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A firm level approach on the effects of IMF programs

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ABSTRACT

This paper evaluates the effects of IMF programs at the firm level and considers the role of firm financing constraints as a channel of transmission. We examine different dimensions of a Fund program, namely participation and scope of conditionality. We find a positive effect of IMF programs on firms' sales growth, such that average sales growth can be up to 26 percent higher in firms exposed to IMF programs, and such effect is persistent over time. We also find evidence that the firms' financing constraint plays a role in the transmission of effects, and alleviation of these constraints improves performance. This paper, aside from providing new evidence on the effectiveness of IMF programs, brings attention to the role (and effectiveness) of official intervention, an important but under-analyzed dimension of international finance.

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1. Introduction

If at one point the era of the IMF as the steward of global capital markets seemed over, the global financial crisis of 2008 and more recently the Covid-19 pandemic have given it a new raison d'être. In particular, adverse global shocks such as the Covid 19 crisis or recent geopolitical tensions have triggered an unprecedented withdrawal of non-resident portfolio flows from emerging markets, increasing the relative importance of official flows. As recently documented by Horn et al. (2020), official lending is much larger than commonly known, often surpassing total private cross-border capital flows, especially in times of global turmoil when private flows generally shrink. In the wake of the recent Covid-19 pandemic for example, about one hundred countries approached the IMF for short-term emergency assistance (around double the number that requested the Fund's assistance in the aftermath of the 2008 financial crisis).[1] This resurgence of official flows and IMF lending in particular is motivation to re-investigate the effectiveness of IMF programs.

There is surprisingly little agreement on the direction and magnitude of the effects of IMF lending.[2] Of even greater significance is the lack of evidence pointing to specific channels through which IMF programs impact the real economy. The extensive dissemination of firm level data, however, now gives the opportunity to pick up on previously immeasurable mechanisms.

This paper takes a new approach to an old debate on the effects of IMF conditionality lending. Rather than drawing conclusions at the country level, we take advantage of micro-level data to explore the effects of IMF conditional lending on firm

[1] The IMF has introduced a set of measures aimed to help developing economies tackling both liquidity (e.g., the Short-term Liquidity Line, or SLL) and solvency problems caused by the pandemic (e.g., the Catastrophe Containment and Relief Trust, or CCRT). Most importantly, the new issuance of $650 billions of new IMF special drawing rights (SDRs) should boost emerging economies' balance sheets. At the same time, the IMF, together with the World Bank, urged G20 countries to establish the DSSI, a form of debt relief that eases financing constraints by deferring debt service repayments.

[2] For a recent survey of the related literature see Balima and Sokolova (2021), who examine 994 estimates of the effects of IMF programs on economic growth as reported by 36 studies, using meta-regression analysis.

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performance, considering growth in firm sales and subsequently how redistribution occurs within the firm. Our approach allows us to perform a more accurate assessment of the effect of IMF lending at the firm level by exploiting both program and firm heterogeneity to investigate effectiveness. We argue that leveraging such heterogeneity to understand the channels through which IMF programs affect the economy is important.

Providing intuitions on micro-level outcomes for IMF programs is relevant both for recipient country policymakers and Fund objectives. A substantial (both theoretical and empirical) literature centered around firm financial frictions has highlighted how shocks to the financial system can affect the real economy differentially based on the composition of the private sector. Recent work by Broner et al. (2021) find that sovereign debt inflows have heterogeneous stock-market effects on domestic firms.[3] Li and Su (2022) show how capital inflows can lead to financial crises through a shortening of corporate debt maturity structure. On the other hand, Fan and Kalemli-Özcan (2016) show the role financial sector reforms have in shaping aggregate corporate savings, and specifically how the effect depends on financing behavior of individual firms. In this paper, we provide an extended, cross-country analysis of these firm-level effects of global capital inflows. The ability of the IMF to impact the financial sector through these large financial flows as well as its reform programs makes it uniquely important.

More specifically, information on firm sales is derived from the World Bank Enterprise Survey (WBES), which provides data on 130,000 firms spread across 139 countries, spanning the years 2003–2018. For information on IMF programs, we incorporate the dataset of Kentikelenis et al. (2016) which includes standard information on Fund programs (arrangement dates, commitment amounts) as well as detailed information on conditionality, for a dataset with over 32,000 conditions in 135 countries, between 1980 and 2016.

Our methodology is part of a growing field of studies utilizing macro–micro approaches.[4] Looking at firm level outcomes allows us not only to make inferences on country level effects, but also to exploit firm heterogeneity and identify potential channels of interest. Furthermore, the availability of detailed data on IMF conditionality allows us to disaggregate IMF lending and observe the differential effects of IMF programs on firm sales. In particular, we look at how an increase in the severity of a program, proxied through the number of binding conditions, impacts firm sales. This paper then contributes to the literature on how IMF effectiveness is contingent conditionality type and, to the best of our knowledge, is the first study that evaluates the effect of the IMF programs on firms' performance.

The scope of the paper, using the outlined methodology, is also to highlight channels and transmission mechanisms through which IMF conditional lending may affect the real economy. As described by Chauvet and Ehrhart (2018), there are two ways through which concessional financial flows may influence firm performance: demand (for example increased demand, financed by IMF loans, is met by firms' production), and supply (IMF loans may affect the productive capacity of firms).[5] More generally, the literature on firm performance points to three main kinds of constraints on firm growth in developing countries: the financing constraint (Beck et al., 2005; Harrison et al., 2004; Choudhary and Limodio, 2022; Fonseca and Matray, 2021), lack of infrastructure (see among others Bluhm et al., 2020; Jedwab and Moradi, 2016), and the institutional environment (e.g., Fisman and Svensson, 2007).

From the demand side, the effects are theoretically ambiguous. On the one hand, IMF disbursements are expected to relax the government borrowing constraints, while on the other hand it is hard to reconcile the IMF intervention with increased government spending, given IMF preference for austerity-oriented measures (see, for example Aiello (2020), Nelson and Wallace (2017)).

We instead choose to focus on one main channel, related to supply-side factors, of how IMF programs may influence firms' performance based on their financing constraints. Besides internal liquidity in the form of retained profits, firms rely on access to external liquidity in order to fund payrolls and other operating cost, and bank credit lines are the principal source to do so (Lins et al., 2010).[6]

Our primary hypothesis is one of a *signaling effect* of IMF programs to investors, as IMF lending could indicate to the international markets renewed confidence in the country. This eventually which translates into easier access to finance at the firm level, as credit conditions in the home country are strongly tied to the degree of financial distress the sovereign is experiencing. The transmission of sovereign risk operates largely through the balance sheets of banks, which especially affects firms with large financing needs.[7] Moreover, restoring a country's creditworthiness, may enhance private capital inflows into the recipient countries, through a *catalytic finance* effect. This could be either directly, when companies access foreign capital markets themselves, or indirectly, when their banks rely on foreign financing. Hence by improving the credit stance of a recipient country, IMF intervention may alleviate the firms' financing constraints and eventually enhance their performance.

[3] More specifically, they present some event studies on Colombia, Czech Republic, Mexico, Nigeria, Romania, and South Africa, which document the effects of official inflows on domestic firms. Instead of sales, as an outcome, they use stock markets returns. They find that while financial and government-related firms exhibit larger cumulative abnormal returns (CARs) tradable firms experience lower CARs.

