionality embedded in the IMF arrangements should act as an incentive for the country's authority, thereby implying better policies and higher growth. This is due to the fact that the approval of new commitments, in general, implies the adoption of sound policies and sustainable adjustment processes and, as a consequence, the announcement of IMF intervention should reinforce policy credibility and increase investors' confidence.

In this work, we evaluate the financial assistance of the IMF by measuring the extent to which Emergency Financing in the form of Rapid Financing Instrument (RFI), and Rapid Credit Facility (RCF) had a catalytic effect on private capital flows during the COVID-19 crisis. Did IMF help to mobilize capital inflows into assisted countries and prevent further outflows?

We find that IMF Emergency Financing had a positive net effect of about 0.2% of GDP on the capital flows of assisted countries (within the month). Specifically, the IMF intervention impacted the flows in MIDCs, while no effect took place for LIDCs, probably due to the low level of financial integration of those countries. The positive effect was mainly driven by Portfolio Flows.

This study makes several contributions. We provide the first measures of the catalytic effects of IMF Emergency Financing on private capital flows during the COVID pandemic. Beyond the COVID-19 context, the results improve our understanding of the role of international financial institutions in improving the resilience of the global financial architecture to global retrenchments of capital flows.

To the best of our knowledge, this is the first analysis that adopts the Covariate Balancing Propensity Score within the framework of the Generalized Method of Moments (Imai & Ratkovic [2014a]) to estimate the catalytic effects of the Fund on capital flows, although matching/weighting is the preferred approach for causal inference in this context. The basic idea of this method is to compare treated countries with untreated ones, which are as similar as possible in terms of observable characteristics. Contrary to the usual matching methods, this procedure allows cleaning for country and time effects by using a weighted fixed effect model. On top of that, this method does not require checking for covariate balance, since this is imposed by construction.

This thesis is structured as follows. Section 1 briefly reviews the related literature and presents the IMF lending package aimed to mitigate the COVID-19 shock. Section 2 describes the data used in the empirical analysis, focusing on the trade-offs involved. Section 3 outlines the empirical strategy by highlighting the identification assumptions of the applied methodologies, and Section 4 presents the main results and related robustness checks. Section 5 concludes. Additional figures and tables are provided in the Appendix.

SECTION 1

RELATED LITERATURE AND IMF LENDING DURING THE COVID-19 PANDEMIC

1.1 Potential Channels

Various theoretical contributions support the view that the IMF can act as a catalyst for attracting other capital flows. Fund financial support can increase the propensity of private investors to hold the financial assets of the assisted countries (see Cottarelli & Giannini [2002a] for a discussion on earlier contributions and on the causal channels involved). The IMF intervention is expected to have catalytic effects on the private capital market through three main channels related to the functions provided by the Fund: **commitment** of the borrowing country, **surveillance**, and **liquidity**.

The conditionalities attached to the IMF financial assistance act as a commitment device for the government of the borrowing country to address time inconsistency problems (Dreher [2009]). Sachs [1989] formalizes the functioning of conditionality in international lending. A debtor government accepts the need for a policy adjustment to obtain the loan, but then the government has the incentive to avoid policy change once the loan is arranged. In these models, the role of conditionalities is to bind the borrowing country to a course of future actions.

International investors also profit from the monitoring role of the IMF. Tirole [2002b] analyzes the moral hazard problem of the borrowing government in an agency framework. The IMF, as a "delegated monitor", alleviates the agency problem of the government, facilitating transparency through systematic disclosure of information.

Lastly, fund financing can directly reduce early liquidations of domestic assets. In a simple static coordination game, Zettlmeyer [2000] shows that the liquidity provided by IFIs can be helpful under certain conditions. First, if financing resources are large enough to rule out the possibility of self-fulfilling runs. Second, in the case that reserves prior to financial assistance are insufficient to cover the outflows of those investors that would want to exit for solvency reasons, even in the absence of a general run. In other words, IMF financing is effective only if the resources are enough to fill all financing gaps.

Contrary to Zettlmeyer [2000], Corsetti et al. [2006] propose a global game model, where the IMF liquidity is always effective. Financial assistance directly reduces liquidation costs against the speculative withdrawal of credit by reducing the amount of illiquid investments that need to be liquidated. Moreover, liquidity support reduces the number of speculators willing to attack a country, as private investors are more likely to roll over their positions.

On the other hand, anti-catalytic effects could relate to the higher losses given the preferred creditor status of the IMF. Extending the theoretical framework proposed by Corsetti *et al.* [2006], Krahnke [2020a] takes into account the additional costs of the IMF lending in the event of default. He shows that there exists a threshold above which high volumes of IMF financing start to reduce private investors' willingness to roll over their debt, resulting in a *crowding-out* effect.

Turning to the relative importance of the aforementioned channels in the context of the COVID-19 pandemic, some specific considerations can be made. First, lending took place mainly via the Rapid Credit Facility (RCF) and the Rapid Financing Instrument (RFI), which do not involve policy conditionality after the IMF Board approves the loan (ex-post conditionality), but the country must meet eligibility requirements and certain pre-conditions to quality. However, eligibility requirements and *pro-forma* practices were formally satisfied for every single RCF/RFI request. In this respect, we expect that the absence of ex-post conditionality and the overall easiness of the Emergency Financing terms prevented the commitment channel to operate.[1]

Second, the Covid pandemic elicited a response "like no other" in terms of number of countries receiving IMF financing. The Fund approved financial assistance to nearly 100 countries (approximately 2/3 of EMDEs) over the period March 2020 - April 2021. Possibly, the widespread distribution of IMF financing could weaken the signaling effects related to the Fund intervention. On the other hand, the fact that during the Coronavirus pandemic there was less of the stigma sometimes attached to IMF financial support may have weakened potential anti-catalytic effects as well. Differently from the idiosyncratic country-specific Balance of Payments problems faced by traditional IMF programs, the COVID-19 pandemic was a global phenomenon, originating a generalized Emerging Market sell-off episode. Therefore, the announcement of new IMF lending should not have been associated with negative signals for the borrowing countries (the COVID-19 crisis was already common knowledge).

Finally, the Fund made financing quickly and abundantly available to the supported countries, approving about $ 108 billions in financial assistance. Even if conditionalities were not binding, the IMF could have catalyzed private capital flows, through liquidity effects and its monitoring role.

1.2 Empirical Review

Measuring the causal impact of IMF financing on capital flows is a difficult task because of selection. Countries do not request and receive IMF support at random, but their selection process is affected by the dynamics of capital flows (outcome variable). Countries generally

[1]The open accessibility requirements raised the concern whether the IMF was providing Emergency Financing on too easy terms, which could allow countries to postpone needed adjustments that would have been required by a traditional Upper Credit Tranche program.

borrow from the Fund, when they face a BOP crisis.

In what follows, I will illustrate and discuss the techniques recently used to deal with selection in this setting, namely matching methods and IV approaches. Specifically, I will illustrate the identification strategies, their advantages, and their main limitations. The common approaches adopted in the literature are:

1. Fixed Effect Panel regressions;
2. Tobit and Heckman selection models;
3. Matching methods, such as propensity score, entropy balancing, and other ML approaches;
4. Instrumental Variables approaches.

It is possible to draw a distinction between approaches 1-3 and 4. The former techniques control for the selection stemming from the observable characteristics of the control and treatment groups. Moreover, these techniques can clean for the selection arising from time-invariant and individual invariant omitted factors, but not from time-varying unobserved con-founders.[2] The latter, namely the IV approach, would be the ideal way to deal with selection, as a valid instrument would avoid the selection bias stemming from both observable and unobservable factors.

Maurini and Schiavone [2021a] adopt matching methods to deal with the selection bias problem. In order to compare the capital flows of countries under traditional IMF programs to countries without a program, they use propensity score matching.[3] Intuitively, the propensity score is a pre-processing technique that matches observations of the treatment group with

[2]Consider the CIA in the context of Fixed Effect panel models.

[3]To be precise Maurini and Schiavone use Entropy Balancing matching, which is a relatively recent generalization of the propensity score (see Hainmuller, [2012c] for the methodological details and its advantages). However, the results they find by using the baseline method are robust to propensity score.

observations of the control group as *similar* as possible. Specifically, it is a 2-step procedure that consists in estimating the conditional probability of being treated (generally by a Logit or Probit model), and, secondly, to apply a matching estimator based on the propensity scores from the first step.When achieving balanced covariate distribution between treated and untreated groups, the propensity score method can avoid the selection problem stemming from the observable characteristics at hand. This technique provides consistent and unbiased estimates, if the ignorability assumption holds. Relatedly, the main limit of this approach lies in the choice of the model used to estimate the propensity score. Indeed, Abadie and Imbens [2016] show that the large sample properties of the estimator are affected by the estimation of the propensity score. Consequently, the estimates of the ATT in the borrowing country depend on the specification of the model in the first step. The main concern for the applicability of matching methods to estimate the catalytic effect during the Covid-19 pandemic is the availability of a suitable set of covariates, because the series of required variables may not be available for LIDCs. Furthermore, the IMF financial support was widespread, which restricts the potential control group, while it will be crucial to exploiting the cross country-variation given the limited time span of interest.

Along the lines of some recent works, [4] Krahnke [2020a] adopts an IV-approach to identify the catalytic effect of traditional IMF programs.As previously argued, the selection bias would imply a violation of the CIA that a fixed effect panel model would not eliminate entirely. In particular, there may exist country-specific unobservable omitted factors affecting both the IMF program participation and capital flows that can be controlled neither by a fixed effect panel model nor by matching methods. The IV-approach is ideally the first best solution since it solves the problem of unobservables as well.[5] Krahnke makes use of an instrumental variable that combines cross-country variation in prior probability of participating in an IMF

[4]See Eichengreen, Gupta, and Mody (2008), Van der Veer and de Jong (2010), Jorra [2012b], Gehring and Lang [2020f], Balima and Sy (2021).

[5]Matching methods could solve for relevant omitted factors, if after the matching process the distribution of the unobservables is balanced between the treatment and the control groups, but this condition cannot be tested.

program with temporal variation in IMF liquidity (measured by the Forward Commitment Capacity):

$$IV_{i,t} = IMFprobability_{i,t} \times ln(IMFliquidity_t)$$

The relevance of the instrument is well supported by economic reasons and suitable testing in the first stage of the 2SLS estimation. Regarding the non-exclusion condition, to be violated omitted factors would have to be correlated with year-specific liquidity and affect capital flows differently in countries with different IMF probabilities. However, IMF liquidity varies primarily because of an institutional rule. Therefore, the instrument proposed is plausibly a valid one. Even though the IV approach is potentially the best solution to address the selection bias problem, the main drawback of this approach is the impossibility to test the exogeneity of the instrument.

