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Yuan Gao Rollinson

Policies as Determinants for Economic Performance

Yuan Gao Rollinson

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Reading Committee:

Theo Eicher, Chair

Thomas Gilbert

Chang-Jin Kim

Stephan Siegel

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Abstract

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Yuan Gao Rollinson

Chair of the Supervisory Committee:
Professor Theo Eicher
Department of Economics

This dissertation studies topics that have substantial policy implications for the public sector, centering around designing, assessing, and implementing policies for improving economic performances at the macro level.

The first chapter first conducts a comprehensive review of the recent development of IMF lending, including an early wave of literature on examining the determinants of IMF lending programs, and the second wave of literature on assessing the impact of IMF lending programs (and the attached conditionality). Based on the comprehensive review and the observed increased reliance of IMF decision-making on the nowcasts data, I identify the nowcasting of IMF lending activities as an important topic for future research that would be particularly useful to aid in implementing IMF lending programs. A gap in the existing literature calls for a comprehensive study to identify the drivers of nowcast inefficiency for IMF lending, based on all available IMF loan programs data up to date.

In light of the first chapter, the second chapter examines the accuracy of the IMF's assessments of crisis nowcasts that predicate program design. Analyzing an unprecedented 602 IMF loan programs from 1992 to 2019, we contradict the popular notion that IMF forecasts are generally optimistic. By disentangling the structure of the nowcast bias, we find the IMF systematically overestimates low-growth recoveries for Low-Income Countries' (LICs) GDPs while underestimating high-growth recoveries. Our unusually large sample allows us to docu-

ment that Non-LICs nowcasts exhibit no statistically significant optimistic/pessimistic bias. We isolate the sources of inefficiencies in IMF nowcasts, including: (i) program objectives, (ii) program conditionality, (iii) geographic regions, (iv) global crises, and (v) geopolitics (elections, conflicts, disasters). In addition, we show that shorter nowcast horizons do not improve accuracy, and that GDP growth nowcasts improved substantially since 2013. Inflation nowcasts continue to struggle with efficiency as recently as 2018.

The third chapter studies the topic of macroprudential policy. It examines empirically the role of macroprudential policy in addressing the effects of external shocks on financial stability. The baseline dynamic panel regressions show that an appreciation of the local exchange rate is associated with a subsequent increase in the domestic credit gap, while a prior tightening of macroprudential policies dampens this effect. These results are strong for small open economies, and robust when we explicitly account for endogeneity of macroprudential policy and changes in exchange rates. We also examine a feedback effect where strong domestic credit pulls in additional cross-border funding, potentially further increasing systemic risk, and find that targeted capital controls can alleviate this effect.

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DEDICATION

To my beloved husband Tobias and daughter Isabelle,
as they are the light of my life
and my strength in completing this dissertation.

Chapter 1

THE RECENT DEVELOPMENT OF IMF LENDING AND NOWCASTING

1.1 *Introduction*

The International Monetary Fund (IMF) was formally formed in 1945 to reconstruct the international monetary system, after the breakdown of international monetary cooperation during the Great Depression and World War II. Its initial functions are to oversee fixed exchange rate arrangements¹ among 29 advanced economies, and provide short-term lending to aid the balance of payments (IMF, 2021e; Jensen, 2004). Providing loans to member countries experiencing actual or potential balance of payments issues has been a core responsibility of the IMF since its creation (IMF, 2021a). IMF's financial assistance is provided in the form of programmatic lending to resolve crises and strengthen countries' economies. The loans are usually disbursed in several installments, conditional on the borrowing country undertaking the mandated economic policies and meeting the performance criteria (the "conditionality") laid down in the loan agreements. Data has played a critical role in IMF lending, as IMF's loan decision, policy prescription, and conditionality design are on the basis of its projection of macroeconomic indicators for the near-term (nowcasts) and the medium-term (forecasts), especially after the 2009 IMF lending reform. In particular, inaccurate IMF nowcasts would have large consequences: overly optimistic nowcasts would underestimate the needs for loan and adjustment policy, and can lead to disruption of loan disbursement if the performance criteria and structural targets are set too high to achieve

¹The fixed exchange rate system, also referred as the Bretton Woods System, required a currency peg to the U.S. dollar, whose value was in turn pegged to the price of gold for the United States. Exchange rates were only allowed to adjust with the IMF's agreement, and for the purpose of correcting a "fundamental disequilibrium" in the balance of payments (IMF, 2021e)

(IMF, 2019); while overly pessimistic nowcasts would overstate financial needs, which may lead to misallocation of resources and fail to sufficiently address the urgent issues of borrowing countries. Until recently, only limited research was devoted to assessing and improving IMF lending nowcasts established at the times of crises, which will be reviewed below.

The role of IMF lending, as well as the scope and content of its conditionality, have been evolving, which has led to further challenges and increased reliance on accurate assessment of economic conditions. First, the role expansion of IMF lending required the IMF country team to assess different economic issues beyond its initial focus of exchange rate policy. Accordingly, the design of conditionality started to incorporate the projection of a broader range of macroeconomic indicators to set the performance criteria. IMF's lending assistances were exclusive to address the balance of payment difficulties and currency crises in the 50s and 60s. It broadened lending for banking and sovereign debt crises in the 70s and 80s (e.g., Mexican 1982 debt crisis), and economic transformation in the early 90s (e.g., the transition of former Soviet Union countries from central planning to market-driven economies) (IMF, 2021e). As a result, the scope of conditionality expanded into new fields such as central government operation, central bank monetary operation, and financial sector regulation, and reforms of public and private sectors. Second, extending IMF lending to emerging markets and low-income countries has imposed several challenges that called for different conditionalities. Compared to advanced economies that usually need temporary IMF financing for the balance of payment and liquidity problems, the emerging markets and low-income countries have minimal access to the private financial markets and have prolonged dependence on IMF loans. Many of them exhibited a chronic dependence on IMF concessionary loan during the 80s to 90s and needed long-term development assistance (Goldsbrough et al., 2002; Reinhart & Trebesch, 2016). This implied a need for policy shift of conditionality toward structural reforms aimed at poverty reduction and sustainable growth, which involved structural benchmarks that are difficult to measure (Polak, 1991; Kentikelenis et al., 2016). In addition, the poor data quality of low-income countries, and limited data availability especially when they are in crises, are the main reasons for the low forecast accuracy (Luna, 2014),

presenting further data uncertainty for IMF program design and conditionality. With the above in mind, accurate assessment of borrowing countries' economic conditions is becoming more challenging, yet still essential for design appropriate conditionality to aid crisis-affected countries.

During 2009-2010, the IMF implemented a series of reforms to strengthen its lending framework, in response to the deepening global economic difficulties post the 2008 Global Financial Crisis (GFC). The reform included major modification of the existing lending facilities and the creation of new lending facilities, to enable IMF to be more flexible and responsive to the evolving needs of crisis-affected countries. As part of the 2009 lending reform, the IMF released its MONA (Monitoring of Fund Arrangements) database and disclosed the associated confidential loan documents (the Executive Board Special documents), making it possible to quantitatively assess IMF lending activities using the nowcast and forecast data.

Literature relates to IMF lending have in general two waves: The first wave focuses on examine the determinants of IMF loan programs, from the perspectives of both political and economic considerations. After IMF introduced its MONA database to the public, IMF's lending decisions and program reviews became more transparent. Also, the MONA database covers more granular data of IMF loan decision-making, compared to annual frequency data used in the previous literature. As a result, the literature focus started to switch to assessing the impact of IMF lending programs (with emphasis on the conditionality attached to the lending programs) on economics conditions, and the unintended consequences on other sectors². With the increasingly important role of IMF lending in stabilizing the global economy and the increasing reliance of IMF decision-making on accurate nowcast data, as highlighted by the Covid-19 crisis, a new research avenue emerges on the topic nowcasting of IMF lending programs.

This paper first presents an overview of the recent development of IMF lending activities

²See [Thomson et al. \(2017\)](#) for the impact of IMF programs on public health, and see [Bredenkamp & Pattillo \(2010\)](#) for the impact on the environment.

in Section 1.2, aim to provide readers the minimum necessary institutional knowledge to understand the present policy issues relate to IMF lending that need to be addressed. It describes the structure and determinants of IMF lending, current lending facilities that are tailored to countries' needs post the 2008 GFC, the impact of IMF lending programs and conditionalities, as well as their heavy reliance on the nowcasts data. Section 1.3 exploits a detailed discussion of the nowcasting topic that have played a critical role in IMF lending decision especially in recent years. Also, I identify a research agenda to further improve the nowcasting for IMF lending. Section 1.4 concludes.

1.2 IMF Lending: Adapt to the Evolving Needs

1.2.1 The Structure and Determinants of IMF Lending

Participation in IMF lending program is a joint decision of the IMF and the member country. A lending program involves policy understandings and the assessment of the near-term and medium-term macroeconomic conditions of the member country, for certain period ahead of the program start date. When a member country encounters an actual or potential balance of payments difficulties, they submit a “letter of intent” to the IMF Executive Board to request a lending program. The letter is based on the negotiation outcomes between country authorities and IMF desk team, which indicates financing need, loan duration, repayment plan, and more importantly - the mandated economic adjustment policy targets (the “conditionality”) and the assessment of macroeconomic conditions within the duration of the program (the “nowcasts” and “forecasts”). After that, IMF desk team submits a “staff report” to assess the program request from the perspective of financing need, necessity and implementability of performance criteria, and government’s commitment to undertake the conditionality. The Executive Board reviews the two documents and makes the decision of approval based on quota-based votes. The “quota” broadly reflect each member country’s relative position in the world economy³ and determines each member’s voting power in IMF decisions and max-

³The current quota formula is a weighted average of GDP (weight of 50 percent), openness (30 percent), economic variability (15 percent), and international reserves (5 percent).

imum amount of financing assistance can obtain from the IMF under normal access (IMF, 2021g).⁴ Member countries may make use of its first tranche of IMF credit (25 percent of the quota) fairly liberally without entailing further policy discussion. Requests for use of IMF resources beyond the first credit tranche require an arrangement (aka, the lending program) between the IMF and the member country.⁵

One of the central criticisms of the IMF relates to the concerns of its political oriented lending behavior. The approval of IMF-supported lending program is subject to a supermajority vote of 85 percent on the Executive Board. The IMF has been criticized that its lending decisions are under the influence or pressure of major IMF shareholders for their own interest or agenda (e.g. U.S., Japan, Germany). Many literature (Blomberg & Lawrence Broz, 2006; Dreher & Vaubel, 2004; Dreher et al., 2008) pointed out the political power imbalances in IMF's governance structure, as a result of voting shares being based primarily on the size and openness of the economies⁶, leaves the emerging economies and low-income countries limited influence over IMF's decision-making process, despite the fact that those countries often request loans and technical assistance from the IMF. The 2010 quota reform, which finally entered into effect in 2016, shifted 6.2 percent of quota shares towards emerging markets and low-income countries, however, the distribution of voting power remained severely imbalanced in favor of the U.S., Japan, and European countries. In particular, as the largest shareholder with over a 16 percent of voting power since IMF was founded⁷, the United States (U.S.) has a unique veto power over major policy decisions, including the approval and cancellation of lending programs⁸. Moreover, the political influence on IMF lending

⁴The quota also determines each members' maximum financial obligation of a member country to the IMF and share in the IMF's unit of account - the Special Drawing Rights (IMF, 2021g).

⁵RFI and RCF have no fund-supported programs, but played important role aiding countries during Covid-19 crises (IMF, 2021c).

⁶It is also argued that the quotas are unbalanced and inappropriately reflective of member countries' relative status in the world economy.

⁷U.S.'s quota share was 31.7 percent when IMF was founded, and is currently 16.51 percent as of May 2021 (IMF, 2021g).

⁸A supermajority vote of 85 or 70 percent is required to approve major IMF decisions

decisions also has applied to its forecasting. As [Dreher et al. \(2008\)](#) find in their study, the major IMF shareholders who possess large voting power and their political allies, tend to receive more optimistic forecasts. The forecast bias relate to IMF lending will be reviewed in section 1.3 below.

It is a consensus that IMF lending decision-making is guided by both economic and political considerations ([Dreher et al., 2008](#); [Breen, 2013](#)). Much research has been devoted to examining the determinants of IMF lending programs, from the perspective of IMF policy objective and member countries' motivation ([Thacker, 1999](#); [Bird & Rowlands, 2001, 2017](#)). Results are sample dependent, varying by sample periods, countries, types of IMF lending programs, empirical methodologies, variable definitions.

The empirical methodologies of explaining IMF program participations are often based on binary outcome models of the Logit ([Thacker, 1999](#); [Andersen et al., 2006](#)) or Probit model ([Conway, 1994](#)). [Barro & Lee \(2005\)](#) instead use a Tobit model, and measure the dependent variable as the fraction of time a country participated in IMF programs. Determinants relates to economic concerns are primarily indicators that reflect the deterioration of external sectors. Suppose t is the start year of an IMF loan program, determinants in levels are usually measured in year $t-1$ while determinants in changes are usually measured based on the (value or percentage) change from $t-1$ to t . [Thacker \(1999\)](#) find the member countries' balance of payment position (only the level variable, not the change), per capita debt level (public and private debt), debt-to-GNP ratio, reserves-to-debt ratio, and interests payment-to-GNP are the robust indicators of the likelihood that a country will receive a IMF loan. Later studies by [Conway \(1994\)](#) and [Barro & Lee \(2005\)](#) have similar findings of the economic determinants, based on different samples.⁹ The three abovementioned studies focus on only two types of IMF lending programs: Stand-by Arrangement (SBA) and Extended Fund Facility (EFF) programs, and excluded other lending programs (e.g. SAF,

⁹[Thacker \(1999\)](#) is based on a group of 87 developing countries over the period of 1985-1994. [Conway \(1994\)](#)'s sample covers 74 developing countries over the period of 1976-1986; [Barro & Lee \(2005\)](#) has a more comprehensive sample of 130 countries during 1975-2000.

ESAF, PRGF) designed for assisting the long-term growth and poverty reduction of the low-income countries. Determinants relates to political concerns stress the overwhelming political influences of the G7 countries (US, Canada, France, Germany, Italy, Japan, and U.K.) in shaping the IMF lending decisions at the advantage of their political and economic interests, in particular the case of the U.S.. Several research, mainly prior to the 2008 GFC, find evidence to support a member country’s political proximity to the U.S. in the international voting space (e.g. proxied by vote consistently with the U.S. in the United Nations General Assembly) being a significant and robust factor in IMF’s lending decision ([Thacker, 1999](#); [Barro & Lee, 2005](#); [Andersen et al., 2006](#); [Dreher et al., 2008](#)).¹⁰ Other political concern is that a country with a longer and deeper previous participation in IMF lending is more likely to receive approval for future IMF loans.¹¹ [Barro & Lee \(2005\)](#) also argues, the loans tend to be larger and more frequent when a country has a bigger quota, more professional staff at the IMF, and when it is more politically and economically connected with the IMF main shareholders.

Variables in above studies are in annual frequency and do not adequately reflect the volatility and surge of deterioration in a particular month before leading to the crises. This implies a potential research gap with accurate assessment of macroeconomic conditions utilizing the currently available higher frequency data provided by the IMF.¹² The launch of IMF’s Monitoring of Fund Arrangements (MONA) database in 2009 made IMF’s lending decisions and program reviews more transparent to the public. Besides covering a large number of macroeconomic indicators across different sectors, the MONA database also cov-

¹⁰[Dreher et al. \(2008\)](#) suspects that many of the arguments about IMF lending may also apply to its forecasting, which I discuss in section 1.3.3 drivers of inaccuracy.

¹¹[Goldsbrough et al. \(2002\)](#) measures the previous participation as the cumulative duration of engagement in IMF loan programs, and defined the “prolonged” users of IMF loans as countries engaged in programs for at least 7 years out of any 10 years. [Ramcharan \(2003\)](#) and [Dreher et al. \(2008\)](#) study the “defensive lending” and argue that IMF may have a perverse incentive to extend new loans to borrowers with large outstanding loans to ensure the existing loans are paid back on schedule.

¹²Thanks to IMF’s efforts to improve dissemination standard, currently most of the economic-concern-related variables become available in monthly or quarterly frequency in the International Financial Statistic (IFS) database

ers granular data of IMF loan decision-making since 1992, such as the loan disbursement schedule, dates of decisions and on which the forecasts and nowcasts were made, and details of conditionality etc. The MONA database facilitated the literature focus switched from examine the determinants of IMF lending program participation to the effects of IMF lending programs (and the attached conditionality) on macroeconomic conditions.

As the “international lender of last resort”, The IMF provides loans to countries at rates that are lower than they would have to pay for financing from the private markets. The IMF’s current financing resources consist of two broad categories: (i) non-concessional lending to the developed countries and emerging markets countries under the General Resources Account (GRA), which focuses on short-term BOP assistance; and (ii) concessional lending to low-income countries¹³ under the Poverty Reduction and Growth Trust (PRGT), which involves long-term financing to aid long-term growth.¹⁴ In addition, IMF complements the above financing facilities with two non-financing facilities, the Policy Support Instrument (PSI) created in 2015 and the Policy Coordination Instrument (PCI) created in 2017, to enable countries to signal commitment to reforms and catalyze financing from other sources, such as creditors, donors, and the general public.

1.2.2 From Goals to Facilities

Over the past 70 years, IMF’s lending expanded from a single facility, the Stand-By Arrangements (SBA) initially created in 1952, to the current 8 lending facilities and 2 non-financing (signaling) facilities as shown in [Figure 1.1](#). The various lending facilities are designed to meet different balance of payment needs, and they are tailored for specific circumstances and member countries’ levels of income and domestic fundamentals. [Table 1.1](#) describes the differences of IMF’s currently available lending facilities by purpose, account type, conditionality, access (limit of loan-to-quota ratio), and duration.

¹³The concessional lending is currently at zero interest rates through June 2021 for the Covid-19 response.

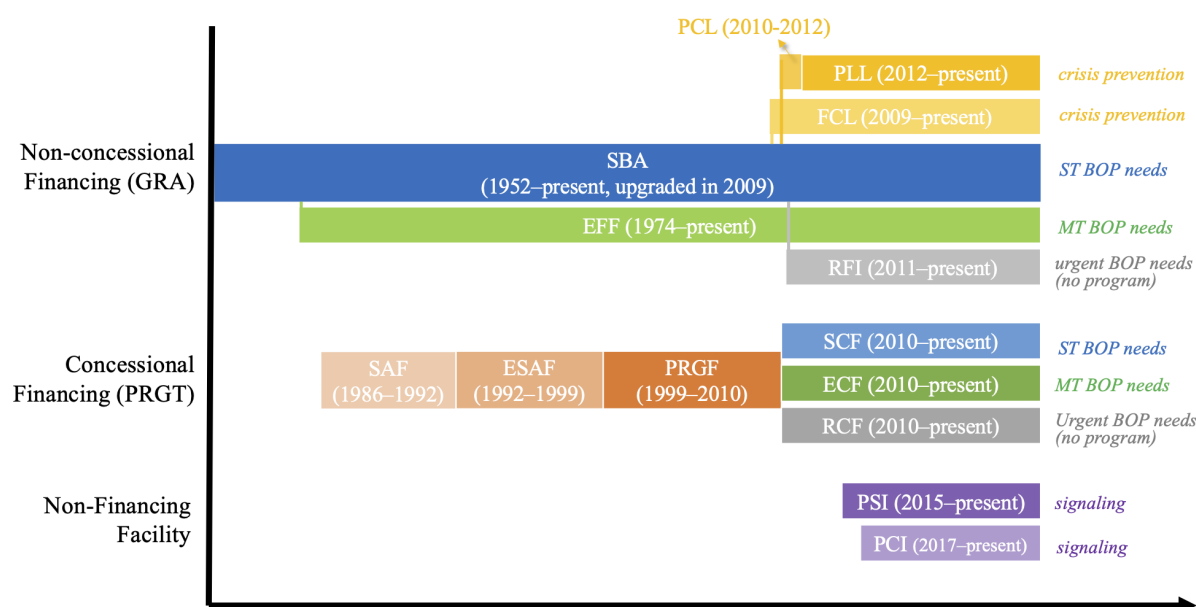
¹⁴[citeBarroLee2005](#) argues that the PRGT-related programs should be viewed more as foreign aid, rather than lending or adjustment programs.

Table 1.1: Description of the Current IMF Lending Facilities

Goal	Facility	Full Name (Year Created)	Non-concessional (GRA) or Concessional (PRGT)?	Conditionality	Limit of Normal Access (Exceptional access is case-by-case)	Duration
Financing facilities						
Present, prospective or potential BOP need	SBA	Stand-By Arrangements (1952, upgraded in 2009)	GRA	Ex-post	annual: 145% quota (245% of quota for Covid-19 response) cumulative: 435% of quota	Up to 3 years, but usually 1-2 years
	SCF	Standby Credit Facility (2010)	PRGT		annual: 100% quota cumulative: 300% of quota	1-2 years
Protracted BOP need/Medium-term assistance	EFF	Extended Fund Facility (1974)	GRA	Ex-post, with focus on structural reforms	annual: 145% quota (245% of quota for Covid-19 response) cumulative: 435% of quota	up to 4 years
	ECF	Extended Credit Facility (2010)	PRGT		annual: 100% quota cumulative: 300% of quota	3-4 years, extendable to 5 years
Actual and urgent BOP need	RFI	Rapid Financing Instrument (2011)	GRA (no program)	No Fund-supported program/ex-post condi, but prior actions possible	annual: 50% quota (100% of quota for Covid-19 response) cumulative: 100% of quota (150% of quota for Covid-19 response)	Outright purchase
	RCF	Rapid Credit Facility (2010)	PRGT (no program)		annual: 50% quota (100% of quota for Covid-19 response) cumulative: 100% of quota (150% of quota for Covid-19 response)	Outright disbursement
Crisis prevention and resolution, for countries with very strong fundamental and policies	FCL	Flexible Credit Line (2009, first program in 2017)	GRA	Ex-ante and annual reviews for the 2year arrangements. No Ex-post cond.	Uncapped	1 or 2 years
Present, prospective or potential BOP need, for countries with sound fundamental and policies	PLL*	Precautionary and Liquidity Line (2012)	GRA	Ex-ante and ex-post. focus on ex-post conditionality aimed at addressing vulnerabilities	500–1000% of quota	6 month (liquidity window) or 1-2 years
Non-financing facilities						
non-financing/signaling instruments	PCI	Policy Coordination Instrument (2017)	GRA and PRGT	Ex-post	no financial support	6 months to 4 years
	PSI	Policy Support Instrument (2015)	PRGT		no financial support	1-4 years, extendable to 5 years

Source: IMF (2021d,f). Note: PLL replaced the PCL (Precautionary Credit Line) in 2012.

Figure 1.1: Development of IMF Lending Facilities



Source: IMF (2021d,f)

Note:

- (a) SAF (Structural Adjustment Facility) was replaced by ESAF (Enhanced Structural Adjustment Facility) in 1992, which was replaced by PRGF (Poverty Reduction and Growth Facility) in 1999.
- (b) PLL replaced the Precautionary Credit Line (PCL) in 2011.
- (c) Full description of currently available lending facilities is in Table 1.1.

The recent IMF lending reforms took place in around 2009, after the 2008 GFC sparked new demands for IMF financing and as a result of strong pressure to reform. The SBA, which has been IMF's workhorse lending instrument for short-term balance of payment problems in the developed and emerging market countries, was upgraded in 2009 to be more flexible and responsive to those countries' needs. As part of this reform, the Poverty Reduction and Growth Facility (PRGF) was succeeded by three individual facilities - Extended Credit Facility (ECF), Rapid Credit Facility (RCF), and Standby Credit Facility (SCF) - under the

PRGT account to provide better support to low-income countries with different needs. Other newly adopted lending facilities (e.g. FCL, PLL etc.) aim to complement the traditional crisis resolution role of the IMF with more effective tools for crisis prevention.

The various goals of lending facilities are described in [Table 1.1](#). The SBA and ECF are to address short-term or potential balance of payments problems, and their programs usually last for 1-2 years. The EFF and ECF are IMF's main tool for medium-term assistance to countries facing protracted balance of payments problems and with a need for structural reforms. Therefore, their program durations are up to 4-5 years. The Rapid Financing Instrument (RFI) and RCF provide rapid loans disbursement to countries with urgent balance of payments needs, without the need for a full-fledged IMF lending program or ex-post conditionality. They are also IMF's main facilities of emergency financing provided to countries that do not have existing IMF lending arrangement to combat the Covid-19 crises. The IMF has also temporarily doubled the normal annual access of RFI and RCF to meet the increased loan demand during the Covid-19. The Flexible Credit Line (FCL) and Precautionary and Liquidity Line (PLL) are to support the crisis prevention role of the IMF, and have higher requirement of countries' fundamentals and stricter ex-ante conditionality (part of the qualification criteria) compared to other lending facilities. The FCL is for liquidity needs of member countries with very strong fundamentals, policies, and track records of policy implementation. The PLL is for liquidity needs of member countries with sound economic fundamentals and policies while fail to meet the qualification requirements for the FCL.

In the light of varying goals, eligibilities, and conditionalities of IMF lending facilities, it is central to accurately assess member countries' economic conditions (e.g. the magnitude of BOP need, fundamentals) and design appropriate conditionality that's suitable to help countries affected by crises. The approval, continuation, or cancellation decision of

an IMF lending programs are all based upon those quantitative and qualitative assessment. Therefore, an appealing future research avenue is to improve IMF’s lending assessment, more specifically, the forecast and nowcast data.

1.2.3 Conditionality and Program Evaluation

The main component of IMF-supported lending programs is the “conditionality”, which refers to a set of conditions and policies required by the IMF in exchange for financial assistance. A loan program is conditional on the country undertaking the agreed economic adjustment policies. The amount of committed loan under an IMF loan arrangement is usually disbursed in quarterly installments, subject to the observance of policy benchmarks and performance criteria (Barro & Lee, 2005; IMF, 2021b). The loans will be withheld if the conditions are not met. In rare cases, the loan programs are cancelled if majority of the conditions are not met (e.g. Argentina 2003 and 2018 SBA; Croatia 2003 SBA; Armenia 2008 PRGF). The system of conditionality serves to help country overcome the economic issues, ensure the country’s loan repayment to the IMF, and, incorporated recently, to promote national ownership of strong and effective policy (IMF, 2021b).

IMF conditionality takes three different forms (IMF, 2020c, 2021b): (i) The most important and common type is the quantitative performance criteria (QPCs). They are quantitatively measurable conditions that relate to macroeconomic condition under the control of the country government. Examples include monetary and credit aggregates, international reserves, fiscal balances, and external borrowing (e.g. ceilings on government deficits, floors on the stock of net foreign assets of the central bank). (ii) Prior actions, sometimes referred as the “ex-ante” conditionality, are the actions a borrowing country agrees go take before the approval of IMF loan programs, or completing a review. (iii) Structural benchmarks (SBs), are reform measures that often are non-quantifiable, such as improve financial sector

operations, develop a monthly monetary survey in line with international standards, reform of pension funds.

There’s a large debate on the effectiveness of IMF conditionality. The advocates claims that the mandated conditions help mitigate the “moral hazard” issue, which arise when a borrowing country maximize their own utility without bearing the full consequences of their actions (e.g. spend excessively on public investment or transfers). Conditionality would reassure the IMF loans will be used for the purpose defined in the loan arrangement, and the country’s loan repayment to the IMF.

On the other hand, conditionality can be contentious, and is not always favored by the member countries’ policy preference, because crisis countries often are required to adopt unpopular policies or reforms that it would not otherwise have undertaken, in exchange for financial assistance. Some studies find that conditionality may lead to non-compliance with program target if a member country’s policy preferences are not well aligned with the IMF policy prescriptions. This may even lead to a policy retreat from reform once the loan program ends ([Reinhart & Trebesch, 2016](#); [Rickard & Caraway, 2019](#)). [Stiglitz \(2002\)](#) and [Rickard & Caraway \(2019\)](#) criticize the “austerity” advocated by IMF conditionality. [Boughton & Mourmouras \(2004\)](#) find conditionality undermines domestic political institutions, and borrowing countries’ sacrifice of policy autonomy in exchange for loan can leave to public resentment. [Garuda \(2000\)](#) finds evidence of IMF program participation exacerbates income inequality and income deterioration of the poorest group, due to the high level of conditionality required by the long-term loan programs designed for the low-income countries.¹⁵ [Dreher et al. \(2015\)](#) studies the political influences on IMF conditionality, and find that countries with closer political connections to the major IMF shareholders (prox-

¹⁵[Garuda \(2000\)](#) control for difference in countries’ initial economic circumstances, and compares the Gini coefficients and income levels of the poorest quintile of sample countries participated in IMF loan programs, with their nonprogram counterparts.

ied by temporary membership of the UN Security Council) receive about 30 percent fewer conditions than other countries participating in IMF loan programs.

Despite of the considerable criticisms on IMF conditionality, the importance of conditionality in IMF programs continuously increased over time, with the average number of conditions per program steadily rising (Dreher, 2009). As an integral part of the IMF 2009 lending reform, the reform of conditionality took place to modernizing its framework, by eliminate the structural performance criteria¹⁶ to reduce the stigma of Fund lending, and focus on “real-time” assessments of program implementation. This approach should also give members a greater sense of ownership and ultimately improve reform prospects. Ex-ante conditionality (i.e., providing full access upfront based on strong fundamentals and policies and established track record of policy implementation. Such as the PLL program and its predecessor PCL) was introduced to reduce the perception of intrusiveness associated with program implementation.

A large body of quantitative research is devoted to understanding the consequences of IMF-supported lending programs and the conditionality. Specifically, have IMF-supposed programs been effective in achieving their objectives of resolving short-term balance of payment problems (via the GRA programs) or improving macroeconomic position and fostering economic growth (via the PRGT programs)? It is undoubting that IMF lending relaxes the short-run borrowing constrains for crises countries and promotes globalization. A number of studies find evidence that IMF lending program is associated with a lower probability of banking crisis in the future. Papi et al. (2015) also find this effect (in reducing the incidence of banking crises) is significantly stronger in recipient countries with higher compliance rate with the conditionalities, but its impact in affecting the depth and cost of banking crises is

¹⁶One of the advantages of the reform is that with the elimination of structural performance criteria, a member does not have to request a waiver for a criterion that was not met, which often is viewed as particularly problematic by members.

unclear. In contract, several studies criticize IMF programs (and strict conditionality) reduce economic growth([Barro & Lee, 2005](#))¹⁷, exacerbate financial crises and economic inequality around the world, lead to lower level of FDI ([Jensen, 2004](#)), and deteriorate democracy ([Barro & Lee, 2005](#)). [Dreher & Gassebner \(2012\)](#) find that governments face an increasing risk of entering a crisis when they remain under an IMF once the economy’s performance improves.

Internally, IMF have been periodically conducted evaluations of program design and implementation ([IMF, 2012, 2019](#)), with the focus on the program compliance and completion rates as well as examine their underlying driving factors. For example, the percentage of measurable conditions have been met, or the percentage of total loans have been disbursed. As [Khan & Sharma \(2001\)](#) pointed out, this is not a straightforward task. The conditionality agreed by the IMF and programs countries relate to a set of policy (quantitative and structural) variables, which may not always lead to the desired macroeconomic outcomes (e.g improvement of BOP and increased GDP growth). Second, the effectiveness of IMF programs is also subject to the influence of unanticipated exogenous shocks that are not assessed by the conditions (e.g. cost of external debt, terms of trade).

The analytical challenges of assessing the impact of IMF programs can be summarized to three points below: First, the non-random selection of countries participated in the IMF loan programs, will confront the issue of sample selection bias. A proper measure of the impact of IMF programs would also require comparing the effects of the programs to their “counterfactuals” - the macroeconomic outcome would have resulted without the IMF programs. A common solution is the Heckman correction method based on two-stage regression to correct the bias, as in [Heckman \(1979\)](#) and [Reinsberg et al. \(2020\)](#).

¹⁷([Barro & Lee, 2005](#)) estimates show that a higher IMF loan-participation rate reduces economic growth. IMF lending does not have significant effects on investment, inflation, government consumption, and international openness.

Second, control for endogeneity that may arise from un-measurable variables such as political factors. [Dreher et al. \(2008\)](#) find many political factors that shape IMF lending decisions also have influence on its forecasts data. Endogeneity may also arise from simultaneity. For example, IMF conditionality attaches to its loan program has impact on economic conditions, while it is also a response to the economic health of a country. A commonly applied approach to address endogeneity is to construct instrument variable that is uncorrelated with the error term ([Barro & Lee, 2005](#); [Reinsberg et al., 2020](#); [Stubbs et al., 2020](#)).

Third, inaccurate forecasts and nowcasts that analysis is based upon. As mentioned at the end of section 1.2.2, IMF program design and evaluation rely on the assessment of economic conditions established at the time of crisis when a member country seek for financial assistance from the IMF. The rational of program design is based upon the lending programs can potentially help countries ameliorate the initial economic problems. Thus, the forecast and nowcast inaccuracy influence not only program scope and design, but also the apparent magnitude and speed of recoveries ([Park, 2006](#), Chapter 9). Many studies find evidence of optimistic bias in IMF's lending forecasts.

The issues of sample selection bias and endogeneity are of importance to study the impact of IMF loan programs. However, they are not directly related to the key question of this paper: how to further improve the nowcasts for IMF lending, in order to support better decision-making and improve the effectiveness of IMF loan programs. Moreover, the first two challenges have been intensively investigated in the existing literature, while the topic of improving IMF lending forecasts and nowcasts have not been adequately studies. In the next section, I will exploit this topic in detail and identify research agendas related to the nowcasting in the context of IMF lending.

1.3 The Nowcasting for IMF Lending

As discussed in the previous section, nowcasting of economic conditions during the crisis are of key interest for IMF's loan decision-making. The nowcasts data also establish a benchmark where the subsequent program performance evaluations and loan disbursements are based upon. Therefore, IMF's commitment to help member countries combat economic and financial crises will require a reliable and accurate nowcast of the near-term conditions in the member countries. As the Covid-19 pandemic raised the demand for IMF lending, the IMF's ability to devise macroeconomic policy responses to large international shocks has come back into the spotlight.

Nowcasting in economics refers to predicting the present, the very near future, and the very recent past state of economic indicators (Bańbura et al., 2011). IMF crisis lending program designs rely on the assessments of current and immediately expected states of the economy. Such one-off forecasts are best described as “nowcasts” as they have to rely on interim data (in lieu of the arrival of official final data) for short-run projections.

As part of the IMF 2009 lending reform framework, the IMF released the MONA database and disclosed the associated confidential loan documents (the Executive Board Special documents), which made it possible to assess IMF lending activities using the nowcast and forecast data. The data information of the most recent lending programs, however, remain largely confidential for security purpose. The econometric methods that IMF's nowcasts were established upon are also not available, which constrained researchers outside of the IMF in improving the nowcast accuracy. For the general public without the inside information of nowcast models, an interesting research avenue is to focus on evaluate the nowcast accuracy and identify potential divers of nowcast bias and inefficiency. Because macroeconomic forecasting is a topic closely related to the nowcasting, below I will also discuss some literature in IMF forecasts when applicable.

1.3.1 Data Source

The Monitoring of Fund Arrangements (MONA) database and the World Economic Outlook (WEO) database have been the two common data sources in the literature on evaluating IMF forecast accuracy. Each database's forecasting process and design is well suited for the purpose it is intended to serve.

The MONA database tracks the performance of IMF loan programs since 1992, in terms of scheduled purchases and reviews, quantitative and structural performance criteria. It collects forecast data of macroeconomic indicators from IMF desk economists in the regional departments. The database includes three years of historical data that predates the program year, t , the nowcast for t , and up to four additional years of future forecasts, for data established at program approval (initial data) and each subsequent review (revised data) (IMF, 2020c). However, the MONA database presents challenges to researchers as it contains a wide range of errors that limits its values of monitoring IMF loan programs. Therefore, data auditing is necessary for a comprehensive study.

Figure 1.2: MONA Database Structure

(Example of Peru Stand-By Arrangement: Nominal GDP in Bil of NCU, $t=2007$)

MONA Review Sequence	Actual data			Nowcasts/Forecast data				
	t-3	t-2	t-1	t	t+1	t+2	t+3	t+4
Program Approval, Jan 2007	238	262	299	318	337	359	384	411
First Review, June 2007	238	262	305	326	346	368	394	422
Second Review, Dec 2007	238	262	305	335	359	383	409	439
Third Review, July 2008	238	262	305	341	383	414	438	467
Last Review, Jan 2009	238	262	303	336	379	414	452	491

Source: MONA database.

In contrast, the WEO database illustrates the unique multi-country aspects of IMF forecasts produced by the Global Projection Model maintained at the IMF Research Department (in consultation with desk economists). Each WEO data vintage (released in April and Octo-

ber each year, started from 1999) includes long series of historical data and 6-year projection data (including the current year) for all IMF member countries and aggregates of country groups (IMF 2020h). WEO forecasts can differ from MONA's forecast because they are model-based estimates that adopt a number of global assumptions in exchange rates, fiscal policy, monetary policy etc (see details in [Carabenciov et al., 2013](#)). WEO forecasts have also been checked for data consistency among the country forecasts made within the same region ([IMF-IEO, 2014](#)). WEO's historical data can also differ from MONA data because they are revised on a continual basis as more information becomes available, and structural breaks are often adjusted to produce smooth series using different techniques.¹⁸ For this reason, WEO historical data are considered the official final data. They are appropriate for the use of surveillance and capacity development on a regular base, not for directly making the loan program decision.

The assessment of nowcast accuracy requires both the nowcasts data and the final realized data. It is critical to examine the nowcasts of IMF programs produced during the crisis, for the year of the crisis in the MONA database, because they reflect IMF's institutional views on the near-term outlook for the borrowing countries at the height of crises. In addition, it is recommended to employ the most recent version of WEO historical data as the official final data to calculate forecast errors for assessment as in [Timmermann \(2007\)](#), rather than using the preliminary final data available from MONA's subsequent reviews as in [Atoyan & Conway \(2011\)](#); [Luna \(2014\)](#); [Eicher et al. \(2019\)](#).

1.3.2 Evaluate Nowcast Accuracy

A voluminous literature describes a common theme of IMF forecasts being optimistic. [Baqir et al. \(2003\)](#) examine 29 MONA program countries to find positive bias, as did [Baqir et al.](#)

¹⁸See notes and data example in <https://www.imf.org/external/pubs/ft/weo/data/WEOhistorical.xlsx>

(2005) in a sample of 94 MONA countries. [Atoyan & Conway \(2011\)](#) examine fiscal and current account balances for 291 MONA program countries to find positive forecast errors. [Luna \(2014\)](#) examines real GDP growth and inflation forecasts for 103 MONA program countries and finds positive forecast bias for countries with “exceptional access to IMF resources.” Neither paper employs formal tests for bias/efficiency. [Genberg & Martinez \(2014\)](#); [Genberg et al. \(2014\)](#) and [Timmermann \(2007\)](#) find optimistic forecast bias using WEO data. Inaccurate IMF nowcasts would impose more challenges for policymakers to restore economy, lead to large consequences. Overly optimistic IMF nowcasts for its lending programs underestimates the needs for loan and economic adjustment policy ([IMF, 2019](#)) and can induce subsequent economic contractions through higher accumulation of public and private debt ([Beaudry & Willems, 2021](#)). Overly pessimistic nowcasts, on the other hand, overstates financial needs and may lead to misallocation of finance and human capital resources. Thus, accurate nowcasts are of key interest for decision-making of IMF lending.

Forecast evaluations are generally assessed from two dimensions, which are also applied to our nowcast evaluations: (i) Efficiency, which measures if forecasts have incorporated all relevant information available at the time forecasts are established. (ii) Bias, which measures the deviation of the forecast data from the actual data.