[4] For example, the availability of geocoded data has produced an emerging strand of literature evaluating aid-effectiveness at the subnational level (Bluhm et al., 2020; Chauvet and Ehrhart, 2018; Del Prete et al., 2019; Gehring et al., 2019; Dreher and Lohmann, 2015 ; Dreher et al., 2021;Marchesi et al., 2021).

[5] Chauvet and Ehrhart (2018) consider ODA which includes both bilateral and multilateral aid flows.

[6] As recently illustrated by Choudhary and Limodio (2021), banks are responsible for the scarcity of long-term finance in low-income countries and liquidity risk is a predominant factor behind this empirical regularity.

[7] Since 2008 there has been burgeoning literature on the linkages between sovereign distress and domestic credit conditions, mostly focused on bank-sovereign "doom loops." Notable contributions include Brunnermeier et al. (2016), Borenstein and Panizza (2009), Gennaioli et al. (2014, 2018), and Bocola (2016).

We test this explicitly by looking at how a program affects sales based on the different financing choices of the firm, most notably by observing the importance of external financing for the firm and the existence of financing constraints at the firm level (e.g., Arellano et al., 2017). We then focus on the extent of IMF intervention by exploring detailed information on program conditionality, and testing whether the effects of binding conditions differentially impact firm performance according to the same channels we considered in the case of participation.

Our main identification strategy is based on an instrumental variable (IV) that combines temporal variation in the IMF's liquidity with cross-sectional variation in a country's prior probability of participating in an IMF program (see Gehring and Valentin, 2020, Lang, 2021). The IMF's liquidity varies primarily because of an institutional rule that requires the IMF to review the financial contributions of its members ("quotas") every five years. For identification, we exploit the fact that the IMF tends to expand its regular clientele in years in which its liquidity is higher, so that countries with an initially lower participation probability are more likely to receive a program in these years. The identifying assumption underlying this approach, which we explain in more detail in Section 4, thus follows a difference-in-differences logic.

In our baseline results, we find a positive impact of IMF program participation on firms' sales growth, and the effect is persistent over time. Specifically, controlling for firm fixed effects, sales are on average 24 percent higher for firms in countries benefiting from IMF lending. We confirm our hypothesis regarding the importance of a firms' financial burden, namely that the main channel of transmission for an IMF program is though the alleviation of the firms' financing constraint. As conditionality is concerned, the time dimension seems to be an important factor to determine its effectiveness: while an increasing number of conditions negatively affects firm performance in the short run, they turn out to enhance firms sales in the medium term. As an additional result, we explore how the increased sales are redistributed within the firm, and we find that participation in an IMF program results in a decline in the labor income share, but only in the short run.

This paper contributes to the literature on the effects of official flows on domestic firms and to the literature on IMF effectiveness. To the best of our knowledge, it is the first study that evaluates the impact of IMF participation on firm performance, providing important insights for the underlying mechanisms behind IMF intervention. Following a period of relative calm, IMF activity is likely to be again under scrutiny as its' share of global financial flows increases (e.g., see Archibong et al., 2021;Chari et al., 2021; Goldfajn et al., 2021;Spence, 2021). Hence, given the resurging importance of the IMF and the multitude of new countries participating in programs, we believe that this is a timely and economically relevant topic. Furthermore, it brings attention to the role (and effectiveness) of official intervention, an important dimension of international finance which needs to be further investigated.

We organize the rest of the paper as follows. In Section 2, we briefly review the related literature. Section 3 discusses the data, while Section 4 illustrates the identification strategies and Section 5 presents the empirical models and the results. Section 6 documents redistribution within the firm and Section 7 presents the robustness analysis. The final Section 8 concludes.

2. Effectiveness of IMF lending

This paper is related to several strands of literature. The first one broadly looks at IMF effectiveness by considering a wide range of dimensions related to an IMF intervention. While some studies find a positive (Bas and Stone, 2014) or insignificant (Atoyan and Conway, 2006) relationship between IMF programs and growth, the majority of empirical studies suggest immediate negative effects (Barro and Lee, 2005;Dreher, 2006; Easterly, 2005; Marchesi and Sirtori, 2011; Przeworkski and Vreeland, 2000). Other studies consider monetary stability, debt management, the containment of external arrears as key goals of IMF programs (Kentikelenis et al., 2016) and even distributional consequences or socio-political consequences of IMF programs (Casper, 2017;Dreher and Gassebner, 2012; Garuda, 2000;Hartzell et al., 2010; Lang, 2021;Oberdabernig, 2013, and Vreeland, 2002).[8] IMF programs have also been associated to reduced inflation and monetary growth, lower risk of currency crises and banking crises, and improved market performance of banks (Dreher and Walter, 2010; Papi et al., 2015; Steinwand and Stone, 2008).

This paper is also related to a growing body of literature which focuses on the effects of concessional financial flows at the subnational-level (rather than country-level). Indeed some advances have been made in the directions of using outcome variables at more disaggregated levels (Bluhm et al., 2020;Chauvet and Ehrhart, 2018; Dreher and Lohman, 2015; Dreher et al., 2020; Marchesi et al., 2021).

Several contributions have considered in more detail the varied conditions attached to IMF financing, finding that conditions are a key mechanism linking IMF lending to policy outcomes.[9] For example, Reinsberg et al. (2018) and Forster et al. (2019) have focused their attention to structural conditions, Reinsberg et al. (2019) focused on labor conditionality, while Rickard and Caraway (2014),Rickard and Caraway, 2019 have focused on public sector conditions.[10] A recent report (IRC 2019) on the effectiveness of the IMF conditionality shows that over about the last ten years a tendency towards more structural conditionality and longer program implementation horizons has emerged and that in the aftermath of an IMF pro-

[8] More recently, Lang (2021) shows that IMF programs substantially increase income inequality and this increase is driven by income losses for the poor. The effect is strongest for IMF programs in democracies, when conditionality is extensive, and when societal actors have little influence on IMF decision-making.

[9] Marchesi et al. (2011) analyze how communication between the IMF and a borrowing country may affect the size and scope of conditionality.

[10] Reinsberg et al. (2019) document that IMF labor market policy reforms significantly reduce both individual and collective labor rights. Rickard and Caraway (2019), find that IMF loans with public sector conditions generate cuts in wages in the short-term, but these cuts do not persist in the longer-term (due to internal political pressure).

gram all relevant macroeconomic variables tend to improve compared with the pre-program period.[11] In sum, the existing evidence suggests some positive adjustment effects regarding financial, fiscal and monetary positions, but the improvement has generally fallen short of expectations, especially in terms of GDP growth and debt reduction.[12]

The success of any IMF program hinges largely on its catalytic effect, namely the propensity of private capital to flow into a country following the adoption of an IMF program. The signaling role of an IMF adoption and its catalytic effects have both been extensively analyzed in the literature with mixed results (among others Chapman et al., 2015; Corsetti et al., 2006; Gehring and Lang 2020; Krahnke, 2020; Marchesi and Thomas, 1999; Marchesi, 2003; Mody and Saravia, 2006; Morris and Shin, 2006).