1.3 Push and Pull Factors

This work broadly relates to the extensive literature on Push and Pull factors as drivers of capital flows. The seminal work of Calvo *et al.* [1993] introduced the conceptual distinction between the country-specific pull factors and the global push factors, providing the analytical framework for much of the empirical analysis that followed. Also, Calvo and Fernandez-Arias [1996] brought push factors at the center of the debate, arguing that pull factors have a weaker role. Some studies have challenged the push-pull framework that implies a dichotomous perspective (country vs. global), thereby looking at Emerging and Advanced Economies separately, while the focus should be on differentials between EMDE and AE variables, such as interest rate and growth differentials (Ahmed and Zlate, [2014b]). Additionally, contagion effects and other forces related to investors' behavior are difficult to classify either as country-specific or as global in nature. Despite its limitations, the push-pull dichotomy offers a simple and intuitive classification of the capital flows drivers, which has guided the selection of some variables in the panel datasets for this analysis as well. Empirically, both push and pull factors matter in explaining the dynamics of capital flows, but their relative importance depends on the type of flows considered, the phase of the financial cycle, and many other factors (see Koepke, [2019] for a recent survey of the empirical literature).

In this respect, the following analysis is more closely related to the strand of the literature that studies extreme episodes in capital flows (Calvo, [2004]; Gosh, 2006), namely sudden stops and surges. Indeed, the underlying question of this work asks whether the policy intervention of the Fund helped assisted economies to recover from the sudden stop that occurred during the Covid crisis, and the time window under analysis is relatively short (2020-21). The factors driving the capital flows during reversal episodes may differ substantially from the ones that do so in "normal times". For example, Fratzscher [2012a] finds that global shocks, such as key crisis events and changes to global liquidity and risk have driven the capital flows both in the Global Financial Crisis and in the recovery.

Although the Covid-19 crisis originated in a totally different kind of shock, still a global shock hit the financial system, and, as a result, capital flows to EMDEs reversed violently. However, EMDEs benefited from the massive monetary and financial stimulus provided by AEs early in the crisis, and especially from the easing actions of the US federal reserve (Obstfeld, [2022]), which is another "push factor" that played a key role during the crisis. Undoubtedly, global factors were the major drivers of capital flows during the Covid EM sell-off. Yet, there is a large degree of heterogeneity with which different countries are affected by the same global shocks. For example, Fratzscher [2012a] also found that during the 2008 crisis and the subsequent recovery EMDEs were exposed to common shocks with different sensitivities, depending on macroeconomic fundamentals (growth, current account, public debt, and deficits), the quality of the institutions, and other country-specific factors. This motivates our attention to pull factors as well as push factors in our empirical analysis.

1.4 IMF response to a crisis "like no other"

In response to the global emergency caused by the Covid-19 pandemic, the International Monetary Fund response to the crisis was focused on making financing quickly and abundantly available to a record number of countries, in the view that measures to unwind these policies could be addressed at a second stage. The Covid pandemic elicitated a response "like no other", in terms of pace, amount, and number of countries receiving IMF financing. Indeed, the Fund approved about $ 108 billions in financial assistance to nearly 100 countries (approximately 2/3 of EMDEs) over the period March 2020- April 2021.[6]

Lending took place mainly via the pre-existing Emergency Financing facilities, namely Rapid Credit Facility and Rapid Financing Instrument, which do not involve policy conditionality after the IMF Board approves the loan (ex-post conditionality), even though the country must meet eligibility requirements and certain preconditions to qualify.[7] Nevertheless, the eligibility requirements and *pro-forma* practices related to the Emergency Financing were formally satisfied for every single RCF/RFI request.

The open accessibility requirements raised the concern whether the IMF was providing Emergency Financing on terms that were too easy without ex-post conditionality, which could allow countries to postpone needed adjustments that would have been required by a traditional Upper Credit Tranche program. This concern may have weakened the signaling effect of the announcement of the IMF intervention that should reinforce the policy credibility instead. Moreover, the RFI/RCF requests were roughly four times as numerous as requests for new, or augmented, non-precautionary UCT arrangements, giving some credence to the worries.

Nonetheless, even though no major policy adjustments were necessary under RCF/RFI, the baseline recommendations included fiscal policy advice on current spending and capital expenditures (*e.g.* Montenegro and South Africa), suggesting that the IMF advice and

[6] See the Draft Issue Paper " The IMF Emergency Response to the Covid-19 Pandemic" (Batini & Cohen-Setton, 2023).

[7] In particular, the relevant decisions state that a country requesting RFI or RFC assistance shall describe in a letter the general policies it plans to pursue to address its balance of payments difficulties, including its intention not to introduce or intensify exchange and trade restrictions.

valuable policy tracker may have reassured international investors.

SECTION 2

DATA

2.1 The Data Challenges

2.1.1 BoP Data on Capital Flows

IMF Balance of Payments is the most common data source not only in the literature of the catalytic role of the Fund, but also in the broader one of capital mobility (see Koepke and Paetzdolv, [2019] for a recent overview on capital flows data and time tracking). Indeed, the IMF Balance of Payments Statistics (BoPS) provides the most comprehensive country coverage and refined methodology, among the traditional datasets on capital flows. BOP capital flows data are generally available both on the gross and net basis for each major sub-component of the financial account (FDI, Portfolio, and Other Investments). This allows to analyze the flows, distinguishing on the base of the BOP classification. For example, Krahnke [2020a] finds that the positive effect of the IMF financial assistance is weakened, if the size of the program exceeds a certain level, due to a crowding-out effect on the debt-type capital inflows (in the Portfolio component). Data are available on a quarterly and annual basis, with a lag of two to four months. Furthermore, the compilation of BoP data is guided by clear accounting principles, which ensure that capital flows are comparable across countries and time. Some accounting principles of these internationally recognized standards are as follows:

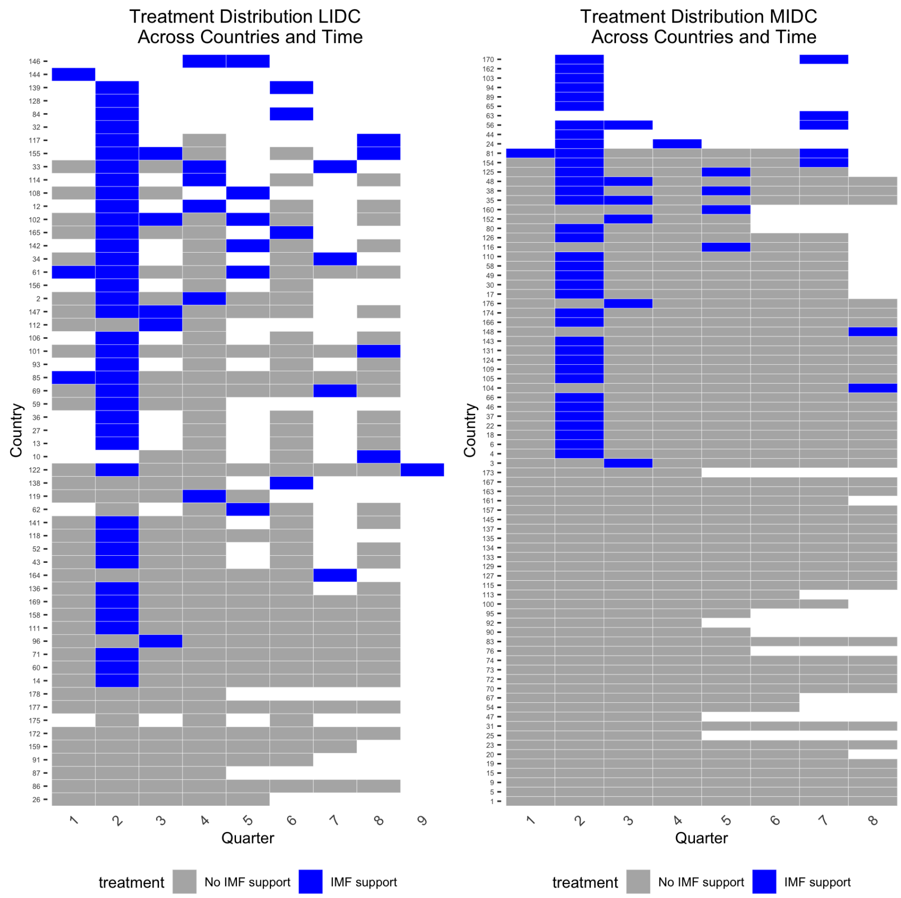
- **Residency:** Capital flows arise from the acquisition and disposal of financial assets and liabilities between residents of different countries.

- **Quadruple entry booking:** Each transaction is recorded twice in each of the two countries' BoP, reflecting the source of funds and the use of funds in each country.

- **Transactions at market value:** To the extent possible, capital flows are recorded using the market value at the time of the transaction. Hence, "valuation effects" do not affect BoP data.

The main advantages of the BoP data are the comprehensive coverage of cross-border transactions and the well-defined accounting methodology. On the other hand, the main limitation of BoP data for this exercise is the relatively low (quarterly) frequency, as the period of interest spans 8 quarters only (2020 Q1 - 2021 Q4), which implies little time variation. Whereas, the empirical analysis of the effects of the Fund's interventions on the capital market are usually based on yearly data over long periods of time (again I refer to Krahnke, [2020a]; Gehring *et al.*, [2020f]). On top of that, the nature of the COVID-19 response is different from the traditional forms of IMF interventions, as extensively discussed in the introduction. In particular, many Low Income countries that are not qualified for the usual IMF financial assistance received emergency financing during the COVID-19 pandemic, and many of those countries are not covered by the BoP dataset, unlike high and middle-income countries. Figure 2.1 illustrates the data challenge with traditional BOP data. The figure is a treatment variation plot, which shows the treatment events (blues rectangles) for each country over time, the control observations (grey rectangles), while the missing country-quarter observations are left black. This plot visualizes the variation of treatment across space and time, in order to help build an intuition about how a comparison of treated and control observations can be made. We pay special attention to whether the treatment varies sufficiently both over time and across countries, because the validity of causal inference relies upon such variations, as emphasized by Imai *et al.* (2021).

Figure 2.1: *Treatment Variation Plot, BOP data*



Note: The left panel displays the spatial-temporal distribution of the IMF intervention for the LIDCs, in which a blue (grey) rectangle represents a treatment (control) country-month observation. A white area represents the months when data on capital flows are not available. The right panel displays the same plot for MIDC.

Firstly, we notice that part of the observations gets lost by using quarterly BoP data, especially for LIDCs (left panel). For instance, consider that the upper rows of the plot display missing series for 7 countries. This accentuates the unbalance between a small control group vs. a large treatment group, a problem that we already have to deal with given the widespread IMF response to the Covid pandemic. The Fund moved quickly in March-April 2020, and disbursed a record amount of financial assistance to a record number of members, restricting greatly the potential control group. Additionally, the quarterly frequency in such a small T panel provides little time variation to be exploited.

On the contrary, the usual pre-Covid studies are based on denser yearly series over longer periods of time. Anyways, country variation for the case at hand may be enough to investigate the catalytic role of the Fund.

Traditional BoP Statistics provides data that directly measure capital flows, which can also be analyzed at the level of the sub-components of the financial account (Portfolio, FDI, and Other Investments).