One commonly used test of efficiency dates back to [Mincer & Zarnowitz \(1969\)](#), which utilizes a regression of the actual data on a constant and the forecasted data. Efficiency requires that the estimate of the constant is zero and that of the slope coefficient is unity. Another form of efficiency test is based on the work of [Nordhaus \(1987\)](#). He exploits dataset contains more than one forecast of the same event, and indicates that an efficient forecast is that all forecast revisions and errors should be uncorrelated. Put differently, if the forecast revisions were correlated, forecast errors would contain systematic information that could be used to improve the forecast. The first test by [Mincer & Zarnowitz \(1969\)](#) is an appropriate

for the assessment of IMF nowcasts because they established prior to the loan programs approval and is not a time-series data. The second efficiency test by Nordhaus (1987), however, are more useful for the assessment of IMF forecasts after the loan programs establish and the subsequent review data become available.

On the other hand, efficiency is a sufficient but not necessary condition for unbiasedness. So, forecast may or may not be biased if it is inefficient. Holden & Peel (1990) subsequently derive a necessary and sufficient condition for unbiased forecasts,¹⁹ which regresses the forecast errors on a constant term. The forecast is considered to be unbiased if the constant term does not significantly differ from zero. To study the accuracy of solely available IMF nowcast at the time of crisis, Pons (2000); Loungani (2001); Eicher et al. (2019) carry out the traditional Holden & Peel (1990) test for unbiasedness. However, unbiasedness is not informative about forecast accuracy. As Mincer & Zarnowitz (1969) and Genberg & Martinez (2014) point out, unbiasedness only means that positive and negative forecast errors tend to cancel each other out over time and forecast errors can still be large and require room for forecast improvement.

In addition, the degree of biasedness can be measured by several forecast error summary statistics: the mean absolute error (MAE), the root mean square error (RMSE), the mean absolute percentage error (MAPE), and Theil's inequality coefficient (U1 and U2) (Theil, 1961; Makridakis et al., 1997; Hyndman & Athanasopoulos, 2018). Such statistics are informative only when evaluate the relative performance of the two or more sets of forecasts (Wallis, 1989; Pons, 2000; Timmermann, 2007), which does not apply to the case of IMF lending forecasts. Most studies on assessing IMF forecast accuracy have found evidence of

¹⁹Both the tests of efficiency by Mincer & Zarnowitz (1969) and Nordhaus (1987) are a form of test for the weak efficiency, accordingly to the definition in Nordhaus (1987). They do not directly test for the unbiasedness (only efficiency implies unbiasedness), while the Holden-Peel test of unbiasedness is stricter and direct test for unbiasedness (more consistent with the concept of "strong efficiency" that minimizes loss function).

systematic optimistic bias and or inefficiency in certain key macroeconomic indicators, implying a scope to improve IMF forecasts ((e.g., [Artis, 1988](#); [Timmermann, 2007](#); [Aldenhoff, 2007](#), in WEO data;) ([Musso & Phillips, 2002](#); [Atoyan & Conway, 2011](#); [Luna, 2014](#); [Eicher et al., 2019](#); [IMF, 2019](#), in MONA data)). However, findings in the literature are shown to be sample-dependent (differ on the choice of country coverage and or sample period). A detailed list of literature related to the quality of IMF forecasts is available from (see Annex 2 in [Genberg & Martinez, 2014](#)).

In relating to forecast evaluations of the MONA database and in the context of IMF-supported programs countries, [Baqir et al. \(2005\)](#) find an optimistic bias in forecasts of GDP growth and inflation, but no bias in forecasts of the current account, based on a sample of 94 programs between 1989 and 2002. [Luna \(2014\)](#) finds optimistic bias in real GDP growth and inflation forecasts of MONA data for 103 IMF loan programs approved during 2002-2011. The bias, however, was significant only for crises countries with exceptional access to IMF financial resources (approved loan amount exceeded the annual limit of 200 percent of the country's quota at IMF, or exceeded a cumulative limit of 600 percent of quota). [Atoyan & Conway \(2011\)](#) focus on 291 programs approved during 1993-2009. They find bias in IMF's forecasts of three macroeconomic indicators (ratios of the fiscal surplus to GDP, ratio of current account surplus to GDP, and real GDP growth), and identify several drivers of forecast errors (we discuss below). [Eicher et al. \(2019\)](#) conducts the first comprehensive evaluation of IMF nowcasts in times of crisis using the MONA nowcasts data. From the full sample of 110 programs approved during 2002-2016, they find real GDP growth nowcasts to be unbiased but inefficient, the nominal GDP growth nowcasts to be both biased and inefficient, and inflation nowcasts to be unbiased and efficient. They have excluded 492 available programs due to potential large outliers or unbalanced data. This approach, however, is unnecessarily restrictive and inaccurate, because about one-third

of their excluded programs contain the accurate information (verified by the official loan documents) and large outliers are simply a defining characteristic of countries requiring IMF financial assistance. Moreover, [Eicher et al. \(2019\)](#) use the preliminary realized data from MONA's final review as a proxy of the official final data, which introduce substantial data error and contaminate their assessment of nowcast accuracy. Most importantly, they do not provide guidance on why IMF lending nowcasts are inefficient or biased.

The literature on evaluating IMF lending forecasts and nowcasts based on the MONA data has only covered small subsamples - out of the total more than 600 available programs in the current database. As the MONA database contains a wide range of errors, it also requires a comprehensive audit of the data before further analysis. In addition, no existing literature has studied how to further explore and identify the potential drivers of nowcasts bias and inefficiency in the context of IMF lending activities. This implies an appealing research agenda to conduct a comprehensive systematic evaluation of all available IMF loan programs up to date, and to assess the potential drivers of nowcast biasedness and inefficiency.

1.3.3 Drivers of Inaccuracy

The nowcast evaluation for IMF lending data generally constrained by the available data series to compare, since only the IMF has access to country crisis data. Hence, researchers without the confidential country data are not able to compare or integrate different nowcasts of countries in crisis, but instead focus on understanding the drivers of inaccuracy for the only available nowcast data. Although my focus is on the lending nowcasting, the forecast evaluations using WEO data also provide useful insights on the potential drivers of nowcast bias and inefficiency. Therefore, below I summarize seven factors identified in literatures that have impacts on IMF forecast accuracy based on both the MONA and WEO data:

- (i) Data quality. IMF staff frequently cited data shortcomings (limited data availability

and poor quality for both low-income countries and program countries in crisis) as the main reason for low forecast accuracy (Luna, 2014). Atoyan & Conway (2011) also point out that mismeasured data on initial conditions is one of the largest contributors to the forecast errors of IMF programs. Eicher et al. (2019) find the subsample of low-income countries is the main driver of forecast biases and inefficiency in the full sample, perhaps reflecting larger shocks and lower data quality.

(ii) IMF program implementation and the conditionality. Forecasts made for IMF program countries are conditional on the successful implementation of conditionality. The increased delays in implementing adjustment policy, or failing to fulfill the conditions of IMF programs possibly contributed to the optimistic bias of forecasts (Atoyan & Conway, 2011; Luna, 2014; IMF, 2019). Goldsbrough et al. (2002) also find that the performance criteria for the programs of IMF prolonged users who have long cumulative IMF program engagement, often receive optimistic bias, especially for the forecasts of real GDP growth and exports growth.

(iii) Regional and global effect. Genberg & Martinez (2014) find WEO forecasts of major national indicators have tended to be consistently over-optimistic in times of regional and global recessions. Similarly, IMF-IEO (2014) find large forecast errors of GDP growth tend to cluster around regional or global recessions. Timmermann (2007) argues while WEO forecasts of nominal GDP growth do incorporate interdependencies among economies to a substantial degree, they fail to fully account for the spillover effect of U.S. and German GDP growth forecasts to many program countries.

(iv) Geopolitical effect. Idiosyncratic factors do play a role in IMF growth forecast errors of program countries, including political transitions, conflicts, and natural disasters (IMF, 2019). Aldenhoff (2007) shows that IMF forecasts are frequently distorted by political bias and find the optimistic bias of industrial countries' long-term growth forecasts significantly

correlated with election dates in the U.S., which he refers as the “political forecasting cycle”.

(v) Forecast horizon. Forecast accuracy in general is expected to decrease as forecast horizons increase ([Armstrong, 2001](#)). [Dreher et al. \(2008\)](#) think this is because the longer time horizon the greater the room for discretionary forecasting. [Pons \(2000\)](#) find the forecast errors increase when the forecast of GDP growth rates is made with a longer horizon (e.g. change from current year forecast to year-ahead forecast). Findings in [Timmermann \(2007\)](#) for the optimistic bias of GDP growth and pessimistic bias of inflation, as well as findings in [Genberg & Martinez \(2014\)](#) for the optimistic bias of GDP growth reinforces this conclusion.

(vi) Institutional reasons. Growth optimism was intrinsic to IMF program design in most cases ([IMF-IEO, 2014](#)). [Dreher et al. \(2008\)](#) find many political factors that influence IMF lending decisions also apply to its forecasting. They conclude that major IMF shareholders who possess large voting powers, and their allies tend to receive more optimistic forecasts. Moreover, [Genberg & Martinez \(2014\)](#) find the experience of IMF desk economist matters for forecast accuracy: desk economists with longer experience make smaller forecast errors, especially in the case of low-income countries.

(vii) Model specification. [Atoyan & Conway \(2011\)](#) and [Aldenhoff \(2007\)](#) call for a better model specification to account for the country-specific effect, because it is a substantial contributor to WEO forecast errors. There is a growing strand of literature on macroeconomic nowcasting aims to improve nowcast accuracy of macroeconomic variables by utilizing the available high-frequency indicators from different sectors (see [Bańbura et al., 2011](#); [Marini, 2016](#)).

1.4 Conclusions

As part of the IMF 2009 lending reform framework, the IMF released the Monitoring of Fund Arrangements (MONA) database and disclosed the associated confidential loan documents.

This made the IMF lending decision more transparent to the public, and provided valuable forecasts and nowcasts data for researchers to quantitatively evaluate IMF lending.

This paper attempts to draw lessons to design IMF lending programs better and to improve their effectiveness. I first conduct a comprehensive review of the recent development of IMF lending, including an early wave of literature on examining the determinants of IMF lending programs, and the second wave of literature on assessing the impact of IMF lending programs (and the attached conditionality).

Based on the comprehensive review and the observed increased reliance of IMF decision-making on the nowcasts data, I identify the nowcasting of IMF lending activities as an important topic for future research that would be particularly useful to aid in implementing IMF lending programs. First, while the existing literature has only covered small subsamples out of the total more than 600 available programs in the current MONA database, and given the fact that the MONA database contains a wide range of errors, a comprehensive audit and evaluation of all available IMF loan programs data up to date is needed for future research. Second, studies on assessing IMF forecasts and nowcasts accuracy have found robust evidence of systematic bias and or inefficiency in key macroeconomic indicators, raising the question as to the drivers of inefficiency. Lastly, I summarize seven factors identified in the literature that impact IMF forecast accuracy, which also applies to the nowcast accuracy for IMF lending. The seven factors include: (i) data quality, (ii) IMF conditionality, (iii) regional and global effect, (iv) geopolitical effect, (v) forecast horizon, (vi) institutional reasons, (vii) model specification. Incorporating the above factors in the future nowcasting for IMF lending will further improve IMF nowcasts efficiency to support better decision-making of loan programs, and ultimately help crisis-affected countries restore economic growth.

Chapter 2

THE ACCURACY OF IMF CRISES NOWCASTS ¹

Most forecasts are wrong, but some are useful ²

2.1 Introduction

The core mission of the International Monetary Fund (IMF) is to ensure the stability of the global economy through surveillance of member economies and lending to crisis countries (IMF, 2021h). As global lender of last resort, IMF crisis loan programs stipulate policy conditions and quantitative economic targets which must be implemented before loan tranches are disbursed (Fischer, 1999). IMF program design and conditionality can be contentious, since the fundamentals of countries' crises often require substantial and often unpopular policy changes to ameliorate the economic conditions that IMF nowcasts establish at the time of crisis.³ IMF nowcasts thus constitute a unique dataset to evaluate the accuracy of the assumptions that constitute the basis for IMF program types, conditions, and loan sizes.⁴ Another important dimension of nowcast accuracy are country program performance evaluations and loan tranche disbursements, which depend on benchmarks established on

¹This chapter is a joint work with Theo Eicher from the University of Washington.

²Variant of George Box's (1976) statistical aphorism.

³See e.g., Feldstein (1998), Stiglitz (2002), who all attribute slow crisis recoveries to IMF conditionality. Dreher et al. (2015)) find that temporary members of the UN Security Council (countries with closer connections to the major IMF shareholders) receive about 30 percent fewer conditions than other countries participating in IMF loan programs.

⁴IMF crisis program designs rely on assessments of the current states of the economy. Such one-off forecasts are best described as "nowcasts" as they have to be based on interim data in lieu of the arrival of regular data vintages, see Bańbura et al. (2011). Some IMF programs include up to 4-year forecasts, but in this paper we examine only nowcasts produced during the crisis year, for the crisis year.

the basis of the original nowcasts. Thus nowcast bias and/or inefficiency influence not only program scope and design, but also the apparent magnitude and speed of recoveries (Park, 2006, Chapter 9).

According to Nordhaus (1987), nowcasts are unbiased and efficient when they incorporate all relevant information available at the time that nowcasts are established. There have been a number of evaluations of IMF forecasts, IMF conditionality, and IMF program efficacy, but previous evaluations focus largely on official final data from the IMF’s World Economic Outlook (WEO) database, not actual crisis nowcast data.⁵ The dataset we use in this paper predates any WEO entries and representing unique, long confidential snapshots of the IMF’s understanding of economies at the pinnacles of crises.

Eicher et al. (2019, EKPC from here forward) also formally examined the accuracy of IMF nowcasts using actual crisis data, not the WEO data. They did not explore the possible sources of bias and inefficiencies, as we do below. EKPC also examined only 110 out of 602 available programs and excluded a large share of observations due to concerns for outliers and database errors. We audit the IMF program database, and verify that about one-third of EKPC’s excluded programs should have been included. Large outliers are simply a defining characteristic of countries requiring IMF crisis assistance. Our audit of the IMF’s Monitoring of Fund Arrangements (MONA) database also allowed us to include an additional 492 programs and an additional 14 years of nowcast data.⁶ To evaluate nowcasts, EKPC used estimates of final data, not official final data which differ substantially (see Figure 2.1). Only 57% of the data lie within 1% of each other, hence EKPC’s nowcast assessments and

⁵See among others: Artis (1988), Artis (1997); Barrionuevo (1993); Beach & Schavey (1999); Loungani (2001); Batchelor (2001); Pons (2000); Aldenhoff (2007); Timmermann (2007). Similar to Batchelor, Beach et al. find IMF WEO forecasts for developing countries overestimate real GDP growth and underestimate inflation, while forecasts for industrialized nations are unbiased and efficient. Artis and Loungani compare IMF forecasts to consensus forecasts and find no substantial differences. Neither of these papers focus on countries in crises or forecasts in times of crises.

⁶Appendix A1 documents our audit of the database, detailing 11 different types of errors and corrections.

results are contaminated by final data errors. Most important, however, is our exploration of the sources of bias and inefficiency in IMF nowcasts, focusing on information IMF economists possessed at the time of the nowcasts.

We introduce several groups of covariates to examine whether their effects were properly accounted for by IMF nowcasts, or whether they were sources of IMF nowcasts' inefficiencies. These regressors relate to (i) international crises, (ii) geographic regions, (iii) conditionality (quantitative and structural performance criteria), (iv) program objectives, (v) loan amount and loan cancellation, (vi) elections, (vii) conflicts, and (viii) natural disasters. Inflation, nominal, and real GDP growth are shown to be associated with different types of conditionality and program aspects that were not properly integrated into IMF nowcasts. For inflation, conditionality effects of ceilings on government deficits and debt were underestimated in IMF nowcasts. For real GDP, ceilings on government credit were overestimated. Nominal GDP features the largest number of conditionality effects improperly integrated into nowcasts, namely ceilings on external debt/arrears, ceilings on government deficit/debt, and reforms of the current and capital accounts.

EKPC found inflation nowcasts to be unbiased and efficient. Using the additionally available programs and official (not estimated) final data that our audit produced, we find IMF inflation nowcasts are actually biased and inefficient. EKPC also found nominal GDP growth nowcasts unbiased and efficient, but our expanded dataset shows both nominal and real GDP growth nowcasts to be inefficient, systematically overestimating low-growth crisis recoveries and underestimating high-growth recoveries. [Luna \(2014\)](#) previously studied and even smaller sample of 103 program countries and found that optimism in IMF crisis forecasts was driven by countries with large loans, but our results in a substantially larger and longer sample do not confirm that loan size affects bias or inefficiency. [Luna \(2014\)](#) also suggested that optimism in IMF forecasts may reflect inadequate program execution on the part of

the countries, but we find no evidence that cancelled programs affect nowcast efficiency.⁷ It is generally expected that forecasts improve as time horizons shorten and IMF-IEO (2014) established the same for WEO forecasts. In contrast, we find that this is not the case for IMF crisis countries nowcasts, which are equally likely to be inefficient and/or biased early or late in the program year for which the nowcast was established. However, exploiting our 28-year time series, we can document that recent GDP growth nowcast accuracy has improved dramatically, while inflation nowcasts continue to struggle with inefficiency until recently.

A voluminous literature describes a common theme that IMF forecasts are thought to be optimistic.⁸ For crisis countries, we show that a focus on average forecast errors, without formal tests, in small samples, provides misleading results. Our approach is novel in that our large sample allows formal tests that decompose the perceived optimism to lay bare the structure of the nowcast inefficiency. The particular manner in which optimistic and pessimistic forecasts average out for high and low-growth crisis recoveries is shown to contain important information. We show that IMF optimism is only a feature for low-growth recoveries in LICs while fast recovering program countries actually suffer from excessively pessimistic IMF nowcasts. Once LICs are purged from the sample, we produce the important result that the remaining share of nearly 250 nowcasts are actually unbiased and efficient.

⁷Luna (2014) also suggests that the IMF nowcasts may be overly optimistic because country authorities provided faulty data to the IMF; we could not find data to test this hypothesis. Another approach to explaining optimistic IMF forecasts bias are based on institutional factors that we also could not explore for lack of data. Genberg & Martinez (2014) find that the IMF desk economists with more years of experience make smaller forecast errors. Beaudry & Willems (2021) find IMF mission chiefs' optimism systematically influences IMF forecasts.

⁸For MONA, Baqir et al. (2003) examines 29 program countries to find positive bias, as did Baqir et al. (2005) in a sample of 94. Atoyan & Conway (2011) examine fiscal and current account balances for 291 program countries and find positive forecast errors. Luna (2014) examines real GDP growth and inflation forecasts for 103 program countries and finds positive forecast bias for countries with "exceptional access to IMF resources". Neither paper employs formal tests for bias or efficiency. Genberg & Martinez (2014) and Timmermann (2007) find optimistic bias in WEO.

Overly optimistic nowcasts for low-growth recoveries may lead to an underestimate of the required financial support and quantitative/reform adjustments. Also, overly optimistic nowcasts for slow-growth recoveries may translate into overly optimistic program targets that are too difficult to reach. LICs’ nowcasts are shown to exhibit the greatest inefficiencies and by far the most optimistic nowcasts for low-growth recoveries. Most unsettling, [Beaudry & Willems \(2021\)](#) show that optimistic bias in IMF GDP growth forecasts can induce subsequent economic contractions through higher accumulation of public and private debt. As the Covid-19 pandemic raises the demand for IMF programs, improved nowcasts and an understanding which areas have produced nowcast inaccuracies are thus more important than ever.

The remainder of the paper is organized as follows. Section 2.2 lays out the methodology, discusses the data, and provides first results. Section 2.3 establishes nowcast bias and/or inefficiency across relevant subsamples. Section 2.4 examines why nowcasts have been inefficient and/or biased, and Section 2.5 investigates if nowcast efficiency changed over time. Section 2.6 checks if time horizons affected nowcast accuracy and Section 2.7 concludes.

2.2 Evaluating Nowcasts: Data and Methodology

2.2.1 Data

Our nowcast data originates with the IMF’s Monitoring of Fund Arrangements (MONA) database ([IMF, 2020c](#)). The database reports the data that the IMF establishes at the time of crisis for 602 programs ranging from 1992 to today. Each program is identified by program-type⁹, approval date, and loan size. The database also reports macroeconomic in-

⁹[IMF \(2021f\)](#) provides a full description of each of the 13 program-types which include Extended Credit Facility (ECF), Extended Fund Facility (EFF), Enhanced Structural Adjustment Facility (ESAF), Exogenous Shock Facility (ESF), Flexible Credit Line (FCL), Structural Adjustment Facility (SAF), Stand-By Agreements (SBA), Standby Credit Facility (SCF), Policy Coordination Instrument (PCI), Precautionary Credit Line (PCL), Precautionary Liquidity Line (PLL), Poverty Reduction and Growth Trust (PRGT),

dicators (in a highly unbalanced manner) for each program country, including three years of historical data that predates the crisis, t , the nowcast for t , and four additional years of forecasts. MONA also reports program conditionality, which the IMF groups into 9 categories of “quantitative” and 11 categories of “structural” (qualitative) performance criteria.¹⁰ Quantitative performance criteria reference numeric benchmarks (e.g., fiscal deficit targets) and structural performance criteria refer to policy and institutional reforms (e.g., income tax reform) that must be implemented by the program. We examine the MONA database’s nowcasts established in crisis year t for program year t . Since IMF projections are based on growth rates, we examine nowcasts for real/nominal GDP growth and inflation from $t-1$ to t . The MONA database also associates each program with Executive Board Documents which have become available in the IMF Archives since 2009. We review these documents in our MONA audit; the list of errors proved extensive (see [Appendix A](#)).

After each program starts, IMF economists review a country’s progress and enter estimates of realized data on a monthly, quarterly or semi-annual basis, depending on the program. EKPC used these subsequent reviews to obtain estimates of final outcome data for t . They accepted an estimate as final data if the review took place 18 months after a program commenced. There are four reasons why this approach is unnecessarily restrictive and inaccurate. First, 48 programs do not have estimates of final data in MONA and were thus excluded by EKPC analysis although official final data exists. Second, the EKPC approach constrained the analysis to programs that lasted at least 18 months, omitting another 14 programs. Third, EKPC excluded another 106 programs due to unbalanced data or due to suspected database errors when observations exceeded four standard deviations from the mean. Fourth, MONA’s estimates of final data can differ substantially from the IMF’s official

Policy Reform Instrument (PSI).

¹⁰See [Table 2.1](#) for conditionality categories. Also see [IMF \(2020a\)](#) MONA database glossary.

final data, which is reported in the IMF’s World Economic Outlook (WEO) database (IMF, 2020d). Figure 2.1 provides a visual representation of the differences between MONA’s estimated final data and WEO’s official final data for real GDP growth – only 57% of the data lie within 1% of each other. Differences between MONA’s estimated final data and WEO’s official final data thus contaminate EKPC’s assessment of nowcast accuracy, as errors in MONA’s estimated final data are attributed to nowcast inaccuracy. Below we evaluate IMF nowcast accuracy only on the basis of official final data obtained from the IMF’s WEO.

2.2.2 A Methodology to Evaluate Nowcast Accuracy

Forecast evaluation exercises generally focus on a range of forecast-error summary statistics that provide performance comparisons for different forecasts, for example, the mean absolute error (MAE) and the root mean square error (RMSE).¹¹ Such statistics are informative only when two or more forecasts are being compared. The forecast evaluation in this paper involves, however, only a single forecast, since the IMF is the sole institution with access to country crisis data; this data remained confidential until 2009 when IMF Archives opened to the public. Hence we cannot hope to compare different nowcasts; we can only examine the accuracy of the solely available nowcast available at the time of crisis. The importance of these IMF nowcasts is thus derived directly from the fact that IMF nowcasters were in a unique position, enjoying exclusive access to confidential country data at the times of crises in order to design the loan programs.

The literature on forecast accuracy evaluation dates back to Mincer & Zarnowitz (1969), who based their analysis on the seminal work of Theil (1961). Theil introduced the concept of a “Prediction-Realization Diagram” as seen in Figure 2.2, which displays IMF nowcasts, F_t , on the horizontal axis. These IMF nowcasts are established at the time of crisis for the

¹¹A review of these statistics is provided by Hyndman & Athanasopoulos (2018)

year in which the program was approved, t , for real/nominal GDP growth and inflation. Official final data, A_t , are plotted on the vertical axis. Mincer-Zarnowitz label the solid 45-degree line the “Line of Perfect Nowcasts” as it represents coordinates where nowcasts equal official final data, $F_t = A_t$. For real GDP growth, the diagram indicates that IMF nowcasts overestimate low-growth recoveries and underestimate high-growth recoveries. A similar pattern exists for nominal GDP growth, but it is obscured by influential observations, which we further examine below. The white shaded area around the dashed regression line represents the 95% confidence interval.

Formally we can test whether nowcasts are unbiased and efficient using Mincer-Zarnowitz regressions, a methodology that has frequently been applied see e.g. [Romer & Romer \(2000\)](#), [Rossi & Sekhposyan \(2011\)](#), or [Granger & Newbold \(2014\)](#):

$$A_t = \alpha + \beta F_t + \varepsilon_t \quad (2.1)$$

where F_t are nowcasts and A_t are official final data. Nowcasts are chosen as the “independent” variable only because they are available before the official final data is published. A forecast is efficient when the forecast error, $A_t - F_t$, is uncorrelated with the forecast. In this case, the slope parameter, β , in [Equation 2.1](#) is unity and the residual variance of the regression equals the variance of the forecast error. To test the accuracy of forecasts, Mincer-Zarnowitz suggest the joint null hypothesis $\alpha = 0$ & $\beta = 1$ to see if forecasts are optimal in that they are unbiased and efficient. Since the estimates of α and β are generally correlated, the individual T statistics provide inappropriate tests and the joint test is required ([Wallis, 1989](#)).

If the Mincer-Zarnowitz null ($\alpha = 0$ & $\beta = 1$) is rejected, nowcasts are inefficient but they may or may not be biased. [Holden & Peel \(1990\)](#) subsequently derived a necessary and

sufficient condition for unbiased nowcasts, which simply tests whether the regression line intersects the Line of Perfect Nowcasts at their respective expected values $E(A_t) = E(F_t)$. When [Holden & Peel \(1990\)](#) test of $A_t - F_t = \gamma + v_t$ rejects the null of $\gamma = 0$, the nowcast is said to be biased. Here it is important to note that this notion of bias implies that nowcasts exhibit no bias at all when they are half of the time 20% higher and half of the time 20% lower than the official final data. This is why Mincer-Zarnowitz stress that unbiasedness is desirable, but not by itself informative about forecast accuracy. Of course, other things being equal, the smaller the bias, the greater the accuracy of the forecast; “other things” here being the distances between the points in the prediction–realization diagram and the Line of Perfect Nowcasts. These distances can be expressed by the variance of the forecast error around its mean, which Mincer-Zarnowitz introduce as an inverse measure of forecast efficiency. Rotations of regression lines to better match Lines of Perfect Forecasts reduce this variance to increase efficiency.

A nowcast is inefficient when it does not incorporate all information that is available at the time the nowcast is established. That is why [Nordhaus \(1987\)](#) noted that the concept of forecast efficiency shares similarity with the concept of stock market efficiency – both imply that efficiency exists when all relevant and available information was considered. Even if the Holden-Peel test indicates unbiased forecasts, the slope coefficient, β , in [Equation 2.1](#) may indicate a statistically significant deviation from unity to suggest inefficiency.

Regressions (2a)-(2c) in [Table 2.2](#) are Mincer-Zarnowitz regressions associated with [Figure 2.2](#) for real/nominal GDP growth and inflation. In regressions (1a)-(1c) we revisit the results in EKPC that were based on their sample of 110 of the available 602 programs. EKPC rejected efficiency but not, on average, bias.¹² In regressions (2a)-(2c) we add the programs

¹²In contrast to EKPC we do not include regional or crisis dummies in the benchmark; the inclusion of such dummies invalidates the Mincer-Zarnowitz null hypothesis. The question of whether IMF nowcasts do not properly integrate regional or global crisis information in is examined in [section 2.4](#).

EKPC omitted, include programs for the additionally available years (1992-2001), and use official, not estimated, final data as in EKPC.

Real GDP growth nowcasts are inefficient but unbiased in EKPC and in our full sample. Slope coefficients are significantly below unity, suggesting low (high) growth recoveries are over (under) estimated. Nominal GDP growth is unbiased and efficient in EKPC, but the inclusion of our additional data eliminates efficiency. We find a much smaller slope coefficient in the Mincer-Zarnowitz regression that is statistically significant deviating from unity. The intercept is also significantly larger than zero, suggesting that IMF nominal GDP growth nowcasts over (under) estimate low (high) growth recoveries, as was the case for real GDP growth. Inflation shows the greatest divergence in results. EKPC found near perfect, unbiased and efficient IMF inflation nowcasts with near unitary slope and zero intercept, suggesting that nowcasts closely match the Line of Perfect Nowcasts. When we include the previously omitted programs, as well as official (not estimated) final data, we find that IMF inflation nowcasts exhibit statistically significant bias and inefficiency. As was the case for GDP growth, IMF nowcasts for inflation over (under) estimate low (high) growth recoveries.

The fact that low GDP growth crisis recoveries are overly optimistically nowcast in the full sample presents major problems for IMF crisis program countries. First, overly optimistic nowcasts for low-growth recoveries may imply an underestimate of the loan requirements and of the quantitative and structural adjustment needs. On the other hand, overly pessimistic nowcasts for the high-growth recoveries overstate programs' financial needs, which may lead to misallocation of resources. Second, overly optimistic nowcasts can translate into overly optimistic program targets and performance criteria (e.g., government revenues or import volumes) that may actually be more difficult or impossible to reach. Third, overly optimistic nowcasts for low-growth recoveries affect program evaluations, as below-par program performance may, in fact, only be due to excessively optimistic nowcasts.

2.3 Does Nowcast Bias and Efficiency Vary by Subsample?

Regressions (2b)-(2c) in [Table 2.2](#) suggest that the assessment of IMF nowcast accuracy may be impacted by influential observations, likely relating to nowcast inaccuracies in high inflation countries. Below we examine whether IMF nowcast accuracy differs by subsamples, specifically subsamples that differ by income and inflation. We use the common World Bank Low-Income Country (LIC) demarcation¹³, and for inflation we use [Dornbusch & Fischer \(1986\)](#) threshold of inflation $> 25\%$ to identify hyperinflation crises. Note that our hyperinflation sample is best described as a sample of anticipated hyperinflation countries, since we code countries for hyperinflation only if IMF nowcasts forecasted hyperinflation. In that sense, hyperinflation was not a surprise to IMF forecasters, they already predicted it in their nowcasts. Hence we evaluate only how accurate IMF nowcasts are for countries and crises for which the IMF expected hyperinflation.¹⁴

Separating the samples by hyperinflation and LICs, we obtain the subsample prediction-realization diagrams in [Figure 2.3](#), suggesting a substantially improved fit and nowcasts for the Non-Hyperinflation & Non-LICs subsample as compared to (2a)-(2c) in [Figure 2.2](#). [Table 2.3](#) reports Mincer-Zarnowitz regressions for the subsamples. For real GDP growth, nowcast accuracy differs substantially across subsamples. The full sample, the Non-Hyperinflation sample, and the LICs sample are all inefficient. Efficiency arises only once we purge the full sample of LICs and hyperinflation nowcasts. This implies that the inefficiency in the full sample is driven by nowcasts for hyperinflation countries and LICs.

The slope coefficients are below unity for all subsamples, indicating overly optimistic

¹³Low-Income Country classification is a time-varying measure based on World Bank's data of GNI per capita in each year. See databank.worldbank.org/data/download/site-content/OGHIST.xls

¹⁴If hyperinflations or even just the events leading up to hyperinflations were a surprise to IMF forecasters (and hence not part of their information set) we would not want to link such surprises to IMF nowcast bias and inefficiency.

nowcasts for low real GDP growth recoveries and overly pessimistic estimates for high-growth recoveries. This effect is greatest for Non-Hyperinflation LICs with an astonishing low β of 0.6, indicating enormous improvement potential for IMF nowcasts for this subsample of 276 programs. By calculating the intersection between the regression line in [Equation 2.1](#) and the Line of Perfect Nowcasts, we find that fragile Non-Hyperinflation LICs, those with below 5% real GDP growth recoveries, may have been impacted by conditionality and performance evaluations that were based on excessively optimistic IMF crisis assessments.

Surprisingly, IMF real GDP growth nowcasts for hyperinflation countries are not statistically significantly biased or inefficient. This is likely an artifact of the high variance in the official final data for hyperinflation countries (see [Table 2.3](#)), which is twice the magnitude of other subsamples variances. High variance outcomes are harder to forecast, resulting in substantially larger forecast errors, as reported in [Table 2.3](#). The large standard errors in the hyperinflation subsample then widen the confidence bands to the point where the Mincer-Zarnowitz F-test cannot rule out that intercept and slope coefficients are consistent with the Line of Perfect Nowcasts (see [Figure 2.3.3a](#)). It is important, however, to note that the vast majority (55%) of hyperinflation events occurred in the very early part of the sample, before 1997. In that sense, the noise introduced by hyperinflation forecast errors is not representative of ongoing nowcast dynamics over the years in the full sample. This is in contrast to the effects of LICs nowcast errors whose contribution to the full sample inefficiency is steady over the 28-year time period.

For nominal GDP growth in hyperinflation countries, we find a pattern similar to what we observed for real GDP growth. In the full sample the slope coefficient is substantially below unity, and [Table 2.3](#) shows that this result is driven entirely by nowcast inaccuracies for hyperinflation countries. The slope coefficient of hyperinflation nowcasts is substantially below unity at 0.666. This indicates again that IMF nowcasts substantially overestimate

nominal GDP growth in “low hyperinflation” recoveries (smaller than 18.6%) and underestimate nominal GDP growth in “high hyperinflation” recoveries. Once we purge the full sample of hyperinflation countries, we find slope coefficients near unity across subsamples (ranging from 0.987 to 1.052), indicating efficient nowcasts.

Interestingly, both the Non-Hyperinflation and the Non-Hyperinflation LICs subsamples exhibit biased nominal GDP growth nowcasts. This is because the positive intercept together with high slope coefficients imply that biased overestimates and biased underestimates cannot average out. Here it is important to note that the bias is driven entirely by LICs. Once LICs are removed from the Non-Hyperinflation sample, the slope is just about unity (1.05) and nowcasts are unbiased and efficient. In summary, for nominal GDP growth we find that the full sample must be exclude (i) hyperinflation countries to achieve efficiency, and (ii) LICs to achieve unbiased nowcasts.

Inflation nowcasts produce the largest accuracy variations across subsamples. Biased and inefficient nowcasts are observed for the full sample, the Non-Hyperinflation sample and the Non-Hyperinflation LICs samples. As in the case of nominal and real GDP growth, once we purge the full sample of Hyperinflation and LICs programs, nowcasts become efficient and unbiased. Slope coefficients are high and exceed unity for Non-Hyperinflation samples (LICs or Non-LICs), indicating that, for Non-Hyperinflation countries, IMF nowcasts underpredict inflation for low inflation recoveries (smaller than 1.7%) and over predict inflation for high inflation events. Only the Hyperinflation subsample exhibits a slope coefficient that is below unity, indicating that IMF nowcasts decisively overestimate “low hyperinflations” and underestimate “high hyperinflations.”

The important finding of the subsample analysis is that we can document Non-Hyperinflation Non-LICs programs (about 42% of the sample) exhibit unbiased and efficient nowcasts for real and nominal GDP growth and for inflation. Hyperinflation countries contribute to bias

and inefficiency, but given that their numbers are concentrated in a few early years in the 28-year sample, the real driver of bias and inefficiency in the full sample are thus Non-Hyperinflation LICs. The analysis raises the question as to the drivers of inefficiency and/or bias, which we explore in the next section.

2.4 Why Are IMF Nowcasts Inefficient?

[Sinclair, Joutz, & Stekler \(2010, SJS\)](#) and [Sinclair, Stelder, & Caraow \(2012\)](#) propose a methodology to investigate potential sources of forecast inefficiencies. They suggest including additional covariates in [Equation 2.1](#) that represent information which was available to forecasters at the time of the forecast.

$$A_t = \alpha + \beta F_t + \delta X_t + \varepsilon_t \quad (2.2)$$

where X_t are additional candidate covariates known to forecasters at time, t . SJS propose the joint null hypothesis of $\beta = 1$ & $\alpha = \delta = 0$ to identify whether the information content of the additional covariates is properly included in the nowcast. If the null is rejected, SJS note that the information contained in X was not fully integrated into the nowcast, identifying a possible source of inefficiency.

We examine eight groups of candidate covariates to test whether they were properly accounted for in IMF nowcasts, specifically, (i) international crises, (ii) regions, (iii) conditionality (quantitative and structural performance criteria), (iv) program objectives, (v) loan amount and loan cancellation, (vi) elections, (vii) conflicts, and (viii) natural disasters to see if nowcasts properly account for their effects.

Two international crises occur in the time period of our data: the 1997 Asian Crisis and the 2008 Global Financial Crisis. The effects of these international crises were not country-specific, and one might suspect that international contagion and crisis spillovers may not

have been fully integrated into IMF nowcasts for individual program countries. [Genberg & Martinez \(2014\)](#) and [IMF-IEO \(2014\)](#) find that IMF WEO forecasts tend to be consistently over-optimistic with larger forecast errors in times of regional and global recessions, and the Asian and Global crises represent two such cases. Here it is important to emphasize that we are examining only sources of inefficiencies based on information available to IMF forecasters at the time of the nowcast. Hence our crisis dummies are set to one only for countries whose programs started after the two international crises commenced.¹⁵ Regional effects proxied by Africa, Asia, and Latin American dummies, which commonly hold explanatory power in growth regressions (see, e.g., [Barro \(1991\)](#) and [Masanjala & Papageorgiou \(2008\)](#) for Africa, and [Fernandez et al. \(2001\)](#) for Asia and Latin America).

Another set of regressors we investigate as possible sources of inefficiencies relates to IMF conditionality. Conditionalities are specifically designed to affect program countries' recoveries, hence nowcasts must exercise particular caution to integrate their effects. For example, conditionality often relates to fiscal discipline and credit targets that directly affect GDP growth and/or inflation. Conditionality comes in two flavors: "quantitative performance criteria" (QPC, e.g., "dollar ceiling on external debt") and "structural performance criteria" (SPC) which refer to policy reforms. The IMF MONA database groups conditionalities into 9 categories of QPCs and 11 categories of SPCs that we add to regressions in [Table 2.4](#).¹⁶

The IMF offers countries a menu of program-types to focus recoveries on different aspects of an economy. Program types address a wide range of issues from external financing difficulties to non-financial reforms, and poverty/growth.¹⁷ We examine if the differential

¹⁵The 1997 Asian Crisis dummy received a "1" for programs that (i) commenced between 7/2/1997 and 1/1/1999, and (ii) that were identified as affected Asian Crisis Countries by [Kaminsky et al. \(2003\)](#). In July 1997, Thailand was forced to float its exchange rate, which is generally seen as the start of the crisis. The 2008 Global Financial Crisis dummy received a "1" for programs that commenced between 9/15/2008 and 9/15/2009, where the start date is the Lehman Brothers' bankruptcy filing date.

¹⁶See [Table 2.1](#) for details.