As we mainly focus on the alleviation of the firms' financing constraint as a channel of transmission of an IMF programs, this paper also relates to the literature that explain how a financial crisis (including a sovereign debt default) may propagate to firms (e.g., Corsetti et al., 2012; Gourinchas et al., 2017; Mendoza and Yue, 2012).[13] A number of papers then look explicitly at financial crises and their effects on the financial sector.[14] Gennaioli et al. (2014, 2018) show that lending by the banking sector can sharply decline in case of a sovereign default, especially if banks hold large amounts of sovereign bonds. In the specific context of developing countries, due to limited information, low collateral value, and a large informal sector, firms primarily produce soft information and are dependent on a banking system that promotes lending relationships (Helpman et al. (2004); Choudhary and Limodio, 2022). [15] In turn, these domestic banks increase credit supply when capital inflows are higher (Baskaya et al., 2017; Schnabl, 2012).[16]

Finally, this paper also broadly relates to a vast international finance literature that studies the effects of capital flows on firms, focusing on sovereign debt inflows, FDI, bank, and equity portfolio flows (e.g., Broner et al., 2021;Baskaya et al., 2017, and Schnabl, 2012). In particular, the closest contribution to ours is the paper by Broner et al. (2021), who show that sovereign debt inflows reduce the domestic interest rate by raising the price of government debt, which benefits banks directly. In turn, banks expand domestic credit benefiting domestic firms (especially those which rely more on external financing).

In summary, we contribute to the existing literature by showing that the gain in financial creditworthiness induced by IMF intervention is passed on to firms operating in the borrowing countries, in turn leading to an increase in sales. We document that the financing channel is central for the firm to exploit the gain in creditworthiness which occurs at the macro-financial level following an IMF program. Finally, using firm level data allows us to investigate more carefully on the channels of transmission of IMF intervention on the recipient countries' economies, an important aspect that has so far been neglected in this literature.

3. Data description

3.1. IMF intervention

Our primary variable of interest is IMF participation. Namely we consider a country to be under an IMF program for years where there are positive disbursements from the IMF to a member country, as reported by IMF Member Financial Data. Broadly, arrangements can be divided into concessional and non-concessional loans. Concessional loans are reserved for low-income countries and are those loans that carry very low interest rates (0–0.5 percent). Our sample period starts effectively in 2000, making the bulk of the programs considered Poverty Reduction and Growth Facility (PRFG), Extended Credit Facility (ECF) as concessional programs, and Stand-By Arrangements (SBA) or Extended Fund Facility (EFF).[17] It is important to note that since our IV is meant for selection into program but not into program type we do not identify the differential effects of concessional versus non-concessional programs. Section 4 explains our IV in greater detail.

[11] Countries are often unwilling or unable to implement reforms. This is, in part, because countries that are strategically important to key principals (especially the US) tend to receive favorable treatment from the IMF (Oatley and Yackee, 2004; Stone, 2008). In these cases, the IMF is less able to credibly threaten to enforce compliance by suspending loans, implying that these borrowers are less likely to comply with conditionality in the first place (Dreher and Jensen 2007; Dreher et al., 2009; Stone, 2008). Other countries fail to comply with reforms due to domestic politics, for example, compliance often breaks down ahead of elections (Dreher, 2003; Dreher, 2006). Sometimes countries simply lack the technical or bureaucratic capacity to follow through on reforms. As a result, only 33 percent of all IMF programs between 1980 and 2015 were fully completed (Reinsberg et al., 2022a,b).

[12] One area in which the effectiveness of IMF programs has proven less than satisfactory is with serial borrowers, i.e. countries that fail to graduate from IMF financial assistance in due course (e.g., Easterly, 2005, Bird et al. 2007; Marchesi and Sabani, 2007a,b).

[13] This literature is vast and considers different outcomes, such as firms' investments, employment, stock prices and productivity. See Andrade and Chhaochharia (2018) and Hébert and Schreger (2017) for some firm-level empirical evidence.

[14] The feedback between distressed banks and sovereigns was most common in the 2008 crisis and the European debt crisis, (e.g., see the so called "doom loops" effect described by Brunnermeier et al. 2016).

[15] In a recent paper, Choudhary and Limodio (2021), based on evidence from Pakistan, shows that banks in low-income countries face severe liquidity risk (due to volatile deposits) and dysfunctional liquidity markets. Overall, such liquidity problems deter the transformation of short-term deposits into long-term loans discouraging investments at the firm level.

[16] Baskaya et al. (2017), using data on Turkey, show the importance of domestic banks' external borrowing for domestic credit growth and this effect is stronger for domestic banks relative to foreign banks. In the case of Peru, Schnabl (2012) finds that after a liquidity shock international banks reduce bank-to-bank lending to domestic banks, which in turn reduce lending to domestic firms.

[17] The heterogeneity in lending arrangements compiled in the raw data from Kentikelenis et al. (2016) is considerable. Other arrangement types include precautionary deals such PLL, PLC, FCL or shock-specific arrangements like ESF or EAND. In the end however the main lending facilities comprise around 87 percent of the sample.

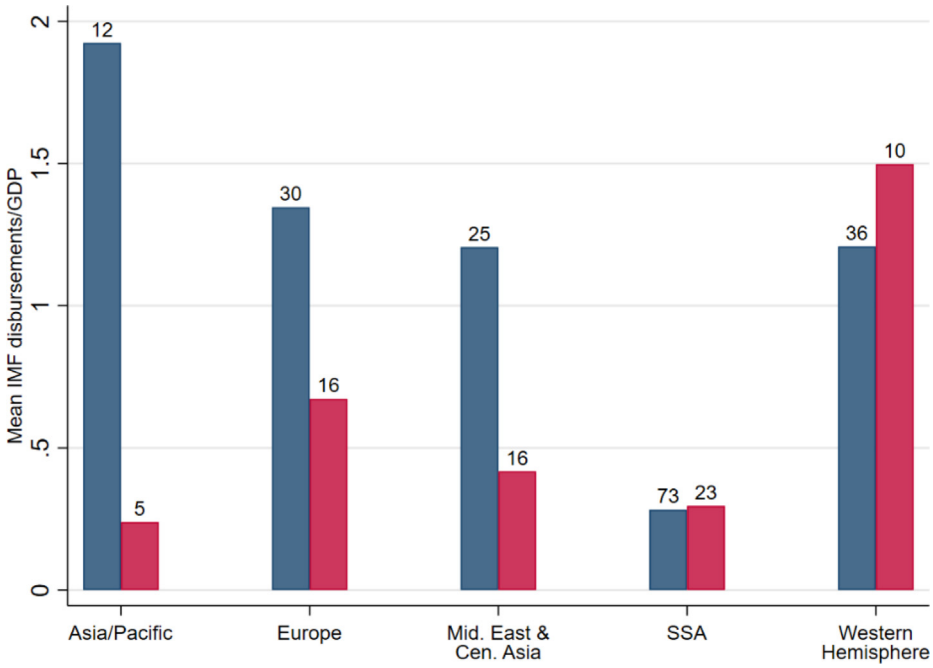

Fig. 1. Average IMF disbursements and total programs by decade and regions. Notes: Plotting mean disbursements and total programs signed by regions and time period. Blue bars correspond to period 2000–2009, red bars 2010–2018. Bar height indicates average disbursement to GDP by category, numbers indicate total programs signed.