2.2 Description of the Data

To perform our empirical analysis, we construct a quarterly panel dataset containing data on capital flows, IMF lending, and a set of control variables, following the empirical literature on the catalytic role of the Fund (see among others Krahnke [2020a], Maurini & Schiavone [2021a]) and additional variables to face the specificities of a crisis *like no other*. The dataset covers all the available EMDEs over the COVID-19 reversal episode (2020-2021), while AEs are excluded since they did not receive IMF financing during the recent crisis.

a) **Treatment Variable.** Information on the treatment variable, that is IMF interventions, is collected from the IMF Covid-19 Lending Tracker database, and various documents on IMF lending. We collect data on all financing types approved during the period 2020Q1–2021Q4, resulting in a sample of 261 lending measures, which are then aggregated at the quarterly and monthly frequencies. The explanatory variable of interest D_{IMF} is an indicator that takes the value of 1, if the country j received IMF financing in the respective time period t. We collect information on the date of approvals, the size of the financing, and their type. According to the IMF conditionality framework, we further organize the financing arrangements into 4 categories: *emergency*, *ex-ante conditionality*, *ex-post conditionality*, and *debt relief*. Appendix Table 5.1 reports the list of the assisted countries by financing category.

b) **Dependent Variable.** The quarterly dataset contains BoP data, which are drawn from the analytic presentation of the IMF's Balance of Payments Statistics for gross capital flows and their sub-components, namely Portfolio, FDI, and Other Investments. Using BoP data, we cover approximately 2/3 of all EMDEs (96 out of 156), but some series of those countries are sparse, especially for Low Income Developing Economies, and the coverage of the available data differs substantially for each sub-component of the financial account.[1] The capital inflows are measured as changes in liabilities of the reporting country's residents held by foreign nationals. Conversely, the outflows are measured by changes in assets.

c) **Controls.** Finally, the dataset includes additional variables that are used as controls, or covariates to perform matching and weighting techniques. In line with the related empirical literature, the variables are chosen because of their ability to explain both the dynamics of capital flows and the countries' participation in IMF programs. The data includes a list of country-specific pull factors: GDP growth (*GDP_gr*) a measure of exchange rate volatility (*vol_e*), inflation rate (*inflation*), and the current account balance as a percentage of GDP (*ca_balance*). In addition, we include push factors, namely the U.S. overnight interest rate (*FFR*), and a measure of global risk aversion (*VIX*). Finally, we include variables affecting the countries' participation in IMF financing programs both on the demand and the supply sides: external financing needs (*EFN*), proxied by current account + capital account - FDI, a measure of international liquidity (*FX reserves in months of imports*), and the level of the financial integration (*FMDI*).

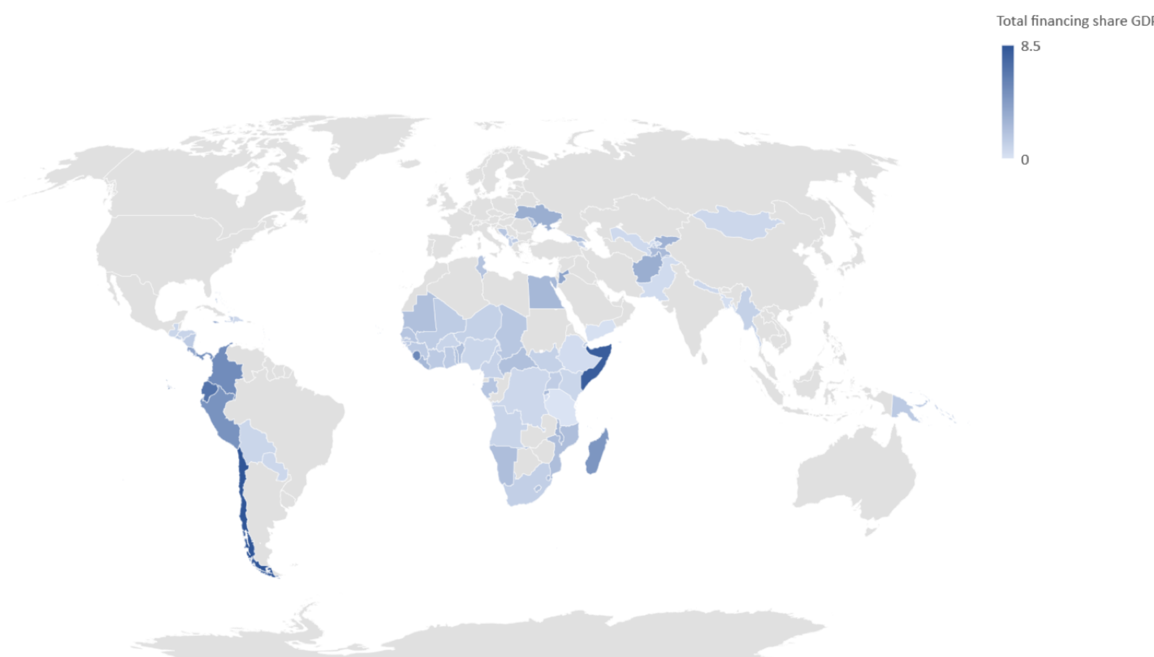
The BoP database we compile covers 130 countries over the period 2010-2022. We focus on Emerging Markets and Developing Economies over the sample period 2020-2021, as no Advanced Economy received financing during the Covid pandemic. As argued by Broner *et*

[1] 96 refers to the coverage of Portfolio Flows. The dataset covers 130 countries for FDI, and 85 for other capital flows.

al. [2013], and Krahnke [2020a], small countries are a concern because they might display an artificially high volume of financial transactions due to their role of tax havens. However, as a robustness check, we only exclude the countries considered as offshore centers, according to the latest update of the IMF Offshore Financial Centers (OFCs).[2] Instead of removing all small countries from the analysis, we restrict the sample in a justified way.

The exact codes of the variables used and the related sources are given in Appendix Table 5.2.

Figure 2.2: *Geographical Distribution of IMF commitments*



Note: Size of IMF arrangements by country. Lending size is measured in percentage of countries' GDP. Data on IMF loans and their size is taken from the COVID-19 Lending Tracker and various program documents. The nominal GDP data is drawn from the IMF WEO database.

[2]The list of OFCs can be accessed online on the IMF's website.

SECTION 3

EMPIRICAL STRATEGY: DEALING WITH SELECTION

This section describes the empirical design used to estimate the effect of IMF financial support on private capital flows. To illustrate the dynamics of the outcome variable over the period of interest, **Figure 4.1** shows the capital flows relative to GDP for the group of the countries that received IMF support (in blue) and the group that did not (in grey). Countries that did not receive financial help from the Fund experienced a stop considerably more severe than supported economies, where the flows bounced back faster to the pre-pandemic levels. However, treated and untreated economies display dissimilar pre-trends of capital flows. Before the Covid episode, the flows generally comove across those countries, but sometimes they diverge, such as in early 2019. Additionally, even before the IMF intervention, the flows of non-supported economies are more volatile than the ones of the supported countries, suggesting that the EMDEs in the treatment group are more resilient to adverse financial conditions *per se*. These differences reflect varying economic fundamentals, degrees of financial stability (*e.g.* external financing needs), financial integration, macroeconomic conditions, and potentially numerous other factors. This heterogeneity adds noise to the analysis of capital flows. Moreover, it may also introduce inconsistency, if the likelihood of receiving IMF support is affected by these factors. We address these challenges using an empirical design that compares the capital flows of the treatment and control groups, while controlling for those factors and mitigating the serious selection problem, due to the endogenous process of request and approval of IMF support. Namely, we adopt a Difference-in-Difference approach in two forms.

The former is a panel Fixed Effect regression that compares the capital flows for countries during the same month, the latter is Covariate Balancing Propensity Score (Imai and Ratkovic, [2015]), which is a weighting technique within the Generalized Method of Moments framework. By using this method we compare treated and untreated countries similar in terms of a set of observable characteristics that explain both the dynamics of capital flows and the participation in IMF programs. Therefore, we start by performing various forms of Fixed Effect regressions, which are the benchmark method for causal inference with panel data, then we move to the matching technique proposed by Imai and Ratkovic [2015], which encompasses the usual Fixed Effect approach as a special case. After obtaining the FE estimates, we investigate how weighting on observables, while exploiting the panel dimensions of the data affects the naïve estimates.

3.1 Treatment Group Definitions

Throughout the rest of this work, we consider three types of IMF financial assistance active during the Covid crisis, from the broadest category to the most restrictive one.

The first is "All Financing", which includes all the new commitments approved over the period January 2020 and December 2021.[1]

The second category of IMF support restricts "All Financing" by excluding the *precautionary* facilities that feature *ex-ante* conditionality, namely the Flexible Credit Line (FCL) and the Precautionary Liquidity Line (PLL). Those financing instruments are designated to give countries with sound policy frameworks and strong economic fundamentals access to large amounts of resources that can be drawn up-front.[2] The nature of these loans differs

[1]This category also includes the loan of St. Vincent and The Grenadines, which received resources in the form of RFC under the Natural Disaster Window, owing to a volcanic eruption that occurred in April 2021, and, therefore, it is not Covid Related.

[2]More precisely, FCL is intended to give countries with very strong fundamentals and policy frameworks access to large amounts of resources that can be drawn up-front; while PLL is meant to provide precautionary support to countries with sound fundamentals, but with some remaining vulnerabilities; it has a shorter

substantially from the Emergency Financing that this analysis aims to evaluate, and, consequently, are excluded from the treatment group. On top of that, the countries qualified for the precautionary facilities are not comparable with the recipient of Emergency Financing, and, as a result, their inclusion in the control group would introduce additional selection bias. Hence, we exclude these countries from the sample.[3]

Finally, we consider the "Emergency Financing" category. This group includes lending via the Rapid Credit Facility (RCF) and the Rapid Financing Instrument (RFI), which were the instrument types the most extensively used by the IMF membership (85 % of the arrangements over the period of interest). Notably, those instruments do not involve policy conditionality after the IMF Board approves the loan, even though the country must meet the eligibility requirements and certain preconditions to qualify. Additionally, the unconventional use of these financial facilities makes it even more relevant to the aims of this analysis to evaluate the effectiveness of the EF category specifically.

3.2 Panel Fixed Effect Approach

In social sciences, fixed effect regression models are widely used as the benchmark method for causal inference with longitudinal data (e.g Angrist and Pischke, [2008]). These models are used to adjust for unobserved individual specific and time-invariant confounders when estimating causal effects in observational studies. Despite the widespread use of this approach, the methodological discussion of fixed effect panel models has taken place from a model-based perspective, with little attention to the causal identification assumptions. In this context, I will outline the assumptions required under fixed effect models and the relationship with the Difference-in-Difference (DiD) identification strategy.

duration and a lower access than FCL. Moreover, part of the SBA arrangements can be ascribed to the precautionary category, as argued by Maurini & Schiavone [2021a]. Those are SBA arrangements requested by countries that do not intend to draw, but they retain the option to do so should they need it.

[3] In this respect, we also distinguish between pandemic and pre-pandemic precautionary arrangements, which include the FCL/PLL that were drawn upon, due to the impact of the pandemic (Mexico, Colombia, and Morocco).