¹⁷We grouped the 13 IMF programs types into 5 program objectives:

focuses of program types have been fully accounted for by IMF nowcasts. In addition, the previous literature on IMF program performance also noted the importance of program-loan size (e.g., [Dreher \(2006\)](#)). [Luna \(2014\)](#) also notes that exceptional access to fund resources imposed particular optimistic bias. Hence, we also examine whether the approved loan size relative to IMF quota¹⁸ is fully accounted for in IMF nowcasts.¹⁹ [Atoyan & Conway \(2011\)](#), [Luna \(2014\)](#), and [IMF \(2019\)](#) all point out that all IMF program forecasts are conditional on the assumption of successful implementation of quantitative and structural reform targets so that failures to implement could contribute to optimistic bias of forecasts. We examine ex-post effects of cancelled programs on nowcast bias and efficiency and found they did not affect results.²⁰

Finally, we examine a block of non-economic regressors that may well exert profound effects on the economy, including elections, conflicts (civil and international), and natural disasters. In its review of program design and conditionality ([IMF, 2019](#)) noted that forecast errors of program countries are impacted by political transitions, conflicts, and natural disasters. Again we are asking if knowledge of such non-economic factors was properly integrated into the nowcasts. The noneconomic dummies exhibit a one only if the event occurred before the program was approved, so that IMF nowcasters were well aware of the events and their effects on GDP growth and inflation.²¹

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- (i) BOP Stabilization: Stand-By Agreements (SBA);
 - (ii) BOP Shocks (precautionary): Exogenous Shock Facility (ESF), Standby Credit Facility (SCF), Flexible Credit Line (FCL), Precautionary Credit Line (PCL), Precautionary Liquidity Line (PLL);
 - (iii) Structural Adjustment Poverty & Reduction and Growth: Structural Adjustment Facility (SAF), Enhanced Structural Adjustment Facility (ESAF), Poverty Reduction and Growth Trust (PRGT);
 - (iv) Non-Financial Reforms: Policy Reform Instrument (PSI), Policy Coordination Instrument (PCI);
 - (v) Long-Term BOP Reforms: Extended Credit Facility (ECF), Extended Fund Facility (EFF).

¹⁸The quota data is from the IMF International Financial Statistics database ([IMF, 2020b](#)).

¹⁹The result is similar if replace the approved loan size with actually drawn loan size.

²⁰Results can be obtained from the authors upon request.

²¹[Table 2.1](#) provides elections, conflicts, and disasters details. Election dummy covers (i) head of state/government elections and (ii) legislative elections. Programs received a “1” if elections occurred

2.4.1 Sources of Nowcast Inefficiency in the Full Sample

[Table 2.4](#) regressions (4.1.a)-(4.3.c) display GDP growth and inflation results for the three key subsamples. We start by discussing the full sample results. For real GDP growth (regression 4.1.a) the null hypothesis that nowcasts could not have been improved by the consideration of additional variables (SJS F-test) is rejected at the 10% level. However, only four covariates are so marginally significant that we don't list them here, reflecting that the initial nowcast was unbiased and efficient.

In contrast, for nominal GDP growth in the full sample, the SJS F-test is rejected at the 1% level (regression 4.2.a), indicating that IMF nowcasts could have been improved if the effects of regions (America) and the effects of the 2008 financial crisis had been properly considered. In addition, there is strong evidence that several conditionality dimensions were also not properly integrated into IMF nominal GDP nowcasts in the full sample. These dimensions include quantitative performance criteria relating to a) reserves, b) external arrears, c) fiscal deficit, d) external debt, and structural performance criteria relating to current/capital account restriction. In addition, nominal GDP growth nowcasts could have been improved through better consideration of the implications of program types that address balance of payments stabilization problems (SBA programs) and poverty reduction & growth (ESAF, SAF, and PRGF programs).

For inflation, the SJS F-test in the full sample is rejected at the 5% level (regression 4.3.a), indicating that the efficiency of IMF nowcasts could have been improved through the consideration of additional covariates such as the effect of 2008 Global Financial Crisis and conditionality. Specifically, the effects of quantitative targets on government and central bank

up to 1 year prior to program start; see [Beck et al. \(2001\)](#) and [IFES \(2020\)](#). Conflict dummy covers intra/inter-state conflicts. Programs received a “1” if (civil) wars occurred up to 1 year prior to program start; see [Harbom et al. \(2009\)](#). Disaster dummy covers natural disasters. Programs received a “1” if disasters occurred up to 1 year prior to program start; see [EM-DAT \(2020\)](#).

credit were not properly integrated into IMF nowcasts. In addition, inflation nowcasts could have been improved through consideration of the effects of structural reforms in economic statistics, trade openness, and state enterprises. As in the case of nominal GDP growth, the effects of balance of payments stabilization programs were also not properly accounted for by IMF inflation nowcasts.

In summary, for the full sample, the effects of the 2008 Global Financial Crisis are the only common factors that were not properly accounted for by IMF nowcasts for GDP growth and inflation. As indicated by the statistically significant crisis coefficient, which is negative across GDP growth and inflation, the IMF nowcasts produced overly optimistic estimates of recoveries during the crisis as previously observed by [Genberg & Martinez \(2014\)](#). In addition, each nowcast has additional distinct factors that were not fully integrated into the nowcasts and whose proper consideration would have improved efficiency. Since we learned in the previous section that the inefficiency of the full sample is decisively driven by inefficiencies in the Non-Hyperinflation LICs sample (and to a lesser extent by early hyperinflation), we examine effects of additional covariates for these two subsamples in [Table 2.4](#).

2.4.2 Sources of Nowcast Inefficiency in the Non-Hyperinflation LICs Sample

In [Table 2.4](#), regressions (4.1.b)-(4.3.b) also indicate that the efficiency GDP growth and inflation nowcasts for Non-Hyperinflation LICs could have been improved. Real GDP growth nowcast inefficiency (regression 4.1.b) was driven by overestimates of growth during the 1997 Asian Crisis, as indicated by the negative coefficient. Additional factors whose consideration could have improved efficiency were reforms of labor and financial markets, as well as the economic effect of elections. For nominal GDP growth, nowcasts efficiency could have been improved if quantitative limit on fiscal deficits, reforms of current/capital accounts and program type (BOP Stabilization, SBA) had been fully integrated. Inflation nowcasts could

have been improved through better integration of the effects of quantitative restrictions on reserves, trade reforms and non-financial reforms along with effects of the poverty and growth programs and the 2008 crisis.

2.4.3 Sources of Nowcast Inefficiency in the Hyperinflation Sample

For hyperinflation countries, there is perhaps the strongest evidence that inefficient real/nominal GDP growth and inflation nowcasts could have been improved. For real GDP growth, we find three highly statistically significant factors that should have been properly integrated: quantitative limits on government credit and deficit, as well as limits on external debt and central bank credit. In terms of structural reforms, we find evidence (regression 4.1.c) that state enterprise reform and central bank statistical/regulatory reforms could have improved the nowcasts. Even the effect of elections is now indicated as a factor that could have improved the nowcasts. For nominal GDP growth nowcasts in hyperinflation countries, the most important factors that could have improved the nowcast were nonfinancial reforms and program type (Balance of Payment Stabilization, SBA). In addition, reforms of public employment and current/capital account openness are statistically significant. For inflation nowcasts, the SJS test is also rejected at the 1% level and the ceiling on central bank credit could have improved efficiency.

2.5 Did Nowcast Bias and Efficiency Change Over Time?

Our MONA nowcast data covers over a quarter century of IMF programs. It is natural to ask whether the accuracy of nowcasts changed over time. It may well be that the advent of better models and improved data collection produced successively better nowcasts. Instead of reporting nowcasts accuracy for each individual year, we report results for rolling 5-year periods. This allows us to keep the number of observations per period roughly similar and

of sufficient size.

Figure 2.4.1 provides visuals of nowcast accuracy over time for the full sample, using Mincer-Zarnowitz regressions that are reported in Appendix Table A.4.1. The black line in Figure 2.4.1 is the value of the β estimates in Table A.4.1 and the dotted lines represent 95% confidence intervals. We observe four distinct periods. First, all nowcasts struggled with bias and/or inefficiency until about 2001. Second, all nowcasts saw a reprieve with unbiased and efficient nowcasts until 2005. Third, another period of bias and/or inefficiency occurred until 2009 (2012 for real GDP growth). Fourth, after 2012, GDP growth nowcasts become unbiased and efficient (with one exception in 2013), but inflation nowcasts continue to struggle with efficiency as recently as 2018. It is fascinating to see that inflation still struggles with inefficiency in recent years while nominal GDP growth nowcasts have become efficient.

In addition to our assessment of nowcast accuracy, we observe that the slope coefficients for nowcasts, β , is almost always smaller than unity until 2014. This implies a long-enduring pattern of overly optimistic nowcasts for low-growth countries, and overly pessimistic nowcasts for high-growth countries. The pattern reverses after 2015 when the slope coefficients start to exceed unity for both inflation and real GDP growth. This suggests that since about 2015 IMF nowcasts become excessively pessimistic (optimistic) for low (high) growth outcomes. We also note that the width of the error bands indicates that the standard errors are roughly similar throughout. The exception is real GDP growth, which experienced a widening of the confidence interval during the 2008 Global Financial Crisis (producing inefficient nowcasts). Nominal GDP growth exhibits extraordinarily large errors in the early 1990s and also during the financial crisis. Inflation bucks the trend with tight standard errors until 2007 and stable error bands for the remaining years.

Above we noted the importance of two subsamples in our study of bias and inefficiency:

especially Non-Hyperinflation LICs introduced forecasts errors that translated into nowcast inaccuracy for the full sample. Given the results above, we are also interested in this subsamples' pattern of nowcast accuracy over time. Even though the hyperinflation sample contains 76 observations, it is too concentrated in the early years of our 28-year sample period, rendering too few observations to produce meaningful 5-year rolling time periods throughout.

For the Non-Hyperinflation LICs sample ([Figure 2.4.2](#)) we find a roughly similar pattern to for the full sample, in that earlier nowcasts are more likely to be inefficient and later nowcasts (since 2014) have become unbiased and efficient for GDP growth. Even inflation is unbiased and efficiently nowcast in the Non-Hyperinflation LICs sample in recent years. This is good news, especially given the nearly unbroken string of biased and/or inefficient nominal GDP growth nowcasts in this sample from 2001–2013.

2.6 *Did Nowcast Horizons Affect Nowcast Accuracy?*

In general, forecast accuracy is expected to decrease as forecast horizons increase (Armstrong 2001). This may be expected to be particularly relevant for IMF nowcasts, as information sets are larger at the end of the year due to the accumulation of several data vintages over the year. Hence, one might well expect nowcast bias and efficiency to improve for programs designed and approved later in the year. In this section we examine whether bias and inefficiency is driven by nowcast horizons. [Figure 2.5](#) provides the visual summary of results for the full sample, and the regression outputs are reported in Appendix [Table A.4.3](#). We examine whether nowcasts produced earlier in the program year exhibit a greater propensity towards bias and inefficiency than those formed later in the year.

The results are surprising, as there is no clear pattern of improved nowcast accuracy when the time horizon shortens. Both real GDP growth and inflation nowcasts exhibit greater

variances early in the year, but these do not necessarily translate into greater bias and/or inefficiency. Indeed, real GDP growth and inflation poignantly produce inefficient and even biased nowcasts later in the year. Surprisingly, inflation nowcasts are the most stable around the slope parameter of unity throughout the year, while nominal GDP growth produces the largest deviations from unity. This could be due to the fact that inflation information is much more readily available (on a monthly basis) than GDP growth (at best quarterly). The divergence in GDP growth and inflation accuracy as the time horizon shortens also implies that GDP growth nowcast errors are not driven by inflation nowcast errors. This finding is supported by the fact that there is no similarity in the pattern of real GDP growth nowcast inaccuracies and either inflation or nominal GDP growth nowcasts.

Biased and inefficient nowcasts are found mid-year in April and May and somewhat surprisingly at the end of the year in November when nowcasts turn excessively optimistic. Nominal GDP growth has the expected bias and inefficiency in January but the next two months are both unbiased and efficient with four additional inefficiencies throughout the year. For inflation, bias is again early in the year but bias and inefficiencies are concentrated mid-year. Overall we see no pattern of either bias and efficiency improving over the year. [IMF-IEO \(2014\)](#) previously found evidence that IMF forecast errors increase with time horizons in WEO data, but their study horizons far exceeded the time period covered in this paper.

2.7 Conclusion

IMF nowcasts established at the time of crisis are the basis for IMF program conditions for countries that request assistance from the lender of last resort. Instead of examining the IMF program efficacy, we formally examine the accuracy of the nowcasts that predicate IMF program design in a dataset that is six times larger than the largest previous study on the subject. We find that GDP growth (real and nominal) and inflation nowcasts are inefficient in

the full sample, a result driven by substantial bias and inefficiency in Low-Income Countries (LICs). We show that these inaccuracies are not a function of the nowcast horizon, and document that GDP growth nowcasts have improved in recent years. In contrast, inflation nowcasts continue to struggle with accuracy until recently.

Instead of documenting the uniform optimism in IMF forecasts, which has largely been accepted as a stylized fact in the previous literature, we dissect the structure of nowcast bias and inefficiency and highlight that only the most vulnerable, low-growth recovering LICs are subject to excessively optimistic forecasts. Nowcasts for fast-growing countries are actually excessively pessimistic, underestimating the speed of the recovery. Once purged of LICs, the remaining sample actually exhibits no statistically significant optimistic/pessimistic bias. Our findings have important implications for LICs crisis countries. IMF conditionality based on overly optimistic nowcasts may affect the likelihood that the country can achieve the conditions, and affect future loan disbursements and program evaluation. In addition, the forecast bias may produce quantitative performance targets that are impossible to reach. As the Covid-19 pandemic has raised the demand for IMF programs, improved nowcasts are thus more important than ever.

The dichotomy between countries that are optimistically or pessimistically assessed raises the question regarding the drivers of the inefficiency in IMF nowcasts. We investigate the sources of nowcast inefficiencies by country subsamples to highlight the factors that were in the IMF forecasters' information sets, but were improperly integrated into nowcasts. Each type of forecast (GDP/Inflation) and each subsample of countries (Full/LICs/Hyperinflation) produces a different set of conditions and program types that were improperly integrated into IMF forecasts. Our work has been made possible through the merger of several IMF databases as well as a comprehensive audit of the data, which was found to include an inordinate amount of errors in the public IMF database. This is noteworthy, since the

databases are the basis of a substantial number of research papers.²²

We leave for future research the pesky question as to how researchers best forecasts recoveries. There is an important literature that notes that models do not provide detailed recovery dynamics and researchers rely instead in their forecasts on previous patterned recoveries. [Loungani & An \(2020\)](#) note the general tendency to assume forecasts are V-shaped. When the V-shaped pattern materializes forecasters are slow to update believes, perhaps for lack of guidance from the models? In future research projects we plan to investigate the shapes of the recoveries associated with program countries? crises to understand how much of the forecast bias and inefficiency is driven by patterned recovery forecasts rather than predicted modeled recovery dynamics.

²²A quick search produces over 2000 papers that have been published on the basis of the IMF MONA Database. Results of our audit and corrected errors are documented in Appendix A.

Table 2.1: Variables

Variable	Data Source	Description
IMF Nowcasts	IMF (2020d) MONA Database. See Appendix A for details	"t-1" to "t" period growth rates for real GDP levels (RGDPC/NGDP_R), nominal GDP levels (NGDP), end-of-period inflation (PCPIC/PCPIE)
Final Realized Data	IMF (2020f) WEO Database	"t-1" to "t" period growth rates for real GDP levels (NGDP_R), nominal GDP levels (NGDP), end of period inflation (PCPIE)
2008 Crisis		Dummy variable for the 2008 Global Financial Crisis. Program received a "1" when program commenced between 9/15/2008 and 9/15/2009. The start date of 2008 crisis is the Lehman Brothers' bankruptcy filing date.
2007 Crisis		Dummy variable of the 1997 Asian Crisis. Program received a "1" for programs that commenced between 7/2/1997 and 1/1/1999; and (ii) the country was also identified as an Asian Crisis Country by Kaminsky, Reinhart, and Vegh (2003). In July 1997, Thailand was forced to float exchange rate, which is generally seen as the start of the crisis.
Regions		Dummy variables for Africa, Americas, Asia, and Europe.
Conditionality: QPCs	IMF (2020b) MONA Database Glossary	Dummy variables given by MONA's "Main Criteria" defined by MONA Glossary .
Conditionality: SPCs	IMF (2020b) MONA Database Glossary	Dummy variables given by MONA's "Indicative Targets" defined by MONA Glossary. We grouped them as: Gen_Gov't_Reform (1.1-1.9), CB_Stats_Regs_Indep.(2.1-2.2), Civil_Service_Wage/Empl. (3), Pension_Reform (4.1-4.2), Gov't_Enterprise_Pricing (5.1-5.3), Financial_Sector_Reform (6.1-6.2), Open_Current&Capital_Account (7), Reduce_Trade_Tariff/Quota (8), Labor_Mkt_Wage/Empl.(9), Improve_Econ_Statistics (10), Legal/Market_Reforms (11.1-11.4)
Program Objectives	IMF (2020a) Crisis Lending Fact Sheet	We grouped IMF loan types into 5 broad categories: (i) BOP_Stablization: Stand-By Agreements (SBA); (ii) BOP_shocks_precautionary: Exogenous Shock Facility (ESF), Standby Credit Facility (SCF), Flexible Credit Line (FCL), Precautionary Credit Line (PCL), Precautionary Liquidity Line (PLL); (iii) Struct_Adj_Poverty_Growth: Structural Adjustment Facility (SAF), Enhanced Structural Adjustment Facility(ESAF), Poverty Reduction and Growth Trust (PRGT); (v) long-term BOP and structural reform assistance: Extended Credit Facility (ECF), Extended Fund Facility (EFF) (iv) Non_Financial_Reforms: Policy Reform Instrument (PSI), Policy Coordination Instrument (PCI).
LoanAmount	IMF (2020d) MONA Database for loan amount drawn, IMF (2020c) IFS Database for quota	Loan Amount is the ratio of drawn loan size to IMF country annual quota in the program start year.
Elections	Beck et al (2001) for data before 1998; IFES (2020) for post 1998 data	Election dummy cover two types of national elections: (i) head of state or government election; (ii) legislative election. Program received a "1" if an election occurred up to 1 year prior to the program start date.
Conflicts	UCDP/PRIO Armed Conflict Dataset by Harbom et al. (2009)	Conflict dummy covers intra-state and inter-state conflicts. Program received a "1" if program country experienced a conflict up to one year prior of program start date.
Disasters	EM-DAT (2020)	Disaster dummy covers natural disasters. Program received a "1" if a disaster occurred up to 1 year prior to the program start date.

Table 2.2: Bias and Inefficiency of IMF Nowcasts

Dependent Variable:	Actual RGDP Growth		Actual NGDP Growth		Actual Inflation	
	(1a)	(2a)	(1b)	(2b)	(1c)	(2c)
	EKPC (2019)	Full dataset	EKPC (2019)	Full dataset	EKPC (2019)	Full dataset
Constant, α	0.016**	0.004	0.014*	0.062***	0.002	0.038***
p-value ($\alpha=0$)	0.013	0.206	0.071	0.000	0.875	0.003
IMF Nowcast, β	0.621***	0.821**	0.926	0.666***	1.091	0.864
p-value ($\beta=1$)	0.010	0.044	0.193	0.000	0.702	0.185
Observations	110	597	110	596	100	595
Adjusted R-square	0.402	0.404	0.742	0.836	0.545	0.810
MZ F-test ($\alpha=0, \beta=1$)	3.460**	2.731*	1.740	13.93***	1.021	8.258***
p-value ($\alpha=0, \beta=1$)	0.035	0.066	0.180	0.000	0.364	0.000
HP T-test ($\gamma=0$)	-0.111	-1.464	1.180	-0.946	1.408	2.595**
p-value ($\gamma=0$)	0.912	0.144	0.240	0.344	0.162	0.010

Note:

- (a) Robust standard errors in parentheses unless otherwise indicated; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
(b) Mincer Zarnowitz Null: Nowcast is unbiased and efficient
(c) Holden Peel Null: Nowcast is unbiased

Table 2.3: Nowcast Accuracy by Subsample

Dependent Variable:	RGDP Growth					NGDP Growth					Inflation				
	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)	(1)	(2)	(3)	(4)	(5)
	All	Non-Hyper	Non-Hyper LICs	Non-Hyper Non-LICs	Hyper Inflation	All	Non-Hyper	Non-Hyper LICs	Non-Hyper Non-LICs	Hyper Inflation	All	Non-Hyper	Non-Hyper LICs	Non-Hyper Non-LICs	Hyper Inflation
Constant, α	0.004	0.008**	0.020***	0.003	-0.008	0.062***	0.003	0.011	-0.004	0.188***	0.038***	-0.004	0.002	-0.010	0.197***
p-value ($\alpha=0$)	0.206	0.010	0.001	0.390	0.234	0.000	0.704	0.520	0.665	0.000	0.003	0.658	0.929	0.168	0.001
IMF Nowcast, β	0.821**	0.775***	0.601***	0.835	0.801	0.666***	1.02	0.987	1.052	0.624***	0.864	1.242	1.262	1.210	0.792**
p-value ($\beta=1$)	0.044	0.002	0.001	0.145	0.437	0.000	0.798	0.919	0.551	0.000	0.185	0.156	0.407	0.129	0.023
Observations	597	526	276	250	71	596	525	276	249	71	595	526	275	251	69
Adjusted R-square	0.404	0.353	0.155	0.486	0.383	0.836	0.665	0.625	0.699	0.834	0.810	0.309	0.207	0.595	0.822
MZ F-test ($\alpha=0$ & $\beta=1$)	2.731*	4.841***	5.685***	1.362	0.988	13.93***	1.685	2.039	0.202	11.26***	8.258***	6.062***	6.222***	1.190	5.8***
p-value ($\alpha=0$ & $\beta=1$)	0.066	0.008	0.004	0.258	0.377	0.000	0.186	0.132	0.818	0.000	0.000	0.003	0.002	0.306	0.005
HP T-test ($\gamma=0$)	-1.464	-0.882	-0.345	-1.001	-1.334	-0.946	1.757*	1.834*	0.508	-1.525	2.595**	2.997***	2.692***	1.345	1.135
p-value ($\gamma=0$)	0.144	0.378	0.730	0.318	0.187	0.344	0.079	0.068	0.612	0.132	0.010	0.003	0.008	0.180	0.260
St Dev: Actual Data	0.050	0.043	0.040	0.042	0.077	0.426	0.137	0.141	0.129	1.051	0.382	0.122	0.145	0.089	0.893
St Dev: Nowcast Error	0.039	0.035	0.038	0.031	0.062	0.261	0.079	0.086	0.071	0.718	0.175	0.102	0.130	0.058	0.430
MAE	2.457	2.264	2.406	2.108	3.882	7.202	4.747	5.285	4.150	25.355	6.482	4.512	5.789	3.111	21.506
RMSE	3.914	3.496	3.838	3.075	6.186	26.089	7.953	8.660	7.088	72.427	17.579	10.286	13.107	5.787	43.108

Note:

- (a) Robust standard errors in parentheses unless otherwise indicated; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
(b) Mincer Zarnowitz Null: Nowcast is unbiased and efficient
(c) Holden Peel Null: Nowcast is unbiased
(d) MAE (mean absolute error), RMSE (root mean square error), are scaled by 100

Table 2.4: Sources of Nowcast Inefficiency by Subsamples

		Real GDP Growth			Nominal GDP Growth			Inflation		
		(4.1.a)	(4.1.b)	(4.1.c)	(4.2.a)	(4.2.b)	(4.2.c)	(4.3.a)	(4.3.b)	(4.3.c)
		Full Sample	Non-Hyper LICs	Hyper Inflation	Full Sample	Non-Hyper LICs	Hyper Inflation	Full Sample	Non-Hyper LICs	Hyper Inflation
Nowcast	Constant, α	-0.007	-0.035	0.084**	0.034	-0.099	-0.008	0.037	-0.155	0.176
	p-value ($\alpha = 0$)	0.533	0.491	0.050	0.336	0.161	0.982	0.553	0.124	0.609
IMF Nowcast, β		0.764*	0.558***	0.946	0.649***	0.947	0.630***	0.837*	1.122	0.802**
	p-value ($\beta = 1$)	0.015	0.003	0.673	0.000	0.699	0.001	0.082	0.688	0.011
2008 Crisis		-0.013*	0.007	-0.022	-0.046***	-0.053	0.323	-0.035*	-0.068*	1.370
		(0.007)	(0.015)	(0.046)	(0.014)	(0.033)	(0.292)	(0.021)	(0.041)	(0.876)
1997 Crisis		-0.012	-0.037**	0.011	-0.028	0.037	-0.194	-0.033	0.013	-0.289
		(0.015)	(0.018)	(0.022)	(0.037)	(0.065)	(0.250)	(0.053)	(0.076)	(0.199)
Africa		0.006	0.041	0.014	-0.038	0.043	-0.169	-0.022	0.081	-0.331
		(0.006)	(0.041)	(0.022)	(0.025)	(0.039)	(0.195)	(0.031)	(0.062)	(0.200)
Americas		0.005	0.032	0.014	-0.056**	0.063	0.299	-0.045	0.084	0.140
		(0.006)	(0.041)	(0.042)	(0.022)	(0.046)	(0.190)	(0.035)	(0.058)	(0.235)
Ceiling External Debt(MT<)		0.007	0.020	0.063	0.040***	0.037	-0.054	0.006	0.011	-0.453
		(0.005)	(0.020)	(0.046)	(0.014)	(0.039)	(0.263)	(0.013)	(0.037)	(0.682)
Floor Int'l Reserves		0.010*	0.014	0.005	0.037*	0.027	0.136	0.023	0.043*	0.133
		(0.005)	(0.011)	(0.024)	(0.021)	(0.021)	(0.164)	(0.016)	(0.024)	(0.146)
Ceiling External Arrears		-0.001	-0.003	0.006	0.033**	0.013	0.029	0.010	0.019	-0.239
		(0.004)	(0.006)	(0.022)	(0.014)	(0.014)	(0.132)	(0.021)	(0.026)	(0.165)
Ceiling Gov't Credit		-0.010**	-0.005	-0.160***	-0.003	0.009	0.026	0.028**	0.011	0.724
		(0.005)	(0.007)	(0.042)	(0.012)	(0.026)	(0.435)	(0.013)	(0.017)	(0.720)
Ceiling Gov't Deficit		-0.004	-0.008	0.041*	-0.058***	-0.021**	-0.302	-0.021	-0.014	-0.026
		(0.004)	(0.005)	(0.022)	(0.018)	(0.011)	(0.203)	(0.021)	(0.024)	(0.156)
Ceiling External Debt(ST)		-0.001	-0.002	-0.085**	-0.020*	-0.009	0.109	0.006	0.014	-0.066
		(0.004)	(0.005)	(0.040)	(0.012)	(0.013)	(0.237)	(0.010)	(0.014)	(0.242)
Ceiling CB Net Dom Assets		0.002	-0.009	0.089***	0.010	-0.013	-0.012	0.041**	-0.006	0.524*
		(0.006)	(0.007)	(0.032)	(0.020)	(0.016)	(0.413)	(0.017)	(0.019)	(0.292)
Civil Service Wage/Empl.		0.003	0.010	0.009	-0.002	-0.002	0.719*	-0.008	-0.009	0.989
		(0.004)	(0.007)	(0.080)	(0.009)	(0.013)	(0.380)	(0.009)	(0.014)	(0.721)
Improve Econ Statistics		0.008*	0.001	-0.144	-0.009	-0.002	0.001	-0.028***	-0.006	0.159
		(0.005)	(0.006)	(0.090)	(0.010)	(0.011)	(0.731)	(0.010)	(0.012)	(1.016)
Open Current&Capital Account		-0.002	0.006	0.002	-0.043**	-0.036**	-0.493*	-0.012	-0.019	-0.230
		(0.005)	(0.006)	(0.021)	(0.021)	(0.015)	(0.261)	(0.018)	(0.023)	(0.158)
Financial Sector Reform		0.002	0.013*	0.031	0.003	0.018	-0.240	-0.001	0.004	-0.244
		(0.005)	(0.007)	(0.025)	(0.015)	(0.015)	(0.209)	(0.018)	(0.022)	(0.256)
Reduce Trade Tariff/Quota		0.001	0.006	-0.025	0.023	-0.005	0.402	-0.029*	-0.051*	0.039
		(0.004)	(0.005)	(0.024)	(0.024)	(0.014)	(0.257)	(0.017)	(0.026)	(0.126)
Gov't Enterprise Pricing		-0.002	0.005	-0.062*	0.017	0.001	0.190	0.033*	-0.004	0.130
		(0.004)	(0.007)	(0.032)	(0.011)	(0.013)	(0.214)	(0.018)	(0.013)	(0.264)
CB Stats Regs Indep.		-0.002	0.001	-0.126***	-0.010	-0.013	-0.074	-0.007	-0.022	-0.244
		(0.004)	(0.008)	(0.038)	(0.011)	(0.016)	(0.250)	(0.013)	(0.021)	(0.335)
Labor Mkt Wage/Empl.		0.009	-0.031*	-	-0.025	-0.003	-	-0.025	-0.009	-
		(0.007)	(0.018)		(0.016)	(0.030)		(0.019)	(0.049)	
BOP Stabilization ¹		-0.005	-0.012	-0.015	0.042**	0.087*	0.398**	0.053***	0.179	0.167
		(0.005)	(0.013)	(0.025)	(0.018)	(0.044)	(0.163)	(0.020)	(0.118)	(0.132)
BOP Shocks Precautionary ²		0.009	0.009	-	0.012	0.020	-	-0.006	0.048	-
		(0.006)	(0.018)		(0.019)	(0.032)		(0.021)	(0.052)	
Struct Adj Poverty Growth ³		0.004	0.002	-	-0.023*	-0.007	-	0.009	0.048*	-
		(0.005)	(0.011)		(0.014)	(0.018)		(0.017)	(0.025)	
Non Financial Reforms ⁴		0.001	-0.004	-0.038	0.008	0.006	0.432**	0.011	0.041***	0.211
		(0.005)	(0.007)	(0.033)	(0.016)	(0.018)	(0.200)	(0.012)	(0.015)	(0.177)
Elections		-0.001	-0.010*	0.013	0.012	-0.019	0.149	-0.007	-0.020	0.012
		(0.004)	(0.005)	(0.015)	(0.018)	(0.013)	(0.174)	(0.012)	(0.018)	(0.116)
Conflicts		-0.005	-0.007	-0.038**	0.000	-0.019	0.005	-0.003	-0.007	-0.033
		(0.005)	(0.006)	(0.016)	(0.012)	(0.012)	(0.120)	(0.013)	(0.019)	(0.149)
Observations		597	276	71	596	276	71	595	275	69
	Adjusted R-squared	0.406	0.199	0.521	0.842	0.643	0.823	0.819	0.231	0.859
	SJS F-test ($\alpha=\delta=0$ & $\beta=1$)	1.368*	2.044***	13.64***	3.263***	1.433**	5.618***	1.573**	1.369*	13.84***
p-value ($\alpha=\delta=0$ & $\beta=1$)		0.081	0.001	0.000	0.000	0.063	0.000	0.021	0.091	0.000

Note:

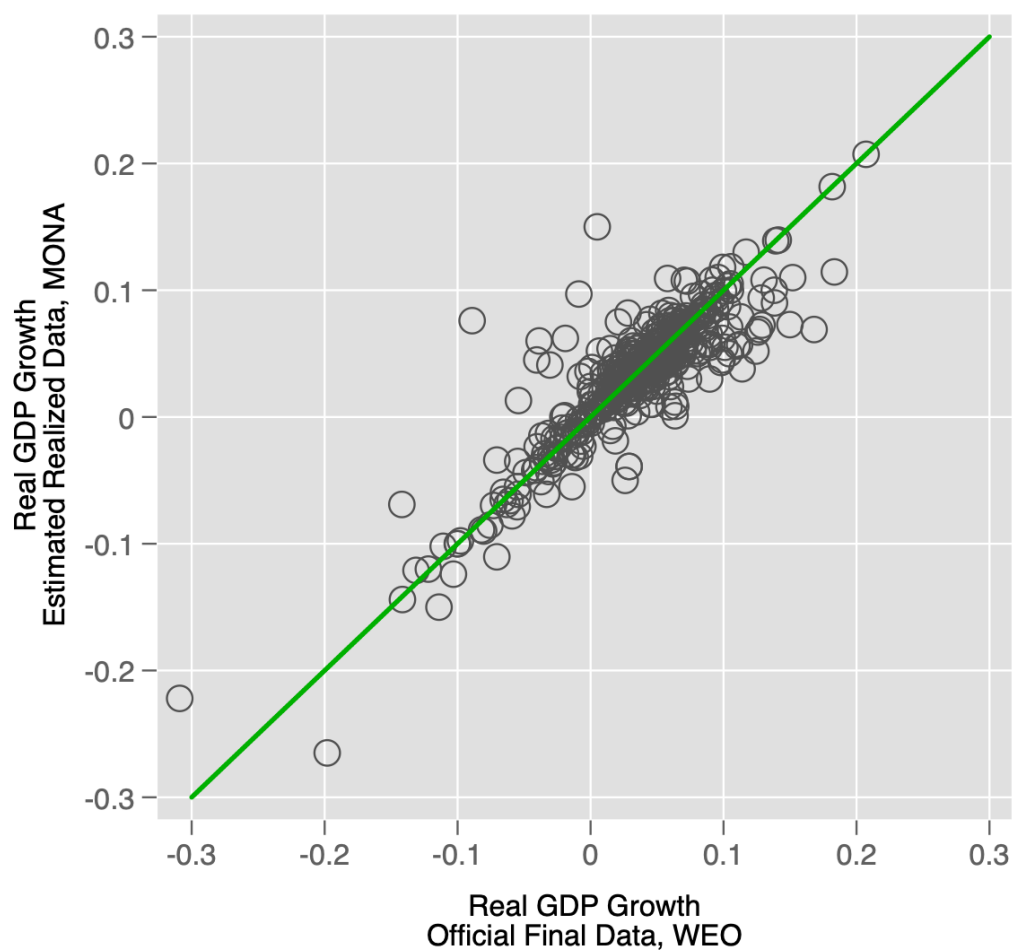
(a) Robust standard errors in parentheses unless otherwise indicated; *** p<0.01, ** p<0.05, * p<0.1

(b) ¹ SBA; ² ESF, SCF, FCL,PCL, PLL; ³ SAF, ESAF, PRGT; ⁴ PSI, PCI. See [Table 2.1](#) for detailed description of programs.

(d) Insignificant variables that were included in the regressions are not included in the table to economize on space. These variables include Regions (Asia), Quantitative conditionality: Ceiling_Domestic_Arrears, Ceiling_New_Arrears/Default; Structural conditionality: Gen_Gov't_Reform, Legal/Market_Reforms, Pension_Reform), NaturalDisasters, LoanAmount.

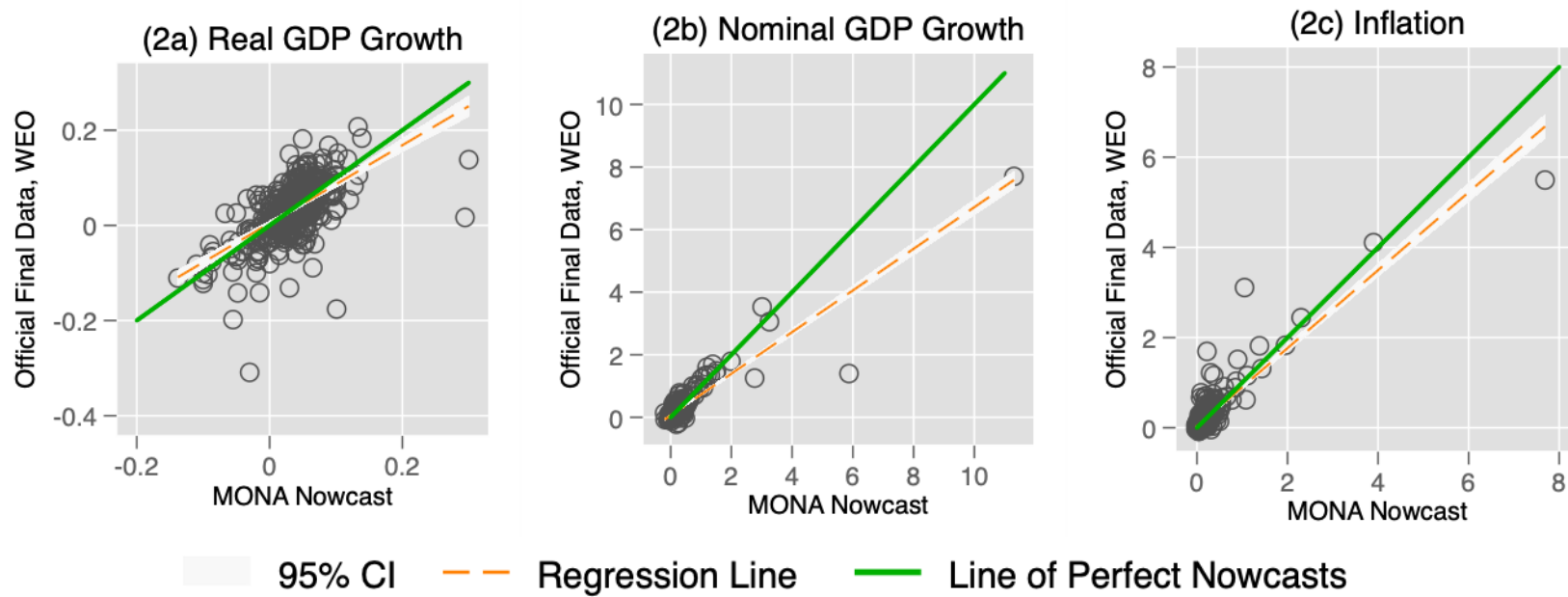
(d) Omitted dummy variables to avoid singularity: Regions (Europe) and Program Objectives ("Long-Term BOP and Structural Reforms" consisting of: ECF and EFF)

Figure 2.1: Official Final Data (WEO) vs. Estimates of Final Data (MONA)
(Real GDP Growth)



Source: Authors' calculations.

Figure 2.2: Prediction-Realization Diagrams
(Full Sample)



Source: Authors' calculations.