In Fig. 1 we plot these disbursements, alongside the number of IMF programs, for the different IMF regional departments, splitting our sample into the period 2000–2009 and the period 2010–2018. There has been a large relative drop in disbursements to Asia, driven by the East Asian financial crises at the turn of the millennia. In general, the size of interventions has diminished in last decade except in Sub-Saharan Africa and the Western Hemisphere, which includes Latin and South America. The number of programs signed per period also fell across regions. For our baseline model, the treatment is constructed at the country-year level. The result is an indicator variable for IMF participation. This dichotomization is standard in the literature, and allows us to capture the effects of being under a program versus not being under a program.

Our second variable of interest is the stringency of IMF programs, as measured by the number of binding conditions. For this we rely on the dataset compiled by Kentikelenis et al. (2016). The authors exclusively use IMF executive board documents (Letters of Intent and Memorandum of Economic and Financial Policies), which are of greater reliability and more comprehensive with respect to similar data.[18] Their dataset contains disaggregated data on IMF conditionality, providing information on 32,261 conditions for 135 different countries over the period 1980–2016. These conditions are categorized by conditionality type (*hard* v. *soft*, quantitative or structural) as well as the relevant affected policy areas. For the purpose of this paper, we focus on this latter dimension.

We focus on the number of conditions as a measure of stringency of IMF programs. Kentikelenis et al. (2016) group conditions for each program into one of 13 mutually exclusive affected policy areas; fiscal reforms, revenues and tax reforms, financial sector and monetary policy reforms, state-owned enterprise reform, state-owned enterprise privatization, external debt reforms, trade and exchange systems reforms, public and private labor reforms, social policies, redistributive policies, institutional reforms, land and environmental reforms, and a residual category.[19] Fig. 2 plots the yearly evolution of the average number of conditions per policy area imposed by the IMF. In the figure, we aggregate policy areas into five distinct groups in order to reduce the granularity of the data. [20]

As can be seen, the increase in total conditionality is driven primarily by conditions in the area of financial reform. Our analysis on IMF conditionality will therefore focus on the total number of conditions and on financial conditions, because of its relative importance in the IMF conditionality toolkit and its direct relation to the financing channel we wish to explore.[21]

[18] The Monitoring of Fund Arrangements (MONA) for example is the IMF's proprietary database on arrangements and conditionality, but is less detailed

[19] The raw data from Kentikelenis et al. (2016) also provides a grouping based on conditionality type, namely whether conditions fall into the categories of Indicative Benchmarks, Prior Actions, Quantitative Performance Criteria, Structural Benchmarks, Structural Performance Criteria, or Performance Criteria. We test for the differential effects of structural conditions (such as Prior Actions, Structural Benchmarks, or Structural Performance Criteria) against quantitative conditions, but we did not find sizeable differences across types of conditions. Results are available upon request.

[20] Table A2, in the online Appendix A, shows the resulting heterogeneity among policy-area reforms.

[21] Our definition of Financial conditions include conditions related to both the financial sector/monetary policy and external debt (see Table A2, in the online Appendix A).

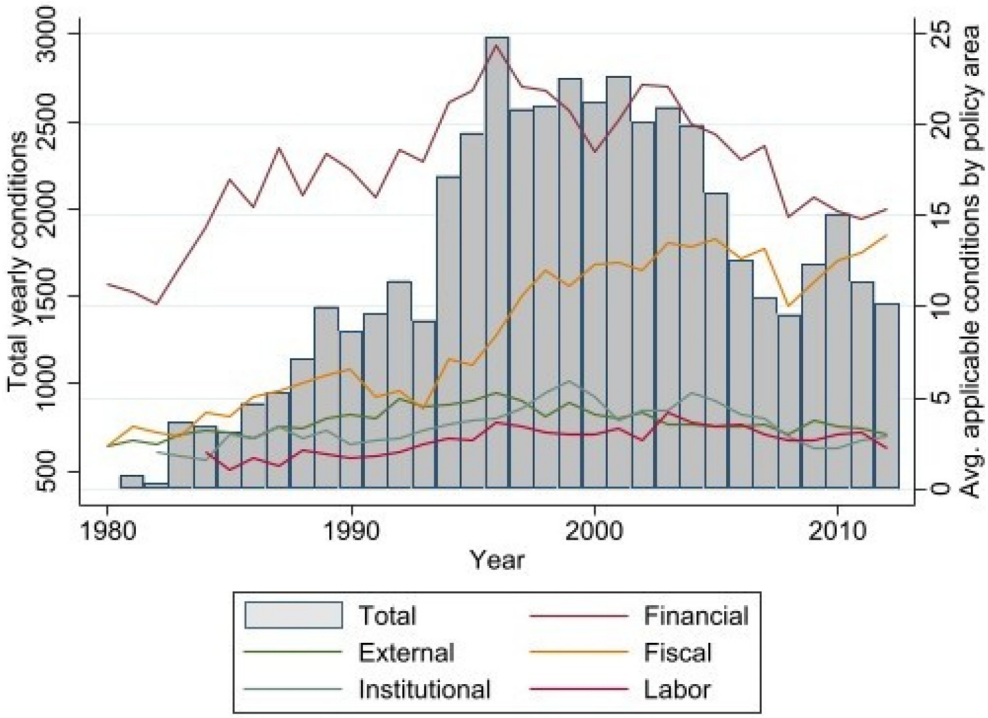

Fig. 2. Total IMF conditions and average conditions by policy area. Notes: Evolution of number of conditions by policy area. Left axis shows total number of binding conditions imposed as part of IMF programs, right axis shows average number of conditions by policy area.

3.2. Firm-level data

The main outcome variables on firm performance come from the World Bank Enterprise Survey dataset. The version of the survey utilized in this paper covers 139 countries between 2003 and 2018, and provides information for approximately 130,000 unique firms over 4 iterations of the survey. One of the advantages of this updated version of the WBES is the availability of multiple questionnaire waves, which gives the possibility to track firms which participate in more than one wave. Within the survey, there are close to 15,000 firms which were recontacted at least once over the different iterations. Detailed information on the number of surveys per country and firms per survey can be found in Table A5, in the online Appendix A.

The survey is constructed to generate a representative sample of the manufacturing and service sectors in a country, with the aim of providing indicators for the investment climate in a country. This means that questions are geared towards assessing the business-related constraints of firms, including administrative, financing, and labor or legal constraints. Interviews are conducted face-to-face by private contractors with business owners or managers, and responses are harmonized across countries for comparability.[22]

The sampling methodology for each country follows a stratified random sampling according to 3 criteria (firm size, sector, geographic location). This effectively allows a random sampling which is more representative of the economic composition of the country, since the likelihood of being selected for an interview is dependent on the individual firms' place in the distribution of firms within a country, as well as its location with respect to geographic areas of economic activity and economically relevant sectors. The population of firms to be sampled is typically derived from official databases or country authorities, but is sometimes selected directly by the World Bank, based on clusters of major economic activity in a country when official sources are weak.