3.2.1 Panel Fixed Effect Linear Model

Consider a balanced longitudinal dataset for N units and T time periods with no missing data. Furthermore, we assume a random sampling of individuals from a population with T fixed. For each time period t and individual i, we observe the outcome variable Y_{it} and the binary treatment variable $X_{it} \in (0, 1)$. The usual two-ways linear regression model with individual and time-fixed effects writes as follows:

$$Y_{it} = \beta X_{it} + \alpha_i + \delta_t + \varepsilon_{it} \tag{3.1}$$

for each $i = 1, 2, \ldots, N$, and $t = 1, 2, \ldots, T$, where α_i is a fixed but unknown intercept for each individual i and similarly δ_t for each time period t. Typically, the strict exogeneity assumption of the disturbance term ε_{it} is assumed to identify β, *i.e* the partial effect of the treatment. Formally, this assumption can be written in the form of the mean independence condition:

$$\mathbb{E}[\varepsilon_{it} | X_{it}, \alpha_i, \delta_t] = 0 \tag{3.2}$$

for each $i = 1, \ldots, N$ and $t = 1, \ldots, T$, where X_{it} is a $(T \times 1)$ vector of treatment variables for unit i. The least squares estimate of β is obtained by OLS:

$$\hat{\beta}_{FE} = \underset{\beta \in \Theta}{\operatorname{argmin}} \sum_{i=1}^{N} \sum_{t=1}^{T} \left\{ (Y_{it} - \bar{Y}_t - \bar{Y}_i) - \beta(X_{it} - \bar{X}_t - \bar{X}_i) \right\}^2 \tag{3.3}$$

where $\bar{X}_i = \sum_{t=1}^{T} X_{it}/T$, $\bar{Y}_i = \sum_{t=1}^{T} /T$, $\bar{X}_t = \sum_{i=1}^{N} X_{it}/N$, and $\bar{Y}_t = \sum_{i=1}^{N} Y_{it}/N$. If the conditional mean independence assumption holds, β can be interpreted as the average contemporaneous effect of X_{it} on Y_{it}.

Turning to the Neyman-Rubin framework for causal inference (Neyman, 1923; Rubin, [2005a]), let $Y_{it}(1)$ represent the potential outcome for individual i at time t under the treatment status $X_{it} = x$ for $x = 0, 1$, where the observed outcome equals $Y_{it}(X_{it})$. Intuitively, Equation 3.3 shows that individuals with no variation in the treatment variable do not affect the estimates of β. Hence, the causal estimand of the average treatment effect among the individuals with some variation in the treatment status (ATT) is:

$$\tau = \mathbb{E}[Y_{it}(1) - Y_{it}(0)|C_i = 1] \tag{3.4}$$

with $C_i = \mathbb{1}(0 < \sum_{t=1}^{T} X_{it} < T)$. In the panel FE setting, this quantity of interest is represented by β. Therefore, $\widehat{\beta}_{FE}$ consistently estimates the Average Treatment Effect on the Treated, if the exogeneity assumption holds, the DiD design and the linear panel FE approach both estimate the same quantity of interest. Hence, panel fixed regressions are commonly used to estimate the Average Treatment Effect, as defined in the difference-in-difference design. In this respect, Wooldridge shows that a pooled OLS regression that includes a treatment indicator, a post-treatment time period dummy, and additional regressors is numerically the same as the full two-way FE estimator, which, in turn, is equivalent to the DiD design (see Wooldridge, [2021c]). Notably, in this setting, there is a single intervention, with common timing. This raises some questions on the extent to which the algebraic equivalence can be extended to more general settings. Yet, if the identification assumptions hold the regression approach and the DiD estimator both converge to the same quantity of interest. This fact motivates the widespread use of the regression approach, regardless of their algebraic equivalence. However, the two identification strategies are based on different conditions that the following discussion will highlight.

3.2.2 Application Panel FE

We begin the empirical analysis with the naïve panel model for country i at time t. We estimate the following model separately for *Emergency* and *All Financing*:

$$\frac{K_{it}}{GDP_{2019}} = \beta D_{it} + \alpha_i + \delta_t + \varepsilon_{it} \tag{3.5}$$

In our application, the outcome variable is the capital flows scaled by the pre-pandemic annual GDP. In line with the literature on capital flows, we scale the flows mainly to avoid size effects. Also, this convenient scaling will allow us to interpret the coefficients as percentage change in the capital flows. The explanatory variable of interest D_{it} is an indicator that takes the value of 1, if the Fund approved financing for country i in month t (see the detailed definition of the treatment group in Section 3.1). Hence, β gives the effect of the contemporaneous IMF intervention, if the conditional mean independence assumption holds. This should capture the signaling effect of the IMF commitments in the financial market, as international investors should be reassured by the intervention of the Fund. The country fixed effects α_i control for the country-specific time-invariant factors. For example, the geographical area is a potentially relevant factor, as the official lenders differ across regions, and the types of official sources of finance available may be correlated with D_{it}, as the decision of a country to request funds from the IMF should be affected by the nature and availability of other external financial sources. Additionally, the geographical region is also correlated with the degree of financial integration, which, in turn, affects the amount of the flows of the country.

The time-fixed effect control for crucial individual invariant time-varying factors, such as the global financial cycle (Helene Rey, [2015b]). In this respect, consider the sudden stop that EMDEs experienced at the beginning of 2020 (Martin *et al.*, [2020d]). During a recessive phase of the cycle, countries are more likely to borrow from the Fund due to increased financing needs.

All in all, the panel regression framework can mitigate part of the endogeneity related to the selection bias problem. Yet, the estimator is still likely to suffer the inconsistency introduced by individual specific and time-varying factors. To exemplify the point, simply consider a factor X_{it} which is both correlated with D_{it} and X_{it}, such as the political risk rating. A country under an unstable political regime is perceived as a highly risky asset by international investors and, therefore, any request of financial assistance is likely to be rejected by the Fund. In such a case, the identifying assumption in 3.2 would be violated, and the estimates would be inconsistent as a result.

3.3 Covariate Balancing Propensity Score

This methodology aims to compare treated units with untreated ones, which are as similar as possible in terms of observable characteristics. The basic idea is that comparing similar units with each other may be useful to draw credible causal inferences, even when using all (unweighted) data is not. In the panel setting, this procedure also allows cleaning for country and time effects by using a weighted fixed effect model. On top of that, it does not require checking for covariate balance, since this is imposed by construction.

Covariate Balancing propensity score is a methodology to estimate the inverse-probability treatment weights for Marginal Structural Models (MSM). The CBPS exploits the usual propensity score as the conditional probability of treatment assignment, but also as a covariate balancing score. Imai and Ratkovic [2014a] introduced this methodology in the cross-sectional setting, showing that CBPS improves the performance of propensity score matching and weighting methods both via computational simulations and empirical applications. In this context, we apply the CBPS to the time-series cross-sectional setting (Imai & Ratkovic, [2015]). This approach incorporates the covariate balancing conditions across multiple time periods, accounting for the panel dimensions of our application.

Before presenting the Covariate Balancing propensity score methodology, we briefly introduce the basic propensity score framework in the cross-sectional setting. The propensity score is defined as the conditional probability of receiving the treatment given the covariates X_i. Following Rosenbaum and Rubin [1983], we assume that the propensity score is bounded away from 0 and 1:

$$0 < Pr(T_i = 1 \mid X_i = x) < 1, \ \forall x \in \mathcal{X} \tag{3.6}$$

For this reason too, in our policy evaluation, we exclude the Advanced Economies from the sample of countries. Indeed, no Advanced Economies received IMF emergency financing during the Covid-19 pandemic and including them in the estimation of the propensity score would imply a violation of this assumption. For instance, the United States, in practice, has 0 probability of receiving support from the Fund, owing to their strong economic fundamentals.

Rosenbaum and Rubin [1983] showed that if one assume the ignorability of the treatment assignment, *i.e.*:

$$\{Y_i(1), Y_i(0)\} \perp\!\!\!\perp T_i \mid X_i \tag{3.7}$$

where $Y_i(t)$ represents the potential outcome under the treatment status $t \in \{0, 1\}$, then the treatment assignment is ignorable given the true propensity score $\pi(X_i)$:

$$\{Y_i(1), Y_i(0)\} \perp\!\!\!\perp T_i \mid \pi(X_i) \tag{3.8}$$

This crucial result implies that a consistent and unbiased estimation of the treatment effects defined in Equation 3.4 is possible by conditioning on the propensity score alone. Therefore, researchers estimate the propensity score in the data (usually via a logit or probit model), and then estimate the treatment effect.

3.3.1 CBPS: the Panel Set-Up

To illustrate the CBPS, we present this methodology for the case of two time periods.

The units are indexed by i, we observe the outcome variable Y_i at the end of the period, the binary treatment T_{ij} for each time period $T = 1, 2$. We are interested in the marginal mean of the potential outcome measured at the end of the second period, $\mathbb{E}[Y_i(\bar{t}_2)]$, where $\bar{t}_2$ denotes the history of treatment events for the i-th unit and can take any of the four possible values , that is $\bar{t}_2 \in \mathrm{T}_2 = \{(0, 0), (0, 1), (1, 0), (1, 1)\}$

We now derive the moment conditions based on the covariate balancing property of the weights for Marginal Structural Models. To do this, we express the moment conditions as function of the weights, defined as follows:

$$w_i\left(\bar{t}_j, \bar{X}_{ij}(\bar{t}_{j-1})\right) := \frac{1}{P\left(\bar{T}_{i,j} = \bar{t}_j | \bar{X}_{i,t}(\bar{t}_{j-1})\right)} = \prod_{j=1}^{J} \frac{1}{P\left(T_{i,j} = t_{i,j} | \bar{X}_{i,j-1}(\bar{t}_{j-1})\right)} \tag{3.9}$$

The weight w_i is a function of the treatment sequence $\bar{t}_j$, and the covariate history $\bar{X}_{i,j}$. Analogously to the standard propensity score framework (Rosenbaum and Rubin, 1983), the weight is simply given by the inverse-probability of treatment, which is conditional on the past treatment and covariate histories, owing to the longitudinal setting.

At the first time period, across all four possible treatment histories, the weight should balance the mean of the baseline covariate, X_{i1}. Therefore, for all $\bar{t}_2 = (t_1, t_2) \in \{(0,0),(0,1),(1,0),(1,1)\}$, we have:

$$\mathbb{E}[X_{i1}] = \mathbb{E}\left[\mathbb{1}\left\{T_{i1} = t_1, T_{i2} = t_2\right\} w_i(\bar{t}_2, \bar{X}_{i2}(t_1)X_{i1})\right] \tag{3.10}$$

The covariate balancing conditions at the second time period are similar to those at time 1, except that the covariates measured at time 2 are possibly functions of the treatment at time 1, that is $X_{i2} = X_{i2}(T_{i1})$. These covariate balancing conditions are as follows:

$$\mathbb{E}[X_{i2}] = \mathbb{E}\left[\mathbb{1}\left\{T_{i1} = t_1, T_{i2} = t_2\right\} w_i(\bar{t}_2, \bar{X}_{i2}(t_1)X_{i2}(t_1)\right] \tag{3.11}$$

3.3.2 Estimation of the Panel CBPS

Since the number of moment conditions is greater than the number of parameters to be estimated, the CBPSs can be estimated by the Generalized Method of Moments (Hansen, 1982) by imposing the covariate balancing conditions above.