Figure 2.3: Prediction-Realization Diagrams for Subsamples

Figure 2.3.1: Non-Hyperinflation & Non Low-Income Countries

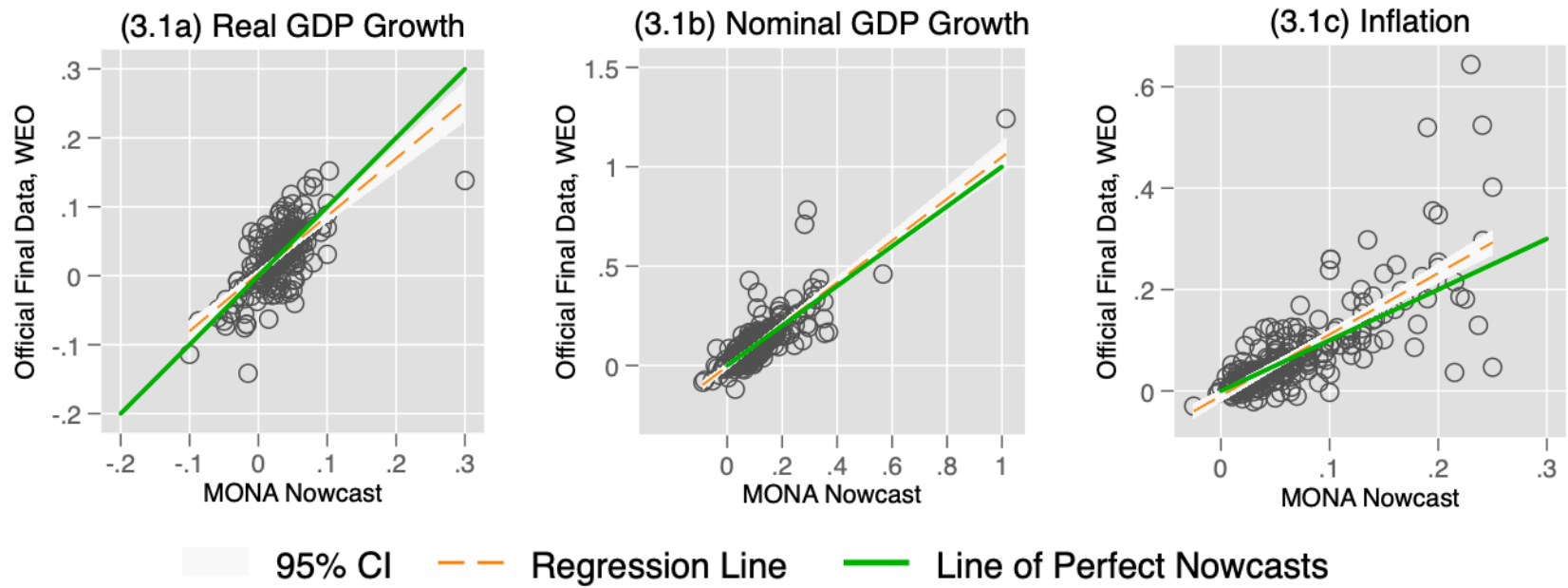


Figure 2.3.2: Non-Hyperinflation & Low-Income Countries

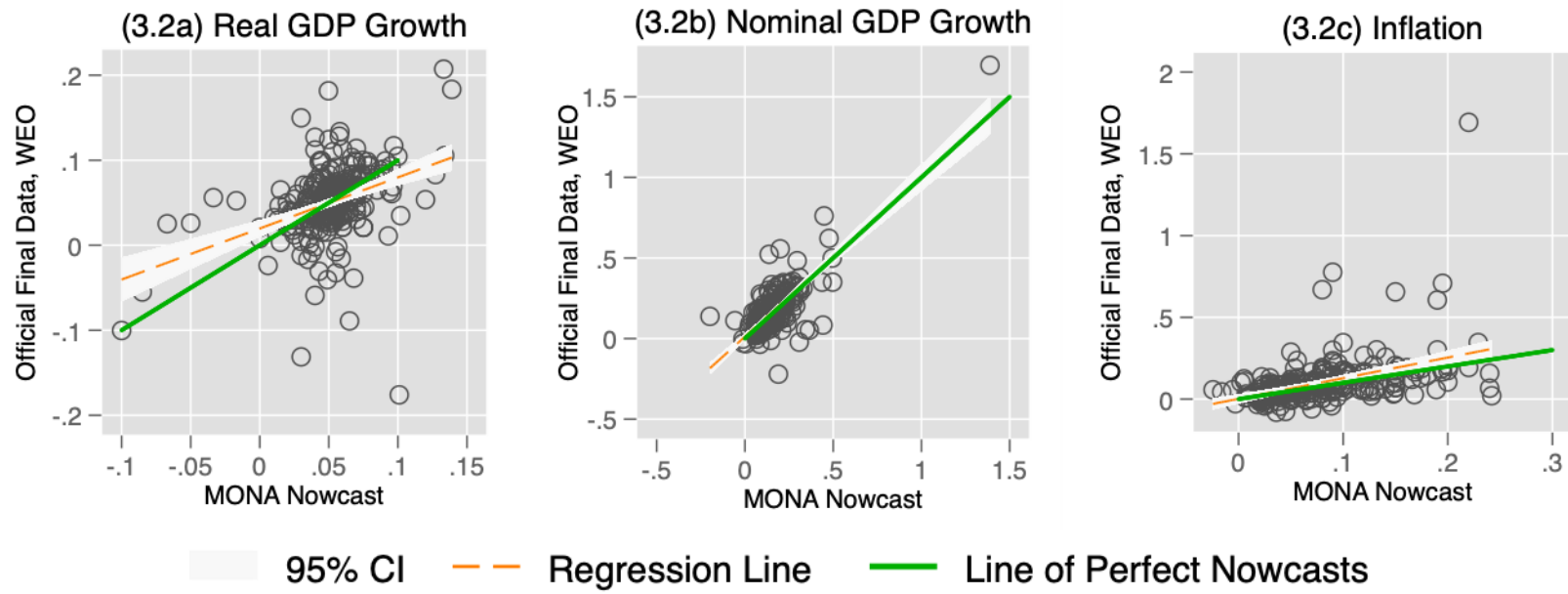
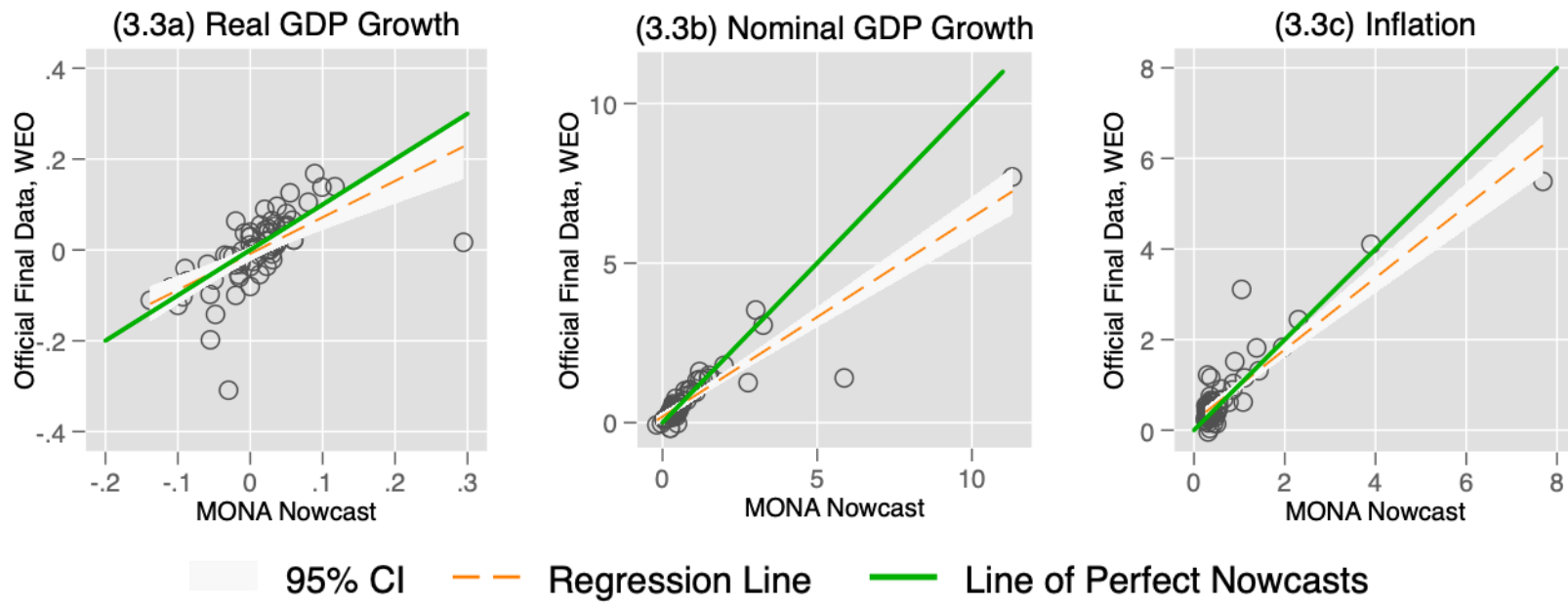


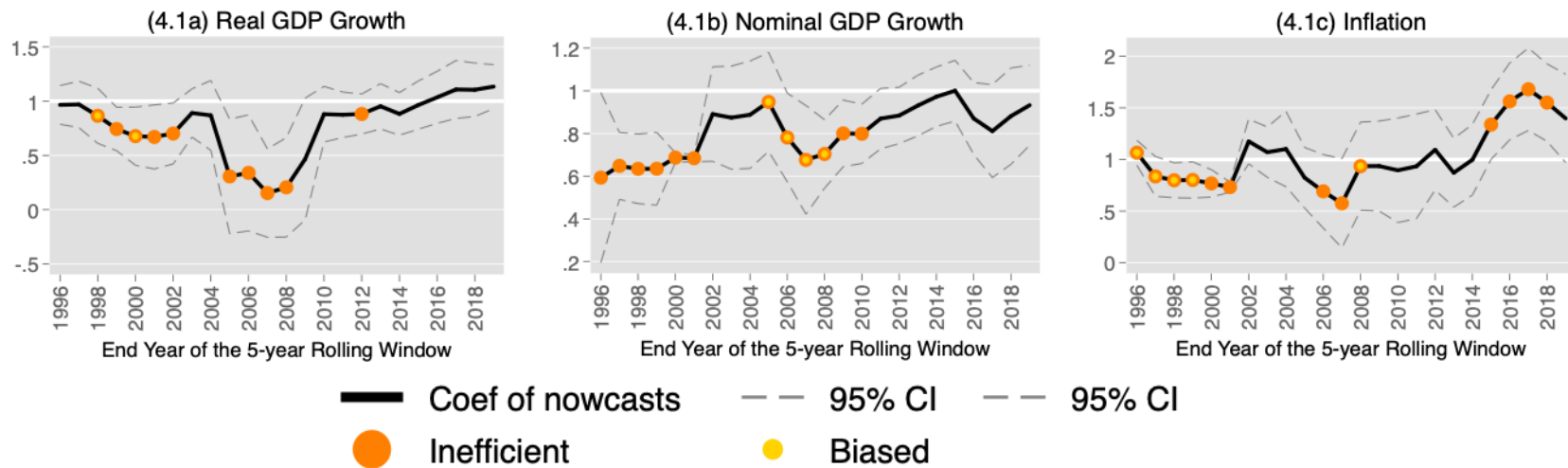
Figure 2.3.3: Hyperinflation Countries



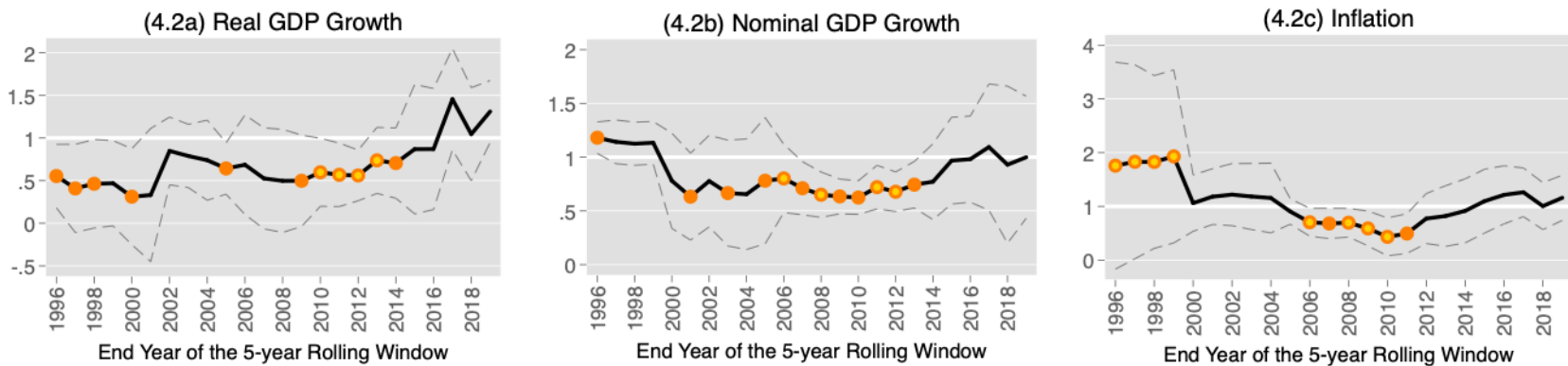
Source: Authors' calculations.

Figure 2.4: Nowcast Bias and Efficiency Over Time

2.4.1: Full sample

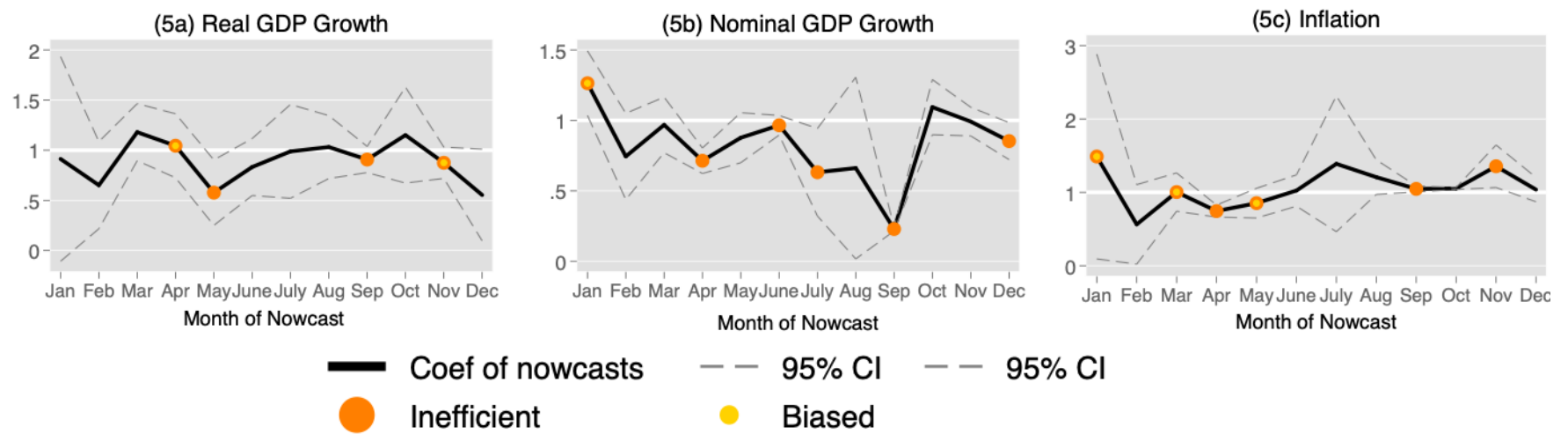


2.4.2: Subsample of Non-Hyperinflation LICs



Source: Authors' calculations.

Figure 2.5: Nowcast Horizons and Nowcast Accuracy
(Full Sample)



Source: Authors' calculations.

Chapter 3

EXCHANGE RATES AND DOMESTIC CREDIT – CAN MACROPRUDENTIAL POLICY REDUCE THE LINK?¹

3.1 Introduction

A recently growing literature explores how movements in exchange rates not only affect macroeconomic outcomes, but can also affect financial conditions and credit developments, which may in turn feed back into the macroeconomic outlook (Blanchard et al., 2016; Shin, 2018; Ghosh et al., 2018; Hofmann et al., 2020; BIS, 2019). The main idea is that a currency appreciation would tend to ease domestic financial conditions, and drive up domestic credit through a number of mutually reinforcing channels. Specifically, an exchange rate appreciation can drive up domestic credit through raising collateral values and net worth of domestic market participants (Krugman, 1999; Céspedes et al., 2004; Bruno & Shin, 2015b). It can also be associated with a reduction in credit spreads and encourage market participants to take greater risks (Hofmann et al., 2020).² Through these mechanisms, an appreciation may become expansionary, in contrast to the standard notion in the earlier literature where an appreciation is held to be contractionary by reducing net exports. Moreover, when an appreciation leads the domestic provision of credit to expand, this can contribute to systemic risk³, and potentially require a policy response on the part of macroprudential policymakers.

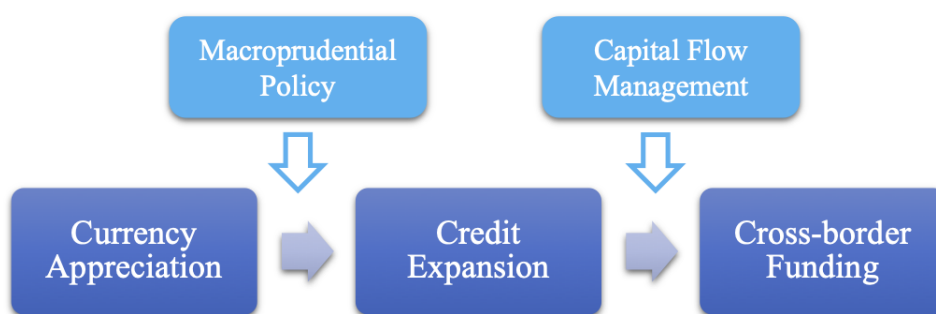
¹This chapter is a joint work with Erlend Walter Nier and Thorvardur Tjoervi Olafsson from the International Monetary Fund.

²Parts of the literature have referred to these mechanisms as the “risk-taking channel” of currency appreciation in the context of cross-border spillovers of monetary policy (Bruno & Shin, 2015a; Borio & Zhu, 2012; Hofmann et al., 2020).

³Increased credit can also in turn affects local asset prices, or is funded through cross-border borrowing. This can magnify the ultimate effects of exchange rates appreciation, potentially giving rise to a build-up

To help guide the use of macroprudential policy, in this paper, we study the link between exchange rate movements and domestic credit in a panel of 62 countries over the period of 2000:Q1 to 2016:Q4, and ask to what extent macroprudential policy can attenuate the effects of currency movements on domestic credit cycles (left-hand side of [Figure 3.1](#)). We also evaluate a complementary role of targeted controls on inflows, when strong developments in credit in turn lead to increases in cross-border borrowing by banks and corporate firms (right-hand side of [Figure 3.1](#)).

Figure 3.1: Exchange Rates, Credit, and Capital Flows



Source: Author's descriptions.

Our empirical focus is on macroprudential policy that addresses the time dimension of systemic risk, that is, procyclical risks from interactions between credit developments and the real economy.⁴ Most existing studies find evidence supporting the notion that macroprudential policies can contain systemic risks, by mitigating potentially excessive increases in credit ([Vandenbussche et al., 2015](#); [IMF-FSB-BIS, 2016](#); [Cerutti et al., 2017a](#); [Galati &](#)

of systemic risk ([Gertler et al., 2007](#); [Borio, 2014](#); [Bruno & Shin, 2015a,b](#); [IMF, 2017](#); [Baskaya et al., 2017](#)).

⁴See [Galati & Moessner \(2013\)](#) for the discussion of cross-sectional dimension of systemic risk, which arises through interlinkages of financial intermediaries.

Moessner, 2018; Alam et al., 2019). However, due to the lack of detailed cross-country information on macroprudential policy or credit developments, the existing literature primarily covers a small set of countries (that have frequently taken macroprudential policy actions as in Fendoğlu (2017)), often over a fairly short period of time (e.g., post global financial crisis), or examining a small subset of macroprudential instruments (e.g., Vandenbussche et al. (2015) and Kuttner & Shim (2016) examine housing-related tools only). Comprehensive evaluations across a large set of economies and over a long period are still limited.

Our analysis deploys a novel database of macroprudential policy actions, the iMaPP database compiled by the IMF (Alam et al., 2019). This database integrates five major existing databases and is the most comprehensive of such databases to date.⁵ Another feature of the iMaPP database we exploit is that it distinguishes policy tightening and loosening actions for each of the categories of tools included in the database. Our data sample consists of a large panel of 62 countries⁶ over the period 2000 to 2016, with observations at the quarterly frequency for 16 categories of macroprudential instruments. It covers economies that have taken frequent macroprudential policy actions (e.g., India, Korea, and Russia) and those that have rarely done so (e.g., Chile and Germany), thereby avoiding sample selection biases that may be present in other studies.

More importantly, our paper differs from the extant literature in that we focus not on the direct effect of macroprudential policy on credit, but on an interaction effect that measures whether macroprudential policy can mitigate the impact of changes in real exchange rates on domestic credit developments. We thereby more squarely address an important issue for small and financially open economies: to what extent is macroprudential policy able to

⁵The original iMaPP database covers 17 instruments for a total of 138 countries since 1999 at a monthly frequency. See its coverage comparison with other existing databases in the Appendix I Table 4 of Alam et al. (2019). Our paper excludes the instrument of SIFI because it is related to the cross-sectional dimension of systemic risk.

⁶The number of countries is mainly constrained by the data availability of domestic credit.

insulate the financial system from the effects of external shocks? For this type of economy, [Gourinchas & Obstfeld \(2012\)](#) find that a rapid increase in leverage and a sharp real currency appreciation emerge consistently as the two most robust and significant predictors of financial crises. Moreover, traditional monetary policy adjustment through increases in short-term interest rates would not be suitable to contain the financial expansion that precedes these crises. Instead, macroprudential policy tightening may be needed to address systemic risks, by reducing the link between the exchange rate and domestic credit.

In addressing these issues empirically, our paper complements [Fendoğlu \(2017\)](#), who examines the effectiveness of macroprudential policy (including capital flow management-related macroprudential measures) in mitigating the impact of capital inflows on the credit-to-GDP gap, for a small sample of 18 emerging market economies. We focus, in a larger sample, on the effect of changes in exchange rates on credit developments, thereby capturing an important transmission channel of external shocks more broadly (see also IMF, 2017)). If macroprudential policy tightening can attenuate the procyclical effects of external shocks, then this would free the hand of monetary policy makers to focus more squarely on managing the effects of domestic shocks on the economy.

Moreover, our paper sheds light on unintended consequences of macroprudential policy, thereby contributing to the existing literature on such spillovers by [Cerutti & Zhou \(2018\)](#); [Avdjiev et al. \(2017\)](#); [Bruno et al. \(2017\)](#); [Cizel et al. \(2019\)](#). In particular, we examine a cross-border leakage effect where stricter macroprudential regulation imposed on domestic lending may lead to increased credit provision from abroad (e.g., [Ahnert et al., 2021](#); [Reinhardt & Sowerbutts, 2015](#)). We embed this analysis of cross-border leakages of macroprudential measures in an empirical framework that examines how strong domestic credit can “pull in” additional capital from abroad ([Borio et al., 2011](#); [Avdjiev et al., 2012](#); [Hahn](#)

et al., 2013).⁷ Our focus here is the effect on the category of “other investment flows”, which includes direct cross-border borrowing, i.e., loans received by corporates from foreign banks, and cross-border funding of the domestic banking system through loans and deposits received from abroad.

In the baseline, first, we find evidence of the linkage between currency appreciation and domestic credit developments, as in the recent empirical literature (Hofmann et al., 2020; Bruno & Shin, 2015a,b; Hahm et al., 2013; Shin, 2018). In particular, an appreciation of the local exchange rate vis-à-vis the U.S. dollar is followed by an increase in the credit-to-GDP gap⁸ in the next quarter. Second, macroprudential policy is found to have a direct effect on domestic credit developments. A tightening macroprudential policy action leads to a reduction in the credit-to-GDP gap in the next quarter. Third, and most importantly, we examine the interaction effect of macroprudential policy in mitigating the impact of the exchange rate on domestic credit, and find that for a given appreciation of the real exchange rate, the subsequent increase in the credit-to-GDP gap is weaker where macroprudential policies had been tightened in the previous quarter. This is important both from a policy perspective and from the point of view of improving the identification of the causal effects of macroprudential measures.

Endogeneity is a common challenge faced by the literature on the effects of macroprudential policy. In our empirical framework, endogeneity may result from (i) potentially omitted effects of economic fundamentals, which may drive both credit and exchange rate changes; (ii) potential reverse causality between macroprudential policy actions and domestic credit,

⁷Most discussions focus on a causal link that runs from capital inflows to credit, where capital inflows lead to an increase in loanable funds for the domestic banking system, and thereby “push up” the supply of domestic credit. However, many studies acknowledge that there is likely to be a two-way relationship, where strong domestic credit can also “pull in” additional capital from abroad (Igan & Tan, 2017; Amri et al., 2016; Lane & McQuade, 2014).

⁸The credit-to-GDP gap was proposed by the Basel Committee as an early-warning indicator of financial crises (BCBS, 2010; IMF-FSB-BIS, 2016), and it captures the time dimension of systemic risk.

where policy action responds to changes in credit. These two issues would bias the estimated coefficients of exchange rate movement and macroprudential policy actions, respectively, and could conceivably contaminate the coefficient estimate for the interaction term between the exchange rate and macroprudential policy. To mitigate the potential endogeneity, we first lag all the independent variables by one-quarter and use the Arellano-Bond difference GMM methodology, thereby following approaches commonly applied in the literature (e.g., [Claessens et al., 2013](#); [Cerutti et al., 2017b](#)). In the absence of suitable instruments, we take further steps by constructing “exchange rate shocks” and “macroprudential policy shocks” that are orthogonal to other covariates. We find that the baseline results are robust after controlling for endogeneity.

In an extension, we find that increases in domestic credit are associated with increases in other investment flows. Moreover, where macroprudential policy is tightened, this leads to further increases in cross-border flows. We interpret this as evidence of policy leakage, where domestic corporates respond to macroprudential tightening by directly borrowing from abroad. On the other hand, targeted capital controls that aim to limit these types of flows appear to be effective. We find that adopting these controls reduces the size of other investment flows that are being pulled in by strong domestic credit, or the tightening of macroprudential policy, thereby limiting the further build-up of systemic risk from direct cross-border borrowing.

The remainder of this paper is organized as follows: Section 3.2 presents the baseline empirical methodology, data, and discusses the baseline findings. Section 3.3 addresses the endogeneity problem. Section 3.4 presents the extension on how domestic credit may fuel capital inflows. Section 3.5 concludes and discusses policy implications.

3.2 The Baseline

3.2.1 Empirical Methodology

In the main part of the paper, we use a dynamic panel framework to investigate the determinants of the credit gap. Our baseline set-up, which we expand on in further analysis, relates the credit gap (denoted Y) to changes in the real exchange rate, macroprudential policy actions, their interactions, as well as controls:

$$\mathbf{Y}_{i,t} = \rho \mathbf{Y}_{i,t-1} + \beta_1 \Delta^4 \mathbf{RER}_{i,t-1} + \beta_2 \mathbf{MaPP}_{i,t-1} + \beta_3 \mathbf{MaPP}_{i,t-1} \times \Delta^4 \mathbf{RER}_{i,t-1} + \theta \mathbf{Z}_{i,t-1} + \mu_i + v_{i,t}$$

$$\text{where } E[\mu_i] = E[v_{i,t}] = E[\mu_i v_{i,t}] = 0$$

$$\text{control variable } \mathbf{Z}_{i,t-1} = [MPS_{i,t-1}, \Delta^4 F_RGDP_{i,t-1}]$$

The subscripts i and t represent country and time (quarter) respectively; μ_i is a fixed effect that captures time-invariant country characteristics, and $v_{i,t}$ is the error term.⁹ $\Delta^4 RER_{i,t-1}$ is the year-over-year log change of real exchange rate, which is lagged by one quarter. $MaPP_{i,t-1}$ an ordinal indicator variable representing the number of macroprudential policy actions by direction (tightening actions net of loosening actions) that are taken during period $t - 1$ in country i . In addition to this measure, we analyze the effects of tightening (T_MaPP) and loosening (L_MaPP) actions separately.

$\mathbf{Z}_{i,t-1}$ is a vector of control variables, which includes the monetary policy stance (MPS) and the consensus forecast of year-over-year real GDP growth ($\Delta^4 F_RGDP_{i,t-1}$), again lagged by one quarter. In contrast to the actual real GDP growth rates widely used in the

⁹The quarterly time-fixed effect, a proxy of the exogenous global push factor, is not considered here. First, potential collinearity arises when the global push factor also affects the domestic drivers of credit developments. Indeed, the inclusion of time-fixed effect makes most coefficients of the domestic drivers statistically insignificant. Second, its inclusion violates the assumption of first order serial correlation in residuals for the dynamic panel model (e.g., p-values of AR(1) test range from 0.35 to 0.9).

existing literature (e.g., Claessens et al., 2013; Akinci & Olmstead-Rumsey, 2018; Cerutti et al., 2017a; Alam et al., 2019), we use the forecasted real GDP growth rates rather than the more widely used actual GDP growth rates to mitigate a potential endogeneity concerns stemming from both credit and the exchange rate being driven by good news about the economy. By including the growth forecast, we measure the effect of the residual variation in the exchange rate that is orthogonal to these effects. A robustness check using the actual GDP growth is reported in Appendix A.1.

The aggregate measure $MaPP_{i,t-1}$ is a lagged ordinal indicator variable representing the number of macroprudential policy actions by direction (tightening actions net of loosening actions) which are taken during period $t - 1$ in country i . For example, a value of +3(-3) represents three policy actions being taken to tighten (loosen) the macroprudential policy stance within the quarter, and a value of 0 represents no action is taken within the quarter.

If a tightening macroprudential action has the effect of reducing the credit gap, we would expect $\beta_2 < 0$ in the equation above. Moreover, by construction, a negative change in $\Delta^4 RER_{i,t-1}$ represents a real exchange rate appreciation. If an appreciation is associated with an increase in the credit gap, we would therefore expect $\beta_1 < 0$.

A key focus of our investigation is the interaction between macroprudential action and the change in the real exchange rate $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$. If macroprudential action is effective in containing the impact of the real exchange rate appreciation on the credit gap, we expect $\beta_3 > 0$, with this effect thereby attenuating the negative coefficient on the real exchange rate.

We estimate the above equation using the Generalized Method of Moments (GMM) estimator developed by Arellano & Bond (1991), to address endogeneity concerns and avoid the Nickell bias¹⁰ arising in the presence of the lagged dependent variable. In addition,

¹⁰The bias of the Least Square Dummy Variables (LSDV) estimator in a dynamic model is generally known

we verify that the Arellano-Bond approach is suitable for our purposes since all further conditions on its use are found to hold.¹¹

We lag the *MaPP* variables by one-quarter, which is consistent with the approach in the previous literature (Akinci & Olmstead-Rumsey, 2018; Cerutti et al., 2017a; Fendoğlu, 2017). We finally lag the real exchange rate, as well as all other independent and control variables to mitigate endogeneity concerns.

In our estimation, the first-differenced lagged dependent variable is instrumented with 1–3 lags of its level. All dependent and control variables are treated as predetermined.¹² We use the forward orthogonal deviation transformation (Arellano & Bover, 1995) to mitigate data gap issues in unbalanced panels. We also use two-step covariance estimates to obtain robust standard errors and to correct their downward bias (Windmeijer, 2005).

3.2.2 Data

Our sample includes 62 economies (35 AEs plus 27 EMEs) as shown in Figure 3.2. These economies have sufficiently good data at quarterly frequency, not just on macroprudential policy measures, but also on credit and GDP, enabling us to compute credit gaps. The sample

as dynamic panel bias or Nickell’s bias (Nickell, 1981). If the lagged dependent variable appears as an independent variable, strict exogeneity of the regressors no longer holds. The LSDV is no longer consistent when N tends to infinity and T is fixed. Given our sample period $T=68$, the theoretical semi-asymptotic bias (assuming N goes to infinity) is -0.0391 in the baseline for an autoregressive coefficient of 0.98 . The actual bias is supposed to be slightly larger for the $N=62$ in our sample. Moreover, a smearing effect arises from the endogeneity (the inconsistency of other independent variables due to the endogeneity of the one variable is smeared across all of the least squares estimators), and a large degree of autocorrelation further magnifies the inconsistency (Kiviet, 1995).

¹¹In addition to the Nickell bias arising in the presence of the lagged dependent variable $Y(t-1)$, these are that the independent variables are not strictly exogenous and are correlated with past errors; a statistically significant linear functional relationship holds, which is confirmed by our baseline result; there is unobserved cross-sectional heterogeneity in the credit gap (denoted as μ_i); there is autocorrelation within individual panel’s error terms $\theta_{i,t}$, which is verified by a $AR(1)$ test in the baseline result (Roodman, 2009).

¹²Predetermined is a weaker restriction than strict exogeneity. The underlying assumption is the current period error term is uncorrelated with current and lagged values of the predetermined variable but maybe correlated with future values.

covers economies that have taken frequent macroprudential policy actions (e.g., India, Korea, and Russia) and those that have rarely done so (e.g., Chile and Germany). The sample period spans from 2000:Q1 to 2016:Q4, as macroprudential policy has been increasingly used across countries since the early 2000. To remove the effect of outliers, we winsorize the top and bottom 1 percent observations of each variable except the dependent variable and the ordinal variable *MaPP*. See [Table 3.1](#) for the description of variables, and [Table 3.2](#) for the summary of statistics.

Dependent variable (Y): Our domestic credit measure is the credit-to-GDP gap, which is the quarterly credit-to-GDP ratio relative to its long-run trend. Following the method proposed by the BCBS (2010), we calculate the gap from a one-sided HP filter using a long-run smoothing parameter $\lambda=400,000$.¹³ Credit is broadly defined in this paper as total claims on the private non-financial sector from both banks and non-bank financial institutions to capture all domestic sources of debt funds for the private sector.¹⁴ We use the financial corporations’ domestic claims on the private sector from the IMF’s International Financial Statistics (IFS) database where available, otherwise, we use depository corporations’ (or monetary) domestic claims on private sector from the same source. For brevity, we refer the credit-to-GDP gap as “credit gap” in the rest of this paper. We consider the simpler 4-quarter change of the credit-to-GDP ratio in a robustness check (see Appendix A.3).

Macroprudential policy stance (MaPP): The data source for macroprudential policy actions is the IMF’s iMaPP database ([Alam et al., 2019](#)), which is, to the best of our knowledge, the most comprehensive database of macroprudential policies to date (covering

¹³Initially taking the first 24 quarters, then computing the trend and cyclical components recursively (adding one quarter at a time).

¹⁴The credit data are not fully harmonized across countries as sometimes they are from surveys reported by different entities within a country. For instance, the credit data for Iceland and Taiwan POC are from the central banks. Allowing this heterogeneity is the only way to provide a sufficient sample coverage across country and time for our study.

17 instruments for a total of 138 countries over the period 1999-2016 at a monthly frequency). We consider an aggregate measure of the macroprudential policy stance (*iMaPP*) as well as two subgroups: borrower-based tools (*MaPP-Br*) and financial institutions-based tools (*MaPP-FI*). Moreover, the iMaPP database allows us to distinguish policy adjustments that amount to a tightening and those that lead to a loosening of macroprudential constraints. We exclude from our index tools used to control risks from systemically important institutions (SIFIs), resulting in an index that aggregates 16 out of the 17 categories in the iMaPP database, since that category of tools is related to the cross-sectional dimension of systemic risk rather than its time dimension. Details of the individual macroprudential instruments covered in each subgroup are available in [Table 3.3](#).

YoY change of the real exchange rate ($\Delta^4 RER$): We use the (lagged) year-over-year log change of the weighted average of the bilateral nominal exchange rate prevailing over the past four quarters¹⁵, which is denoted in national currency relative to the USD, and deflated by the U.S. consumer price index (CPI) against domestic CPI. An appreciating real exchange rate can fuel the build-up in credit through multiple channels as described in section 3.1. By convention, a negative change in $\Delta^4 RER_{i,t-1}$ represents a real exchange rate appreciation, so we expect a negative coefficient.

Forecasted YoY growth of real GDP ($\Delta^4 F_RGDP$): We include the consensus forecast of future GDP growth as a control, since this serves to mitigate a potential endogeneity problem, when “good news” about the economy leads the exchange rate to appreciate and at the same time stimulates credit. We construct the forecasted quarterly year-over-year real GDP growth by taking a weighted average of the current year’s and next year’s forecasted growth rates

¹⁵The weighted average of variable X is calculated as $X_{WA} = 0.4 \times X + 0.3 \times l1.X + 0.2 \times l2.X + 0.1 \times l3.X$. We also use the simple average and the true year-over-year log change of real exchange rate, and found results to be consistent with using the weighted average (while the significance level and size of coefficients slightly dropped).

from the Consensus Forecast.¹⁶ For five countries (Iceland, Lebanon, Luxembourg, Malta, and Mongolia) for which consensus forecasts are not available, we apply the same weighted average method but use the realized forward growth rates instead as a “perfect foresight” measure. Optimism with respect to short-run economic outcomes is expected to drive up both credit demand and supply, so a positive coefficient is expected. We use the actual GDP growth rates in a robustness check.

Monetary policy stance (MPS): We use (lagged) central bank policy rates to capture the monetary policy stance. For countries that have implemented unconventional monetary policies during the sample period (Euro Area, U.S., U.K., and Japan), we use the so-called shadow policy rates estimated by Krippner (2016). As a monetary policy tightening is generally found to reduce aggregate demand and increase the cost of borrowing, we expect a negative coefficient.

3.2.3 Baseline Finding

Table 3.4 presents the baseline regression results on the effects of the exchange rate and macroprudential policy on the credit gap.

We show results for the aggregated index of macroprudential policy action, which includes all measures (*iMaPP*) and two subgroups of macroprudential policy, the borrower-based tools (*MaPP_Br*) and the financial institutions-based tools (*MaPP_FI*). The first two columns shown for each group are based on a measure of net tightening (tightening actions net of loosening actions); and the last two columns of each group separate the tightening (T) and loosening actions (L). We emphasize four results:

¹⁶The first quarter carries full weight of the forecasted real GDP growth of current year, based on the forecast in January; the second quarter gives 3/4 weight to the current year and 1/4 weight to the next year, based on the forecast in April; the third quarter gives equal weights to the current year and next year, based on the forecast in July; the fourth quarter carries full weight of the next year, based on the forecast in October.

First, exchange rate movements have a measurable effect on domestic credit developments (column 1). In particular, a 10 percent real exchange rate appreciation is associated with a subsequent increase in the credit gap of 0.5 percentage points of GDP. This is on the same order of magnitude as the median credit gap in the sample (0.33 percent of GDP) and therefore economically meaningful. The size of the effect turns out to be robust across different specifications (with the size of the effect measured as between 0.5–0.6 percentage points) and statistically highly significant (typically at the 1 percent level).

Second, macroprudential policy has a direct effect on domestic credit developments. In particular, a net tightening of macroprudential policy is estimated to decrease the credit gap by 0.875 percentage points of GDP in the next quarter, which again exceeds the median and is roughly 0.08 of the standard deviation of the credit gap. Looking at different groups of macroprudential policy tools, the effect is stronger for borrower-based than financial institutions-based tools (columns 5 and 9), in line with prior literature ([Cerutti et al., 2017a](#); [Fendoğlu, 2017](#)). Considering tightening and loosening actions separately (column 3), the effect is strong and significant for tightening actions (1.168 percentage points of GDP) but insignificant for loosening actions.

Third, in addition to having a direct impact on the credit gap, macroprudential policy has the effect of weakening the extent to which exchange rate movements impact credit developments. This effect is reflected in the coefficient of the interactions term $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$ which shows a strong and statistically significant effect of macroprudential policy in mitigating the effect of the exchange rate on the credit gap. This mitigating effect holds for both the aggregated and the two subgroups of macroprudential policy, that is for both borrower-based and financial institutions-based policies. In economic terms, a one standard deviation increase in $iMaPP$ is estimated to reduce the sensitivity of the credit gap to real exchange rate movements by 0.82 percentage points of GDP. This effect is again stronger for

the borrower-based tools, with a one standard deviation increase in such tools being estimated to reduce the sensitivity of the credit gap to the real exchange rate by 1.4 percentage points of GDP, compared with 0.9 percentage points of GDP for the financial institutions-based tools.

Fourth, and importantly, we find evidence that a prior relaxation of macroprudential policy can have a strong and significant effect in increasing the extent to which an exchange rate shock affects the credit gap. This is evident from looking at the interaction terms for tightening and loosening actions separately (column 4). Columns 8 and 12 reveal that this effects appears to be driven by loosening of financial institutions-based tools rather than borrower-based ones.

As for the control variables, the estimated coefficients have the expected sign and are highly significant: A one percentage point tightening of monetary policy is estimated to reduce the credit gap by about 0.24–0.29 percentage points of GDP, while expectations of improved macroeconomic conditions increase the credit gap, with a one percent improvement in annual forecasted real GDP growth leading to an increase of the credit gap by about 0.45–0.5 percentage points of GDP.

The coefficients of the lagged dependent variable appear close to unity, but a Fisher-type panel unit root test confirms that the credit gap variable is stationary (in at least one panel).¹⁷ Also, the large autoregressive coefficient is partially due to the setting of forward orthogonal deviation transformation in the GMM estimation that is needed to account for unbalanced panels.