The final sample used in our analysis covers 135 developing and emerging market countries: 22 Asian, 52 African, 31 Latin America, and 30 Eastern European. We drop countries defined as being in conflict during survey years, since these countries tend to experience abnormally high growth rates in the reconstruction years following violence, and survey participation and integrity is also compromised in years of conflict. [23] Only a small set of countries, however, are affected by this filtering, namely countries like Afghanistan, Iraq, or the Democratic Republic of Congo, which are effectively dropped from the analysis.

[22] The survey targets formal (registered) firms with 5 or more employees in the primary manufacturing and services sectors. Firms with 100 percent government or state ownership are not eligible for interview.

[23] We consider a very stringent definition of conflict, based on the World Bank *Global Spread Of Conflict By Country And Population*. Conflict is defined as having both 20 percent or more of a countries geographic area under conflict as well as at least 10 percent of the population affected.

Following this trimming, the distribution of firms and the respective re-contact rate, defined as the share of firms per country that are observed in more than one wave of the survey, is rather homogeneous.

We observe 38,870 unique firms in the whole of Africa, with about 18 percent of firms being recontacted. In Asia we instead observe 29,542 unique firms, of which 16 percent are recontacted, while Latin America has 28,688 unique firms which participated in the survey, but a larger percentage of these (33 percent) were successfully recontacted. Finally, across Eastern Europe we have data on 26,744 unique firms with 18 percent of them being recontacted. Within regions, the recontact rate varies by country, where smaller countries rarely participate in multiple waves of the survey.

Data on firm sales within the survey are reported at time t and $t-2$. We use these two points to construct the average firm sales growth over the 3 years. This lag structure also means that our sample effectively covers the years 2000 to 2018. We also log transform sales because of large differences in the values both across firm size within countries as well as across countries. From the survey we extract a large set of firm level controls, which we describe more carefully in Section 5 below. Similarly, the WBES provides information on 51 stratified industries of operation for the firms, which we group into the nine macro-industries. These industry identifiers allow us to construct industry-year dummies to account for time-varying heterogeneity. Table A1, in the online Appendix A, reports the distribution of firms within these sectors.

Our final macro–micro dataset then matches country-level variables,including IMF data, and firm-level variables at the country-year level. Fig. 3 plots this information by displaying the share of the years from 1980 to 2018 for which a given country was under an IMF conditionality-based program, based upon the SDR commitments after signing of an agreement with the IMF. Overlaid to this are unique number of firms recorded per country represented in the WBES, where it can be clearly seen that the overlap is strong, with some notable exceptions such as Namibia which participated in the WBES but did not sign any IMF agreements.

As a final part to our analysis, we merge our firm level data with a dataset compiled by Isaka and Paul (2019), who use the same World Bank Enterprise Survey data to compute the share of income accruing to the workers for each firm.[24] Following Zhou (2016), the labor income share (LIS) at the firm level is defined as:

$$LIS_{i,t} = \frac{Compensationofemployee_{i,t}}{Totalsales_{i,t}} \tag{1}$$

Where compensation of employees is the total annual cost of labor (including wages, bonuses, and social payments). Using this definition, we can use almost all observations in our dataset, including services and other sectors. Some observations however are found far beyond its expected range. These values may bias our estimation, so we detect outliers as follows. First, we take the logarithm of labor income share. Then we apply the three-standard-deviation rule; observations that are more than three standard deviations away from the mean are dropped. We utilize the same methodology as in our baseline specification for firm sales to analyze the impact of an IMF program on the labor income share.

4. Endogenous Selection into IMF Programs

We want to test whether the presence of an IMF program in a given country may affect the growth of firm sales in that country. The fundamental methodological issue with this question is that selection into IMF programs is obviously not random. On the contrary, "treated" countries typically experience an economic crisis when entering into a program, which directly affects the performance of their firms. As a consequence, simple comparisons between treated and non-treated will not yield causal effects, but instead will capture the negative bias resulting from omitted variables and reverse causality.

Following Gehring and Lang (2020) and Lang (2021), we take the Funds's liquidity ratio, defined as the share of liquid resources over liquid liabilities, as a proxy for the lenders budget constraint and interact it with the recipient-specific probability of receiving a loan from the IMF as an instrument for IMF intervention. This strategy follows a difference-in-differences logic and is similar to shift-share or Bartik instruments.[25] The IV equation is then the following:

$$IV_{j,t}^{IMF} = IMFliquidityratio_t \text{x} IMFprobability_{j,t} \tag{2}$$

where *IMF probability* is the (time-varying) share of years between 1980 and 2018 that country j received an IMF loan, while IMF resources is the temporal variation of *IMF liquidity*, which is defined as the organization's liquid resources divided by its liquid liabilities. The data on IMF liquidity derives from the original dataset of Lang (2021), which we extend up to 2018. Figure B1, in the online Appendix B, plots the natural log of the liquidity ratio and its components. Liquid resources are composed primarily of usable currencies and SDR on the Funds' balance sheet, and at times include additional borrowings when

[24] The labor income share is essentially a macroeconomic concept, defined as the share of national income allocated to labor, and is generally computed from aggregate data by dividing total labor compensation by national income (GDP). However, even this computation does not give us the labor income share that we seek to obtain because it overlooks contributions from self-employment (Krueger, 1998; Gollin, 2002). If the earnings of the self-employed are taken as capital income as in the conventional method, then it may underestimate the true value of labor income share and bias international comparisons (Guerriero, 2012).

[25] Earlier work in this area focuses on shocks affecting donor countries such as the variation in steel production to instrument aid from China (Dreher et al. 2021) or on temporal variation in US wheat production to instrument US food aid (Nunn and Qian 2014). Bartik shift-share instruments were typically used in the labor and migration literature (e.g., Autor et al., 2013;Altonji and Card, 1991). See Goldsmith-Pinkham et al. (2020) for a discussion of Bartik style instruments.

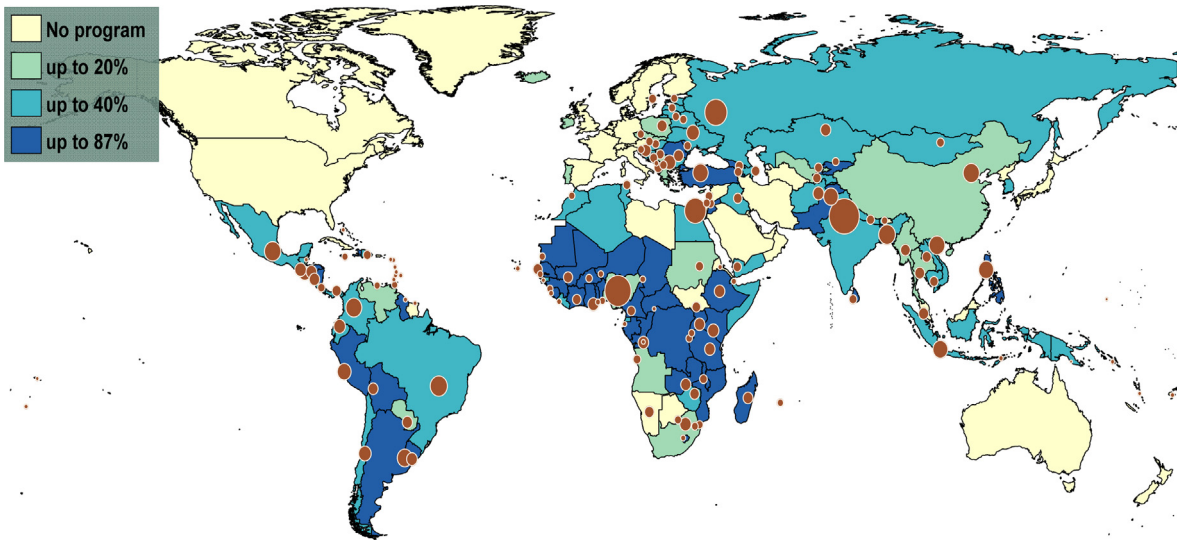

Fig. 3. Share of years under IMF program and representation of WBES countries. Notes: Average number of years from 1980 to 2018 a country is under an IMF program as measured by the presence of positive SDR commitments in a given year t. Size of bubbles proportional to the number of distinct firms sampled in given country under WBES.