Thus, the optimal GMM estimator is given by:

$$\widehat{\beta}_{GMM} = \underset{\beta \in \Theta}{\operatorname{argmin}}\, vec(G)^T W^{-1} vec(G) \tag{3.12}$$

where the sample moment conditions are given by:

$$G = \frac{1}{N} \sum_{i=1}^{N} \begin{bmatrix} (-1)^{T_{i1}} w_i X_{i1} & (-1)^{T_{i2}} w_i X_{i1} & (-1)^{T_{i1}+T_{i1}} w_i X_{i1} \\ 0 & (-1)^{T_{i2}} w_i X_{i2} & (-1)^{T_{i1}+T_{i1}} w_i X_{i2} \end{bmatrix}, \tag{3.13}$$

and their covariance $\mathbb{W}$ is given by:

$$\mathbb{W} = \frac{1}{N}\mathbb{E}\left\{\begin{bmatrix} 1 & (-1)^{T_{i1}+T_{i2}} & (-1)^{T}_{i2} \\ (-1)^{T_{i1}+T_{i2}} & 1 & (-1)^{T}_{i1} \\ (-1)^{T_{i2}} & (-1)^{T_{i1}} & 1 \end{bmatrix} \otimes w_i^2 \begin{bmatrix} X_{i1}X_{i1}^T & X_{i1}X_{i2}^T \\ X_{i2}X_{i1}^T & X_{i2}X_{i2}^T \end{bmatrix} \mid X_{i1}, X_{i2}\right\} \tag{3.14}$$

Specifically, we use an efficient 2-step estimator, where the weighting matrix is given by a low-rank approximation described in Imai and Ratkovic [2014a].[4]

3.3.3 Application CBPS

The approach used in this analysis involves two stages of modeling. The first stage model estimates the probability of participating in an IMF-supported program. The second stage model estimates the effect on capital flows by using a Fixed Effect regression weighted by the inverse probability obtained from the first step of the procedure. This approach is doubly robust in the sense that the weighted Fixed Effect Estimator (WFE) is consistent unless both treatment and outcome models are misspecified.

In our analysis, the first stage model (CBPS) writes:

$$D_{it} = \delta_t + V_{it}\phi + v_{it} \tag{3.15}$$

where countries and time periods are indexed by i and t respectively, D is the IMF dummy previously introduced, and V is a set of relevant predictors of IMF financial assistance, including specific variables for the Covid pandemic as well.

The second stage is a linear weighted fixed effect model, where the estimator is given by:

$$\hat{\beta}_{WFE} = \underset{\beta \in \Theta}{\operatorname{argmin}} \sum_{i=1}^{N}\sum_{t=1}^{T} W_{it}\left\{(Y_{it} - \bar{Y^*}_t - \bar{Y^*}_i) - \beta(D_{it} - \bar{D}_t^* - \bar{D}_i^*)\right\}^2$$

where $\bar{D^*}_i = \sum_{t=1}^{T} D_{it}/T$, $\bar{Y^*}_i = \sum_{t=1}^{T}/T$, $\bar{D^*}_t = \sum_{i=1}^{N} D_{it}/N$, $\bar{Y^*}_t = \sum_{i=1}^{N} Y_{it}/N$, and the

[4] This runs substantially faster than the continuous-updating estimator also described in Imai and Ratkovic [2014a].

weights are given by the first stage model.

The covariates V that enter the first stage model are progressively included in different specifications from the least to the most complete. We select the variables to explain both the countries' participation in IMF programs and the dynamics of capital flows. The former category of variables, in turn, includes characteristics that affect the demand for Fund financing: *External Financing Needs*, as countries request the IMF Emergency support when facing BoP difficulties; buffers, such as foreign exchange reserves in months of imports; income level, since a fragile economy is more likely to ask support.

On the supply side, we construct and include a variable that captures the country past relationship with the Fund, owing to the fact that having an established relationship with the IMF facilitates the process to obtain Fund financing. More precisely, this variable is given by the average of an indicator that takes value 1, if a country was under an IMF program (the average is taken over 10 years time).

The latter broad category consists in pull factors. These include the current account balance, inflation, and geographic region.

3.4 Impulse Response Functions by Local Projections

To track the effect of the IMF intervention on capital flows we estimate Impulse Response Functions (IRFs), using the method of Local Projections (Jorda, [2005b]). The specification we consider takes the following form:

$$\Delta_h k_{i,t-1} = \beta^h D_{i,t} + \alpha_i^h + \delta_t^h + u_{i,t+h} \tag{3.16}$$

where subscript i indexes countries, subscript t indexes quarters, k is net capital flows scaled by annual GDP (total flows, portfolio, FDI, or others) or gross capital flows, D is an indicator that takes value 1 at the time of the approval, α_i denotes country-fixed effects, δ_t denotes quarter-fixed effects, and $u_{i,t}$ is an error term. Let $\Delta_h k_{i,t-1} = k_{i,t+h} - k_{i,t-1}$ denote the response variable of interest from the base quarter $t-1$ (before IMF intervention) up the quarter $t+h$, with $h = 0, 1, 2, 3, 4$. Using these definitions, we are interested in estimating the dynamic multipliers of $\Delta_h k_{i,t-1}$ to the approval of IMF financial support. Hence, the equation above is re-estimated for each IRF horizon $h = 0, 1, 2, 3, 4$ (with $h = 1$ indicating the quarter when IMF lending is approved, *i.e.* the contemporaneous effect). To consistently estimate β using the standard Fixed Effects approach, strict exogeneity of the explanatory variables conditional on the unobserved fixed effects needs to be satisfied ($\mathrm{E}(u_{i,t}|D_{i,t}, \ldots, D_{i,T}, \delta_1, \ldots, \delta_T, \alpha_i) = 0$ for all $t = 1, \ldots, T$). This assumption implies that receiving IMF financing is uncorrelated with the idiosyncratic error in each time period ($\mathrm{E}(D_{i,s}u_{i,t}) = 0$ for all $s, t = 1, \ldots, T$. This is a stronger assumption than assuming zero contemporaneous correlation in the model given in Section 2.2.2 for OLS consistency.

The baseline empirical strategy combines the Local Projection approach just described with Covariate Balancing Propensity Score (Imai and Ratkovic, [2014a]). We simply estimate the aforementioned series of regressions for each horizon h, using the weights given by the Covariate Balancing Propensity Score procedure. Thus, we use a 2-step procedure, where we estimate the weights from the CBPS sets of moment conditions, and then we perform weighted panel fixed effect regressions, taking the weights from the first step.

To sum up the methodological discussion, Table 3.1 lists the identifying assumptions for each model considered. First, the strict exogeneity assumption holds, if the error term ε_{it} is mean independent on the IMF treatment conditional on country-specific time-invariant factors and time-varying but country-invariant factors.

Second, the propensity score weighting consistently estimates the partial effect of the IMF financing, if, in turn, the propensity score is consistently estimated. In other words, we do not require D_{it} to be exogeneous in the model for capital flows, but only to consistently estimate the parameter ϕ in the probability model. For instance, we possibly allow for simultaneity between capital flows and the IMF treatment.

Finally, the MSM based on Covariate Balancing Propensity Score is doubly robust, in the sense that for its consistency either the former or the latter assumptions are required to hold. On top of that, the CBPS is robust to mild misspecifications of the probability model (Imai and Ratkovic, [2014a]).

Table 3.1: *Models and related Identification Assumptions*

Approach	**Estimation**	**Assumption**
Panel FE	OLS	$E[\varepsilon_{it} \mid X_{it}, \alpha_i, \delta_t] = 0$
Propensity Score Weighting	MLE	$E[\{Y_{it}(1), Y_{it}(0)\}_{t=1}^{t} \perp\!\!\!\perp \pi(V_{it})] = 0$
Covariate Balancing Propensity Score MSM	GMM+OLS	either (1) or (2)*

Note: all identification assumptions are given for the panel setting. *Moreover, the CBPS is robust to mild misspecifications of the probability model for the propensity score, and the over-identified system of balancing conditions allows to perform a specification test, using Hansen's J-statistic.

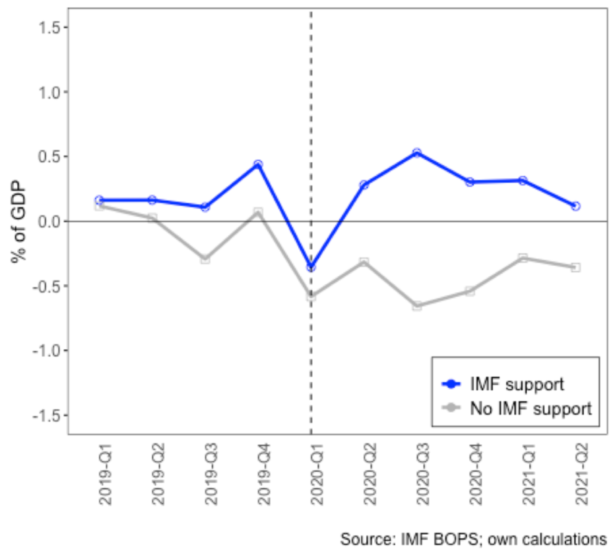
SECTION 4

RESULTS

4.1 Graphical Evidence

We visually inspect Portfolio investments, which is the most volatile component of the BoP financial account, as it should be the most responsive component to IMF policy intervention as well. Figure 4.1 shows the mean of net portfolio capital flows over time. Both the group that received IMF support and the group that did not experienced a drastic stop in portfolio flows in the first quarter of 2020. After the IMF intervention, which was concentrated in March-April 2020, the flows bounced back faster in assisted countries (blue line). Specifically, the treatment group is “All Financing”, and the figure suggests that catalytic effects of the IMF intervention were in place, with a difference between the two groups ranging from 0.5% to 1% of GDP. However, the pre-trends are dissimilar and the difference could be explained by selection rather than true catalytic effects. Relatedly, the countries in the treatment group and those in the control group are deeply heterogeneous, and such heterogeneity could explain the different dynamics in the capital flows.

Figure 4.1: *Portfolio Net Inflows, EMDEs*

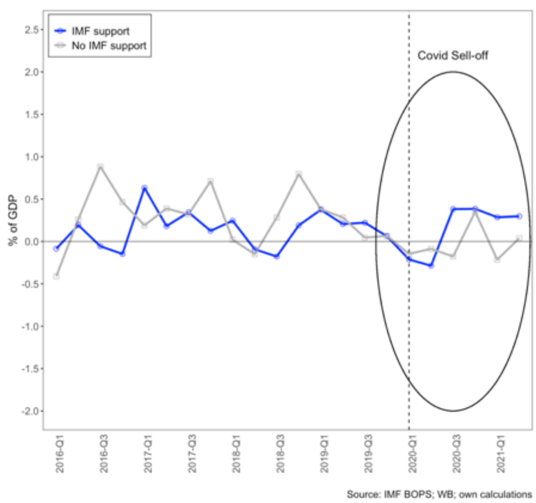


Note: The figure shows the net portfolio inflows scaled by the GDP as of 2019. The treatment group is composed of all countries that received IMF support during the Covid pandemic. A country enters the treatment group in the quarter when it receives IMF financing.