Furthermore, the instrument lag choice is validated by the p-value of AR(1) and AR(2) at the bottom of [Table 3.4](#). Results yield small p-values of AR(1) about 2 percent, so the null hypothesis of no first order autocorrelation in first differences is rejected as expected. The

¹⁷See [Phillips & Moon \(2000\)](#) for their review of recent econometric methods of panel unit root tests and their discussion on the inadequacy of those tests.

instrument lag choice yields all AR(2) p-values above the 10 percent threshold (ranging from 23–37 percent), so the null hypothesis of no first order autocorrelation in levels (AR(2)) is not rejected, suggesting the second lags are appropriate instruments for their current values.

3.3 Addressing Endogeneity

A common challenge faced by the literature on the effects of macroprudential policy is the endogeneity problem that arises when an independent variable is correlated with the error term. In our empirical framework, endogeneity can arise in two ways. First, it can be related to potentially omitted variables, such as omitted economic fundamentals that may drive both credit and the exchange rate, thereby inducing biased coefficient estimates. Second, endogeneity can arise from potential reverse causality between macroprudential policy actions and domestic credit ([Galati & Moessner, 2018](#); [Alam et al., 2019](#)). Macroprudential policy actions are not taken in a vacuum, but may be taken in response to macroeconomic and financial developments, which may be the same variables used to assess their effects. In our context, when we want to assess the effect of the policy action on the credit gap, but the credit gap is used as a signal for policy actions by policymakers, this can result in reverse causality. In particular, when high values of the credit gap are likely to prompt a tightening, this could induce a positive correlation that would bias the estimates on the impact of tightening, which are expected negative, up towards zero (attenuation bias).

We mitigate the risk of biased estimates due to endogeneity in three ways, with the first being commonly applied in the literature (e.g., [Claessens et al., 2013](#); [Cerutti et al., 2017a](#)):

- First, already in our baseline set up, we lag the macroprudential indicator and control variables by one-quarter and also include the lagged dependent variable. Estimation is then using the Arellano-Bond difference GMM methodology, which is suitable for independent variables that are not strictly exogenous.

- Second, we focus on the interaction term of $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$. This should suffer less from an endogeneity bias, on the assumption that changes to exchange rates are not commonly taken into consideration when setting macroprudential policy. The change in the exchange rate then functions as exogenous shifter of the effect of prior macroprudential action, reducing the potential endogeneity problem.¹⁸
- Third, and finally, in the absence of suitable instruments, we construct more “exogenous” exchange rate shocks and macroprudential policy shocks, and include them in the main regression. The advantage of this approach is that, by construction, these shocks are orthogonal both to the credit gap and other covariates. We discuss the detailed methodology and result below.

3.3.1 *Distilling Exchange Rate Shocks*

We first focus on potential omitted economic fundamentals that are not included in the baseline regression, but may simultaneously be driving both the real exchange rate and credit developments. For instance, it is conceivable that, in addition to growth expectations, inflation and current account developments affect the exchange rate, and these might also affect the credit gap. This would result in an omitted variable bias in that the coefficient on the exchange rate would reflect both the causal effect and the correlation induced through the omitted fundamentals, thereby overstating the former. While this need not be the case, this bias could conceivably then also affect the interaction term between the exchange rate and macroprudential policy.

¹⁸This reasoning is in line with recent econometric theory developed by [Bun & Harrison \(2019\)](#). These authors study inference in models in which there is both an endogenous (x) and an exogenous explanatory variable (w). Their analysis shows that, under fairly general conditions, the interaction term between x and w can be identified consistently using OLS, even if it is difficult to find valid instruments for the base effect of x on y, and the OLS estimator of this effect is inconsistent.

To address this concern, we attempt to distill more “exogenous” exchange rate shocks for use in our main regressions. The proposed two-stage procedure we use here is similar in spirit to those used in [Auerbach & Gorodnichenko \(2013\)](#) for fiscal policy, [Furceri et al. \(2016\)](#) for monetary policy, and [Ahnert et al. \(2021\)](#) for macroprudential policy. First, we “purge” the impact of domestic fundamental factors on the real exchange rate by running a fixed-effect regression of the exchange rate movement on those domestic fundamentals,¹⁹ and in the second stage we use the residuals from this regression to replace $\Delta^4 RER_{i,t}$ in our baseline dynamic panel regression with those (lagged) “purged shocks.” The first stage regressions have the following forms:

$$\Delta^4 \mathbf{RER}_{i,t} = \beta_1 \Delta^4 \mathbf{Inflation}_{i,t} + \beta_2 \Delta^4 \mathbf{F_RGDP}_{i,t} + \beta_3 \Delta^4 \mathbf{CA_Deficit}_{i,t} + \eta_i + e_{i,t}$$

Where $\Delta^4 Inflation$ is the year-over-year change of the CPI, $\Delta^4 F_RGDP_{i,t-1}$ is the year-over-year real GDP growth, and $\Delta^4 CA_Deficit$ is the year-over-year change in the current account deficit (positive values entail a greater deficit while negative values a move towards surplus). Data availability means that the size of the sample is slightly reduced from 62 to 60 economies when the current account variable is included in the first-step regression.

[Table 3.5](#) shows the results of the first stage. Unsurprisingly, while individual coefficients on some of the fundamentals come out significant, the overall explanatory power of the first-stage regressions is low (R-square is about 0.04), in line with the well-known notion that exchange rate movements are difficult to explain, and are therefore to a considerable extent “random” ([Rossi, 2013](#)). On the other hand we find that both growth forecasts and inflation

¹⁹Results of the second stage regressions are robust given different combinations of domestic fundamental variables considered in the first stage (we have also tried the quarterly change of domestic demand), and they all yield very low explanatory power in the first stage fixed-effect regression (overall R-square less than 0.05).

developments are significant in explaining movements in exchange rates, with the effect of the GDP forecast overall the strongest.

Table 3.6 shows the results of the second stage that uses “exchange rate shocks,” obtained as the residuals from the first-stage regressions, instead of the actual change in the real exchange rate, as a determinant of the credit gap. We find that, while there is a very slight drop in the size of the coefficient on the exchange rate, relative to the baseline, the results on this effect continue to hold strongly. Moreover, the ability of macroprudential policy to affect the credit gap directly and indirectly, by reducing the impact of the exchange rate shocks on credit, continues to hold.²⁰

As an alternative, we have attempted to instrument the domestic real exchange rate fluctuations in the baseline regression using a standard instrumental variable approach, with potential instruments in the first stage regression including time fixed effects, average changes in real exchange rates against the dollar in countries within the same region, as well as changes in commodity prices from Gruss & Kebhaj (2019). We found, however, that the results throughout suffered from a “weak instruments” problem, in that the correlation of the predicted values with the actual change in real exchange rates is low, then reducing also the correlation with credit developments that we investigate in the second stage. We take away from these exercises that country-specific variation in exchange rate movements account for an important part of the co-movement between exchange rates and the credit gap, and that an approach that retains this information is better able to address the endogeneity problem.

Overall, we conclude that it is crucial to control for endogeneity induced by omitted

²⁰A commonly used technique is to bring the omitted variable into the main regression. The disadvantage is that this does not generate information on the drivers of the exchange rate changes. Moreover, it accounts less well for the endogeneity at issue here. Empirically, we find that although the coefficient of forecasted growth is significant in the baseline, when we include inflation and changes in the current account in the main regressions these variables are not significant in affecting the credit gap. The results on other variables are largely unchanged (results not shown).

economic fundamentals in our context. We find that including GDP forecasts in the specification is already a quite powerful way of doing so. Constructing more explicit exchange rate shocks can also be useful, but does not lead to major changes in the estimates when compared to a baseline that already controls for growth expectations.

3.3.2 *Using Policy Shocks*

As set out above, when estimating the effects of macroprudential policy on credit, a further endogeneity concern arises from reverse causality – when macroprudential policy is not random, but reacts to credit developments. When we want to measure the effects of policy action on credit developments, this can lead the estimated coefficients to be biased down towards zero—the so-called attenuation bias (see also [Alam et al., 2019](#)). While our baseline regression already attempts to mitigate this bias, by using the lag of the macroprudential indicator, we here go a step further, by using macroprudential policy “shocks.”

Specifically, the identification of macroprudential policy shocks follows a three-step method closely following [Brandao-Marques et al. \(2020\)](#), and related again also to the literature that computes policy shocks for monetary ([Furceri et al., 2016](#)) and fiscal policy ([Auerbach & Gorodnichenko, 2013](#)):

- Step 1: we estimate an ordered probit model of the macroprudential policy indicator variable²¹ conditional on observables. As independent variables we use the year-over-year change of the quarterly credit-to-GDP gap, the change of the real exchange rate ($\Delta^4 RER_{i,t}$ as used in the baseline), the quarterly change of net capital inflows (as percent of GDP), an indicator of lagged policy actions (the sum of lags one to four of the quarterly macroprudential policy indicator), and a country indicator to capture

²¹For this exercise we recategorize the macroprudential indicator into five outcomes: tightening by two or more actions (+2), tightening by one action (+1), no change (0), loosening by one action (-1), or loosening by two or more actions (-2). We then use an ordered probit that accounts for these five buckets.

cross-sectional heterogeneity. The ordered-probit (first stage regression) is shown in [Table 3.7](#).

- Step 2: we compute the “expected” macroprudential policy stances using the probabilities obtained from the ordered probit regression conditional on the independent variables.
- Step 3: we compute the macroprudential policy shocks as the actual macroprudential indicators minus their expected values. Thus, positive values represent tightening shocks and negative values represent loosening shocks.

By construction, the shocks are orthogonal to credit developments, as measured by the past changes in the credit gap, helping to reduce endogeneity. In addition, importantly, they are orthogonal to exchange rate changes, thereby mitigating concerns that macroprudential policy might respond to exchange rate movements.

When we replace the variable $MaPP_{i,t-1}$ in the baseline regression with the macroprudential policy shocks identified above ($MaPP_Shock_{i,t-1}$), the estimation result, shown in [Table 3.8](#), is roughly consistent with the baseline. In particular, all interaction terms are essentially the same. However, at the margin we find that the base effects of macroprudential action are measured larger and more significant compared to the baseline. This holds in particular for the coefficients on the aggregate indicator and the financial institutions-based indicator, suggesting that these coefficients are measured with less bias when using the shocks.

When inspecting the first-step regressions ([Table 3.7](#)), consistent with this, we find that the coefficient on the change in the credit gap is sizable and statistically significant at the five and one per cent levels in the regressions explaining the overall and financial-institutions-based indicators, respectively, while the credit gap is not significant in the regressions ex-

plaining the use of borrower-based tools. This suggests that the former actions respond more strongly to credit developments, relative to the borrower-based tools, creating a greater potential for attenuation bias in the measurement of policy effects. As regards the first-step results, it is worth noting also that the change in the exchange rate does not enter statistically significant in any of the regressions, in line with our prior that macroprudential policy does not tend to react to movements in the exchange rate.

Overall, we find that when using macroprudential policy shocks, the attenuation bias from reserve causality is reduced. This affects mainly the base effect of macroprudential policy, and in particular the financial-institutions-based tools while interactions of all types of tools with the change in the exchange rate are not affected substantially when using macroprudential policy shocks in place of the indicator we have in the baseline.

3.4 Extension: The Feedback Effect from Credit to Capital Inflows

Up until now, we have focused on the role of exchange rate shocks in driving domestic credit developments and the ability of macroprudential policy to attenuate these effects. This section extends our analysis to examine feedback effects that run from credit developments, and policy levers that may be deployed to affect them, to specific types of capital inflows (right-hand side of [Figure 3.1](#)). More specifically, we hypothesize that both strong domestic credit demand and policies that tamp down domestic credit—potentially both monetary and macroprudential policy—can lead to increases in the so-called “gross other investment inflows” that capture cross-border borrowing by domestic financial institutions and non-financial corporates. Evidence in favor of this hypothesis would shine a light on the unintended spillover effects of these domestic policies. In this context, we also examine whether targeted capital controls can reduce gross other investment inflows and thereby reduce the feedback effect.

As noted already, there is likely to be a two-way causation between capital flows and credit. On the one hand, strong credit demand may cause domestic financial institutions and corporates to “pull in” capital from abroad, as domestic funding sources are exhausted or constrained (Avdjiev et al., 2012; Hahm et al., 2013). On the other hand, where capital inflows are strong for other reasons, this may reduce the cost of domestic credit and thereby “push up” domestic credit supply (Mendoza & Terrones, 2012). In general equilibrium, both these effects would lead us to observe a positive correlation between measures of credit and measures of capital inflows.

In our analysis we regress gross other inflows on the lagged credit gap as one way of controlling for reverse causality. In addition, and importantly, we include quarter time-fixed effects to account separately for all global push factors. Inclusion of these quarter fixed effects should render the variation in “other capital flows” that is left to be explained by the credit gap (and other domestic variables) orthogonal to global push factors that might lead to common variations in capital inflows driving changes in the supply of domestic credit.²² We acknowledge, however, that it is difficult to rule out that the coefficient on the credit gap still captures a “push” effect that runs from inflows to credit to some extent.²³ We therefore prefer to interpret the coefficient on the credit gap in the analysis below as measuring a conditional correlation that is consistent with a feedback relationship, rather than measuring a causal effect.

This analysis is embedded in otherwise standard “push-and-pull” capital flow regressions

²²An alternative way to understand this point is to imagine that we were to run a two-step estimation, where other capital inflows are first regressed on a set of time-fixed effects, in order to purge the outcome variable of the effects of global push factors, and where the residual would be used in a second step regression on domestic pull factors, including the credit gap. Basic econometric theory, known as the “Frisch-Waugh-Lovell Theorem” implies that the coefficients of the second step regression would be the same as those shown in our results. See further (Davidson & MacKinnon, 1993, page 19).

²³For instance, it is possible that different countries have different sensitivity to global push factors. Our regression set-up cannot control for that, since our data are at the country level, and therefore do not offer sufficient degrees of freedom to include interactions between time- and country-fixed effects.

with domestic factors that have been identified in the existing literature as driving capital inflows, including monetary policy and growth expectations, as well as the “catch-all” push factor discussed above. The main variables we use are the following:

Capital inflows (CFLOW): As our dependent variable, we consider gross other investment inflows.²⁴ These types of flows are most likely to reflect unintended effects from the domestic policies under investigation, since they have been found to exhibit the most robust positive association with domestic credit unconditionally (IMF, 2017; Igan & Tan, 2017). The capital inflows data are expressed in terms of gross other investment inflows within quarter t as a percent of GDP in the previous quarter $t-1$. The data source is the IMF Financial Flows Analytics (FFA) database.

Push factors: As discussed above, we include quarterly time-fixed effects (μ_t), following Ahnert et al. (2021), to account for global “push” factors that affect all countries equally to the fullest possible extent. The benefit of these time fixed effects is that they control all global factors that are common across countries in each period. This can include changes in global risk aversion, the monetary policy stance in advanced economies, and other variables that may be difficult to observe. Time-fixed effects can help control all these push factors in a parsimonious way.

Pull factors: In line with the existing literature, we include monetary policy and the forecasted real GDP growth as domestic pull factors (with variable definitions and data sources as before). These variables are meant to capture to what extent tight monetary policy and a positive GDP growth outlook attract cross-border flows.

Capital controls (FARI): Different from the majority of previous studies that consider

²⁴Gross inflows are net purchases of domestic assets by foreign agents while net flows subtract net purchases of foreign assets by domestic agents. Other investment inflows capture all other investments flows than direct investment, portfolio equity and debt, financial derivatives, and reserves. It includes currency and deposits, loans, insurance and pensions, trade credit and advances, other accounts payable, and SDR allocations.

broad-based measures of capital controls and apply indicators on inflow and outflow restrictions of all types of flows, we use an index of controls that are targeted at the type of “other investment (banking and corporate) inflows” we investigate in the regression. The relevant “Financial Accounts Restrictiveness Index” (FARI) is compiled based on source data from the IMF’s Annual Report on Exchange Arrangements and Exchange Restrictions (AREAER). Following [Klein \(2012\)](#), we distinguish between long-standing capital controls (“walls”), measured as the level of the index on “other investment inflows” for each country in a given quarter, and temporary adjustments (“gates”), measured as incremental changes (+1 for net tightening and -1 for net loosening actions) in such controls.

Our regressions equations are given below. We regress gross other capital flows on the credit gap (Y), lagged by one quarter, and we also include lagged policy variables, with separate regressions for long-standing controls and episodic controls. As explained further just below, our main interest is on the interaction effect between measures of capital controls (i.e., the walls and the gates) and the credit gap: do such controls reduce the extent to which high domestic credit stimulates direct borrowing from abroad?

[Cerdeiro & Komaromi \(2021\)](#) point out the difficulty of identifying empirically the effects of capital controls. On the one hand, these controls do not vary much over time, reducing the power of standard fixed effects regressions. On the other hand, their level could be correlated with a number of country-specific factors, exposing a random effects regression to potential omitted variables. They argue in favor of an identification through interaction effects, by showing in a simple model that capital controls not only affect the unconditional mean of flows, but importantly the sensitivity of flows to various push and pull factors; a point that is usually not exploited in regressions that neglect interactions and assume an additive linear effect of capital controls on the level of capital flows.

Our regressions equations are given below. We regress gross other capital flows on the

credit gap (Y), lagged by one quarter, and we also include lagged policy variables, with separate regressions for long-standing controls and episodic controls.

- The “Walls” Effect of Capital Controls (levels, $FARI$) + $MaPP$

$$\begin{aligned} \mathbf{CFLOW}_{i,t} = & \rho \mathbf{CFLOW}_{i,t-1} + \beta_1 \mathbf{Y}_{i,t-1} + \beta_2 \mathbf{FARI}_{i,t-1} + \beta_3 \mathbf{Y}_{i,t-1} \times \mathbf{FARI}_{i,t-1} \\ & + \beta_4 \mathbf{MAPP}_{i,t-1} + \beta_5 Y_{i,t-1} \times \mathbf{MAPP}_{i,t-1} + \beta_6 \mathbf{MPS}_{L,t-1} + \beta_7 \mathbf{Y}_{l,t-1} \times \mathbf{MPS}_{i,t-1} \\ & + \theta \Delta^4 \mathbf{F_RGDP}_{i,t} + \theta_t \mu_t + \theta_i \alpha_i + v_{i,t} \end{aligned}$$

- The “Gates” Effect of Capital Controls (1-quarter change, $\Delta FARI$) + $MaPP$

$$\begin{aligned} \mathbf{CFLOW}_{i,t} = & \rho \mathbf{CFLOW}_{i,t-1} + \beta_1 \mathbf{Y}_{i,t-1} + \beta_2 \Delta \mathbf{FARI}_{i,t-1} + \beta_3 \mathbf{Y}_{i,t-1} \times \Delta \mathbf{FARI}_{i,t-1} \\ & + \beta_4 \mathbf{MAPP}_{i,t-1} + \beta_5 Y_{i,t-1} \times \mathbf{MAPP}_{i,t-1} + \beta_6 \mathbf{MPS}_{L,t-1} + \beta_7 \mathbf{Y}_{l,t-1} \times \mathbf{MPS}_{i,t-1} \\ & + \theta \Delta^4 \mathbf{F_RGDP}_{i,t} + \theta_t \mu_t + \theta_i \alpha_i + v_{i,t} \end{aligned}$$

Following [Cerdeiro & Komaromi \(2021\)](#), we include country-fixed effects, in addition to the time-fixed effects, in order to control as tightly as possible for time-invariant omitted variables at the country level.²⁵ For identification, our main interest is then on the interaction effect $\mathbf{Y}_{i,t-1} \times \Delta \mathbf{FARI}_{i,t-1}$ between the credit gap and measures of capital controls: do such controls reduce the extent to which high domestic credit stimulates direct borrowing from abroad? In addition, we include interaction terms to analyze how other policies interact with the credit gap (the domestic pull factor of interest) in affecting the level of other investment

²⁵The random effect regression is considered in a robustness check (see Appendix B). It is based on the de-meaned variables (of capital inflows and monetary policy stance) and shock variables (of macroprudential policy shocks, and the shock of capital inflow control for the “gate” effect only). The average credit-to-GDP ratio over sample period by country is considered as an additional country-specific “pull” factor in the random effects regressions.

flows.

The estimation here does not use the previously applied dynamic panel (GMM) model, in the main since the assumption of autocorrelation within individual panel’s error terms $v_{i,t}$ is not satisfied in the estimated equation of the feedback effect. The p-value of AR(1) test ranges from 0.62 to 0.8, implying strong evidence against the first order serial correlation of residuals in differences, and thus would invalidate the moment conditions used in the dynamic panel estimation (Roodman, 2009).²⁶

Throughout, we include all three policy variables: monetary policy, macroprudential policy, and capital controls. Table 3.9 has results for a measure of the breadth of capital account restrictions across the relevant category (the “walls” effect in the language of Klein (2012)), while Table 3.10 has results for incremental changes in such controls (the “gates” effect). Appendix Table B1 reports results for macroprudential policy shocks that account for reverse causality (as above in subsection 3.3.2); and Appendix Table B2 reports the finding of a robustness check applying the random effects approach.

There are four noteworthy findings: First, we find evidence that supports the idea of a feedback effect from credit to other investment inflows that arises when a strong domestic credit cycle leads to greater cross-border borrowing by banks and corporates. As indicated by the coefficients of Y_{t-1} , a 10 percentage points of GDP increase in the domestic credit gap (close to a one-standard deviation change) is associated with a rise of cross-border capital inflows in the subsequent quarter, ranging from 0.9 to 1.3 percentage points of GDP.

Second, we find that a tight monetary policy stance also leads to a higher level of cross-

²⁶The trade-off is that we may not fully account for the Nickell bias in the estimation of the effect on the lagged dependent variable. The semi-asymptotic bias (assuming N goes infinity) for our sample $T=68$ is -0.0262 for an autoregressive coefficient equals to 0.72 in the extension section, compared with the bias of -0.0391 in the baseline given its autoregressive coefficient of 0.98. Although we are not able to quantitatively assess the degree of smearing effect arises from the endogeneity for the two cases, it is reasonable to assume that the smearing effect in the feedback regression is smaller and may not be a major concern.

border capital inflows in the subsequent quarter. A 1 percentage point increase in the policy rate is associated with an increase of about 1.7 percentage points in the ratio of other investment inflows to GDP. The coefficients on interactions between monetary policy and credit are not significantly different from zero (coefficients are very close to zero and with relatively large standard errors). This implies that the effect of monetary policy on inflows does not appear to depend on whether the level of the domestic credit gap is high or low.

Third, we find that macroprudential policy tightening is also associated with increases in other investment flows, at the margin, with the result being significant for borrower-based tools and in particular when using macroprudential policy shocks (see Appendix Table B1). This is in line with the idea that domestic macroprudential policy tightening is associated with cross-border leakages, that is, tends to further increase borrowing from abroad. An average tightening action of borrower-based tools is found to lead to an increase in cross-border borrowing on the order of 1.3–1.5 percentage points of GDP. This is intuitive as the borrower-based tools restrict borrowing from domestic banks, which can be circumvented by borrowing directly from abroad. On the other hand, financial institution-based tools do not tend to have such strong effects on the domestic credit provision, potentially reducing incentives for circumvention. This might explain why we find them to have an insignificant role in affecting other investment flows.

Fourth, and in contrast, targeted long-standing capital controls (“walls”) can have sizeable effects in reducing cross-border funding, in particular by affecting the interactions between credit and these other capital inflows. The coefficients of the direct impact of long-standing controls already is negative and significant (at the 10 percent significance level), and these controls also have a significant indirect effect in mitigating the impact of a high credit gap in increasing cross-border funding (again significant at the 10 and sometimes 5 percent

level).²⁷ Hence, it appears that these long-standing targeted controls are indeed acting as a “wall” when the credit “tide” is up and risks pulling in additional capital from abroad.

As for the episodic controls (Table 3.10), both the direct effect and indirect effect of capital controls have the expected negative coefficients, although they are not always statistically significant. When we use “shocks” to capital controls, the statistical significance of the base effect improves in some regressions (see Appendix Table B2), pointing to the presence of a reverse causality bias that needs to be addressed.²⁸ However, the interaction effects between episodic controls and the credit gap remain insignificant throughout. The associated standardized coefficients of episodic controls (added in blue) are also much smaller than those found for the long-standing controls. These findings are in line with prior evidence suggesting that the effects of incremental changes in capital controls (“gates”) are weaker, relative to the effect of pre-existing controls (“walls”).

Overall, this evidence suggests that macroprudential policy and capital controls have a complementary role in mitigating systemic risk, with long-standing and targeted capital controls useful when strong domestic credit pulls in additional capital inflows and this cannot be controlled by macroprudential measures and would be exacerbated by monetary tightening. Our comparison of “walls” and “gates” also suggests that when targeted controls are applied to specific types of flows to complement domestic macroprudential measures, these controls may be more effectively deployed in a precautionary manner, ahead of the occurrence of a surge in such flows.

²⁷These results broadly hold for the random effect model, although the direct effects of capital controls are smaller and less significant.

²⁸Shocks to capital controls are computed in a manner analogous to the way we construct macroprudential policy shocks, by first running an ordered probit of the gates measure of controls on the same set of variables used to compute macroprudential shocks, including the credit gap, the change in the real exchange rate and the net capital inflow, and then using the difference between the actual value on the indicator and its conditional expectation.

3.5 Conclusion and Policy Implication

This paper examines the effectiveness of macroprudential policies in attenuating the impact of real exchange rate movements on domestic credit cycles. Our main result is that macroprudential policy is effective not only in dampening domestic credit, but also in reducing the procyclical impact of external shocks on credit – a novel finding in the literature.

Using dynamic panel regression and a new and comprehensive dataset of macroprudential policy measures that covers a large sample of 62 economies over the period of 2000:Q1-2016:Q4 period, we find robust evidence that exchange rate appreciation is associated with subsequent increases in the credit-to-GDP gap – a well-known early warning indicator of future financial crises. Importantly, we find that tighter macroprudential policies can mitigate this effect, thereby insulating the economy from procyclical effects of external shocks on credit. These findings are robust to employing a range of methods to address endogeneity of the change in the exchange rate or the use of macroprudential measures.

We also find evidence supporting the effectiveness of targeted capital inflow controls, especially long-standing ones, in dampening a feedback effect that arises when strong domestic credit leads to increases in borrowing from abroad, and tighter monetary and macroprudential policies would further stoke such cross-border borrowing. This points to the benefits of a potential complementary use of macroprudential policies and capital inflow controls, even as alternatives and tradeoffs should be considered carefully.

Table 3.1: Description of Variables

Variable	Symbol	Description	Sources
<u>Dependent variable:</u>			
Credit-to-GDP gap	Y	Calculated by one-sided HP filter using smoothing parameter=400,000. ^(a) ^(b)	IMF IFS; BIS; central banks
<u>Independent variables:</u>			
Macroprudential policy stance	$MaPP$ T_MaPP L_MaPP	+1 represents a tightening action is taken within the quarter; -1 represents a loosening action is taken within the quarter; 0 represents no policy action is taken within the quarter. <i>iMaPP</i> is the aggregated measure, <i>MaPP_{Br}</i> is the borrower-based measure, and <i>MaPP_{FI}</i> is the financial institutions-based measure.	IMF iMaPP Database (Alam et al. 2019)
YoY change of real exchange rate	$\Delta^4 RER$	Year-over-year log change of weighted average ^(c) of quarterly bilateral nominal exchange rate in NC per USD, deflated by U.S. CPI against domestic CPI. Positive value = depreciation against USD.	IMF IFS
<u>Control variables:</u>			
Monetary policy stance	MPS	Central bank policy rates. Use shadow policy rates for Euro Area, U.S., U.K., and Japan.	IMF IFS, BIS, and Krippner (2016)
Forecasted YoY real GDP growth	$\Delta^4 F_RGDP$	Year-over-year quarterly log change of forecasted real GDP growth. It is a weighted average of current year's and next year's forecasted growth rates ^(d)	Consensus Forecast, IMF WEO
<u>Other variables:</u>			
YoY real GDP growth	$\Delta^4 RGDP$	Year-over-year log change of quarterly real GDP.	IMF WEO
Capital inflows	$CFLOW$	Gross other investment inflows within quarter t (as percent of GDP in $t-1$).	IMF FFA
Capital control	$FARI$	Financial account restriction index on gross other investment inflows, range 0-1 (higher values indicate more restrictive system).	IMF AREAER

Note: (a) Initially take the first 24 quarters, then compute the trend and cyclical components recursively (add one quarter at a time).

(b) We use the financial corporations' domestic claims on private sector from IMF IFS where available, otherwise, we use depository corporations (or monetary) domestic claims on private sector from IMF IFS. The credit data of Iceland and Taiwan POC are from the central banks.

(c) The weighted average of variable X is calculated as $X_{WA} = 0.4 \times X + 0.3 \times l1.X + 0.2 \times l2.X + 0.1 \times l3.X$

(d) See footnote 15.

Table 3.2: Summary of Statistics

Variable	Mean	Median	St. Dev	Min	P25	P75	Max	Obs
<i>Y</i>	-1.227	0.330	11.785	-99.806	-2.970	3.572	63.642	4,112
$\Delta^4 RER$	-0.502	-0.675	9.198	-23.474	-6.246	4.844	25.934	4,152
<i>MPS</i>	4.172	3.301	5.159	-5.728	1.000	6.000	26.000	4,083
$\Delta^4 RGDP$	3.256	3.199	3.676	-8.781	1.319	5.504	12.296	4,212
$\Delta^4 F_RGDP$	3.169	3.071	2.292	-3.517	1.776	4.613	9.225	4,216
<i>CFLOW</i>	5.514	1.502	25.824	-60.800	-1.702	6.632	169.592	4,120
<i>FARI</i>	0.181	0.167	0.166	0	0.083	0.250	0.750	3,904
$\Delta FARI$	-0.001	0.000	0.025	-0.333	-0.250	0.250	0.500	3,903
<i>iMaPP</i>	0.095	0.000	0.626	-4	0	0	6	4,216
<i>MaPP_Br</i>	0.020	0.000	0.229	-2	0	0	3	4,216
<i>MaPP_FI</i>	0.070	0.000	0.543	-4	0	0	5	4,216

Source: Authors' calculations.

Note: We winsorize the top and bottom 1% observations of each variable except the dependent variable *Y* and the categorical variable *MaPP*.

Table 3.3: Definition of Macroprudential Policy Instruments

	Instrument	Definition
1	CCB	A requirement for banks to maintain a countercyclical capital buffer. Implementations at 0% are not considered as a tightening in dummy-type indicators.
2	LVR	A limit on leverage of banks, calculated by dividing a measure of capital by the bank's non-risk-weighted exposures.
3	LLP	Loan loss provision requirements, which include dynamic provisioning and sectoral provisions (e.g. for impaired or housing loans).
4	Capital	Capital requirements for banks, which include risk weights, systemic risk buffers, and minimum capital requirements. Countercyclical capital buffers and capital conservation buffers are captured in their sheets respectively and thus not included here.
5	LTV	Limits to the loan-to-value ratios, mostly targeted at housing loans, but also includes those targeted at automobile loans.
6	DSTI	Limits to the debt-service-to-income ratio and the loan-to-income ratio, which restrict the size of debt services or debt relative to income.
7	LFC	Limits on foreign currency (FC) lending, and rules or recommendations on FC loans.
8	RR	Reserve requirements (on domestic or foreign currency) for macroprudential purposes.
9	Tax	Taxes and levies applied to specified transactions, assets, or liabilities, which include stamp duties, capital gain taxes, and levies on banks' noncore funding.
10	SIFI	Identification of and additional buffer requirements for global and domestic systemically important financial institutions.
11	Liquidity	Regulations for liquidity and funding risks, including minimum requirements for liquidity coverage ratios, liquid asset ratios, net stable funding ratios, and core funding ratios.
12	LTD	Limits to the loan-to-deposit (LTD) ratio and penalties for high LTD ratios.
13	LFX	Limits on net or gross open foreign exchange (FX) positions, limits on FX exposures and FX funding, and currency mismatch regulations.
14	LCG	Limits on growth or the volume of aggregate credit, the household-sector credit, or the corporate-sector credit by banks, and penalties for high credit growth.
15	Conservation	Requirements for banks' capital conservation buffers.
16	LoanR	Loan restrictions, that are more tailored than those captured in "14.LCG". They include loan limits and prohibitions, which may be targeted at loan characteristics (e.g., the maturity, the size, the LTV ratio and the type of interest rate of loans), bank characteristics (e.g., mortgage banks), and other factors. Restrictions on foreign currency lending are captured in "7.LFC". External debt restrictions for banks are captured in "13. LFX".
17	Other	Explicitly macroprudential regulation not included elsewhere, such as stress testing, restrictions on profit distribution and limits on foreign investment.

Source: Alam et al. (2019). We exclude SIFI from the original database to construct the aggregated macroprudential policy measure because this paper focuses on the time dimension of systemic risk.

Table 3.4: Baseline Result

Variables	iMaPP				Borrower-based tools				Financial institutions-based tools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y_{t-1}	0.982*** (0.020)	0.989*** (0.018)	0.986*** (0.020)	0.991*** (0.018)	0.988*** (0.020)	0.988*** (0.019)	0.987*** (0.019)	0.988*** (0.017)	0.983*** (0.021)	0.989*** (0.019)	0.987*** (0.021)	0.991*** (0.020)
$\Delta^4 RER_{t-1}$	-0.050** (0.021)	-0.054*** (0.018)	-0.053** (0.022)	-0.052*** (0.019)	-0.053** (0.021)	-0.060*** (0.018)	-0.054** (0.021)	-0.056*** (0.019)	-0.051** (0.021)	-0.053*** (0.020)	-0.054** (0.021)	-0.050*** (0.019)
$MaPP_{t-1}$	-0.875* (0.463)	-0.737 (0.461)			-2.231** (1.016)	-1.990* (1.087)			-0.416 (0.502)	-0.365 (0.397)		
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$		0.144*** (0.048)				0.253* (0.151)				0.148*** (0.049)		
T_MaPP_{t-1}			-1.168** (0.524)	-0.852* (0.503)			-2.000 (1.395)	-1.620 (1.597)			-1.149 (0.728)	-0.810 (0.624)
$T_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.130** (0.061)				0.270** (0.135)				0.112* (0.067)
L_MaPP_{t-1}			0.027 (0.754)	-0.630 (0.709)			-3.034** (1.535)	-2.886 (1.878)			0.504 (1.184)	-0.028 (0.934)
$L_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.159*** (0.042)				0.224 (0.201)				0.160*** (0.052)
MPS_{t-1}	-0.269*** (0.074)	-0.246*** (0.069)	-0.264*** (0.073)	-0.245*** (0.073)	-0.290*** (0.073)	-0.273*** (0.072)	-0.285*** (0.073)	-0.290*** (0.074)	-0.262*** (0.069)	-0.240*** (0.072)	-0.263*** (0.071)	-0.249*** (0.072)
$\Delta^4 F_RGDP_{t-1}$	0.504*** (0.077)	0.462*** (0.085)	0.503*** (0.080)	0.453*** (0.089)	0.496*** (0.074)	0.453*** (0.079)	0.492*** (0.073)	0.473*** (0.076)	0.465*** (0.077)	0.447*** (0.078)	0.473*** (0.082)	0.457*** (0.083)
Observations	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
AB AR(1) test - p value	0.00183	0.000741	0.00157	0.000754	0.00180	0.000833	0.00160	0.000735	0.00279	0.00133	0.00184	0.00121
AB AR(2) test - p value	0.394	0.257	0.264	0.250	0.350	0.255	0.373	0.278	0.390	0.297	0.281	0.253
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.5: “Purging” the Real Exchange Rate

	(1)	(2)
	FE	FE
$\Delta^4 \text{Inflation}_t$	0.252*** (0.061)	0.260*** (0.069)
$\Delta^4 F_RGDP_t$	-2.111*** (0.166)	-2.215*** (0.182)
$\Delta^4 CA_Deficit_t$		-0.000 (0.000)
Constant	5.259*** (0.612)	5.211*** (0.650)
Observations	4,151	3,804
R-squared	0.041	0.040
# of Economies	62	60

Source: Authors' calculations.

Note:

(a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) FE regression (1): $\Delta^4 \text{RER}_{i,t} = \beta_1 \Delta^4 \text{Inflation}_{i,t} + \beta_2 \Delta^4 \text{RGDP}_{i,t} + \eta_i + e_{i,t}$

(c) FE regression (2): $\Delta^4 \text{RER}_{i,t} = \beta_1 \Delta^4 \text{Inflation}_{i,t} + \beta_2 \Delta^4 \text{RGDP}_{i,t} + \beta_3 \Delta^4 \text{CA_Deficit}_{i,t} + \eta_i + e_{i,t}$

Table 3.6: Robustness — Results with “Purged” Exchange Rate Shocks

Variables	Baseline				$\Delta^4 RER$ = Residuals from FE Regression (1)				$\Delta^4 RER$ = Residuals from FE Regression (2)			
	iMaPP				iMaPP				iMaPP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\Delta^4 RER_{t-1}$	-0.050** (0.021)	-0.054*** (0.018)	-0.053** (0.022)	-0.052*** (0.019)	-0.044** (0.022)	-0.054*** (0.021)	-0.044** (0.022)	-0.046** (0.020)	-0.033* (0.020)	-0.043** (0.019)	-0.033* (0.019)	-0.039** (0.018)
$MaPP_{t-1}$	-0.875* (0.463)	-0.737 (0.461)			-0.866** (0.414)	-1.351*** (0.512)			-0.940* (0.481)	-1.461*** (0.424)		
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$		0.144*** (0.048)				0.206** (0.085)				0.188** (0.078)		
T_MaPP_{t-1}			-1.168** (0.524)	-0.852* (0.503)			-1.161** (0.471)	-1.566*** (0.592)			-1.476*** (0.515)	-2.034*** (0.618)
$T_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.130** (0.061)				0.168** (0.067)				0.157** (0.071)
L_MaPP_{t-1}			0.027 (0.754)	-0.630 (0.709)			0.009 (0.832)	-0.711 (0.927)			0.293 (0.898)	-0.570 (0.786)
$L_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.159*** (0.042)				0.257 (0.159)				0.281* (0.149)
Observations	3,842	3,842	3,842	3,842	3,841	3,841	3,841	3,841	3,544	3,544	3,544	3,544
# of Economies	62	62	62	62	62	62	62	62	60	60	60	60

Source: Authors' calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) FE regression (1): $\Delta^4 RER_{i,t} = \beta_1 \Delta^4 Inflation_{i,t} + \beta_2 \Delta^4 RGDP_{i,t} + \eta_i + e_{i,t}$ (c) FE regression (2): $\Delta^4 RER_{i,t} = \beta_1 \Delta^4 Inflation_{i,t} + \beta_2 \Delta^4 RGDP_{i,t} + \beta_3 \Delta^4 CA_Deficit_{i,t} + \eta_i + e_{i,t}$

Table 3.7: Results from Ordered Probit Regression

	iMaPP	MaPP_Br	MaPP_FI
VARIABLES	(1)	(2)	(3)
$\Delta^4 Y_{t-1}$	0.009** (0.004)	0.006 (0.006)	0.011*** (0.004)
$\Delta^4 RER_{t-1}$	0.001 (0.001)	0.001 (0.002)	0.001 (0.001)
$\Delta^4 NCFLOW_{t-1}$	0.000 (0.000)	0.000 (0.001)	0.000 (0.000)
$\sum_{s=-4}^{-1} MaPP_s$	0.110*** (0.014)	0.029 (0.021)	0.117*** (0.014)
Constant cut1	-2.076*** (0.169)	-2.882*** (0.335)	-2.165*** (0.175)
Constant cut2	-1.383*** (0.163)	-2.167*** (0.308)	-1.483*** (0.168)
Constant cut3	1.490*** (0.163)	2.168*** (0.308)	1.585*** (0.168)
Constant cut4	2.193*** (0.166)	2.950*** (0.319)	2.345*** (0.173)
Observations	3,699	3,699	3,699

Source: Authors' calculations.

Note:

(a) Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(b) The constant cut points represent the thresholds of predicted cumulative normal distribution of the dependent variable corresponding to its different categorical values.