complementary resources are raised to boost lending capacity. Liquid liabilities instead are the sum of reserve tranche positions and outstanding borrowing. We rely on IMF Annual Reports, published yearly in April, as well as the April update of the Fund Resource and Liquidity Position.[26]

Our main identification strategy is based on an instrumental variable (IV), which combines the temporal variation in IMF liquidity with the cross-sectional variation in a country's prior probability of participating in an IMF program. This strategy exploits the fact that the IMF tends to expand its regular clientele in years in which its liquidity is higher, so that countries with an initially lower participation probability are more likely to receive a program in these years (as displayed in Figure B4, in the online Appendix B). Controlling for year fixed effects (which captures the IMF liquidity component of the interaction term) as well as for the individual time-varying, country-specific probability component of the interaction term, the identifying assumption underlying this approach follows a difference-in-differences logic. What we investigate is the differential effect of IMF's liquidity on the present participation in an IMF program in countries with a high compared to a low probability of receiving IMF loans.

Given the difference-in-difference structure of the identification strategy, the exclusion restrictions would be violated if there were some unobservable, time-varying trend affecting sales differently across countries based on their past exposure to IMF programs. There are several reasons why we think this is unlikely. First, one of the key features of this methodology is the fact that the IMF's liquidity varies primarily because of an institutional rule that requires the IMF to review the financial contributions of its members (quotas) every five years (Lang, 2021, Gehring and Lang, 2020). The timing of this variation is therefore exogenous to both global economic cycles and country-specific trends in firm sales. Again, even if there were evidence of correlation between the two, it would only bias the results if the correlation was contingent on a country's past participation in IMF programs.

Another main source of IMF liquidity depends on its borrowing from a group of members under the New Arrangements to Borrow (NAB), which are typically activated in the event of a major crisis. Hence, one could argue that NAB's might violate the exclusion restrictions due to the tie between liquidity and crisis. However, this liquidity boost would be problematic to the extent that it differentially affects countries with high vs. low probability of participating in an IMF program. To the contrary, our marginal effects show that the IMF tends to expand its clientele in years of high liquidity rather than financing the usual countries supporting the idea that a large positive shift in liquidity deriving from additional borrowing (such as NAB) would be redistributed among all borrowers. Furthermore, Lang (2021) shows that this identification strategy is robust to dropping years after 2008, when the majority of NABs were disbursed. Unfortunately, given the survey structure of our data we are not able to carry a similar robustness test.

[26] For *IMF probability*, we start the count of years of past IMF participation in 1980 and thus 24 years before our observation period starts. This ensures that the variable does not fluctuate strongly from one year to the next for the early years of the sample and increases the plausibility of the exclusion restriction because it is determined by earlier periods.

Finally, we plot the IMF liquidity ratio over our period of estimation alongside the GDP trends in countries, distinguishing between different degrees of past IMF participation.[27] The trends (which are shown in Figure B2, in the online Appendix B) are clearly parallel and not obviously correlated to IMF liquidity.

4.1. Selection into conditionality

The same endogeneity concerns holding for selection into an IMF program apply when considering the severity of the program a country is assigned. We proxy this severity by the type and number of policy-area related conditions imposed as part of the borrowing arrangement. From an identification perspective, selection into a more "severe" program is not random. We argue that countries which are experiencing economic downturns are more likely to require intrusive conditionality. Furthermore, the total number of conditions depends on series of unobservable characteristics that introduce an omitted variable bias.

The identification strategy adopted here is similar to the one by Forster et al. (2019), and again follows a similar reasoning of the compound IV strategy by Lang (2021) explained at the beginning of this section; that is, IMF flexibility towards borrowers is reduced in years where its budget constraint is binding. In this context, as shown by Forster et al. (2019), a preferable proxy for budget constraint would be given by the number of countries under an IMF program in a given year (rather than IMF liquidity).

As more countries require assistance, Funds resources are stretched and therefore programs on average entail more conditions to balance demand with the available resources. On the other hand, the time-varying average number of conditions per policy area for a given country captures the government bargaining position with the IMF. More specifically, as shown by Dreher and Jensen (2007) and Dreher et al. (2009), countries receiving more conditions by the Fund would have a lower bargaining power and hence would tend to obtain a greater number of conditions also in the future.[28] In other words, once in a program, the probability of having more conditions for countries that already received more conditions in the past is higher when Fund budget is tight, as the Fund needs to be more selective in its allocation of funds and more conditions are imposed for countries with a higher historical average (lower bargaining position).[29] The marginal effects displayed in Figure B5, in the online Appendix B, confirm the proposed mechanism, namely that the greater the number of countries under an IMF program per year, the greater the effect of past conditionality on the current number of conditions. Together, these two predict the variation in number of conditions per policy area.[30] Formally, we can write the instrument as:

$$IV^{IMF}_{j,p,t} = CountriesunderIMFprogram_t \text{x} Average number of conditions_{j,p,t} \tag{3}$$

where p stands for each policy area in a given country j. We therefore run separate regressions, considering first total conditions and then financial conditions, and plot separately the first-stage effects on number of conditions for these two policy areas (see Figure B5 reported in the online Appendix B). Because the IV works to identify the extent of IMF programs, the regression is run on a sub-sample of countries under an IMF program. The instrument follows the same diff-in-diff logic as the original instrument, and the same caveats apply. The exclusion restrictions are satisfied so long as variables correlated to the number of countries under an IMF program do not affect firm sales growth differently in countries with high versus low average number of conditions, conditional on all our sets of controls and fixed effects. A typical argument could be that global financial crises lead to an increase in the number of countries under a program, but it is unlikely that these global shocks affect firm sales differentially based on countries past exposure to specific IMF conditionality.

5. Empirical strategy and results

Our preferred model for assessing the impact of an IMF program on firm performance is a two-stage least squares (2SLS) which takes the following general form:

$$\begin{aligned} 1^{st}Stage: IMF_{j,t} = \; & \alpha_1 (IMFprobability_{j,t} * IMFliquidity_t) \\ & + \alpha_2 IMFprobability_{j,t} + \beta X_{i,k,j,t} + \mu_j + \tau_t + \varepsilon_{j,t} \end{aligned} \tag{4}$$

$$2^{nd}Stage: g_{i,k,j,(t,t-2)} = \alpha \widehat{IMF}_{j,t} + \beta X_{i,k,j,t} + \gamma Z_{j,t} + \delta IMFprobability_{j,t} + \tau_{k,t} + \mu_{j/i} + \varepsilon_{i,k,j,t} \tag{5}$$

where g is our outcome variable for firm i, in industry k, and country j. $\widehat{IMF}_{j,t}$ is our variable of interest measuring IMF intervention. X is a set of time varying firm-level characteristics, while Z is a set of time varying country-level variables. We also add the time-varying share of years that country j was under an IMF program. This means that we control for the initial, pre-

[27] Due to the survey structure of the data, it is not possible to construct country-specific trends over time in sales.