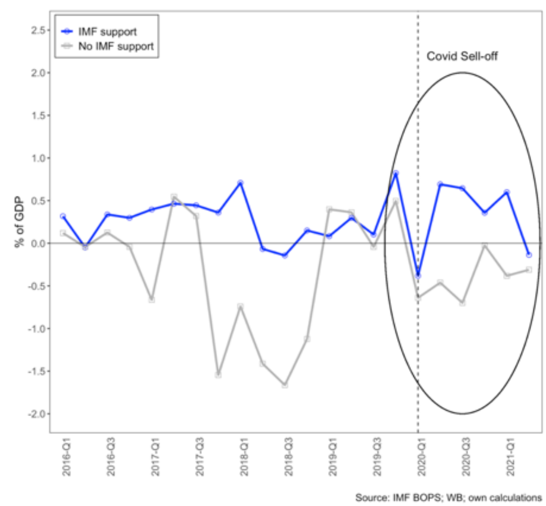
In the attempt to compare countries that are more similar to each other, we show an additional plot of the evolution of capital flows, grouping by income classes. We consider 3 mutually exclusive groups: Lower Middle Income Developing Countries, Lower Middle Income, and Upper Middle Income Developing Countries. These figures show that the difference between treated and untreated EMDEs was mainly driven by Upper Middle Income countries, whereas for Low Income countries the picture is not so clear-cut. Also, by extending the time span we note that the pre-trends of the groups are far from being parallel, meaning that it is difficult to attribute the observed difference in flows to the Fund intervention. Appendix figure 5.1 groups the countries by sovereign credit ratings, using S&P ratings for the available countries. We find that assisted countries had higher net capital flows than non-assisted ones only in the high-rating group. Finally, we group the countries by geographic regions, as the available sources of finance greatly differ across regions. The positive finding for "high-performing" countries robustly emerges from different groupings. In contrast, the other components of the BoP financial account (Foreign Direct Investments and Other Investments) do not display considerable differences between the treated and untreated countries.

Figure 4.2: *Portfolio Net Inflows by Income Groups*

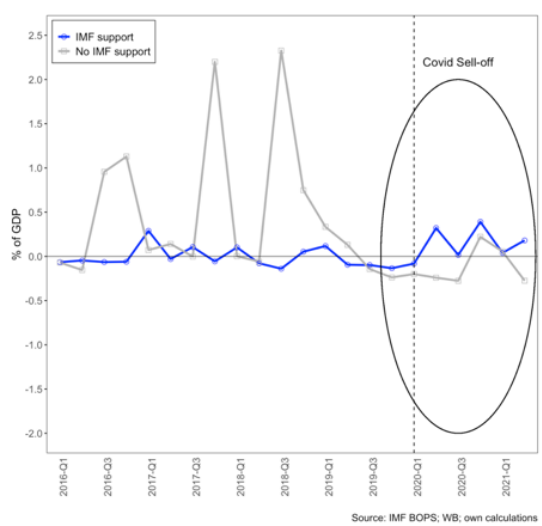
(a) *LMI*



(b) *UMI*



(c) *LIDC*



Note: The figure shows the net portfolio inflows scaled by the GDP as of 2019 by income groups. The MIDCs by the IMF categorization are further split into Lower Middle Income and Upper Middle Income, according to the WB classification. In each panel, the treatment group is composed of all countries that received IMF support during the Covid pandemic.

4.2 Econometric Analysis

We start our econometric analysis, using the fixed effect approach with BoP data on total capital flows. Table 4.1 reports the estimates from the baseline two-way panel regression. Adopting this baseline technique, we clean for country-specific time constant factors and time-varying global factors. We find that the approval of IMF financing is associated with an increase in capital flows of about 0,9% of GDP within the quarter of intervention for Middle Income countries. In contrast, Low Income countries experienced a negative but negligible change in capital flows after the IMF intervention (which can be taken as a null effect). This finding is consistent with the graphical evidence provided in the previous section. Furthermore, the positive estimates are driven by portfolio flows.

Yet, this naïve approach does not address the selection problem seriously. In particular, the observed evolution of capital flows shows that the parallel pre-trends assumption is unlikely to hold, and, consequently, causal claims cannot be made. The estimates could be the result of selection effects. Traditional programs were affected by a selection that biased the estimates downward, as typically countries facing balance of payments difficulties requested to participate in IMF programs. In other terms, these low-performing countries can be thought to have unobserved terms positively correlated with IMF participation and negatively correlated with capital flows. For the Covid-19 episode, instead, the direction of the bias is less clear *a-priori* for the very nature of the crisis: the majority of EMDEs faced difficulties.

Table 4.1: *Net Capital Flows BoP, Emergency Financing*

	Dependent variable:		
	Net Capital Flows		
	(EMDE)	(LIDC)	(MIDC)
Dummy IMF	0.621	-0.033**	0.877*
	(0.406)	(0.005)	(0.477)
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Sample Size	874	210	664
Note:	*p<0.1; **p<0.05; ***p<0.01		

Note: The table reports the estimates for the average treatment effect of the IMF approval, where we use the BoP capital flows data. The estimates are given in percentage of GDP. Heteroskedastic-robust clustered (across countries) standard errors are presented in parenthesis. The treatment group is "Emergency Financing".

To correct for selection, we apply CBPS, following Imai and Ratkovic [2015]. Then, we perform weighted panel FEs regressions with weights given by CBPS. By doing so, we aim to compare treated and untreated countries which are similar in terms of observables while controlling for country and time fixed effects. In the first stage model, we progressively include the categories of variables previously discussed. These are: (i) external financing needs and buffers, (ii) foreign exchange reserves, (iii) financial market depth index (FMDI), which proxies the level of financial integration, and (iv) additional pull factors, namely GDP growth and inflation.

At the same time, we want to track the effect of the IMF financial assistance over time. Thus, we produce IRFs by Local Projections (Jorda, 2005), given by our baseline two-way fixed effect specification. Table 4.2 reports the estimates for the cumulative average treatment effect after 1 quarter from the IMF approval ($h = 2$). Specifically, it displays the estimates given by the naïve baseline FE model and the coefficients from the CBPS-weighted FE approach. The results given by the unweighted regressions are robust when controlling for selection stemming from observables. We find that IMF financial support is associated with an average increase in EMDEs by +0.67% of GDP when controlling for all covariates at hand. The effect is driven by MIDCs while LIDCs had a lower impact but still significant after 1 quarter lag, suggesting that the baseline unweighted estimates were lightly upward biased for MIDCs and, conversely, downward biased for LIDCs. This confirms the intuition that selection effects were weaker during the global Covid pandemic than they usually are for traditional IMF-supported programs (see Krahnke [2020a] among others). Notably, the results are robust across specifications, and the positive effect of MIDCs always drives the effect for EMDEs. Instead, we do not find a considerable effect for Low Income countries, even after controlling for selection (the effect on portfolio flows is significant after 1 quarter but relatively small in size).

Figure 4.3 shows the IRFs for the IMF intervention to net portfolio flows over 1-year horizon, given by the most complete specification of CBPS (specification 3 in Table 4.2). The IMF support was associated with an increase in the capital flows of assisted countries that persisted for 3 quarters. MIDCs experienced stronger effects than LIDCs but also dissipated faster. These differences in the catalytic effect of Fund financing between Middle Income and Low Income countries may reflect the different roles that Fund financing has in countries with different income fundamentals. To better understand how the CBPS weighting scheme affects the baseline estimates, we produce the IRFs by iteratively including one variable at each step. The IRFs across specifications are reported in Appendix figure 5.2.

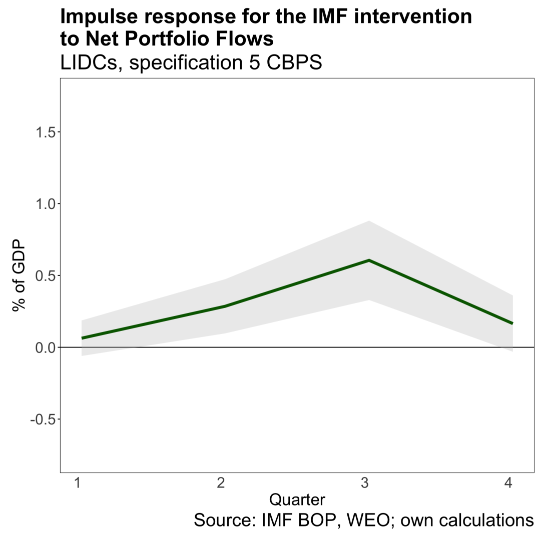
Table 4.2: *Net Portfolio Flows BoP, Emergency Financing CBPS*

	Response : Net Portfolio Flows, **h = 2**		
	(Unweighted)	(2)	(3)
Dummy IMF	0.689***	0.600***	0.670***
EMDE	(0.129)	(0.123)	(0.132)
Sample Size	970	970	970
Dummy IMF	0.241*	0.232*	0.286**
LIDC	(0.125)	(0.124)	(0.114)
Sample Size	236	236	236
Dummy IMF	1.076***	0.880***	0.927***
MIDC	(0.215)	(0.195)	(0.201)
Sample Size	734	734	734
Cov CBPS			
EFN		✓	✓
FX Reserves		✓	✓
GDP gr.		✓	✓
FMDI			✓
Individual FE	✓	✓	✓
Time FE	✓	✓	✓
Note:			*p<0.1; **p<0.05; ***p<0.01

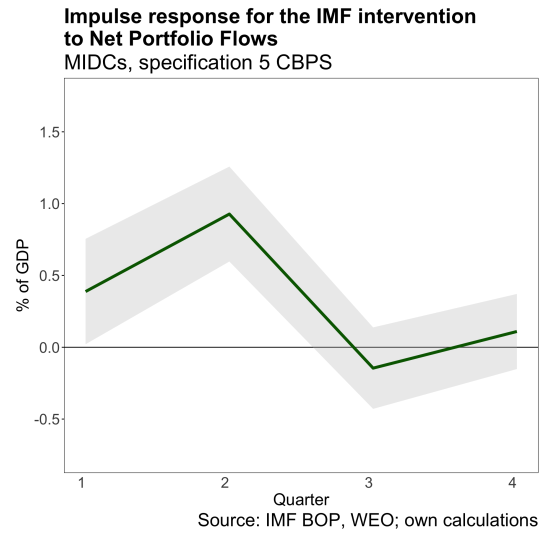
Note: The table reports the estimates given by the unweighted FE model and the CBPS weighted FE regressions. The estimates give the cumulative effect after 1 quarter from the IMF approval (h=2), in percentage of GDP. Heteroskedastic-robust clustered (across countries) standard errors are presented in parenthesis. The treatment group is "Emergency Financing".