Table 3.8: Robustness — Results with Macroprudential Policy Shocks

Variables	iMaPP				Borrower-based tools				Financial institutions-based tools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y_{t-1}	0.988*** (0.019)	0.993*** (0.017)	0.989*** (0.019)	0.991*** (0.017)	0.993*** (0.018)	0.990*** (0.017)	0.992*** (0.019)	0.991*** (0.018)	0.989*** (0.019)	0.993*** (0.018)	0.988*** (0.019)	0.989*** (0.015)
$\Delta^4 RER_{t-1}$	-0.058*** (0.020)	-0.055*** (0.019)	-0.057*** (0.021)	-0.053*** (0.019)	-0.054** (0.022)	-0.058*** (0.021)	-0.056** (0.023)	-0.058*** (0.022)	-0.057** (0.023)	-0.054*** (0.020)	-0.060*** (0.020)	-0.053*** (0.019)
$MaPP_Shock_{t-1}$	-1.088** (0.490)	-1.240** (0.529)			-1.988** (0.884)	-1.891* (0.986)			-1.157** (0.562)	-1.343** (0.576)		
$MaPP_Shock_{t-1} \times \Delta^4 RER_{t-1}$		0.178*** (0.048)				0.240* (0.136)				0.192*** (0.050)		
$T_MaPP_Shock_{t-1}$			-2.090*** (0.702)	-1.979** (0.797)			-1.970 (1.247)	-1.658 (1.389)			-2.870*** (0.931)	-2.522** (1.069)
$T_MaPP_Shock_{t-1} \times \Delta^4 RER_{t-1}$				0.215*** (0.077)				0.317* (0.181)				0.251** (0.107)
$L_MaPP_Shock_{t-1}$			0.914 (0.814)	0.392 (0.795)			-2.371 (1.455)	-1.865 (1.650)			1.784 (1.359)	1.161 (1.207)
$L_MaPP_Shock_{t-1} \times \Delta^4 RER_{t-1}$				0.126*** (0.044)				0.012 (0.203)				0.116** (0.047)
MPS_{t-1}	-0.315*** (0.080)	-0.276*** (0.074)	-0.269*** (0.081)	-0.250*** (0.075)	-0.312*** (0.068)	-0.301*** (0.073)	-0.315*** (0.078)	-0.289*** (0.079)	-0.287*** (0.079)	-0.263*** (0.076)	-0.249*** (0.082)	-0.221*** (0.075)
$\Delta^4 F_RGDP_{t-1}$	0.469*** (0.084)	0.452*** (0.088)	0.450*** (0.091)	0.426*** (0.089)	0.443*** (0.072)	0.459*** (0.077)	0.450*** (0.076)	0.470*** (0.079)	0.485*** (0.077)	0.466*** (0.084)	0.461*** (0.088)	0.432*** (0.088)
Observations	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
AB AR(1) test - p value	0.00341	0.000832	0.00135	0.000387	0.00422	0.00210	0.00411	0.00184	0.00348	0.000896	0.000471	0.000253
AB AR(2) test - p value	0.640	0.439	0.311	0.203	0.583	0.434	0.592	0.506	0.625	0.424	0.271	0.162
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Leakages and the “Walls” Effect of Capital Controls
(The Fixed Effect Model)

	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
The “Walls” Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.713*** (0.032) <i>0.713</i>	0.713*** (0.032) <i>0.713</i>	0.711*** (0.032) <i>0.711</i>	0.713*** (0.032) <i>0.713</i>	0.713*** (0.033) <i>0.713</i>	0.713*** (0.032) <i>0.713</i>	0.711*** (0.032) <i>0.711</i>	0.713*** (0.032) <i>0.712</i>	0.713*** (0.032) <i>0.712</i>	0.713*** (0.032) <i>0.712</i>	0.711*** (0.032) <i>0.711</i>	0.713*** (0.032) <i>0.712</i>	0.713*** (0.032) <i>0.712</i>
Y_{t-1}	0.091** (0.037) <i>0.057</i>	0.090** (0.037) <i>0.057</i>	0.128** (0.044) <i>0.081</i>	0.088** (0.037) <i>0.055</i>	0.103* (0.048) <i>0.065</i>	0.089** (0.037) <i>0.056</i>	0.126** (0.045) <i>0.080</i>	0.086** (0.036) <i>0.054</i>	0.102* (0.048) <i>0.065</i>	0.091** (0.037) <i>0.058</i>	0.129** (0.044) <i>0.082</i>	0.092** (0.038) <i>0.058</i>	0.104* (0.048) <i>0.066</i>
$FARI_{t-1}$	-4.933* (2.341) <i>-0.045</i>	-5.004* (2.356) <i>-0.046</i>	-6.116* (3.110) <i>-0.056</i>	-5.014* (2.346) <i>-0.046</i>	-5.218* (2.419) <i>-0.048</i>	-5.161* (2.386) <i>-0.047</i>	-6.251* (3.144) <i>-0.057</i>	-5.298* (2.403) <i>-0.048</i>	-5.378* (2.459) <i>-0.049</i>	-4.907* (2.342) <i>-0.045</i>	-6.034* (3.114) <i>-0.055</i>	-4.919* (2.362) <i>-0.045</i>	-5.127* (2.409) <i>-0.047</i>
$MaPP_{t-1}$		0.144 (0.096) <i>0.004</i>	0.130 (0.089) <i>0.004</i>	0.091 (0.189) <i>0.003</i>	0.134 (0.102) <i>0.004</i>	1.474** (0.461) <i>0.044</i>	1.443** (0.466) <i>0.043</i>	1.159 (0.656) <i>0.035</i>	1.466** (0.461) <i>0.044</i>	-0.076 (0.169) <i>-0.002</i>	-0.090 (0.160) <i>-0.003</i>	-0.048 (0.172) <i>-0.001</i>	-0.089 (0.175) <i>-0.003</i>
MPS_{t-1}	0.174** (0.074) <i>0.047</i>	0.174** (0.074) <i>0.047</i>	0.170** (0.071) <i>0.046</i>	0.175** (0.074) <i>0.048</i>	0.165** (0.071) <i>0.045</i>	0.176** (0.074) <i>0.048</i>	0.171** (0.072) <i>0.047</i>	0.176** (0.075) <i>0.048</i>	0.167** (0.072) <i>0.045</i>	0.174** (0.074) <i>0.047</i>	0.169** (0.071) <i>0.046</i>	0.173** (0.073) <i>0.047</i>	0.165** (0.071) <i>0.045</i>
$Y_{t-1} \times FARI_{t-1}$			-0.110** (0.048) <i>-0.031</i>				-0.107* (0.049) <i>-0.030</i>				-0.111** (0.048) <i>-0.031</i>		
$Y_{t-1} \times MaPP_{t-1}$				0.021 (0.034) <i>0.006</i>				0.104 (0.082) <i>0.029</i>				-0.017 (0.019) <i>-0.005</i>	
$Y_{t-1} \times MPS_{t-1}$					-0.004 (0.005) <i>-0.013</i>				-0.004 (0.005) <i>-0.013</i>				-0.004 (0.005) <i>-0.013</i>
$\Delta^4 F_RGDP_{t-1}$	0.570 (0.366) <i>0.074</i>	0.568 (0.365) <i>0.074</i>	0.560 (0.358) <i>0.073</i>	0.566 (0.365) <i>0.073</i>	0.569 (0.360) <i>0.074</i>	0.561 (0.364) <i>0.073</i>	0.554 (0.357) <i>0.072</i>	0.556 (0.363) <i>0.072</i>	0.563 (0.359) <i>0.073</i>	0.571 (0.366) <i>0.074</i>	0.564 (0.359) <i>0.073</i>	0.572 (0.366) <i>0.074</i>	0.573 (0.361) <i>0.074</i>
Constant	0.567 (2.358)	0.601 (2.348)	0.906 (2.254)	0.602 (2.346)	0.727 (2.346)	0.642 (2.363)	0.941 (2.268)	0.700 (2.355)	0.769 (2.362)	0.553 (2.347)	0.862 (2.252)	0.559 (2.347)	0.682 (2.345)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603	3,603
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.725	0.725	0.726	0.726	0.725	0.726	0.726	0.726	0.726	0.725	0.726	0.725	0.725
Within R-square	0.631	0.631	0.631	0.631	0.631	0.632	0.632	0.632	0.632	0.631	0.631	0.631	0.631

Source: Authors’ calculations.

Note: (a) Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Table 3.10: Leakages and the “Gates” Effect of Capital Controls
(The Fixed Effect Model)

	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE	FE
The “Gates” Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.718*** (0.035) <i>0.719</i>	0.719*** (0.035) <i>0.719</i>	0.719*** (0.035) <i>0.719</i>	0.719*** (0.035) <i>0.719</i>	0.718*** (0.036) <i>0.719</i>	0.718*** (0.035) <i>0.719</i>	0.718*** (0.035) <i>0.719</i>	0.718*** (0.035) <i>0.718</i>	0.718*** (0.036) <i>0.718</i>	0.718*** (0.035) <i>0.719</i>	0.718*** (0.035) <i>0.719</i>	0.718*** (0.036) <i>0.718</i>	0.718*** (0.036) <i>0.718</i>
Y_{t-1}	0.097** (0.033) <i>0.064</i>	0.096** (0.033) <i>0.064</i>	0.097** (0.033) <i>0.064</i>	0.095** (0.034) <i>0.063</i>	0.106** (0.041) <i>0.071</i>	0.096** (0.033) <i>0.064</i>	0.096** (0.034) <i>0.064</i>	0.093** (0.033) <i>0.062</i>	0.105** (0.041) <i>0.070</i>	0.097** (0.033) <i>0.065</i>	0.097** (0.033) <i>0.065</i>	0.099** (0.034) <i>0.066</i>	0.107** (0.041) <i>0.071</i>
$\Delta FARI_{t-1}$	-0.318 (0.184) <i>-0.005</i>	-0.352* (0.180) <i>-0.005</i>	-0.338 (0.207) <i>-0.005</i>	-0.342* (0.173) <i>-0.005</i>	-0.344* (0.171) <i>-0.005</i>	-0.345* (0.175) <i>-0.005</i>	-0.327 (0.205) <i>-0.005</i>	-0.367* (0.174) <i>-0.006</i>	-0.340* (0.167) <i>-0.005</i>	-0.294* (0.159) <i>-0.004</i>	-0.278 (0.183) <i>-0.004</i>	-0.307 (0.169) <i>-0.005</i>	-0.286* (0.150) <i>-0.004</i>
$MaPP_{t-1}$		0.111 (0.070) <i>0.003</i>	0.110 (0.069) <i>0.003</i>	0.077 (0.129) <i>0.002</i>	0.101 (0.074) <i>0.003</i>	1.303** (0.519) <i>0.039</i>	1.306** (0.517) <i>0.040</i>	0.984 (0.590) <i>0.030</i>	1.294** (0.512) <i>0.039</i>	-0.087 (0.156) <i>-0.003</i>	-0.088 (0.152) <i>-0.003</i>	-0.057 (0.147) <i>-0.002</i>	-0.099 (0.164) <i>-0.003</i>
MPS_{t-1}	0.161* (0.076) <i>0.045</i>	0.161* (0.076) <i>0.045</i>	0.161* (0.076) <i>0.045</i>	0.162* (0.076) <i>0.045</i>	0.150* (0.073) <i>0.042</i>	0.162* (0.077) <i>0.045</i>	0.162* (0.077) <i>0.046</i>	0.161* (0.077) <i>0.045</i>	0.152* (0.074) <i>0.043</i>	0.161* (0.076) <i>0.045</i>	0.161* (0.076) <i>0.045</i>	0.160* (0.075) <i>0.045</i>	0.150* (0.073) <i>0.042</i>
$Y_{t-1} \times \Delta FARI_{t-1}$			-0.012 (0.028) <i>-0.002</i>				-0.015 (0.027) <i>-0.002</i>				-0.013 (0.028) <i>-0.002</i>		
$Y_{t-1} \times MaPP_{t-1}$				0.015 (0.026) <i>0.004</i>				0.105 (0.076) <i>0.030</i>				-0.023 (0.016) <i>-0.007</i>	
$Y_{t-1} \times MPS_{t-1}$					-0.003 (0.004) <i>-0.010</i>				-0.003 (0.004) <i>-0.010</i>				-0.003 (0.004) <i>-0.011</i>
$\Delta^4 F_RGDP_{t-1}$	0.494 (0.368) <i>0.064</i>	0.491 (0.367) <i>0.064</i>	0.490 (0.367) <i>0.063</i>	0.490 (0.368) <i>0.063</i>	0.493 (0.363) <i>0.064</i>	0.485 (0.367) <i>0.063</i>	0.483 (0.366) <i>0.063</i>	0.478 (0.366) <i>0.062</i>	0.486 (0.363) <i>0.063</i>	0.495 (0.368) <i>0.064</i>	0.494 (0.367) <i>0.064</i>	0.495 (0.368) <i>0.064</i>	0.497 (0.364) <i>0.064</i>
Constant	-2.554 (1.815)	-2.541 (1.814)	-2.539 (1.815)	-2.545 (1.815)	-2.454 (1.779)	-2.534 (1.808)	-2.532 (1.808)	-2.504 (1.804)	-2.448 (1.772)	-2.563 (1.814)	-2.561 (1.814)	-2.551 (1.805)	-2.474 (1.779)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783	3,783
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.724	0.724	0.724	0.724	0.724	0.725	0.725	0.725	0.725	0.724	0.724	0.724	0.724
Within R-square	0.633	0.633	0.633	0.633	0.633	0.634	0.634	0.634	0.634	0.633	0.633	0.633	0.633

Source: Authors’ calculations.

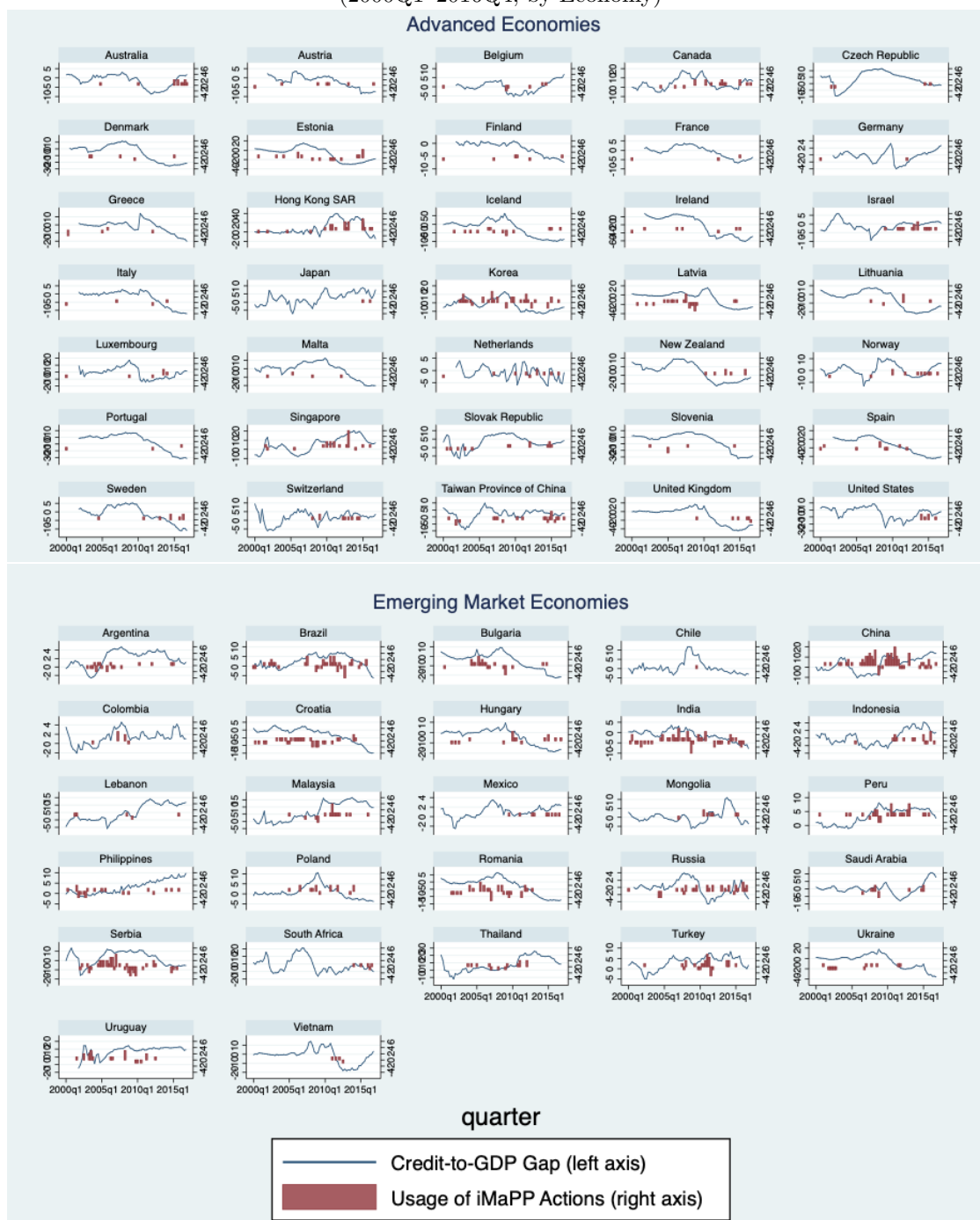
Note: (a) Robust standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Figure 3.2: Credit-to-GDP Gap and Usage of iMaPP Actions
(2000Q1–2016Q4, by Economy)



Source: Authors' calculations.

Note: Country classification is based on the latest WEO.

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Appendix A

THE ACCURACY OF IMF CRISES NOWCASTS

Appendix A.1 to A.3 address three dimensions of the MONA database: Section A1 (“MONA Audit”) covers results of our MONA audit including a list of (corrected) errors. Section A2 (“MONA Harmonization”) describes the construction of the time series of IMF program nowcasts from 1992 to 2019. Section A3 (“Our Dataset”) details our dataset.

Appendix A.4 reports the regressions results for Figures 2.4.1, 2.4.2, and 2.5 in the main body of the paper.

A.1 MONA Audit

The MONA database presents challenges to researchers as it contains a wide range of errors. To complicate matters, the database does not identify release dates, so updates/corrections to the database cannot be identified by vintages. Results of our audit are based on the MONA version downloaded November 19, 2020, which is archived at tinyurl.com/monamirror. We audited MONA and corrected the following 11 different types of errors:

Data Entry Errors

- (1) Temporal Errors (the right data entered for the wrong program year)
- (2) Zeros Identify Missing Values
- (3) Data Entered with Wrong Signs
- (4) Typos and Spelling Mistakes

(5) Wrong Line Items Entered

Inconsistencies

(6) Currency Unit Magnitude Inconsistencies

(7) Indicator Variable Inconsistencies

(8) Rates vs Level Inconsistencies

(9) Base Year Inconsistencies

Corrected Data from IMF Archives (Executive Board Documents)

(10) Missing Data Corrected

(11) Outliers Verified and Corrected

Important Note: Errors cited in this paper that are corrected in future work or in the MONA database must cite this paper as the source.

A.1.1 Temporal Errors

MONA identifies “t” as the program year and reports data from t-3 to t+4. If the program year was, for example, the year 2000 for country x, the database reports 1997 to 2004 data for that program. At times, data entry confused the program year to create temporal errors by entering data t data (supposedly 2000 data) as t-1 data (1999 data). This error occurs in several forms: either (i) for an entire program, (ii) for all variables but only some reviews of a particular program, or (iii) for only some variables in only some reviews of a particular program. Table A.1.1 lists 10 programs that suffer from this error. Each program features

multiple incidences of the error.

A.1.2 Zeros Identify Missing Values

MONA does not possess a consistent indicator for missing values. At times missing entries are identified as “0,” “.”, “ ”, “NA”, or “NULL.” Using zeros as missing values creates problems when some variables (e.g., debt or inflation) take zero as actual values. For inflation, we replaced exact zeros as missing data or corrected the entry if we observed four or more consecutive years of exact zero inflation, after auditing every zero for t and $t-1$. Table A.1.2 lists 29 missing values which related to GDP and inflation that were erroneously entered as zeros in MONA. We identified an additional 10935 instances where exact zeros are missing values (e.g., for national accounts, exchange rates, or investment data) but we do not include that list in our Table, which pertains only to variables used in our paper.

A.1.3 Data Entered with Wrong Sign

Our audit of the MONA database produced 593 instances of variables entered with the wrong (negative) sign. This is a common issue for trade data in MONA (although the database instructions/definitions indicate otherwise). Such sign errors are prevalent and affect a wide range of variables including consumption and investment data. Errors are also not necessarily consistent within any given program, which may express a variable at times with or without a negative sign. We do not list these items in a Table since they do not affect the variables in our paper.

A.1.4 Typos and Spelling Mistakes

Table A.1.4 lists 49 instances of typos, where either (i) the entire number is wrong, (ii) one decimal is wrong, (iii) one additional integer is added, (iv) one integer is missing, (v) the wrong country is identified as the program country, (vi) the wrong year is identified as the program year, or (vii) a variable is misspelled.

A.1.5 Wrong Line Item Entered

When data entry fell into the wrong line in the loan document, data for variable x (the line item above or below) is, at times, accidentally entered for variable y. Table A.1.5 lists 2 the instances relevant to our paper.

A.1.6 Currency Unit Magnitude Inconsistencies

Table A.1.6 lists 19 instances where unit magnitudes for a given variable are inconsistent across reviews in a given program.

A.1.7 Unit Indicator Variable Inconsistency

There are 293 instances for real/nominal GDP and inflation where the indicated (i) currency magnitude, (ii) currency (USD vs. SDR), or (iii) indicator currency unit (index vs rate) is incorrect. In addition, there are 5301 of such instances for variables that do not pertain to this paper. Since our paper is in growth rates, we do not report these instances.

A.1.8 Rates vs. Levels Inconsistencies

Table A.1.8 lists 28 instances for GDP/inflation where MONA indicates growth rates (e.g. GDP growth) but the data entered is in levels, or when a variable is supposed to be the inflation rate, but the data entered is an index.

A.1.9 Unit Inconsistency: Base Years

Table A.1.9 lists 23 instances when a variable in the same program is expressed in different base years. Since MONA does not contain a separate variable that indicates the base year, any revaluation, currency reform, or change in base years cannot be easily discerned in the database. When changes in base years were apparent, we adjusted observations to have the same base year and converted the data to growth rates. Two programs had to be dropped since the base year and/or base year currency reform conversions were unclear.

A.1.10 Missing Data

Table A.1.10 lists the 17 missing observations in the original MONA database that we updated with GDP and Inflation data from the IMF archives (Executive Board Documents).

A.1.11 Verified / Corrected Outliers

Table A.1.11 reports 92 outliers for real/nominal GDP and inflations that we audited/verified/corrected for program year t and $t-1$. After all missing datapoints were audited, we audited all observations that were 3 standard deviations from the mean. After the first round of audited outliers, the distribution changed and we audited yet another round

of outliers 3 standard deviations from the mean to verify that large deviations represent the actual data.

A.2 *MONA Harmonization*

The IMF MONA database consists of two parts: Part 1: 1992-2002 and Part II: 2002-today. The newer MONA files report real GDP in local currency and in levels, while the older MonA files report it in growth rates. We calculated growth rates for the newer MONA files. Newer MONA files also report “end-of-period CPI” in levels (as an index), while older MONA files report “end-of-period CPI” prices as inflation rates (in percentages). Again we calculated inflations rates from the level data for newer MONA files.

A.3 *Our Dataset*

To construct our dataset, we used the World Economic Outlook (WEO) data as “official final data” and MONA data issued at the time the program was approved as “nowcasts.” To ensure consistency, we mapped MONA data that are levels with WEO data in levels and then calculated growth rates. There are exceptions: First, when end-of-period inflation data was missing in MONA or from Executive Board Documents in IMF archives, we used period-average inflation data; seven such instances are documented in Table A.3.1 below. When WEO official final data was missing, we used data from MONA’s last review; 29 instances are documented in Table A.3.1 below. When WEO data seemed to contain errors we also used MONA’s last review data; one instance is documented in Table A.3.1. below.

We used growth rates as the unit of analysis paper because IMF projections are based on the growth rates. Also, level data in MONA is at times report in different units or magnitudes (as described in Appendix A.1.7 and A.1.8). Focusing on growth rates resolved the inconsistency issues. Our current dataset includes 602 programs for three variables: real GDP growth, nominal GDP growth, and inflation from 1992-2019. We had to drop two

programs due to unresolved issues (see Table A.1.9). The data coverage differs slightly for each variable due to variations in the availability of original data in the IMF Archives:

- Real GDP growth data covers 597 programs. We dropped 3 programs with missing final data (in both WEO and MONA).
- Nominal GDP growth data covers 596 programs. We dropped 2 programs with missing nowcasts data and 2 programs with missing final data (in both WEO and MONA).
- Inflation rate data covers 595 programs. We dropped 5 programs with missing final data (in both WEO and MONA).

Table A.1: MONA Audit: List of Corrected Errors

Table A.1.1: Temporal Errors

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
1	7	Estonia	1993	All	Last	All	corrected data using IMF archives (moved data one year forward)
2	15	El Salvador	1993	All	Last	All	corrected data using IMF archives (moved data one year forward)
3	132	Sierra Leone	1995	NI_Y, RGDPC	R0	All	corrected data using IMF archives (moved data one year forward)
4	256	Indonesia	1998	NI_Y, PCPIC, RGDPC	All	All	corrected data using IMF archives (moved data one year backward)
5	275	Indonesia	1999	ENDA, NGDP, PCPIC, RGDPC	All	All	corrected data using IMF archives (moved data one year backward)
6	552	Dominican Rep.	2005	All	All	All	corrected data using IMF archives (moved data two years backward)
7	571	Madagascar	2006	All	Last	All	corrected data using IMF archives (moved data one year forward)
8	579	Gabon	2007	All	R1–Last	All	corrected data using IMF archives (moved data one year forward)
9	687	Tanzania	2012	BFOL_O	Last	All	corrected data using IMF archives (moved data two years backward)
10	695	Solomon Islands	2012	FMB, FBA, FCG, FDS, FGS, FFS	R1–Last	All	corrected data using IMF archives (moved data one year forward)

Table A.1.2: Zeros Identify Missing Values

<i>Count</i>	Arrange- ment	Country Name	Prog .	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
1	127	Georgia	1995	NGDP	R0	t-3	data corrected using IMF archives
2	250	Mauritania	1997	PCPIC	R0	t-1	data corrected using IMF archives
3	264	Central African Republic	1998	PCPIC	R0	t-1	Zero verified by EBS document
4	281	Argentina	1998	PCPIC	R0	t-2 to t-1	Zero verified by EBS document
5	337	Indonesia	2000	PCPIC	R0	t-1	Zero verified by EBS document
6	507	Albania	2002	NGDP	R6	All	Replaced zeros with missing values
7	507	Albania	2002	NGDP_R	R6	All	Replaced zeros with missing values
8	507	Albania	2002	PCPIE	R6	All	Replaced zeros with missing values
9	526	Nepal	2003	NGDP	R2-R3	t-3 to t-2	Replaced zeros with missing values
10	526	Nepal	2003	NGDP_R	R2-R3	t-3 to t-2	Replaced zeros with missing values
11	544	Mozambique	2004	NGDP	R1-R2	t+4	Replaced zeros with missing values
12	544	Mozambique	2004	NGDP_R	R1-R2	t+4	Replaced zeros with missing values
13	551	Niger	2005	NGDP_R	R5	t-3	Replaced zeros with missing values
14	560	Benin	2005	PCPIE	R0	t-1 to t+4	data corrected using IMF archives
15	590	Liberia	2008	PCPIE	R7	All	Replaced zeros with missing values
16	656	Senegal	2010	PCPIE	R6-R7	All	Replaced zeros with missing values
17	662	Romania	2011	NGDP_R	R7-R8	t+4	Replaced zeros with missing values
18	678	Burundi	2012	NGDP	R0	t+3 to t+4	Replaced zeros with missing values
19	678	Burundi	2012	NGDP_R	R0	t+3 to t+4	Replaced zeros with missing values
20	681	Niger	2012	PCPIE	R2-R7	All	Replaced zeros with missing values
21	685	Gambia, The	2012	PCPIE	R0	All	data corrected using IMF archives
22	708	Burkina Faso	2013	NGDP	R0	t+4	Replaced zeros with missing values
23	708	Burkina Faso	2013	NGDP_R	R0	t+4	Replaced zeros with missing values
24	709	Albania	2014	PCPIE	R2-R3	All	data corrected using IMF archives
25	714	Tanzania	2014	PCPIE	R0	All	data corrected using IMF archives
26	729	Senegal	2015	PCPIE	R3	All	Replaced zeros with missing values
27	731	Sao Tome and Principe	2015	PCPIE	R0	All	data corrected using IMF archives
28	739	Rwanda	2016	PCPIE	R0	t to t+4	data corrected using IMF archives
29	741	Iraq	2016	PCPIE	R0	All	data corrected using IMF archives

Table A.1.4: Typos and Spelling Mistakes

<i>Count</i>	<i>Arrangement</i>	<i>Country Name</i>	<i>Prog. Year</i>	<i>Mnemonic</i>	<i>Review Type</i>	<i>Year (t-3 -t+4)</i>	<i>Correction</i>
1	15	El Salvador	1993	programyear	R2	All	programyear corrected
2	18	Latvia	1993	programyear	R1–Last	All	programyear corrected
3	75	Turkey	1994	PCPIC	R0	All	data corrected using IMF archives
4	84	Algeria	1995	DL	Last	All	data corrected using IMF archives
5	117	Albania	1994	countryname	All	All	wrong countryname corrected
6	117	Albania	1994	countrycode	All	All	wrong countryname corrected
7	127	Georgia	1995	NGDP	R0	t	data corrected using IMF archives
8	132	Sierra Leone	1995	programyear	R0	All	programyear corrected
9	136	Haiti	1995	PCPIC	R0	t	data corrected using IMF archives
10	143	Pakistan	1996	programyear	R0–R1	All	programyear corrected
11	156	Guyana	1996	BK SP	Last	All	data corrected using IMF archives
12	160	Russian Federation	1995	PCPIC	R0	All	data corrected using IMF archives
13	205	Vietnam	1996	boarddocno	R1	All	board document typo corrected
14	206	Congo, Rep.	1996	GCECY	R0	All	data corrected using IMF archives
15	207	Ethiopia	1997	reviewtype	All	All	reviewtype lables corrected
16	212	Kyrgyz Republic	1997	ENDA	Last	All	data corrected using IMF archives
17	230	Burkina Faso	1996	programyear	R0	All	data corrected using IMF archives
18	242	Bosnia Herzegovina	1998	RGDPC	R0	t-3	data corrected using IMF archives
19	250	Mauritania	1997	PCPIC	R0	t-3 to t-1	data corrected using IMF archives
20	274	Ukraine	1998	NGDP	Last	All	data corrected using IMF archives
21	274	Ukraine	1998	PCPIC	R0	All	data corrected using IMF archives
22	274	Ukraine	1998	programyear	R5–R6	All	programyear corrected
23	295	Yemen	1998	NGDP	R0	t	data corrected using IMF archives
24	402	Moldova	2000	BXS O	R0	All	data corrected using IMF archives
25	506	Bosnia Herzegovina	2002	NGDP	R0	All	data corrected using IMF archives
26	510	Argentina	2003	NGDP	R0	All	data corrected using IMF archives
27	510	Argentina	2003	NGDP_R	R0	All	data corrected using IMF archives
28	510	Argentina	2003	PCPIE	R0	All	data corrected using IMF archives
29	521	Ghana	2003	PCPIE	R0	All	data corrected using IMF archives
30	527	Nicaragua	2002	programyear	R10	All	programyear corrected
31	535	Uruguay	2002	NGDP_R	R0	All	data corrected using IMF archives
32	535	Uruguay	2002	PCPIE	R0	All	data corrected using IMF archives
33	539	Dominican Rep.	2003	NGDP	R0	All	data corrected using IMF archives
34	539	Dominican Rep.	2003	NGDP_R	R0	All	data corrected using IMF archives
35	539	Dominican Rep.	2003	PCPIE	R0	All	data corrected using IMF archives
36	545	Peru	2004	PCPIE	R0	All	data corrected using IMF archives
37	560	Benin	2005	PCPIE	R0	All	data corrected using IMF archives
38	560	Benin	2005	boarddocno	R0	All	board document typo corrected
39	564	Iraq	2005	NI	R0	All	data corrected using IMF archives
40	572	Haiti	2006	GCENL	Last	All	data corrected using IMF archives
41	598	Djibouti	2008	NGDP_R	R0	All	data corrected using IMF archives
42	607	Congo, Rep.	2008	NGDP	R0	All	data corrected using IMF archives
43	628	Kyrgyz Rep.	2008	reviewtype	All	All	reviewtype lables corrected
44	681	Niger	2012	programyear	R8	All	programyear corrected
45	707	Mali	2013	NCG	R0	All	data corrected using IMF archives
46	724	Ukraine	2015	reviewtype	All	All	reviewtype lables corrected
47	734	Kenya	2016	reviewtype	All	All	reviewtype lables corrected
48	764	Mauritania	2017	programyear	R0–R4	All	programyear corrected
49	All	All	All	intialenddate	All	All	spelling error corrected

Table A.1.5: Wrong Line Item Entered

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
1	560	Benin	2005	PCPIE	R0	All	data corrected using IMF archives
2	610	Sao Tome and Principe	2009	NGDP_R	All	All	data corrected using IMF archives

Table A.1.6: Wrong Currency Unit Entered

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
1	23	Pakistan	1993	indicatorcurrency of NGDP	All	All	Changed to "NCU (billions)"
2	70	Poland	1994	ENDA	R0	All	Divided value by 10000
3	70	Poland	1994	FMB	R2	All	Divided value by 1000
4	70	Poland	1994	FNDA	R3	All	Divided value by 1000
5	70	Poland	1994	NGDP	R1	All	Divided value by 1000
6	75	Turkey	1994	ENDA	Last	All	Divided value by 1000
7	84	Algeria	1995	ENDA	R0	All	Divided value by 1000
8	164	Russian Federation	1996	ENDA	R0	All	Divided value by 1000
9	164	Russian Federation	1996	FMB	R6	All	Divided value by 1000
10	164	Russian Federation	1996	FNDA	R7	All	Divided value by 1000
11	164	Russian Federation	1996	NGDP	R5	All	Divided value by 1000
12	199	Croatia	1997	ENDA	Last	All	Divided value by 1000
13	317	Turkey	1999	NGDP	R0	All	Divided value by 1000
14	398	Bulgaria	2002	ENDA	R0	All	Divided value by 1000
15	398	Bulgaria	2002	NGDP	R4	All	Divided value by 1000
16	517	Croatia	2003	indicatorcurrency of NGDP	R0-R1	All	Changed to "NCU (billions)"
17	517	Croatia	2003	indicatorcurrency of PCPIE	R0-R1	All	Changed to "Index Number"
18	566	Grenada	2006	NGDP_R	Last	All	Multiplied value by 10
19	580	Mozambique	2007	PCPIE	R0	All	Divided value by 1000

Table A.1.8: Rates vs. Levels Inconsistencies

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
1	119	Argentina	1992	NGDP	R5–R6	All	corrected to rates
2	510	Argentina	2003	NGDP_R	R0	All	corrected to rates
3	510	Argentina	2003	PCPIE	R0	All	corrected to rates
4	521	Ghana	2003	PCPIE	R0	All	corrected to rates
5	527	Nicaragua	2002	PCPIE	R0	All	corrected to rates
6	535	Uruguay	2002	NGDP_R	R0	All	corrected to rates
7	535	Uruguay	2002	PCPIE	R0	All	corrected to rates
8	539	Dominican Republic	2003	NGDP_R	R0	All	corrected to rates
9	539	Dominican Republic	2003	PCPIE	R0	All	corrected to rates
10	545	Peru	2004	PCPIE	R0	All	corrected to rates
11	556	Turkey	2005	PCPIE	All	All	corrected to rates
12	560	Benin	2005	PCPIE	R0	All	corrected to rates
13	562	North Macedonia, Rep.	2005	PCPIE	R0-R1	All	corrected to rates
14	564	Iraq	2005	PCPIE	R0	All	corrected to rates
15	566	Grenada	2006	PCPIE	R0	All	corrected to rates
16	572	Haiti	2006	PCPIE	R0	All	corrected to rates
17	580	Mozambique	2007	PCPIE	R0	All	corrected to rates
18	588	Iraq	2007	PCPIE	R0	All	corrected to rates
19	591	Honduras	2008	PCPIE	R0	All	corrected to rates
20	598	Djibouti	2008	NGDP_R	R0	All	corrected to rates
21	610	Sao Tome and Principe	2009	NGDP_R	All	All	corrected to rates
22	685	Gambia, The	2012	PCPIE	R0	All	corrected to rates
23	709	Albania	2014	PCPIE	R0	All	corrected to rates
24	714	Tanzania	2014	PCPIE	R0	All	corrected to rates
25	718	Yemen	2014	PCPIE	R0	All	corrected to rates
26	731	Sao Tome and Principe	2015	PCPIE	R0	All	corrected to rates
27	739	Rwanda	2016	PCPIE	R0	All	corrected to rates
28	741	Iraq	2016	PCPIE	R0	All	corrected to rates

Table A.1.9: Unit Inconsistency: Base Years

<i>Count</i>	<i>Arrange- ment</i>	<i>Country Name</i>	<i>Prog. Year</i>	<i>Mnemonic</i>	<i>Review Type</i>	<i>Year (t-3 -t+4)</i>	<i>Correction</i>
1	16	Kyrgyz Republic	1993	All	All	All	Unresolved. dropped
2	108	Kazakhstan	1994	All	All	All	Unresolved. dropped
3	532	Sierra Leone	2001	PCPIE	Last	All	Converted to rates
4	533	Tanzania	2000	PCPIE	Last	All	Converted to rates
5	538	Burundi	2004	PCPIE	Last	All	Converted to rates
6	547	Zambia	2004	PCPIE	Last	All	Converted to rates
7	549	Bulgaria	2004	PCPIE	Last	All	Converted to rates
8	554	Kyrgyz Republic	2005	PCPIE	Last	All	Converted to rates
9	561	Sao Tome and Principe	2005	PCPIE	Last	All	Converted to rates
10	565	Albania	2006	PCPIE	Last	All	Converted to rates
11	567	Moldova	2006	PCPIE	Last	All	Converted to rates
12	568	Paraguay	2006	PCPIE	Last	All	Converted to rates
13	596	Burundi	2008	PCPIE	Last	All	Converted to rates
14	617	Romania	2009	PCPIE	Last	All	Converted to rates
15	619	Ghana	2009	PCPIE	Last	All	Converted to rates
16	620	Sri Lanka	2009	PCPIE	Last	All	Converted to rates
17	623	Angola	2010	PCPIE	Last	All	Converted to rates
18	625	Congo, Democ. Rep.	2010	PCPIE	Last	All	Converted to rates
19	635	El Salvador	2010	PCPIE	Last	All	Converted to rates
20	678	Burundi	2012	PCPIE	Last	All	Converted to rates
21	697	Jamaica	2013	PCPIE	Last	All	Converted to rates
22	704	Romania	2013	PCPIE	Last	All	Converted to rates
23	712	Seychelles	2014	PCPIE	Last	All	Converted to rates