[28] Dreher and Jensen (2007) show that countries voting in line with the US in the United Nation General Assembly (UNGA) received IMF loans with fewer conditions. Dreher et al. (2009) show that temporary members of the United Nations Security Council (UNSC), on average, receive a smaller number of conditions during their mandate.

[29] An additional possibility could also be that more conditions are imposed in a country that in the past had already more conditions, because incomplete implementation of previous conditions requires more conditions in a new program.

[30] Figure B6, in the online Appendix B, plots the total number of binding conditions against the number of countries under an IMF program for a given year.

determined *IMF probability* in both stages while year fixed effects absorb the level effect of IMF liquidity. Hence, for identification we only need to assume the exogeneity of the interaction term conditional on its two constituent terms (as well as the fixed effects and the control vector X). We then include industry-year dummies $\tau_{k,t}$, in order to control for industry time-varying heterogeneity and $\varepsilon_{i,k,j,t}$ is the error term. We include either country or firm fixed effects according to the specification (country dummies when using a pooled model, otherwise firm fixed effects, and with standard errors clustered at the country level). Finally, to avoid extremely fast-growing firms driving the results, we exclude the top one percent of sales growth from the sample distribution. Our main specification considers as the outcome variable the average firm sales growth, measured as the change in (log) firm sales between t and t-2.[31] We then control for a number of characteristics at the firm level, following the same specification of Chauvet and Ehrhart (2018). First we take *Sales*, in logarithm, measured at one lag with respect to the dependent. Firm *Size* takes the value one for firms with fewer than 20 employees, the value two for firms with between 20 and 100 employees, and three for firms with more than 100 employees. We also consider the characteristics of firm ownership using two variables, *State* and *Foreign*. *State* is a dummy variable which is equal to one when part of the firm is (partially) owned by the state, while *Foreign* is a dummy variable which is equal to one when part of (or all) the firm is owned by a foreign individual or company. Finally, we include information on whether the firm is outward looking using *Export*, which is a dummy variable equal to one when the firm exports part of or all its sales, either directly or indirectly (as a supplier to exporting firms). These firm-level characteristics are measured in year t since we do not have their predetermined value at year t-2.

At the country level, we control for a country's *GDP per capita* and *GDP growth rate*. Both variables are averaged over a three-year period. We also control for the size of the country using the logarithm of the *Population*. Finally, we consider the quality of institutions using the ICRG index of *Corruption*, where a higher value of this variable refers to a higher quality of economic institutions.

Our sample consists of rather large, formal firms: around 22 percent are outward looking (exporting either directly or indirectly) and the average size is about 20 employees. Furthermore, around 50 percent of firms rely on some form of external sources of financing (defined as borrowing from either bank, non-bank financial institutions, or on credit), and about 70 percent of firms report financial obstacles.[32] Table A3, in the online Appendix A, shows some basic summary statistics, while Table A4 presents a description and source of all the variables used in the analysis.

5.1. IMF participation

We now provide our baseline results, where we look at the effect of participating in an IMF program on firm sales growth. Columns 1 to 4 of Table 1 shows our results for a simple pooled OLS, a two-stage least squares (2SLS), a fixed-effects model, and a 2SLS with fixed-effects, respectively. Columns 5–8 repeat this structure but with a lagged value for our variable of interest, IMF program. Beginning with a pooled OLS model allows us to utilize the entire sample without restricting ourselves to the subsample of firms that were recontacted over different iterations of the survey. All specifications contain a series of firm and country-level controls which are shown, as well as industry-year fixed effects to account for time-varying unobservable heterogeneity. Models without firm fixed effects contain country dummies, while instead when firm fixed effects are used the country dummies are dropped because of collinearity.

Among the firm-level controls, the coefficients of *Foreign* and *Exports* are both positive and significant almost always, suggesting that outward-looking firms and firms which are foreign-owned tend to have higher growth rates. *Size* is also positive and significant suggesting that larger firms also tend to have a positive growth of sales. [33] Interestingly, the coefficient of *State* is positive but not always significant. Among the country-level controls, countries with greater *Corruption* experience lower firm sales growth, while the coefficients of both *Population* and *GDP Growth* are not significant. Both the coefficients of *GDP per capita* and *Sales* suggest a catching-up effect: countries with lower level of development and firms with lower initial sales tend to experience higher growth of sales.

Turning to the relationship between IMF participation and firm sales growth, we see that the coefficient on IMF program is always positive and statistically significant in the IV specifications. The first-stage results show the coefficient for our instrument, which is always negative as expected. Kleibergen-Paap tests provide further evidence in support of identification.

Table 1 shows our baseline results. In columns 1 and 3, where we estimate a pooled OLS and a fixed-effects model without instrumenting, we do not find statistically significant results. Instead, when instrumenting for IMF participation, as in column 2, we find that sales increase by 26 percent for firms in countries under a Fund program and the coefficient is significant at the five percent level. When we control for firm fixed-effects we find comparable results; sales increase by about 24 percent when instrumenting for participation in a program (column 4), with the coefficient being significant at the one percent level. Similarly, we find evidence of positive, medium-term effects. In column 6, where we use an instrumented lagged value

[31] More precisely, since in the WBES all data on sales are reported for the last fiscal year, our outcome variable would consider the average difference in log sales between the last fiscal year (t-1) and the reported sales from 3 years ago (t-3). For notational simplification, we label these as t and t-2.

[32] More precisely, 49 percent of contacted firms in our sample report a non-zero amount of working capital financed from external sources, while 69 percent of firms reported that access to finance presents at least some obstacle to operations.

[33] There is a strong correlation between *Size, Foreign*, and *Exports*, as most of the larger firms in the sample are those firms which tend to export or be a foreign subsidiary; something which is standard in the literature on international trade (see, among others, Melitz 2002; Helpman et al. (2004)).

Table 1
IMF participation and firm sales growth.