Figure 4.3: *CBPS weighted IRFs, complete specification*

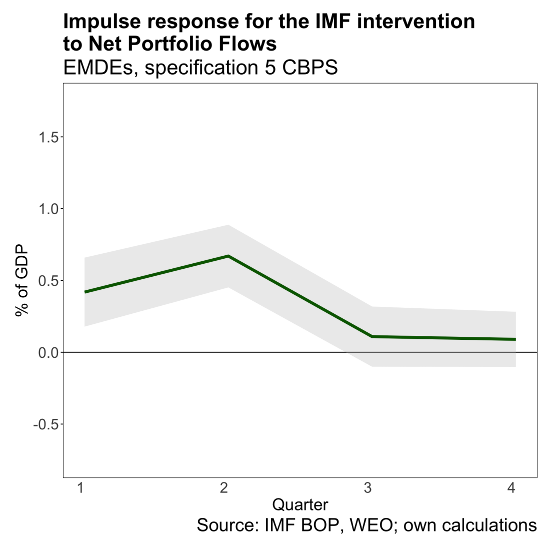
(a) *LIDC*



(b) *MIDC*



(c) *EMDE*



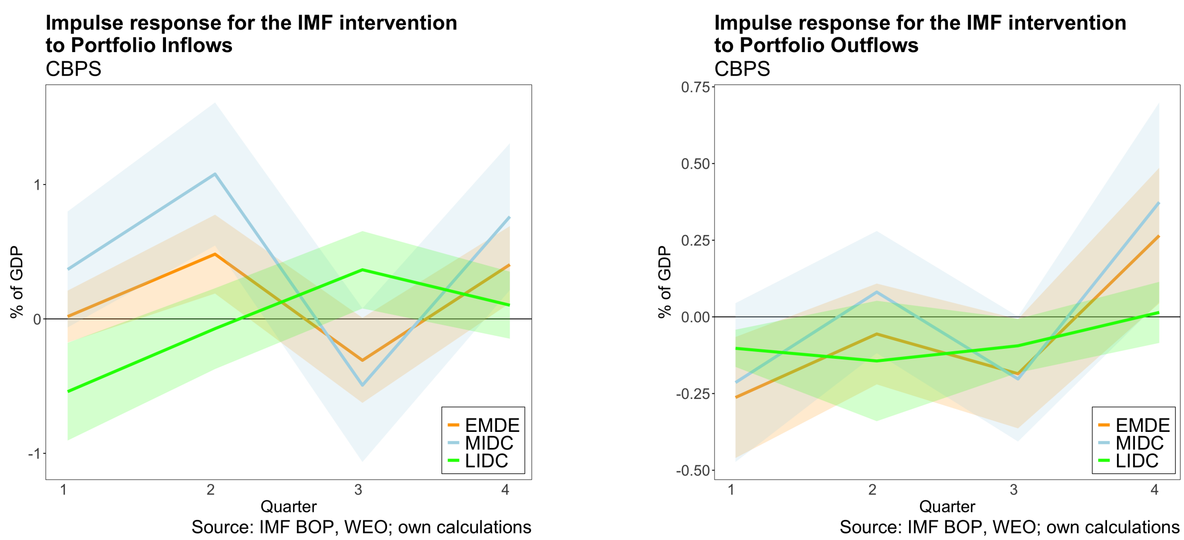
Note: The figure shows IRFs for the IMF intervention to net portfolio flows over 1-year horizon, given by the most complete specification of the CBPS approach. Heteroskedastic-robust clustered 90% confidence level bands are presented.

We have estimated a positive net effect for EMDEs, especially driven by MIDCs. Nonetheless, we study the impact of IMF support on gross flows as well. In our view, both measures are relevant. Analyzing net flows reflects the contribution of IMF assistance to external adjustment, while the impact on gross flows helps to understand whether the decisions of international investors (inflows) or domestic investors (outflows) were affected by the IMF intervention. Krahnke [2020a] and other empirical contributions analyze the catalytic effect of the IMF through the lenses of gross inflows by foreign investors since IMF financial assistance should affect the confidence of international investors in particular.

Figure 4.4 shows the IRFs to gross portfolio inflows (left panel) and to gross portfolio outflows (right panel). In line with the existing empirical literature on the catalytic role of the Fund, we find that the positive net effect is determined by an increase in gross portfolio inflows. In other words, the IMF intervention attracted capital from foreign investors.

Figure 4.4: *CBPS weighted IRFs, Gross Portfolio Flows*

(a) *Gross Portfolio Inflows* (b) *Gross Portfolio Outflows*



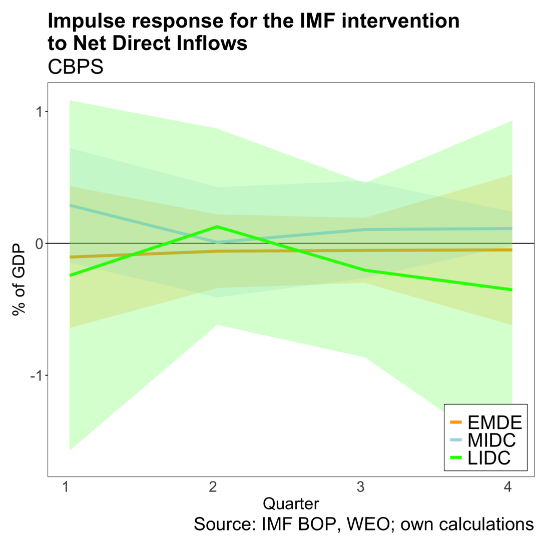
Note: The figure shows IRFs for the IMF intervention to gross portfolio flows over 1-year horizon, given by the most complete specification of the CBPS approach. Heteroskedastic-robust clustered 90% confidence level bands are presented.

4.2.1 FDI and Other Investments

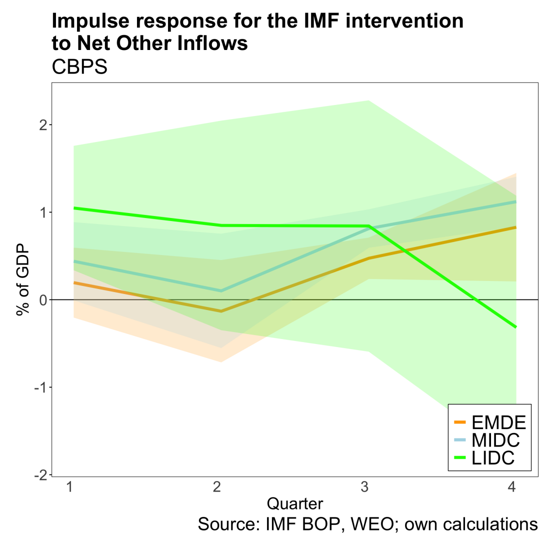
In the previous subsection, we investigated the effect of IMF support on portfolio flows, which is the most volatile and responsive component of the BoP financial account. We now turn to the analysis of Foreign Direct Investments (FDI) and Other Investments. Figure 4.5 shows the IRFs to net FDI and to net Others. Differently from portfolio flows, the other 2 sub-components of the BoP capital flows did not benefit from the intervention of the Fund in the short run. The estimates are low and insignificant, with the exception of a weak positive effect on net Other Investments for MIDCs (smaller and less clear-cut than the effect on portfolio flows). We conclude that the overall catalytic effect was driven by portfolio flows.

Figure 4.5: *unweighted IRFs, FDI and Other Investments*

(a) *Net FDI*



(b) *Net Other Investments*



Note: The figure shows IRFs for the IMF intervention to net foreign direct investments (left panel) and other investments (right panel) over 1-year horizon, given by the most complete specification of the CBPS approach. Heteroskedastic-robust clustered 90% confidence level bands are presented.

4.2.2 Emergency Financing Vs. other financing facilities

Previously, we have investigated the effect of IMF *Emergency Financing* (RFI/RCF), as this is the focus of our analysis. Yet, we also ask whether all Covid support has an effect on capital flows on average. As expected, the estimates do not differ substantially between the two treatment groups (*All Financing* and *Emergency Financing*), due to the fact that the financing took place mainly via RFI and RCF. However, we also notice that the effect of Emergency Financing was stronger than the effect of the other financing facilities. In particular, traditional UCT programs display lower and insignificant estimates (even though the lack of statistical significance may be simply related to the small sample of treated countries). Anyways, Emergency Financing has reassured private investors, in support of the policy strategy for the IMF response to the pandemic.[1]

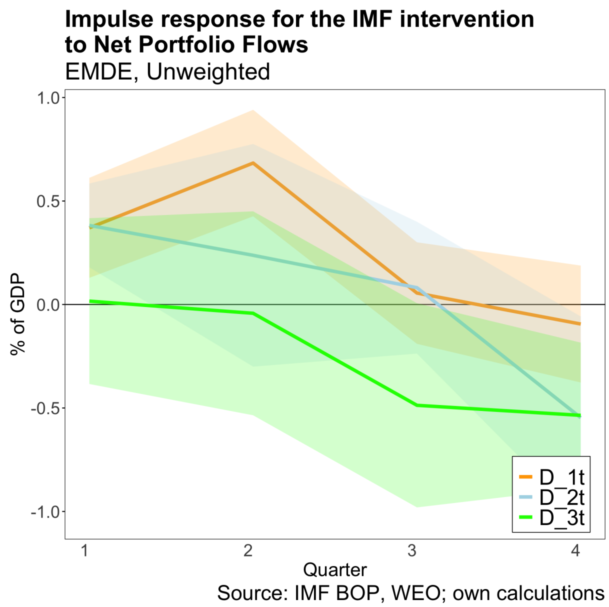
4.2.3 Robustness Checks

CBPS is a generalization of the propensity score (PS) weighting. Thus, we perform PS as a robustness check. In line with our preferred methodology, we find a positive effect for EMDEs, mainly driven by MIDCs. However, we also find a weak positive effect for LIDCs, probably related to an imperfect covariate balance.

An additional issue regards the timing of the catalytic effects. So far, we have investigated the impact of the Fund intervention on the capital flows at a given quarter (and its cumulative effect over time), as the dummy takes value 1 only at the time of intervention. This is because we expect the *seal of approval effect* to materialize quickly as the approval is announced, mainly through liquidity and surveillance channels. We explore several definitions of the dummy, from the baseline single-period to multi-period dummies which take the value of 1 at the time period of the approval and the subsequent periods (2 or 3 periods). We confirm our intuition by obtaining lower estimates and insignificant when using multi-period dummies. The single-period dummy is the best one to timely identify the *catalytic effect*.

[1] In the same spirit, we also explore 3 additional intermediate groups by excluding progressively the following measures from all financing: FCL, PLL; UCT follow-up; UCT augmentations. By doing so, we confirm that EF had the strongest effect.

Figure 4.6: *Unweighted IRFs of Portfolio Net Inflows, EMDEs, Single Vs. Multi-period dummy*



Note: The figure shows IRFs for the IMF intervention to net portfolio flows over 1-year horizon, for different definitions of the dummy IMF (single-period, 2, and 3 period dummies). Heteroskedastic-robust clustered 90% confidence level bands are presented.

SECTION 5 CONCLUSIONS

In this work, we document evidence in favor of the IMF policy response to the COVID-19 pandemic. The Fund Emergency Financing in the form of Rapid Financing Instrument and Rapid Credit Facility had a positive effect on the private capital flows of assisted countries, with an average impact of about 0.7% of (annual) GDP after 1 quarter from the approval. Yet, the effect on the capital market varied greatly across countries. The average effect on EMDEs was mainly driven by middle income developing economies, while we find a lower and negligible effect for low income developing economies. The results are robust to different identification strategies and a wide set of model specifications. Moreover, these results are in line with the descriptive graphical evidence.