Table A.1.10: Missing Data Filled

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Year (t-3 -t+4)	Correction
<i>1</i>	5	Czech Republic	1993	NGDP	Last	All available	entered data using IMF archives
<i>2</i>	95	Ukraine	1995	RGDPC	R0	All available	entered data using IMF archives
<i>3</i>	127	Georgia	1995	NGDP	R0	All available	entered data using IMF archives
<i>4</i>	203	Senegal	1997	PCPIC	R0	All available	entered data using IMF archives
<i>5</i>	277	Senegal	1998	PCPIC	R0	All available	entered data using IMF archives
<i>6</i>	327	Senegal	1999	PCPIC	R0	All available	entered data using IMF archives
<i>7</i>	418	Turkey	2002	NGDP	R0	All available	entered data using IMF archives
<i>8</i>	537	Serbia & Montenegro	2002	PCPIE	R0	All available	entered data using IMF archives
<i>9</i>	545	Peru	2004	PCPIE	R0	All available	entered data using IMF archives
<i>10</i>	624	Maldives	2009	NGDP_R	R0	All available	entered data using IMF archives
<i>11</i>	650	Kosovo, Rep.	2010	PCPIE	R0	All available	entered data using IMF archives
<i>12</i>	685	Gambia, The	2012	PCPIE	R0	All available	entered data using IMF archives
<i>13</i>	709	Albania	2014	PCPIE	R0	All available	entered data using IMF archives
<i>14</i>	714	Tanzania	2014	PCPIE	R0	All available	entered data using IMF archives
<i>15</i>	731	Sao Tome and Principe	2015	PCPIE	R0	All available	entered data using IMF archives
<i>16</i>	739	Rwanda	2016	PCPIE	R0	All available	entered data using IMF archives
<i>17</i>	741	Iraq	2016	PCPIE	R0	All available	entered data using IMF archives

Table A.1.11: Verified / Corrected Outliers

<i>Count</i>	<i>Arrange- ment</i>	<i>Country Name</i>	<i>Prog. Year</i>	<i>Mnemonic</i>	<i>Correction</i>
1	10	Ethiopia	1992	RGDPC	verified using IMF archives
2	16	Kyrgyz Republic	1993	NGDP	verified using IMF archives
3	16	Kyrgyz Republic	1993	PCPIC	verified using IMF archives
4	17	Lao People'S Dem. Rep.	1993	NGDP	not available in IMF loan archives, dropped
5	17	Lao People'S Dem. Rep.	1993	PCPIC	verified using IMF archives
6	17	Lao People'S Dem. Rep.	1993	RGDPC	verified using IMF archives
7	19	Lithuania	1993	PCPIC	verified using IMF archives
8	75	Turkey	1994	PCPIC	typo fixed using IMF archives
9	80	Bulgaria	1994	PCPIC	verified using IMF archives
10	82	Moldova	1994	NGDP	verified using IMF archives
11	82	Moldova	1994	PCPIC	verified using IMF archives
12	82	Moldova	1994	RGDPC	verified using IMF archives
13	93	Cambodia	1994	NGDP	verified using IMF archives
14	93	Cambodia	1994	PCPIC	verified using IMF archives
15	93	Cambodia	1994	RGDPC	verified using IMF archives
16	108	Kazakhstan	1994	NGDP	verified using IMF archives
17	108	Kazakhstan	1994	PCPIC	verified using IMF archives
18	118	Congo, Republic Of	1994	PCPIC	WEO data typo? Used MONA last review
19	127	Georgia	1995	NGDP	typo fixed using IMF archives
20	127	Georgia	1995	PCPIC	verified using IMF archives
21	132	Sierra Leone	1995	RGDPC	temporal issue fixed using IMF archives
22	134	Belarus	1995	NGDP	verified using IMF archives
23	134	Belarus	1995	PCPIC	verified using IMF archives
24	136	Haiti	1995	NGDP	not available in IMF loan archives, dropped
25	136	Haiti	1995	PCPIC	typo fixed using IMF archives
26	136	Haiti	1995	RGDPC	verified using IMF archives
27	139	Kyrgyz Republic	1994	NGDP	verified using IMF archives
28	139	Kyrgyz Republic	1994	PCPIC	verified using IMF archives
29	139	Kyrgyz Republic	1994	RGDPC	verified using IMF archives
30	150	Togo	1996	RGDPC	verified using IMF archives
31	158	Ghana	1995	PCPIC	verified using IMF archives
32	160	Russian Federation	1995	PCPIC	typo fixed using IMF archives
33	170	Cambodia	1995	NGDP	verified using IMF archives
34	170	Cambodia	1995	PCPIC	verified using IMF archives
35	170	Cambodia	1995	RGDPC	verified using IMF archives
36	174	Guinea-Bissau	1996	PCPIC	verified using IMF archives
37	181	Bulgaria	1996	PCPIC	verified using IMF archives
38	187	Uzbekistan	1996	PCPIC	verified using IMF archives
39	202	Bulgaria	1997	NGDP	verified using IMF archives
40	202	Bulgaria	1997	PCPIC	verified using IMF archives
41	202	Bulgaria	1997	RGDPC	verified using IMF archives
42	210	Romania	1997	PCPIC	verified using IMF archives
43	222	Mexico	1995	PCPIC	verified using IMF archives
44	228	Sierra Leone	1997	PCPIC	verified using IMF archives
45	228	Sierra Leone	1997	RGDPC	verified using IMF archives
46	242	Bosnia & Herzegovina	1998	RGDPC	typo fixed using IMF archives

(Table A.1.11 continued)

Count	Arrange- ment	Country Name	Prog. Year	Mnemonic	Correction
47	256	Indonesia	1998	NGDP	verified using IMF archives
48	256	Indonesia	1998	PCPIC	temporal issue fixed using IMF archives
49	256	Indonesia	1998	RGDPC	temporal issue fixed using IMF archives
50	275	Indonesia	1999	NGDP	temporal issue fixed using IMF archives
51	275	Indonesia	1999	PCPIC	temporal issue fixed using IMF archives
52	275	Indonesia	1999	RGDPC	temporal issue fixed using IMF archives
53	295	Yemen	1998	NGDP	typo fixed using IMF archives
54	506	Bosnia & Herzegovina	2002	NGDP	typo fixed using IMF archives
55	508	Argentina	2003	PCPIE	not available in IMF loan archives, dropped
56	510	Argentina	2003	NGDP	typo fixed using IMF archives
57	510	Argentina	2003	NGDP_R	typo fixed using IMF archives
58	510	Argentina	2003	PCPIE	typo fixed using IMF archives
59	520	Gambia, The	2002	NGDP_R	verified using IMF archives
60	521	Ghana	2003	PCPIE	typo fixed using IMF archives
61	532	Sierra Leone	2001	NGDP_R	verified using IMF archives
62	535	Uruguay	2002	NGDP_R	typo fixed using IMF archives
63	535	Uruguay	2002	PCPIE	typo fixed using IMF archives
64	539	Dominican Republic	2003	NGDP	typo fixed using IMF archives
65	539	Dominican Republic	2003	NGDP_R	typo fixed using IMF archives
66	539	Dominican Republic	2003	PCPIE	typo fixed using IMF archives
67	546	Ukraine	2004	NGDP	verified using IMF archives
68	552	Dominican Republic	2005	NGDP	temporal issue fixed using IMF archives
69	552	Dominican Republic	2005	NGDP_R	temporal issue fixed using IMF archives
70	552	Dominican Republic	2005	PCPIE	temporal issue fixed using IMF archives
71	564	Iraq	2005	NGDP_R	verified using IMF archives
72	566	Grenada	2006	NGDP_R	verified using IMF archives
73	588	Iraq	2007	PCPIE	verified using IMF archives
74	598	Djibouti	2008	NGDP_R	typo fixed using IMF archives
75	601	Seychelles	2008	PCPIE	verified using IMF archives
76	607	Congo, Republic Of	2008	NGDP	typo fixed using IMF archives
77	610	Sao Tome and Principe	2009	NGDP_R	typo fixed using IMF archives
78	611	Armenia	2009	NGDP	verified using IMF archives
79	611	Armenia	2009	NGDP_R	verified using IMF archives
80	611	Armenia	2009	PCPIE	verified using IMF archives
81	625	Congo, Democratic Rep.	2010	NGDP	verified using IMF archives
82	625	Congo, Democratic Rep.	2010	NGDP_R	verified using IMF archives
83	625	Congo, Democratic Rep.	2010	PCPIE	verified using IMF archives
84	643	Sierra Leone	2010	NGDP	verified using IMF archives
85	643	Sierra Leone	2010	NGDP_R	verified using IMF archives
86	643	Sierra Leone	2010	PCPIE	verified using IMF archives
87	661	Kenya	2011	PCPIE	verified using IMF archives
88	724	Ukraine	2015	PCPIE	verified using IMF archives
89	733	Mozambique	2016	PCPIE	verified using IMF archives
90	737	Suriname	2016	PCPIE	verified using IMF archives
91	737	Suriname	2016	PCPIE	verified using IMF archives
92	770	Argentina	2018	PCPIE	verified using IMF archives

Table A.2: MONA Harmonization

Old MONA, 1992–2002 (Mnemonic and Description)		New MONA, 2002–today (Mnemonic and Description)		Harmonization
NGDP	Nominal GDP (in level)	NGDP	Nominal GDP (in level)	Use the level of nominal GDP to calculate the growth rates for both
RGDPC	Real GDP growth rate (in percent)	NGDP_R	Real GDP (in level)	Use the level of real GDP in the new MONA to calculate the real GDP growth, then combine with the old MONA
PCPIC	End-of-period CPI Inflation rate (in percent)	PCPIE	End-of-period CPI Index (in level)	Use the CPI index in the new MONA to calculate the annual CPI inflation, then combine with the old MONA

Table A.3: Construction of Our Dataset

<i>Count</i>	Arrange- ment	Country Name	Prog. Year	Mnemonic	Review Type	Correction
1	5	Czech Republic	1993	NGDP	Last	Used MONA "L" data, WEO data missing
2	5	Czech Republic	1993	PCPIC	Last	Used MONA "L" data, WEO data missing
3	5	Czech Republic	1993	RGDPC	Last	Used MONA "L" data, WEO data missing
4	7	Estonia	1993	NGDP	Last	Used MONA "L" data, WEO data missing
5	7	Estonia	1993	PCPIC	Last	Used MONA "L" data, WEO data missing
6	7	Estonia	1993	RGDPC	Last	Used MONA "L" data, WEO data missing
7	19	Lithuania	1993	PCPIC	Last	Used MONA "L" data, WEO data missing
8	35	Cameroon	1993	NGDP	Last	Used MONA "L" data, WEO data missing
9	75	Turkey	1994	PCPIC	R0	Used the period-average data
10	110	North Macedonia, Rep.	1995	PCPIC	Last	Used MONA "L" data, WEO data missing
11	118	Congo, Republic Of	1994	PCPIC	Last	Use MONA "L" data, WEO data 1 order of magnitude larger than any other source including World Bank
12	119	Argentina	1992	PCPIC	Last	Used MONA "L" data, WEO data missing
13	121	Lithuania	1995	PCPIC	Last	Used MONA "L" data, WEO data missing
14	121	Lithuania	1995	RGDPC	Last	Used MONA "L" data, WEO data missing
15	122	Zimbabwe	1992	PCPIC	Last	Used MONA "L" data, WEO data missing
16	122	Zimbabwe	1992	RGDPC	Last	Used MONA "L" data, WEO data missing
17	126	Argentina	1995	PCPIC	Last	Used MONA "L" data, WEO data missing
18	127	Georgia	1995	PCPIC	Last	Used MONA "L" data, WEO data missing
19	130	Zimbabwe	1992	PCPIC	Last	Used MONA "L" data, WEO data missing
20	130	Zimbabwe	1992	RGDPC	Last	Used MONA "L" data, WEO data missing
21	140	Zimbabwe	1994	PCPIC	Last	Used MONA "L" data, WEO data missing
22	140	Zimbabwe	1994	RGDPC	Last	Used MONA "L" data, WEO data missing
23	145	Argentina	1996	PCPIC	Last	Used MONA "L" data, WEO data missing
24	160	Russian Federation	1995	PCPIC	R0	Used the period-average data
25	171	Georgia	1996	PCPIC	Last	Used MONA "L" data, WEO data missing
26	203	Senegal	1997	PCPIC	R0	Used the period-average data
27	217	Venezuela	1996	NGDP	Last	Used MONA "L" data, WEO data missing
28	250	Mauritania	1997	PCPIC	R0	Used the period-average data
29	277	Senegal	1998	PCPIC	R0	Used the period-average data
30	327	Senegal	1999	PCPIC	R0	Used the period-average data
31	397	Yugoslavia	2001	NGDP	Last	Used MONA "L" data, WEO data missing
32	397	Yugoslavia	2001	PCPIC	Last	Used MONA "L" data, WEO data missing
33	397	Yugoslavia	2001	RGDPC	Last	Used MONA "L" data, WEO data missing
34	537	Serbia and Montenegro	2002	PCPIE	R0	Used the period-average data
35	537	Serbia and Montenegro	2002	NGDP	Last	Used MONA "L" data, WEO data missing
36	537	Serbia and Montenegro	2002	PCPIE	Last	Used MONA "L" data, WEO data missing
37	537	Serbia and Montenegro	2002	NGDP_R	Last	Used MONA "L" data, WEO data missing

A.4 Regressions for Figures 2.4.1, 2.4.2, and 2.5

Table A.4.1: Regression Output for Figure 2.4.1

(Nowcast Bias and Efficiency Over Time, rolling 5-year averages, full sample)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	1992 to 1996	1993 to 1997	1994 to 1998	1995 to 1999	1996 to 2000	1997 to 2001	1998 to 2002	1999 to 2003	2000 to 2004	2001 to 2005	2002 to 2006	2003 to 2007	2004 to 2008	2005 to 2009	2006 to 2010	2007 to 2011	2008 to 2012	2009 to 2013	2010 to 2014	2011 to 2015	2012 to 2016	2013 to 2017	2014 to 2018	2015 to 2019
Real GDP Growth																								
Constant, α	-0.003	-0.004	-0.003	0.005	0.008	0.010	0.010	0.008*	0.008	0.034***	0.034***	0.048***	0.043***	0.020	0.003	0.001	0.001	0.000	0.006	0.000	-0.002	-0.003	-0.004	-0.005
p-value ($\alpha = 0$)	0.520	0.410	0.636	0.253	0.190	0.122	0.107	0.096	0.268	0.003	0.005	0.000	0.001	0.149	0.718	0.810	0.903	0.943	0.223	0.920	0.684	0.612	0.471	0.190
IMF Nowcast, β	0.966	0.970	0.865	0.744**	0.677**	0.670**	0.702**	0.891	0.869	0.307**	0.339**	0.154***	0.206***	0.467*	0.880	0.875	0.882	0.953	0.883	0.964	1.035	1.108	1.105	1.134
p-value ($\beta = 1$)	0.705	0.784	0.296	0.012	0.018	0.029	0.038	0.335	0.421	0.011	0.016	0.000	0.001	0.062	0.354	0.236	0.206	0.656	0.238	0.746	0.769	0.422	0.397	0.188
Observations	151	180	194	192	184	172	155	136	112	94	83	72	71	75	90	87	94	87	80	60	66	62	62	65
Adjusted R-squared	0.483	0.434	0.383	0.372	0.246	0.236	0.310	0.351	0.240	0.0644	0.0845	0.0153	0.0319	0.171	0.449	0.531	0.577	0.627	0.674	0.765	0.759	0.760	0.735	0.770
MZ F-test ($\alpha=0, \beta=1$)	0.991	1.256	3.445**	3.893**	4.034**	2.880*	2.382*	1.666	0.705	4.904***	4.401**	12.98***	6.173***	2.185	0.883	2.341	3.705**	0.236	0.809	0.0875	0.0883	0.392	0.365	0.963
p-value ($\alpha=0, \beta=1$)	0.374	0.287	0.034	0.022	0.019	0.059	0.096	0.193	0.496	0.009	0.015	0.000	0.003	0.120	0.417	0.102	0.028	0.790	0.449	0.916	0.916	0.677	0.696	0.387
HP T-test ($\gamma=0$)	-1.150	-1.520	-2.34**	-1.563	-1.840*	-1.402	-0.827	1.511	0.861	1.022	0.835	1.064	-0.270	-1.186	-0.650	-0.996	-1.212	-0.369	0.594	-0.283	-0.289	0.403	-0.091	-0.442
p-value ($\gamma=0$)	0.252	0.130	0.020	0.120	0.067	0.163	0.409	0.133	0.391	0.309	0.406	0.291	0.788	0.240	0.518	0.322	0.229	0.713	0.554	0.778	0.774	0.688	0.928	0.660
Nominal GDP Growth																								
Constant, α	0.141**	0.113***	0.104***	0.084***	0.056***	0.046***	0.018	0.021	0.026*	0.020	0.043***	0.062***	0.063***	0.027**	0.027**	0.019	0.014	0.004	0.009	-0.001	0.006	0.013	0.011	0.007
p-value ($\alpha = 0$)	0.029	0.000	0.000	0.000	0.000	0.000	0.198	0.132	0.096	0.128	0.001	0.000	0.000	0.041	0.038	0.117	0.213	0.724	0.409	0.845	0.458	0.148	0.221	0.336
IMF Nowcast, β	0.593**	0.648***	0.635***	0.635***	0.687***	0.684***	0.891	0.874	0.888	0.948	0.781**	0.677**	0.704***	0.801**	0.799***	0.869*	0.884*	0.931***	0.972***	1.001***	0.870***	0.811*	0.882***	0.933***
p-value ($\beta = 1$)	0.046	0.000	0.000	0.000	0.000	0.000	0.332	0.305	0.379	0.657	0.039	0.013	0.000	0.013	0.005	0.070	0.085	0.335	0.695	0.994	0.128	0.088	0.298	0.475
Observations	149	178	193	190	183	172	155	135	112	94	83	72	71	76	91	88	95	88	80	60	66	62	62	65
Adjusted R-squared	0.621	0.846	0.841	0.851	0.962	0.972	0.624	0.653	0.652	0.757	0.603	0.495	0.554	0.618	0.584	0.618	0.643	0.641	0.638	0.746	0.718	0.612	0.665	0.759
MZ F-test ($\alpha=0, \beta=1$)	2.554*	14.54***	13.70***	13.02***	399.4***	771.7***	0.902	1.411	2.251	3.739**	7.593***	11.62***	12.97***	3.259**	4.352**	1.692	1.561	0.702	0.467	0.0430	1.821	1.504	0.766	0.480
p-value ($\alpha=0, \beta=1$)	0.081	0.000	0.000	0.000	0.000	0.000	0.408	0.248	0.110	0.028	0.001	0.000	0.000	0.044	0.016	0.190	0.215	0.498	0.629	0.958	0.170	0.230	0.469	0.621
HP T-test ($\gamma=0$)	-0.711	-1.113	-1.079	-1.057	-0.731	-0.946	0.335	0.550	1.532	1.874*	2.023**	2.492**	2.811***	-0.165	0.104	0.466	0.008	-0.546	0.962	-0.269	-1.536	-0.859	-0.029	0.084
p-value ($\gamma=0$)	0.478	0.267	0.282	0.292	0.466	0.346	0.738	0.583	0.128	0.064	0.046	0.015	0.006	0.870	0.917	0.643	0.994	0.586	0.339	0.789	0.129	0.393	0.977	0.933
Inflation																								
Constant, α	0.048***	0.095***	0.092***	0.070***	0.048***	0.035***	-0.004	0.001	0.003	0.015	0.031***	0.033**	0.022	0.014	0.015	0.008	-0.003	0.002	-0.002	-0.019***	-0.029***	-0.031***	-0.024***	-0.016*
p-value ($\alpha = 0$)	0.005	0.000	0.000	0.000	0.000	0.000	0.661	0.912	0.825	0.109	0.005	0.012	0.117	0.371	0.343	0.622	0.798	0.857	0.848	0.028	0.002	0.001	0.007	0.094
IMF Nowcast, β	1.066	0.836*	0.799**	0.802**	0.768***	0.732***	1.174	1.070	1.101	0.823	0.691*	0.575*	0.935	0.936	0.895	0.934	1.093	0.872	0.997	1.338**	1.562***	1.680***	1.551***	1.397*
p-value ($\beta = 1$)	0.283	0.094	0.020	0.027	0.001	0.000	0.114	0.569	0.584	0.235	0.088	0.054	0.763	0.771	0.680	0.795	0.635	0.446	0.986	0.049	0.004	0.001	0.005	0.068
Observations	151	180	193	191	183	171	153	134	111	93	82	72	71	76	90	87	94	87	79	60	66	62	62	65
Adjusted R-squared	0.769	0.815	0.769	0.789	0.811	0.927	0.642	0.700	0.640	0.560	0.438	0.347	0.597	0.570	0.507	0.531	0.655	0.517	0.582	0.761	0.779	0.801	0.803	0.766
MZ F-test ($\alpha=0, \beta=1$)	5.523***	7.571***	10.12***	7.855***	11.82***	67.04***	1.699	0.633	1.310	1.367	5.323***	3.781**	5.216***	1.420	1.743	0.363	0.258	1.275	0.125	2.535*	5.204***	6.317***	4.489**	1.732
p-value ($\alpha=0, \beta=1$)	0.005	0.001	0.000	0.001	0.000	0.000	0.186	0.532	0.274	0.260	0.007	0.028	0.008	0.248	0.181	0.696	0.773	0.285	0.883	0.088	0.008	0.003	0.015	0.185
HP T-test ($\gamma=0$)	3.109***	2.157**	2.228**	1.658*	0.701	-0.093	1.514	1.105	1.485	0.210	1.230	0.253	2.499**	1.265	1.282	0.518	0.698	-1.585	-0.453	0.339	0.769	0.817	1.304	1.220
p-value ($\gamma=0$)	0.002	0.032	0.027	0.099	0.484	0.926	0.132	0.271	0.140	0.834	0.222	0.801	0.015	0.210	0.203	0.606	0.487	0.117	0.652	0.736	0.445	0.417	0.197	0.227

Note:

(a) Robust standard errors in parentheses unless otherwise indicated; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(b) Mincer Zarnowitz Null: Nowcast is unbiased and efficient

(c) Holden Peel Null: Nowcast is unbiased

Table A.4.2: Regression Output for Figure 2.4.2
(Nowcast Bias and Efficiency Over Time, rolling 5-year averages, Non-Hyperinflation LICs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
	1992 to 1996	1993 to 1997	1994 to 1998	1995 to 1999	1996 to 2000	1997 to 2001	1998 to 2002	1999 to 2003	2000 to 2004	2001 to 2005	2002 to 2006	2003 to 2007	2004 to 2008	2005 to 2009	2006 to 2010	2007 to 2011	2008 to 2012	2009 to 2013	2010 to 2014	2011 to 2015	2012 to 2016	2013 to 2017	2014 to 2018	2015 to 2019
Real GDP Growth																								
Constant, α	0.017	0.024*	0.021	0.024*	0.031**	0.032*	0.009	0.015*	0.014	0.021**	0.019	0.030*	0.031*	0.034**	0.031***	0.030***	0.030***	0.023**	0.022**	0.009	0.008	-0.027*	-0.006	-0.017**
p-value ($\alpha = 0$)	0.121	0.068	0.118	0.064	0.026	0.095	0.438	0.083	0.195	0.013	0.245	0.068	0.083	0.024	0.006	0.008	0.002	0.026	0.047	0.673	0.680	0.065	0.638	0.043
IMF Nowcast, β	0.553**	0.408**	0.463**	0.469**	0.311**	0.331*	0.848	0.789	0.739	0.643**	0.686	0.526	0.497	0.498*	0.597**	0.568**	0.561***	0.738	0.706	0.869	0.871	1.455	1.045	1.311*
p-value ($\beta = 1$)	0.020	0.026	0.043	0.038	0.017	0.093	0.450	0.261	0.271	0.021	0.283	0.113	0.100	0.065	0.048	0.024	0.005	0.178	0.155	0.725	0.710	0.126	0.864	0.089
Observations	65	81	95	105	108	104	90	72	56	43	36	35	35	34	38	33	36	35	32	24	25	22	19	21
Adjusted R-squared	0.154	0.059	0.067	0.070	0.015	0.011	0.151	0.170	0.117	0.102	0.175	0.154	0.134	0.169	0.360	0.475	0.549	0.560	0.529	0.500	0.524	0.756	0.612	0.747
MZ F-test ($\alpha=0, \beta=1$)	3.603**	2.865*	2.681*	2.280	2.97*	1.449	0.307	2.120	0.879	3.565**	0.701	1.860	1.601	3.125*	5.161**	4.670**	5.821***	4.178**	2.627*	0.120	0.093	2.521	0.585	2.556
p-value ($\alpha=0, \beta=1$)	0.033	0.063	0.074	0.107	0.056	0.240	0.737	0.128	0.421	0.037	0.503	0.172	0.217	0.058	0.011	0.017	0.007	0.024	0.089	0.887	0.912	0.106	0.568	0.104
HP T-test ($\gamma=0$)	-0.981	-1.229	-1.368	-0.732	-1.100	-0.590	0.301	1.330	0.338	0.707	0.325	0.664	0.309	0.996	2.11**	2.185**	2.241**	2.14**	1.391	0.239	0.094	-0.334	-0.973	-0.507
p-value ($\gamma=0$)	0.330	0.223	0.175	0.466	0.274	0.557	0.764	0.188	0.737	0.484	0.747	0.511	0.759	0.326	0.042	0.036	0.032	0.040	0.174	0.813	0.926	0.742	0.343	0.618
Nominal GDP Growth																								
Constant, α	-0.022	-0.016	-0.008	-0.006	0.039	0.055**	0.041	0.053*	0.058*	0.045	0.050**	0.059***	0.077***	0.076***	0.070***	0.061***	0.068***	0.042**	0.039	0.011	-0.001	-0.021	0.003	-0.003
p-value ($\alpha = 0$)	0.115	0.318	0.622	0.661	0.182	0.031	0.123	0.064	0.062	0.159	0.028	0.005	0.000	0.000	0.000	0.003	0.000	0.038	0.152	0.687	0.982	0.430	0.940	0.922
IMF Nowcast, β	1.181**	1.142	1.125	1.134	0.780	0.632*	0.777	0.666	0.655	0.781	0.802	0.711**	0.651***	0.635***	0.624***	0.721***	0.678***	0.744**	0.772	0.968	0.981	1.094	0.930	0.999
p-value ($\beta = 1$)	0.016	0.167	0.219	0.183	0.327	0.074	0.302	0.180	0.184	0.456	0.215	0.023	0.002	0.000	0.000	0.009	0.001	0.021	0.196	0.870	0.923	0.742	0.843	0.998
Observations	65	81	96	105	108	104	90	71	56	43	36	35	35	34	38	33	36	35	32	24	25	22	19	21
Adjusted R-squared	0.793	0.764	0.732	0.742	0.330	0.260	0.337	0.273	0.262	0.417	0.532	0.501	0.499	0.562	0.623	0.656	0.646	0.656	0.534	0.491	0.543	0.560	0.409	0.548
MZ F-test ($\alpha=0, \beta=1$)	3.109*	0.986	1.107	1.496	1.388	2.740*	1.926	2.576*	2.398*	5.048**	3.829**	4.733**	8.044***	10.72***	11.860***	5.444***	8.178***	2.931*	1.099	0.250	0.0730	1.139	0.0880	0.0436
p-value ($\alpha=0, \beta=1$)	0.052	0.377	0.335	0.229	0.254	0.069	0.152	0.083	0.100	0.011	0.032	0.016	0.001	0.000	0.000	0.009	0.001	0.067	0.346	0.781	0.930	0.340	0.916	0.957
HP T-test ($\gamma=0$)	0.929	0.856	1.285	1.597	0.758	0.576	1.278	1.214	1.138	1.510	1.912*	1.472	2.071**	1.268	1.150	2.188**	2.275**	0.576	0.941	0.695	-0.349	-1.051	-0.430	-0.271
p-value ($\gamma=0$)	0.356	0.395	0.202	0.113	0.450	0.566	0.205	0.229	0.260	0.139	0.064	0.150	0.046	0.214	0.258	0.036	0.029	0.568	0.354	0.494	0.730	0.305	0.672	0.789
Inflation																								
Constant, α	-0.018	-0.024	-0.025	-0.036	0.012	-0.001	-0.004	-0.001	0.004	0.011	0.036***	0.033**	0.046***	0.050***	0.056***	0.049***	0.029	0.009	0.004	-0.003	-0.010	-0.009	0.007	-0.002
p-value ($\alpha = 0$)	0.785	0.706	0.644	0.460	0.445	0.931	0.810	0.923	0.795	0.306	0.007	0.021	0.002	0.001	0.000	0.001	0.104	0.650	0.853	0.836	0.547	0.431	0.635	0.882
IMF Nowcast, β	1.759	1.831	1.828	1.931	1.061	1.183	1.220	1.185	1.157	0.905	0.705**	0.683**	0.697**	0.590**	0.433***	0.495***	0.775	0.822	0.918	1.098	1.215	1.263	1.006	1.160
p-value ($\beta = 1$)	0.434	0.362	0.309	0.255	0.817	0.486	0.452	0.550	0.630	0.418	0.027	0.029	0.027	0.015	0.002	0.009	0.332	0.525	0.780	0.734	0.419	0.239	0.979	0.435
Observations	64	80	94	105	108	104	90	72	56	43	36	35	35	34	38	33	36	35	32	24	25	22	19	21
Adjusted R-squared	0.173	0.181	0.181	0.205	0.167	0.191	0.250	0.398	0.409	0.424	0.331	0.313	0.381	0.271	0.148	0.166	0.350	0.413	0.460	0.601	0.575	0.332	0.385	0.533
MZ F-test ($\alpha=0, \beta=1$)	6.100***	4.872**	3.497**	2.362*	1.359	0.570	0.549	0.947	1.639	0.554	4.163**	3.103*	5.699***	6.264***	10.590***	6.514***	1.736	0.253	0.0526	0.0772	0.343	0.759	0.205	0.425
p-value ($\alpha=0, \beta=1$)	0.004	0.010	0.034	0.099	0.261	0.567	0.580	0.393	0.204	0.579	0.024	0.058	0.007	0.005	0.000	0.004	0.192	0.778	0.949	0.926	0.713	0.481	0.817	0.660
HP T-test ($\gamma=0$)	1.959*	2.240**	2.199**	1.925*	1.403	1.068	1.044	1.265	1.508	0.572	2.090**	1.517	2.859***	2.307**	2.016*	1.333	1.071	-0.557	-0.263	0.355	0.369	0.352	0.655	0.593
p-value ($\gamma=0$)	0.055	0.028	0.030	0.057	0.164	0.288	0.299	0.210	0.137	0.570	0.044	0.138	0.007	0.028	0.051	0.192	0.292	0.581	0.794	0.726	0.716	0.728	0.521	0.560

Note:

(a) Robust standard errors in parentheses unless otherwise indicated; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

(b) Mincer Zarnowitz Null: Nowcast is unbiased and efficient

(c) Holden Peel Null: Nowcast is unbiased

Table A.4.3: Regression Output for Figure 2.5
(Nowcast Horizons and Nowcast Accuracy, by month of nowcast, full sample)

	(1) Jan	(2) Feb	(3) Mar	(4) Apr	(5) May	(6) June	(7) July	(8) Aug	(9) Sep	(10) Oct	(11) Nov	(12) Dec
Real GDP Growth												
Constant, α	-0.000	0.017	-0.012*	-0.011	0.011	0.012	-0.002	0.002	0.007	0.002	-0.002	0.010
p-value ($\alpha = 0$)	0.995	0.190	0.100	0.145	0.137	0.126	0.838	0.759	0.105	0.796	0.681	0.172
IMF Nowcast, β	0.913	0.651	1.18	1.045	0.577**	0.832	0.989	1.032	0.907	1.151	0.875	0.554*
p-value ($\beta = 1$)	0.866	0.111	0.210	0.780	0.0122	0.243	0.961	0.839	0.157	0.518	0.113	0.0556
Observations	72	28	61	55	50	70	65	33	37	22	33	71
Adjusted R-squared	0.164	0.245	0.573	0.615	0.254	0.410	0.439	0.474	0.661	0.452	0.765	0.379
MZ F-test ($\alpha=0, \beta=1$)	0.582	1.428	1.403	2.905*	3.576**	1.309	0.232	0.157	3.854**	0.324	2.911*	2.038
p-value ($\alpha=0, \beta=1$)	0.561	0.258	0.254	0.0635	0.0357	0.277	0.794	0.856	0.0307	0.727	0.0694	0.138
HP T-test ($\gamma=0$)	-0.675	0.0635	-1.345	-2.382**	-0.570	1.356	-0.602	0.569	0.786	0.800	-1.785*	-1.165
p-value ($\gamma=0$)	0.502	0.950	0.184	0.0208	0.571	0.180	0.550	0.574	0.437	0.432	0.0837	0.248
Nominal GDP Growth												
Constant, α	-0.008	0.052	0.013	0.068***	0.004	0.022**	0.069**	0.045	0.093***	0.007	-0.008	0.020**
p-value ($\alpha = 0$)	0.607	0.108	0.411	0.002	0.818	0.018	0.012	0.198	0.000	0.746	0.371	0.023
IMF Nowcast, β	1.264**	0.744*	0.968	0.714***	0.877	0.965	0.632**	0.662	0.229***	1.094	0.992	0.853**
p-value ($\beta = 1$)	0.0247	0.098	0.748	0.000	0.171	0.325	0.021	0.293	0.000	0.326	0.870	0.0289
Observations	70	28	61	55	50	71	64	33	37	22	33	72
Adjusted R-squared	0.725	0.667	0.677	0.962	0.822	0.969	0.757	0.353	0.764	0.907	0.921	0.637
MZ F-test ($\alpha=0, \beta=1$)	3.557**	1.510	0.473	28.80***	1.520	2.940*	3.367**	0.972	5908***	1.149	1.046	3.604**
p-value ($\alpha=0, \beta=1$)	0.034	0.240	0.625	0.000	0.229	0.060	0.041	0.389	0.000	0.337	0.363	0.032
HP T-test ($\gamma=0$)	2.146**	0.873	0.689	-0.720	-1.439	1.329	-0.715	-0.209	-1.147	1.492	-1.458	-0.375
p-value ($\gamma=0$)	0.035	0.390	0.494	0.475	0.157	0.188	0.477	0.836	0.259	0.151	0.155	0.709
Inflation												
Constant, α	-0.007	0.034*	0.022*	0.072***	0.051**	0.032	-0.031	-0.013	-0.008	0.017	-0.023*	-0.000
p-value ($\alpha = 0$)	0.866	0.085	0.089	0.009	0.016	0.126	0.447	0.157	0.412	0.574	0.082	0.961
IMF Nowcast, β	1.488	0.563	1.003	0.744***	0.852	1.024	1.388	1.207*	1.047**	1.052***	1.355**	1.037
p-value ($\beta = 1$)	0.488	0.109	0.981	0.000	0.149	0.823	0.405	0.083	0.026	0.000	0.018	0.655
Observations	72	28	61	55	50	69	64	31	37	23	33	72
Adjusted R-squared	0.318	0.260	0.710	0.930	0.446	0.602	0.641	0.910	0.978	0.974	0.876	0.758
MZ F-test ($\alpha=0, \beta=1$)	1.900	1.811	4.672**	23.96***	3.264**	1.235	0.390	1.644	2.862*	39.05***	3.127*	0.125
p-value ($\alpha=0, \beta=1$)	0.157	0.183	0.013	0.000	0.047	0.297	0.679	0.211	0.071	0.000	0.058	0.882
HP T-test ($\gamma=0$)	1.730*	-0.301	2.293**	-0.012	1.771*	1.476	0.831	0.612	-0.009	1.114	1.382	0.484
p-value ($\gamma=0$)	0.088	0.765	0.025	0.991	0.083	0.145	0.409	0.545	0.993	0.277	0.176	0.630

Note:

- (a) Robust standard errors in parentheses unless otherwise indicated; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$
(b) Mincer Zarnowitz Null: Nowcast is unbiased and efficient
(c) Holden Peel Null: Nowcast is unbiased

Appendix B

EXCHANGE RATES AND DOMESTIC CREDIT – CAN MACROPRUDENTIAL POLICY REDUCE THE LINK?

B.1 Baseline Robustness: Using Actual Rather than Forecasted GDP Growth

In the baseline result of Table 4 in the main body of the paper, we control for the impact of the forecasted real GDP growth on domestic credit development. In this section, we replace the forecasted growth rates with the realized growth rates, i.e., the (lagged) year-over-year log change of quarterly real GDP from the IMF World Economic Outlook (WEO) database (Δ^4RGDP). Actual GDP growth rates are more commonly used in the existing literature (Fendoglu 2017; Cerutti, Claessens, and Laeven 2017). However, they may, in the context of our exercises, be inferior as a control for potential simultaneity issues that arise when both credit and the exchange rate changes are being driven by economic expectations, for example related to good news about economic prospects.

Overall, results presented in Appendix [Table B.1](#) are consistent with Table 4. However, they also suggest that GDP forecasts are a much stronger control than past realizations of GDP. While the coefficient of Δ^4RGDP is positive and statistically significant, as was found in other studies, the size of the coefficient is only about a half of that for the GDP forecast: a one percent increase in the actual real GDP growth rates leads to an increase of the credit gap by about 0.24–0.29 percentage points of GDP, compared with about 0.45–0.5 percentage points of GDP for the forecasted real GDP growth. This suggests that the evolution of credit gaps is more closely related to the expectation of future growth than to past growth in GDP. Moreover, and by contrast, the coefficient on the real exchange rate movement is somewhat

larger when we use actual GDP compared to when we use the growth forecast, consistent with the idea that the larger coefficient is capturing some of the “good” news effects we control for when using forecast GDP. The coefficient of the interactions term $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$, on the other hand, roughly remain with the same magnitudes and levels of significance.

B.2 Baseline Robustness: Effects of Macroprudential Policies by Country Characteristics

We next examine whether the effects vary by country characteristics (Appendix [Table B.2](#)). We initially divide the country sample into two groups: the G7 countries, which are considered to be the source countries of global capital flows, and the non-G7 group, which tend to receive these flows.

We first present results for specifications that use both macroprudential policy shocks and exchange rate shocks, as introduced separately in the sections above, for the full sample of countries. The results, shown in columns 1–4, document once again that macroprudential policy shocks have significant effects in reducing credit, and that there is an interaction, where in the presence of a shock to the domestic real exchange rate, a macroprudential tightening reduces the expansionary effect of the appreciation on the credit gap.

The results for different sample splits are shown in columns 5–12. Overall, the effects are strong for the non-G7 group, but not for the G7. In particular, the base effect of the exchange rate on credit developments is significant and strong for the non-G7 group while it is insignificant for G7 countries (as indicated by the coefficients of $\Delta^4 RER_{i,t-1}$ in columns 9–12). The same holds for the extent to which tightening macroprudential policy can weaken the interactions between currency and credit movements, being strong and significant for non-G7 but insignificant for G7 countries.

We further divide the non-G7 group into more and less financially open economies. Using the de facto financial account openness index developed by Lane and Milesi-Ferretti (2007),

we take the average index for each country over the sample period (we take 2015 value for the index in 2016). The more financially open (closed) non-G7 economies are categorized as those having an average index above (below or equal to) the sample median. We find that the interaction effects between macroprudential policy and exchange rate shocks remain strong and significant in the more financially open group, despite the reduction in the sample size to 27 countries, while both base and interaction effects are measured statistically insignificant for the group of relative more financially closed economies (columns 13–21). This leads us to conclude that the effects are strongest for small open economies, and less relevant statistically for either advanced economies or relatively closed EMDEs.

B.3 Baseline Robustness: Alternative Measure of Domestic Credit Developments

We finally consider an alternative and simpler measure of domestic credit developments. Although the credit gap is a well-established broad-based indicator of systemic risk in the time dimension (Drehmann 2013), it relies on a statistical filtering of the aggregate credit series that is subject to well-known issues regarding end-date biases, structural breaks, and the parameters driving the filtering. To side-step these criticisms, we employ an alternative and simpler measure, which is the four-quarter change of the credit-to-GDP ratio. As does the credit gap, this measure continues to relate credit aggregates to the size of the economy. It does not however, rely on any filtering method.