Nevertheless, the findings presented need further investigation. The analysis would require a more careful selection of the covariates that should explain both the dynamics of capital flows and the participation in the IMF financial programs. For example, a more systematic approach would be to apply feature selection methods. Additionally, the analysis of the conventional BoP data could be complemented with the use of big data high-frequency proxies of capital flows. For example, the same methodology could be applied using "SPR's SWIFT Monitor", which uses cross-border inter-bank transactions sent through the SWIFT network to proxy for international capital flows. More specifically, one could use the SWIFT international transactions monitor based on MT 103 messages: "both outflows and inflows are recorded. These transactions can be useful high-frequency proxies for international remittances and capital flows (IMF, 2021)". This innovative data source would provide greater time and country variation to draw causal inference.

Regarding the methodology adopted, the main limitation is that the CBPS weighting correction method would provide inconsistent estimates, in the presence of country-specific time-varying omitted factors that are not accounted for by the observable covariates. Nevertheless, we have shown that selection (the main source of endogeneity) is less of a concern than for traditional IMF programs, given the global nature of the COVID-19 pandemic.

On top of that, the method for Impulse Response Functions by Local Projections (Jorda, [2005b]) should be used for longer time spans of analysis. In this respect, the use of the big data high-frequency proxy, such as at the monthly frequency, would provide a compelling solution for this issue as well.

The inability of the IMF financial assistance to catalyze capital flows towards EMDEs with poor fundamentals should be explained. Probably, the IMF financial assistance that was provided on very easy terms to basically all the requesting countries was not a credible signal to induce a crowding-in effect toward fragile economies. At best, the emergency financing to poor and low-credit countries mitigated the urgent BoP needs, helping to avoid default, but not inducing catalytic effects on private capital flows.

Moreover, we note that generally Low Income Developing Countries are not financially integrated (inspecting the financial market depth index). The IMF intervention in countries with little access to private capital markets is not supposed to attract capital flows from foreign investors. A related interesting research question would then be to study whether IMF financing helped to catalyze official financial support from other IFIs, but this is out of the scope of this analysis.

Appendix

Appendix A: Treatment/Control Groups Composition

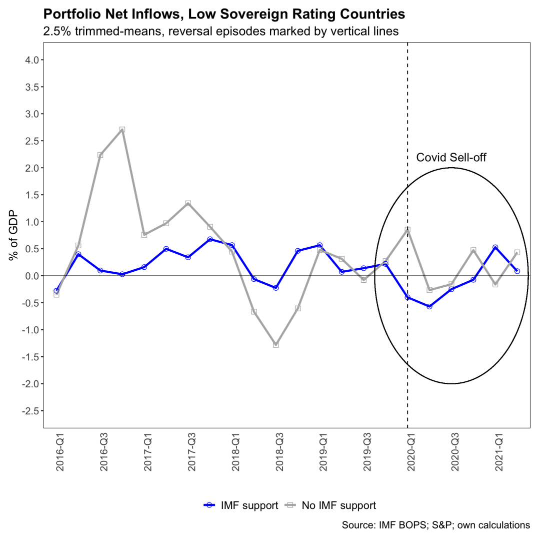
Table 5.1: *Treatment Groups Compositions*

Emergency Financing		**Traditional UCT**	**FCL/PLL**
Afghanistan	Lesotho	Afghanistan	Chile
Albania	Moldova	Angola	Colombia
Burundi	Madagascar	Armenia	Morocco
Benin	Maldives	Benin	Mexico
Burkina Faso	North Macedonia	Barbados	Panama
Bangladesh	Mali	Cameroon	Peru
Bahamas	Myanmar	Congo D.M.	
Bosnia and Herzegovina	Montenegro	Costa Rica	
Bolivia	Mongolia	Ecuador	
Central African Republic	Monzambique	Egypt	
Cote d'Ivoire	Mauritania	Gabon	
Cameroon	Malawi	Georgia	
Congo D.R.	Namibia	Gambia	
Comoros	Niger	Honduras	
Cabo Verde	Nigeria	Jordan	
Costa Rica	Nicaragua	Kenya	
Djibouti	Nepal	Moldova	
Dominica	Pakistan	Madagascar	
Dominican Republic	Panama	Mauritania	
Ecuador	Papua New Guinea	Niger	
Egypt	Paraguay	Nepal	
Ethiopia	Rwanda	Sudan	
Gabon	Senegal	Senegal	
Ghana	Solomon Islands	Somalia	
Guinea	Sierra Leone	Sao Tome & Principe	
Gambia	El Salvador	Suriname	
Guinea Bissau	South Sudan	Seychelles	
Equatorial Guinea	Sao Tome & Principe	Chad	
Grenada	Eswatini	Togo	
Guatemala	Seychelles	Uganda	
Haiti	Chad	Ukraine	
Jamaica	Tajikistan		
Jordan	Tonga		
Kenya	Tunisia		
Kyrgyz Republic	Tanzania		
Kosovo	Uganda		
Liberia	Uzbekistan		
St.Lucia	St. Vincent and the Grenadines		
Samoa	South Africa		

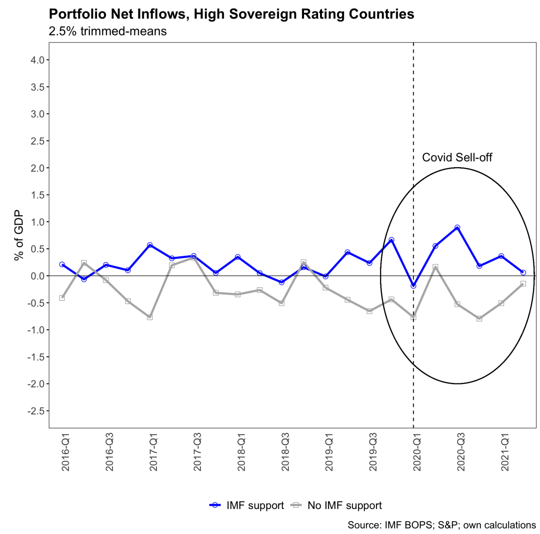
Appendix B: Evolution of Portfolio Flows by Credit Rating

Figure 5.1: *Portfolio Net Inflows by Rating Groups*

(a) *Low Rating*



(b) *High Rating*



Note: The figure shows the net portfolio inflows scaled by the GDP as of 2019 by credit rating groups. In each panel, the treatment group is composed of all countries that received IMF support during the Covid pandemic.

Appendix C: Variables Sources

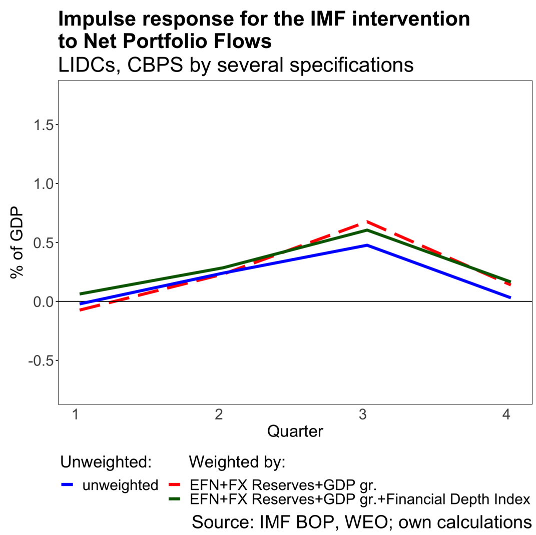
Table 5.2: *Variables Definitions and Sources*

Variable	Definition [Code]	Source
net_portfolio	Portfolio gross inflows - Portfolio gross outflows scaled by 2019 GDP [BFPL_BP6 - BFPA_BP6]	BoP
net_fdi	Foreign Direct Investments gross inflows - Foreign Direct Investments gross outflows scaled by 2019 GDP [BFDL_BP6 - BFDA_BP6]	BoP
net_other	Other Investments gross inflows - Other Investments gross outflows scaled by 2019 GDP [BFOL_BP6 - BFPA_BP6]	BoP
Dummy IMF	Indicator of IMF lending: takes 1 at the time of IMF intervention	IMF COVID-19 Lending Tracker
EFN	External Financing Needs, proxied by CA+KA-FDI	WEO
FX Reserves	FX reserves measured in months of imports	WEO
GDP gr	Pre-crisis forecast of 2020 nominal GDP growth [NGDP_R]	WEO
FMDI	Level of financial integration, proxied by Financial Market Depth Index [FD_FME_IX]	Financial Development Index Database
Lending Size	Commitments as a share of GDP	Covid-19 Tracker
Income per capita	Nominal Income per capita	WEO
Inflation	Percentage change in CPI	IFS
Current Account Balance	Balance on goods and services scaled by GDP	IFS
Reserves	Total reserves excluding gold	IFS
Openness	(I + X)/GDP	DOT
Exchange Rate Volatility	Variance of exchange rate scaled by the mean	IFS
Lending Rate differential	Lending interest rate differential vis-à-vis the US	IFS
Fed Fund Rate	FFR	FRED
VIX	VVIX	CBOE

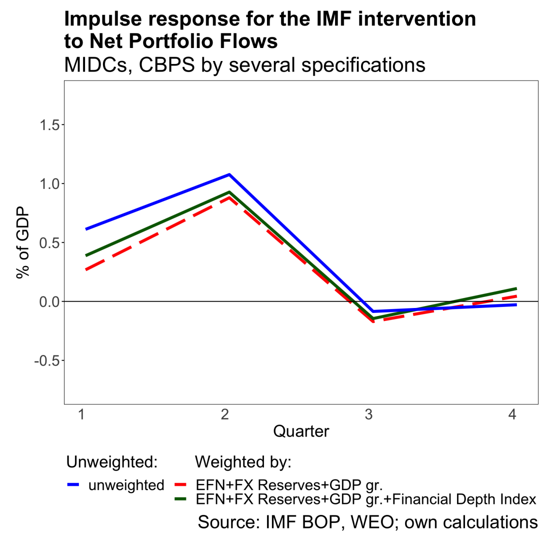
Appendix D: CBPS IRFs across specifications

Figure 5.2: *CBPS weighted IRFs by Income Groups*

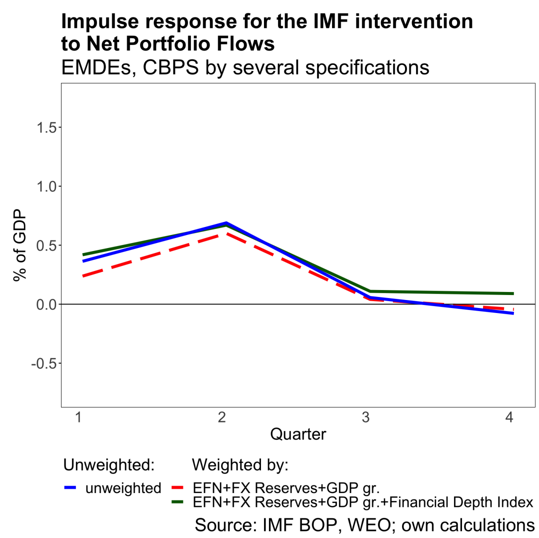
(a) *LIDC*



(b) *MIDC*



(c) *EMDE*



Note: The figure shows IRFs for the IMF intervention to net portfolio flows over 1-year horizon, across several specifications of the CBPS approach. Confidence bands are not presented here to facilitate readability .

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