When using this alternative outcome variable, our specification continues to employ exchange rate shocks, macroprudential shocks, as well as monetary policy and GDP forecasts as controls. The results, shown in Appendix [Table B.3](#), document that currency appreciations are associated with increases in credit also using this alternative measure, with results for increases in the ratio of credit to GDP as statistically significant as those for the credit gap. The direct effects of macroprudential policy on the alternative measure of credit appear

somewhat weaker, even as the effect remains statistically highly significant for the borrower-based tools. The strength of the interaction effects, on the other hand, is quite similar to the results using the credit gap, and economically if anything somewhat larger. For example, the coefficient of $MaPP_{i,t-1} \times \Delta^4 RER_{i,t-1}$ in column 2 is 0.446 in the bottom panel, compared with 0.256 when using the credit gap (upper panel).

Looking at different groups and specifications of macroprudential policies, the detailed results line up slightly differently. For instance, relaxations of macroprudential policy continue to strengthen the effect of appreciation on credit and this now applies to both borrower-based and financial institution-based tools. Overall, however, the headline results on the link between exchange rates and credit and the interaction effects of macroprudential policy in reducing this link carry over to this alternative measure of credit.

We finally return to the credit gap as the outcome variable and report the results for individual policy tools. Appendix [Table B.4](#) indicates strong and significant interaction effects for caps on loan-to-value (LTVs) in particular. Those results should be interpreted with caution, however, since when it comes to individual tools, the number of macroprudential actions recorded in the database is in general quite small.

B.4 Robustness Check of the Extension “Feedback Effect”

We conduct robustness check for the feedback effect in the extension section (Table 9 and 10). For assessing the “walls” effect of capital controls with macroprudential policy shocks, Appendix [Table B.5](#) presents the result in a fixed effect model, and [Table B.6](#) presents the result in a random effect model. The random effect model considers de-meaned variables (of capital inflows and monetary policy stance). The average credit-to-GDP ratio over sample period by country is included as an additional country-specific “pull” factor in the random effect regressions. For assessing the “gate” effect of capital controls with macroprudential

policy shocks, [Table B.7](#) presents the result in a fixed effect model, and [Table B.8](#) presents the result in a random effect model—with inclusion of capital inflow control shocks.

When using the constructed macroprudential policy shocks, similar to results in the main paper, we find robust evidence that macroprudential policy tightening is associated with increase in other investment flows, with the result being significant for borrower-based tools. When we further add the constructed shocks of “capital inflow controls” (see [Appendix Table B.8](#)), the statistical significance of the base effect of capital control improves in some regressions.

Table B.1: Robustness — Results with Actual GDP Growth Rates

Variables	iMaPP				Borrower-based tools				Financial institutions-based tools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y_{t-1}	0.983*** (0.017)	0.990*** (0.016)	0.984*** (0.018)	0.988*** (0.017)	0.987*** (0.017)	0.987*** (0.016)	0.988*** (0.017)	0.987*** (0.016)	0.986*** (0.019)	0.990*** (0.017)	0.984*** (0.017)	0.990*** (0.017)
$\Delta^4 RER_{t-1}$	-0.056*** (0.021)	-0.056*** (0.018)	-0.052** (0.022)	-0.049*** (0.019)	-0.058*** (0.020)	-0.061*** (0.017)	-0.057*** (0.020)	-0.058*** (0.018)	-0.058*** (0.020)	-0.057*** (0.019)	-0.057*** (0.020)	-0.047** (0.022)
$MaPP_{t-1}$	-0.894** (0.404)	-0.745** (0.350)			-2.269** (0.965)	-1.955** (0.996)			-0.483 (0.515)	-0.392 (0.477)		
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$		0.158*** (0.050)				0.264* (0.145)				0.171*** (0.049)		
T_MaPP_{t-1}			-1.030** (0.437)	-0.752 (0.487)			-2.097 (1.381)	-1.640 (1.381)			-0.890 (0.707)	-0.721 (0.872)
$T_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.128* (0.067)				0.303** (0.154)				0.094 (0.081)
L_MaPP_{t-1}			-0.331 (0.747)	-1.155 (0.739)			-3.156** (1.582)	-2.664 (1.834)			0.231 (1.155)	-0.468 (0.690)
$L_MaPP_{t-1} \times \Delta^4 RER_{t-1}$				0.191*** (0.060)				0.093 (0.264)				0.201*** (0.046)
MPS_{t-1}	-0.220*** (0.067)	-0.186*** (0.061)	-0.206*** (0.069)	-0.180** (0.080)	-0.241*** (0.062)	-0.209*** (0.063)	-0.248*** (0.064)	-0.235*** (0.064)	-0.208*** (0.066)	-0.183*** (0.063)	-0.197*** (0.063)	-0.197** (0.077)
$\Delta^4 RGDP_{t-1}$	0.290*** (0.052)	0.264*** (0.047)	0.277*** (0.049)	0.269*** (0.050)	0.262*** (0.053)	0.239*** (0.049)	0.256*** (0.050)	0.252*** (0.045)	0.267*** (0.057)	0.255*** (0.055)	0.252*** (0.048)	0.257*** (0.053)
Observations	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
AB AR(1) test - p value	0.002	0.001	0.002	0.001	0.002	0.001	0.002	0.001	0.003	0.001	0.003	0.002
AB AR(2) test - p value	0.356	0.226	0.290	0.311	0.328	0.242	0.347	0.280	0.365	0.252	0.298	0.285
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.2: Effectiveness of Macroprudential Policies by Country Characteristics

	All (baseline) iMaPP				G7 iMaPP				non-G7 iMaPP				non-G7: Financially open iMaPP				non-G7: Financially closed iMaPP			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(18)	(19)	(20)	(21)
$\Delta^4 RER_{Shock_{t-1}}$	-0.053** (0.021)	-0.052** (0.021)	-0.049** (0.020)	-0.046** (0.020)	0.115 (0.139)	4.449 (3.148)	0.870 (0.595)	-1.114 (1.205)	-0.059** (0.024)	-0.056** (0.023)	-0.058** (0.024)	-0.060*** (0.020)	-0.066** (0.032)	-0.056** (0.025)	-0.067** (0.030)	-0.049 (0.033)	-0.048 (0.032)	-0.040 (0.032)	-0.051 (0.034)	-0.094*** (0.017)
$MaPP_{Shock_{t-1}}$	-1.114** (0.492)	-1.794*** (0.600)			0.570 (1.171)	-80.857 (55.555)			-0.741 (0.533)	-1.391** (0.620)			0.201 (0.607)	-0.464 (0.573)			0.064 (0.336)	0.097 (0.285)		
$MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$		0.256*** (0.087)				15.358 (9.654)				0.243*** (0.091)				0.314*** (0.100)				-0.014 (0.053)		
$T_MaPP_{Shock_{t-1}}$			-2.460*** (0.717)	-2.944*** (0.758)			-7.225 (7.761)	125.037 (94.092)			-2.095*** (0.689)	-2.747*** (0.856)			-1.416 (1.028)	-1.798 (1.358)			-0.309 (0.496)	-0.839 (0.969)
$T_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				0.228** (0.096)				-31.393 (27.581)				0.231** (0.091)				0.301 (0.214)				0.113 (0.102)
$L_MaPP_{Shock_{t-1}}$			0.616 (0.811)	-0.433 (1.118)			22.217 (22.322)	0.000 (0.000)			0.761 (0.788)	-0.022 (0.980)			1.741* (0.894)	0.887 (0.994)			0.676 (0.720)	1.444 (0.933)
$L_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				0.260 (0.159)				2.604 (4.145)				0.231 (0.166)				0.346*** (0.071)				-0.230 (0.200)
MPS_{t-1}	-0.303*** (0.079)	-0.265*** (0.077)	-0.295*** (0.083)	-0.266*** (0.072)	-0.011 (0.789)	-13.593 (8.843)	-6.165 (5.668)	-14.144 (10.006)	-0.268*** (0.076)	-0.242*** (0.072)	-0.256*** (0.075)	-0.240*** (0.068)	-0.184** (0.093)	-0.231* (0.124)	-0.222** (0.097)	-0.197* (0.103)	-0.255** (0.108)	-0.231* (0.124)	-0.254** (0.121)	-0.202 (0.148)
$\Delta^4 F_RGDP_{t-1}$	0.595*** (0.097)	0.570*** (0.103)	0.595*** (0.095)	0.564*** (0.094)	0.319 (3.161)	6.393 (6.350)	2.986 (2.701)	-2.944 (3.691)	0.568*** (0.099)	0.544*** (0.099)	0.569*** (0.101)	0.545*** (0.092)	0.696*** (0.152)	0.539*** (0.154)	0.658*** (0.157)	0.595*** (0.183)	0.321*** (0.104)	0.296*** (0.101)	0.292*** (0.113)	0.265*** (0.093)
Observations	3,505	3,505	3,505	3,505	406	406	406	406	3,099	3,099	3,099	3,099	1,558	1,558	1,558	1,558	1,541	1,541	1,541	1,541
# of Economies	62	62	62	62	7	7	7	7	55	55	55	55	27	27	27	27	28	28	28	28
AB AR(1) test - p value	0.00324	0.000464	0.00121	0.000261	0.0955	0.400	0.180	0.342	0.0102	0.00196	0.00497	0.00109	0.0489	0.0222	0.0427	0.0217	0.000325	0.000300	0.000281	0.00430
AB AR(2) test - p value	0.652	0.575	0.305	0.312	0.877	0.529	0.265	0.442	0.661	0.491	0.404	0.309	0.491	0.422	0.397	0.419	0.702	0.677	0.813	0.693
Hansen test - p value	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1	1

Source: Authors' calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We use the de facto financial account openness index developed by Lane and Milesi-Ferretti (2017). We take the average index for each country over the sample period (we take 2015 value for the index in 2016). The more financially open (closed) non-G7 economies are categorized as those having an average index above (below or equal to) the sample median.

Table B.3: Robustness — Alternative and Simpler Measure of Credit Developments

Variables	iMaPP				Borrower-based tools				Financial institutions-based tools			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Y=Credit-to-GDP Gap (baseline)												
$\Delta^4 RER_{Shock_{t-1}}$	-0.053** (0.021)	-0.052** (0.021)	-0.049** (0.020)	-0.046** (0.020)	-0.049** (0.025)	-0.054** (0.024)	-0.049** (0.022)	-0.035* (0.018)	-0.054** (0.022)	-0.046** (0.021)	-0.051** (0.020)	-0.055*** (0.021)
$MaPP_{Shock_{t-1}}$	-1.114** (0.492)	-1.794*** (0.600)			-2.110** (1.021)	-4.030** (1.608)			-1.131** (0.552)	-1.528*** (0.524)		
$MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$		0.256*** (0.087)				0.506** (0.210)				0.185** (0.080)		
$T_MaPP_{Shock_{t-1}}$			-2.460*** (0.717)	-2.944*** (0.758)			-1.932 (1.646)	-2.744* (1.661)			-3.358*** (1.046)	-3.557*** (1.157)
$T_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				0.228** (0.096)				0.245** (0.098)				0.214** (0.104)
$L_MaPP_{Shock_{t-1}}$			0.616 (0.811)	-0.433 (1.118)			-2.677* (1.516)	-6.619* (3.438)			1.451 (1.216)	1.087 (1.172)
$L_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				0.260 (0.159)				1.052*** (0.353)				0.132 (0.081)
Observations	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62
Y=4-quarter change of credit/GDP ratio												
$\Delta^4 RER_{Shock_{t-1}}$	-0.105** (0.047)	-0.114** (0.050)	-0.100** (0.044)	-0.012 (0.056)	-0.108** (0.047)	-0.114** (0.053)	-0.108** (0.050)	-0.058 (0.037)	-0.103** (0.050)	-0.107** (0.045)	-0.099** (0.047)	-0.046 (0.051)
$MaPP_{Shock_{t-1}}$	0.151 (0.912)	-0.926 (0.913)			-2.524* (1.509)	-3.866*** (1.392)			0.697 (0.843)	-0.257 (0.825)		
$MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$		0.446** (0.222)				0.457 (0.379)				0.345* (0.204)		
$T_MaPP_{Shock_{t-1}}$			-1.656 (1.398)	-0.954 (1.205)			-3.644** (1.526)	-1.964 (2.191)			-1.078 (1.816)	-0.794 (1.404)
$T_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				-0.034 (0.223)				-0.267 (0.215)				0.044 (0.165)
$L_MaPP_{Shock_{t-1}}$			2.746 (2.202)	-0.394 (1.792)			1.242 (3.425)	-5.159 (4.213)			3.310 (2.806)	0.900 (1.567)
$L_MaPP_{Shock_{t-1}} \times \Delta^4 RER_{Shock_{t-1}}$				0.807*** (0.264)				1.803*** (0.403)				0.718*** (0.205)
Observations	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505	3,505
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62

Source: Authors' calculations.

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table B.4: Effectiveness of Macroprudential Policies by Individual Instrument

	iMaPP	Borrower-based tools		Financial institutions-based tools												Other	
	iMaPP	LTV	DSTI	CCB	LVR	Capital	Conservation	LLP	LFC	Tax	LTD	LCG	LoanR	Liquidity	RR	LFX	OT
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
<u>Use MaPP Stance (same setup in Baseline)</u>																	
$\Delta^4 RER_{t-1}$	-0.054*** (0.018)	-0.055*** (0.019)	-0.061*** (0.021)	-0.057*** (0.020)	-0.058*** (0.020)	-0.055*** (0.021)	-0.056*** (0.022)	-0.057*** (0.021)	-0.049*** (0.017)	-0.056*** (0.020)	-0.054*** (0.021)	-0.053*** (0.020)	-0.053*** (0.018)	-0.055*** (0.017)	-0.059*** (0.022)	-0.058*** (0.021)	-0.058*** (0.022)
$MaPP_{t-1}$	-0.737 (0.461)	-2.852** (1.196)	-3.056 (2.149)	-1.754 (4.620)	-10.757 (9.850)	-2.920 (2.278)	-3.817 (5.637)	0.328 (4.414)	-19.412 (22.121)	2.482 (2.545)	-12.841 (8.859)	-3.342 (4.203)	0.370 (1.947)	-7.331** (2.876)	1.511 (1.322)	-5.319 (8.622)	-2.090 (3.379)
$MaPP_{t-1} \times \Delta^4 RER_{t-1}$	0.144*** (0.048)	0.370** (0.168)	0.499 (0.329)	0.416 (0.409)	1.766 (1.323)	0.291 (0.307)	0.111 (0.198)	0.943 (0.585)	-2.750 (3.993)	-0.327 (0.394)	-0.867 (2.655)	0.207 (0.411)	0.128* (0.069)	0.205 (0.507)	0.122 (0.099)	1.222 (1.213)	0.406 (0.560)
$\Delta^4 RER_{t-1}$	-0.052*** (0.019)	-0.055*** (0.019)	-0.056** (0.023)	-0.062*** (0.020)	-0.058*** (0.020)	-0.063*** (0.020)	-0.054** (0.021)	-0.052*** (0.019)	-0.054*** (0.019)	-0.055** (0.022)	-0.056*** (0.020)	-0.053*** (0.019)	-0.052*** (0.018)	-0.054*** (0.016)	-0.058** (0.026)	-0.056*** (0.019)	-0.059*** (0.021)
T_MaPP_{t-1}	-0.852* (0.503)	-2.195 (2.164)	-4.124* (2.275)	1.585 (6.711)	-10.757 (9.850)	-2.022 (2.209)	-3.713 (6.173)	7.282 (5.797)	-27.235 (31.534)	2.957 (2.887)	-19.571 (15.316)	-7.095 (6.152)	-2.658 (2.562)	-7.501* (3.958)	1.547 (1.777)	-6.584 (9.783)	-4.685 (4.677)
$T_MaPP_{t-1} \times \Delta^4 RER_{t-1}$	0.130** (0.061)	0.420** (0.209)	0.464* (0.275)	1.935 (1.398)	1.766 (1.323)	0.497 (0.390)	0.222 (0.360)	1.064* (0.590)	-3.163 (4.350)	-0.438 (0.478)	0.187 (3.231)	-0.271 (0.613)	-0.178 (0.251)	0.447 (0.614)	0.098 (0.180)	1.391 (1.410)	0.096 (0.630)
L_MaPP_{t-1}	-0.630 (0.709)	-4.035** (1.724)	12.817 (11.959)	20.394 (108.692)	0.000 (0.000)	-13.453 (9.666)	-18.346 (41.921)	-19.544 (17.403)	7.114 (19.691)	43.257 (48.675)	0.466 (16.042)	0.440 (6.333)	4.712 (6.912)	-4.985 (4.395)	1.446 (1.723)	2.674 (32.135)	.
$L_MaPP_{t-1} \times \Delta^4 RER_{t-1}$	0.159*** (0.042)	0.200 (0.250)	0.546 (0.740)	-3.848 (10.355)	0.000 (0.000)	-1.051 (0.867)	-2.770 (7.201)	2.506 (1.844)	-0.102 (4.359)	0.554 (2.267)	-5.596 (8.985)	0.203 (0.508)	-0.031 (0.271)	-0.211 (1.010)	0.152 (0.180)	6.953 (10.345)	4.452 (4.733)
# of Overall MaPP Actions	417	58	28	5	13	91	30	27	13	30	1	1	30	34	20	16	20
# of Tightening Actions	758	93	37	7	13	116	32	41	17	33	7	10	40	49	218	24	21
# of Loosening Actions	341	35	9	2	0	25	2	14	4	3	6	9	10	15	198	8	1
Observations	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842	3,842
# of Economies	62	62	62	62	62	62	62	62	62	62	62	62	62	62	62	62	62

Source: Authors' calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) The coefficient of tightening action of “other” tools is omitted due to its insufficient action number, as indicated in the table.

Table B.5: Leakages and the “Walls” Effect of Capital Controls (b)

(The Fixed Effect Model with Macprudential Policy Shocks)

Variables	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE	(8) FE	(9) FE	(10) FE	(11) FE	(12) FE	(13) FE
The “Walls” Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.713*** (0.032) <i>0.713</i>	0.719*** (0.024) <i>0.720</i>	0.717*** (0.023) <i>0.719</i>	0.719*** (0.023) <i>0.721</i>	0.719*** (0.024) <i>0.720</i>	0.719*** (0.024) <i>0.720</i>	0.717*** (0.024) <i>0.719</i>	0.719*** (0.023) <i>0.720</i>	0.719*** (0.024) <i>0.720</i>	0.719*** (0.023) <i>0.720</i>	0.717*** (0.023) <i>0.719</i>	0.719*** (0.023) <i>0.720</i>	0.718*** (0.024) <i>0.720</i>
Y_{t-1}	0.091** (0.037) <i>0.057</i>	0.085** (0.034) <i>0.055</i>	0.121** (0.038) <i>0.078</i>	0.084** (0.034) <i>0.054</i>	0.099* (0.044) <i>0.063</i>	0.085** (0.035) <i>0.054</i>	0.120** (0.039) <i>0.077</i>	0.081** (0.033) <i>0.052</i>	0.098* (0.044) <i>0.063</i>	0.086** (0.034) <i>0.055</i>	0.122** (0.038) <i>0.079</i>	0.086** (0.034) <i>0.055</i>	0.099** (0.044) <i>0.064</i>
$FARI_{t-1}$	-4.933* (2.341) <i>-0.045</i>	-6.109* (2.778) <i>-0.054</i>	-7.344* (3.660) <i>-0.064</i>	-6.000* (2.822) <i>-0.053</i>	-6.419* (2.846) <i>-0.056</i>	-6.304* (2.817) <i>-0.055</i>	-7.522* (3.706) <i>-0.066</i>	-6.241* (2.839) <i>-0.055</i>	-6.619** (2.899) <i>-0.058</i>	-6.017* (2.773) <i>-0.053</i>	-7.264* (3.668) <i>-0.064</i>	-6.045* (2.814) <i>-0.053</i>	-6.332* (2.844) <i>-0.056</i>
$MaPP_Shock_{t-1}$		0.165 (0.110) <i>0.004</i>	0.154 (0.104) <i>0.004</i>	0.126 (0.169) <i>0.003</i>	0.154 (0.110) <i>0.004</i>	1.520** (0.498) <i>0.040</i>	1.501** (0.507) <i>0.039</i>	1.270* (0.680) <i>0.033</i>	1.517** (0.502) <i>0.040</i>	-0.094 (0.188) <i>-0.002</i>	-0.104 (0.179) <i>-0.003</i>	-0.082 (0.191) <i>-0.002</i>	-0.108 (0.192) <i>-0.003</i>
MPS_{t-1}	0.174** (0.074) <i>0.047</i>	0.210** (0.090) <i>0.054</i>	0.205** (0.088) <i>0.053</i>	0.210** (0.090) <i>0.054</i>	0.199** (0.087) <i>0.051</i>	0.211** (0.091) <i>0.054</i>	0.206** (0.089) <i>0.053</i>	0.211** (0.091) <i>0.054</i>	0.201** (0.088) <i>0.052</i>	0.209** (0.090) <i>0.054</i>	0.204** (0.088) <i>0.052</i>	0.209** (0.089) <i>0.054</i>	0.199** (0.087) <i>0.051</i>
$Y_{t-1} \times FARI_{t-1}$			-0.106** (0.046) <i>-0.030</i>				-0.104* (0.048) <i>-0.030</i>				-0.106** (0.046) <i>-0.030</i>		
$Y_{t-1} \times MaPP_Shock_{t-1}$				0.024 (0.038) <i>0.006</i>				0.099 (0.083) <i>0.025</i>				-0.008 (0.015) <i>-0.002</i>	
$Y_{t-1} \times MPS_{t-1}$					-0.004 (0.004) <i>-0.013</i>				-0.004 (0.004) <i>-0.013</i>				-0.004 (0.004) <i>-0.013</i>
$\Delta^4 F_RGDP_{t-1}$	0.570 (0.366) <i>0.074</i>	0.602 (0.415) <i>0.077</i>	0.595 (0.410) <i>0.076</i>	0.599 (0.416) <i>0.077</i>	0.604 (0.411) <i>0.077</i>	0.593 (0.415) <i>0.076</i>	0.586 (0.409) <i>0.075</i>	0.588 (0.415) <i>0.075</i>	0.595 (0.410) <i>0.076</i>	0.604 (0.416) <i>0.077</i>	0.597 (0.410) <i>0.076</i>	0.604 (0.417) <i>0.077</i>	0.606 (0.412) <i>0.078</i>
Constant	0.567 (2.358)	-0.434 (2.477)	-0.101 (2.356)	-0.460 (2.475)	-3.221 (2.246)	-0.317 (2.479)	-3.027 (2.251)	-0.302 (2.477)	-3.188 (2.243)	-0.471 (2.474)	-3.099 (2.262)	-0.462 (2.465)	-0.310 (2.430)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,603	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.725	0.733	0.733	0.733	0.733	0.733	0.733	0.734	0.733	0.733	0.733	0.733	0.733
Within R-square	0.631	0.635	0.635	0.635	0.635	0.636	0.636	0.636	0.636	0.635	0.635	0.635	0.635

Source: Authors’ calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Table B.6: Leakages and the “Walls” Effect of Capital Controls (c)

(The Random Effect Model with Macroprudential Policy Shocks)

Variables	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE
The “Walls” Effect of Capital Controls + MaPP													
<i>CFLOW_DM_{t-1}</i>	0.721*** (0.032) <i>0.726</i>	0.729*** (0.025) <i>0.728</i>	0.728*** (0.025) <i>0.727</i>	0.729*** (0.025) <i>0.728</i>	0.729*** (0.025) <i>0.728</i>	0.729*** (0.025) <i>0.728</i>	0.728*** (0.025) <i>0.727</i>	0.729*** (0.025) <i>0.728</i>	0.729*** (0.025) <i>0.728</i>	0.728*** (0.025) <i>0.728</i>	0.727*** (0.025) <i>0.727</i>	0.728*** (0.025) <i>0.728</i>	0.729*** (0.025) <i>0.728</i>
<i>Y_{t-1}</i>	0.069*** (0.024) <i>0.054</i>	0.067*** (0.022) <i>0.053</i>	0.088*** (0.026) <i>0.070</i>	0.065*** (0.022) <i>0.052</i>	0.066*** (0.022) <i>0.052</i>	0.067*** (0.023) <i>0.053</i>	0.087*** (0.027) <i>0.069</i>	0.063*** (0.022) <i>0.050</i>	0.065*** (0.022) <i>0.051</i>	0.067*** (0.023) <i>0.053</i>	0.088*** (0.026) <i>0.070</i>	0.068*** (0.022) <i>0.054</i>	0.066*** (0.022) <i>0.052</i>
<i>FARI_{t-1}</i>	-2.723* (1.574) <i>-0.031</i>	-2.718 (1.748) <i>-0.029</i>	-3.000 (2.028) <i>-0.032</i>	-2.665 (1.750) <i>-0.029</i>	-2.710 (1.763) <i>-0.029</i>	-2.751 (1.747) <i>-0.030</i>	-3.025 (2.026) <i>-0.033</i>	-2.681 (1.731) <i>-0.029</i>	-2.742 (1.760) <i>-0.030</i>	-2.697 (1.748) <i>-0.029</i>	-2.981 (2.032) <i>-0.032</i>	-2.713 (1.776) <i>-0.029</i>	-2.688 (1.762) <i>-0.029</i>
<i>MaPP_Shock_{t-1}</i>		0.138 (0.110) <i>0.004</i>	0.129 (0.107) <i>0.004</i>	0.101 (0.162) <i>0.003</i>	0.137 (0.112) <i>0.004</i>	1.487*** (0.495) <i>0.048</i>	1.471*** (0.500) <i>0.047</i>	1.241* (0.638) <i>0.040</i>	1.488*** (0.496) <i>0.048</i>	-0.121 (0.184) <i>-0.004</i>	-0.128 (0.179) <i>-0.004</i>	-0.107 (0.184) <i>-0.003</i>	-0.122 (0.187) <i>-0.004</i>
<i>MPS_DM_{t-1}</i>	0.156** (0.071) <i>0.032</i>	0.179** (0.077) <i>0.035</i>	0.176** (0.077) <i>0.034</i>	0.180** (0.077) <i>0.035</i>	0.178** (0.078) <i>0.035</i>	0.180** (0.079) <i>0.035</i>	0.178** (0.078) <i>0.034</i>	0.180** (0.079) <i>0.035</i>	0.180** (0.080) <i>0.035</i>	0.179** (0.077) <i>0.035</i>	0.176** (0.076) <i>0.034</i>	0.178** (0.077) <i>0.034</i>	0.178** (0.078) <i>0.034</i>
<i>Y_{t-1} × FARI_{t-1}</i>			-0.061* (0.035) <i>-0.021</i>				-0.059* (0.035) <i>-0.021</i>				-0.062* (0.035) <i>-0.022</i>		
<i>Y_{t-1} × MaPP_Shock_{t-1}</i>				0.023 (0.035) <i>0.007</i>				0.096 (0.080) <i>0.030</i>				-0.010 (0.014) <i>-0.003</i>	
<i>Y_{t-1} × MPS_DM_{t-1}</i>					-0.001 (0.005) <i>-0.002</i>				-0.001 (0.005) <i>-0.002</i>				-0.001 (0.005) <i>-0.002</i>
<i>Δ⁴F_RGDP_{t-1}</i>	0.354 (0.235) <i>0.056</i>	0.365 (0.258) <i>0.057</i>	0.362 (0.257) <i>0.057</i>	0.363 (0.259) <i>0.057</i>	0.365 (0.257) <i>0.058</i>	0.359 (0.258) <i>0.057</i>	0.356 (0.257) <i>0.056</i>	0.355 (0.259) <i>0.056</i>	0.360 (0.257) <i>0.057</i>	0.367 (0.259) <i>0.058</i>	0.363 (0.258) <i>0.057</i>	0.367 (0.260) <i>0.058</i>	0.367 (0.258) <i>0.058</i>
<i>Avg Credit/GDP</i>	0.003 (0.003) <i>0.009</i>	0.003 (0.003) <i>0.011</i>	0.002 (0.002) <i>0.008</i>	0.003 (0.003) <i>0.011</i>	0.003 (0.002) <i>0.011</i>	0.003 (0.003) <i>0.010</i>	0.002 (0.002) <i>0.008</i>	0.003 (0.003) <i>0.010</i>	0.003 (0.002) <i>0.011</i>	0.003 (0.003) <i>0.011</i>	0.002 (0.002) <i>0.008</i>	0.003 (0.003) <i>0.011</i>	0.003 (0.002) <i>0.011</i>
Constant	1.482 (1.617)	1.496 (1.623)	1.666 (1.730)	1.457 (1.633)	1.494 (1.626)	1.590 (1.637)	1.755 (1.748)	1.538 (1.625)	1.587 (1.639)	1.499 (1.618)	1.672 (1.725)	1.508 (1.625)	1.496 (1.620)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,603	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326	3,326
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.629	0.635	0.635	0.635	0.635	0.636	0.636	0.636	0.636	0.635	0.635	0.635	0.635

Source: Authors’ calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Table B.7: Leakages and the “Gates” Effect of Capital Controls (b)

(The Fixed Effect Model with Macroprudential Policy Shocks and Capital Control Shocks)

Variables	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
	(1) FE	(2) FE	(3) FE	(4) FE	(5) FE	(6) FE	(7) FE	(8) FE	(9) FE	(10) FE	(11) FE	(12) FE	(13) FE
The “Gates” Effect of Capital Controls + MaPP													
$CFLOW_{t-1}$	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.728</i>	0.725*** (0.028) <i>0.727</i>	0.725*** (0.028) <i>0.727</i>	0.725*** (0.028) <i>0.727</i>	0.725*** (0.028) <i>0.727</i>
Y_{t-1}	0.094** (0.029) <i>0.063</i>	0.094** (0.029) <i>0.063</i>	0.094** (0.029) <i>0.064</i>	0.092** (0.030) <i>0.062</i>	0.103** (0.037) <i>0.069</i>	0.093** (0.030) <i>0.063</i>	0.094** (0.030) <i>0.063</i>	0.090** (0.029) <i>0.061</i>	0.102** (0.037) <i>0.069</i>	0.094** (0.029) <i>0.063</i>	0.094** (0.029) <i>0.064</i>	0.094** (0.030) <i>0.064</i>	0.103** (0.037) <i>0.070</i>
$\Delta FARI_Shock_{t-1}$	-0.295 (0.191) <i>-0.003</i>	-0.321 (0.180) <i>-0.004</i>	-0.279 (0.227) <i>-0.003</i>	-0.305 (0.167) <i>-0.003</i>	-0.313* (0.170) <i>-0.004</i>	-0.336* (0.177) <i>-0.004</i>	-0.286 (0.229) <i>-0.003</i>	-0.371* (0.177) <i>-0.004</i>	-0.330* (0.168) <i>-0.004</i>	-0.268 (0.173) <i>-0.003</i>	-0.222 (0.212) <i>-0.003</i>	-0.276 (0.181) <i>-0.003</i>	-0.260 (0.163) <i>-0.003</i>
$MaPP_Shock_{t-1}$		0.116 (0.096) <i>0.003</i>	0.114 (0.094) <i>0.003</i>	0.088 (0.124) <i>0.002</i>	0.107 (0.093) <i>0.003</i>	1.335** (0.561) <i>0.035</i>	1.341** (0.560) <i>0.035</i>	1.068 (0.637) <i>0.028</i>	1.329** (0.556) <i>0.035</i>	-0.119 (0.176) <i>-0.003</i>	-0.123 (0.171) <i>-0.003</i>	-0.110 (0.170) <i>-0.003</i>	-0.131 (0.180) <i>-0.003</i>
MPS_{t-1}	0.188* (0.088) <i>0.050</i>	0.189* (0.090) <i>0.051</i>	0.190* (0.090) <i>0.051</i>	0.191* (0.090) <i>0.051</i>	0.179* (0.087) <i>0.048</i>	0.190* (0.091) <i>0.051</i>	0.191* (0.091) <i>0.051</i>	0.190* (0.091) <i>0.051</i>	0.179* (0.088) <i>0.048</i>	0.190* (0.090) <i>0.051</i>	0.190* (0.090) <i>0.051</i>	0.189* (0.089) <i>0.050</i>	0.178* (0.087) <i>0.048</i>
$Y_{t-1} \times \Delta FARI_Shock_{t-1}$			-0.019 (0.025) <i>-0.002</i>				-0.023 (0.024) <i>-0.003</i>				-0.020 (0.025) <i>-0.003</i>		
$Y_{t-1} \times MaPP_Shock_{t-1}$				0.022 (0.025) <i>0.006</i>				0.105 (0.073) <i>0.029</i>				-0.009 (0.012) <i>-0.002</i>	
$Y_{t-1} \times MPS_{t-1}$					-0.003 (0.004) <i>-0.010</i>				-0.003 (0.004) <i>-0.009</i>				-0.003 (0.004) <i>-0.010</i>
$\Delta^4 F_RGDP_{t-1}$	0.518 (0.415) <i>0.067</i>	0.518 (0.415) <i>0.066</i>	0.517 (0.414) <i>0.066</i>	0.517 (0.416) <i>0.066</i>	0.520 (0.411) <i>0.067</i>	0.510 (0.415) <i>0.065</i>	0.508 (0.414) <i>0.065</i>	0.504 (0.415) <i>0.064</i>	0.512 (0.411) <i>0.065</i>	0.520 (0.416) <i>0.067</i>	0.519 (0.415) <i>0.066</i>	0.520 (0.416) <i>0.067</i>	0.522 (0.412) <i>0.067</i>
Constant	-1.278 (2.404)	-4.237 (2.412)	-1.287 (2.437)	-4.250 (2.418)	-4.139 (2.363)	-1.217 (2.431)	-4.234 (2.419)	-1.188 (2.426)	-1.123 (2.425)	-1.313 (2.439)	-4.269 (2.425)	-1.306 (2.435)	-4.171 (2.373)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,522	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.732	0.732	0.732	0.732	0.732	0.732	0.732	0.733	0.732	0.732	0.732	0.732	0.732
Within R-square	0.637	0.637	0.638	0.638	0.638	0.638	0.638	0.638	0.638	0.637	0.638	0.638	0.638

Source: Authors’ calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.

Table B.8: Leakages and the “Gates” Effect of Capital Controls (c)

(The Random Effect Model with Macroprudential Policy Shocks and Capital Control Shocks)

Variables	Baseline		iMaPP			Borrower-based tools				Financial institutions-based tools			
	(1) RE	(2) RE	(3) RE	(4) RE	(5) RE	(6) RE	(7) RE	(8) RE	(9) RE	(10) RE	(11) RE	(12) RE	(13) RE
The “Gates” Effect of Capital Controls + MaPP													
CFLOW_DM_{t-1}	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.733*** (0.027) <i>0.733</i>	0.733*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.733*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>	0.732*** (0.027) <i>0.733</i>
Y_{t-1}	0.071*** (0.020) <i>0.059</i>	0.071*** (0.020) <i>0.059</i>	0.071*** (0.020) <i>0.059</i>	0.069*** (0.020) <i>0.057</i>	0.068*** (0.022) <i>0.057</i>	0.070*** (0.020) <i>0.058</i>	0.071*** (0.020) <i>0.059</i>	0.066*** (0.020) <i>0.055</i>	0.068*** (0.022) <i>0.056</i>	0.071*** (0.020) <i>0.059</i>	0.071*** (0.020) <i>0.059</i>	0.071*** (0.020) <i>0.059</i>	0.068*** (0.022) <i>0.057</i>
ΔFARI_Shock_{t-1}	-0.294* (0.170) <i>-0.004</i>	-0.322** (0.160) <i>-0.005</i>	-0.286 (0.200) <i>-0.004</i>	-0.306** (0.147) <i>-0.004</i>	-0.319** (0.159) <i>-0.004</i>	-0.337** (0.161) <i>-0.005</i>	-0.293 (0.205) <i>-0.004</i>	-0.369** (0.161) <i>-0.005</i>	-0.334** (0.160) <i>-0.005</i>	-0.268* (0.159) <i>-0.004</i>	-0.228 (0.189) <i>-0.003</i>	-0.279* (0.170) <i>-0.004</i>	-0.264* (0.159) <i>-0.004</i>
MaPP_Shock_{t-1}		0.121 (0.100) <i>0.004</i>	0.119 (0.098) <i>0.004</i>	0.094 (0.128) <i>0.003</i>	0.120 (0.100) <i>0.004</i>	1.381** (0.566) <i>0.045</i>	1.385** (0.564) <i>0.045</i>	1.107* (0.631) <i>0.036</i>	1.381** (0.566) <i>0.045</i>	-0.117 (0.178) <i>-0.004</i>	-0.121 (0.173) <i>-0.004</i>	-0.105 (0.169) <i>-0.003</i>	-0.119 (0.181) <i>-0.004</i>
MPS_DM_{t-1}	0.164** (0.071) <i>0.033</i>	0.163** (0.070) <i>0.033</i>	0.163** (0.070) <i>0.033</i>	0.164** (0.071) <i>0.033</i>	0.161** (0.071) <i>0.033</i>	0.164** (0.072) <i>0.033</i>	0.164** (0.072) <i>0.033</i>	0.165** (0.072) <i>0.033</i>	0.162** (0.073) <i>0.033</i>	0.163** (0.070) <i>0.033</i>	0.163** (0.070) <i>0.033</i>	0.162** (0.069) <i>0.033</i>	0.161** (0.071) <i>0.033</i>
Y_{t-1} × ΔFARI_Shock_{t-1}			-0.017 (0.025) <i>-0.003</i>				-0.020 (0.024) <i>-0.003</i>				-0.018 (0.025) <i>-0.003</i>		
Y_{t-1} × MaPP_Shock_{t-1}				0.021 (0.024) <i>0.007</i>				0.106 (0.072) <i>0.036</i>				-0.012 (0.012) <i>-0.004</i>	
Y_{t-1} × MPS_DM_{t-1}					-0.001 (0.003) <i>-0.003</i>				-0.001 (0.003) <i>-0.003</i>				-0.001 (0.003) <i>-0.003</i>
Δ⁴F_RGDP_{t-1}	0.250 (0.224) <i>0.040</i>	0.252 (0.225) <i>0.040</i>	0.251 (0.224) <i>0.039</i>	0.252 (0.225) <i>0.040</i>	0.252 (0.224) <i>0.040</i>	0.246 (0.225) <i>0.039</i>	0.245 (0.224) <i>0.039</i>	0.244 (0.226) <i>0.038</i>	0.247 (0.224) <i>0.039</i>	0.254 (0.225) <i>0.040</i>	0.253 (0.224) <i>0.040</i>	0.253 (0.225) <i>0.040</i>	0.255 (0.224) <i>0.040</i>
Avg Credit/GDP	0.003 (0.003) <i>0.012</i>	0.003 (0.003) <i>0.012</i>	0.003 (0.003) <i>0.012</i>	0.003 (0.003) <i>0.012</i>	0.003 (0.002) <i>0.012</i>	0.003 (0.003) <i>0.011</i>	0.003 (0.003) <i>0.011</i>	0.003 (0.003) <i>0.011</i>	0.003 (0.002) <i>0.012</i>	0.003 (0.003) <i>0.012</i>	0.003 (0.003) <i>0.012</i>	0.003 (0.002) <i>0.011</i>	0.003 (0.002) <i>0.012</i>
Constant	-2.157 (1.678)	-2.165 (1.685)	-2.160 (1.681)	-2.198 (1.685)	-2.161 (1.692)	-2.110 (1.696)	-2.103 (1.692)	-2.119 (1.699)	-2.105 (1.703)	-2.164 (1.689)	-2.158 (1.685)	-2.148 (1.671)	-2.160 (1.696)
Time-fixed Effect	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter	Quarter
Observations	3,522	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506	3,506
# of Economies	61	61	61	61	61	61	61	61	61	61	61	61	61
Overall R-square	0.637	0.637	0.637	0.637	0.637	0.637	0.637	0.638	0.637	0.637	0.637	0.637	0.637

Source: Authors’ calculations.

Note: (a) Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

(b) We drop Taiwan POC in the sample due to missing data of financial account restriction index.

(c) The blue italic numbers are standardized coefficients, representing the change of standard deviation in the dependent variable by one standard deviation change in corresponding independent variables.

(d) Both of the capital inflows and capital control measures are based on the gross other investment inflows.