	Contemporaneous				Lagged			
	Pooled OLS (1)	2SLS (2)	FE (3)	2SLS FE (4)	Pooled OLS (5)	2SLS (6)	FE (7)	2SLS FE (8)
IMF participation	0.015	0.261**	0.025	0.240***	0.057**	0.440*	0.037	0.375**
	(0.66)	(2.04)	(0.91)	(3.54)	(2.29)	(1.71)	(1.31)	(2.49)
(ln) Sales(t-1)	-0.045***	-0.045***	-0.076***	-0.076***	-0.046***	-0.046***	-0.076***	-0.075***
	(-13.17)	(-12.60)	(-8.32)	(-8.80)	(-13.24)	(-12.78)	(-8.18)	(-6.86)
State	-0.0001	0.005	-0.001	-0.007	-0.0006	0.004	-0.001	-0.006
	(-0.09)	(0.42)	(-0.03)	(-0.16)	(-0.06)	(0.39)	(-0.03)	(-0.14)
Foreign	0.037***	0.034***	0.026**	0.020	0.037***	0.035***	0.026**	0.019
	(7.15)	(5.87)	(2.06)	(1.55)	(7.14)	(6.46)	(2.07)	(1.45)
Exports	0.026***	0.025***	0.028***	0.030***	0.027***	0.027***	0.028***	0.034***
	(6.28)	(5.86)	(2.77)	(2.74)	(6.31)	(6.15)	(2.78)	(2.69)
Size	0.088***	0.088***	0.066***	0.065***	0.087***	0.088***	0.064***	0.052***
	(13.07)	(12.60)	(5.49)	(5.68)	(13.11)	(12.83)	(5.29)	(3.86)
(ln) GDP per Capita	-0.074	-0.081	0.051	0.056	-0.042	0.173	0.072	0.282**
	(-0.94)	(-0.77)	(0.61)	(0.53)	(-0.54)	(1.07)	(0.85)	(1.98)
GDP Growth	-0.003	-0.002	0.002	-0.001	-0.005	-0.021	0.001	-0.018
	(-0.71)	(-0.34)	(0.65)	(-0.19)	(-1.40)	(-1.54)	(0.18)	(-1.40)
Population	-0.050	-0.118	-0.217	-0.510	-0.037	0.162	-0.214	-0.128
	(-0.18)	(-0.24)	(-0.62)	(-1.15)	(-0.13)	(0.33)	(-0.60)	(-0.20)
Corruption	0.007	-0.055	-0.007	-0.056	0.003	-0.054	-0.007	-0.067
	(0.29)	(-0.88)	(-0.27)	(-1.04)	(0.15)	(-0.87)	(-0.31)	(-1.10)
IMF probability		-0.329		0.206		-0.537		-0.757*
		(-0.72)		(0.56)		(-1.19)		(-1.75)
First stage: Instrument		-1.004***		-1.366***		-0.695**		-1.235***
		(-2.91)		(-5.21)		(-2.09)		(-2.97)
Observations	77524	77524	10586	10586	77524	77524	10586	10586
R2	0.152	0.039	0.225	0.065	0.153	0.021	0.226	0.004
Kleibergen Paap (p-value)		0.037		0.006		0.057		0.014
Panels			5114	5114			5114	5114
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	NO	NO	YES	YES	NO	NO	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	NO	NO	YES	YES	NO	NO

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Column 3 uses the within estimator with firm fixed effects. Column 4 uses an IV estimator with firm fixed effect. The coefficient Instrument in this case is the IV IMF liquidity x IMF probability for the first stage in our IV models. Columns 5–8 use the same estimators as in columns 1–4, but the variable of interest IMF participation is lagged by one period. All models include industry-year dummies and firm and country level controls. Kleibergen-Paap p-values are for the underidentification LM test. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p < 0.01, **<p0.05, *p < 0.1.

of IMF participation, we find a roughly 44 percent increase in average firm sales growth, with the coefficient being significant only at the ten percent level. Controlling for firm fixed-effects and instrumenting for participation in a program, we find that sales increase by about 38 percent (column 8), with the coefficient being significant at the one percent level. This result is in line with the previous literature, according to which the effects of IMF programs tend to be stronger when measured on longer horizons (see Balima and Sokolova, 2021).

Our results would correspond to an average firm growth of about 3.6 percent (column 2) and 3.4 percent (column 4), which are comparable with the empirical evidence currently available in the literature regarding the effects of a country's IMF program participation on its output growth (e.g., Bas and Stone, 2014; Binder and Bluhm, 2017).[34] Such effects are notable, and we attribute them to a signaling effect of IMF participation. In a nutshell, the main intuition is that the adoption of an IMF program signals a country's "good intent" (as in Marchesi and Thomas, 1999; Gehring and Lang, 2020), which is then rewarded by either some commercial debt restructuring (Marchesi, 2003) or private capital inflows (e.g., Mody and Saravia, 2006,Morris and Shin, 2006; Krahnke, 2020). In turn, such catalytic effect both improves the recipient's financial markets and gives sovereign borrowers some fiscal space. We should also emphasize that this effect can realize both at the firm level (e.g., as financially constrained firms are able to borrow more or more easily) as well as at the country level as domestic (and foreign operating on domestic soil) credit institutions become more able and more willing to lend. In the next section we provide some evidence on the channels through which IMF intervention is expected to affect firm performance.

5.2. Channels of transmission

In general, the literature points out that financial flows can have both demand and supply side effects on firms (e.g., Chauvet and Ehrhart, 2018,Marchesi et al., 2021). On the demand side, the effects are theoretically ambiguous. On the

[34] Given our sample firm average sales growth of 14 percent.

one hand, IMF programs are expected to alleviate the government borrowing constraints, and hence increase the size of government budgets. This effect would be especially pronounced for firms which are large, state-owned, or operate almost exclusively in sectors directly affected by government expenditure. On the other hand, given its historical preference for austerity-oriented measures, it is hard to reconcile the IMF programs with a boost in government spending.

From a supply-side perspective, we test empirically if the presence of an IMF program may have an impact on firm sales through some specific firm financial characteristic.[35] The reason we focus on this channel is related to our working hypothesis, namely that the Fund is expected to release the financial burden of firms operating in recipient countries, which would in turn lead to an increase in sales. Concretely, a reduction in financial frictions could occur through a *signaling effect*. This improves the balance sheets of domestic financial institutions holding sovereign bonds by reducing a country's sovereign risk, in turn spurring on increased lending. Moreover, such signaling effect, by restoring a country's creditworthiness, may enhance private capital inflows into the recipient countries, benefiting directly firms or financial institutions through *catalytic finance*. Considering both effects is important to our analysis. For example, in emerging economies, a typical catalytic finance mechanism is likely to be more relevant, given the importance of foreign capital flows in these countries. Instead, in low-income countries, given the importance of domestic bank lending to the financial system, the reduction of systemic risk has positive effects on financing for firms. Furthermore, in general more liquid capital markets also imply less problems with sourcing when the production process requires imports or inputs external to the firm.

Therefore, we postulate that a firm achieves growth in sales due to an improved access to finance after the approval of an IMF program. To measure this, we distinguish those firms whose main source of financing comes from external channels (such as commercial banks, suppliers credit, other financial institutions).[36] In line with the existing literature (e.g., Andrade and Chhaochharia, 2008; Arellano et al., 2017;Broner et al., 2021), we expect that firms relying more on external finance should be affected more.[37] Moreover, we look specifically to the value of credit opening (*Loan* approval). Since in most cases such loans are made by private commercial banks, the effect of this variable can be interpreted along the intensive margin.[38] As we described in Section 2, firms in developing countries heavily rely on access to external liquidity, especially banks, in order to fund their operating cost (Lins et al., 2010; Choudhary and Limodio, 2022).

We then look at those firms that have explicitly declared to have experienced some *Financial obstacles*. In addition, we include *Size*, as it is a good proxy for access to finance and may affect the ability to benefit more from the IMF intervention.[39]

Finally, we check whether *Exporting* firms may benefit differentially from an IMF program with respect to non-exporting ones. Two contrasting effects should be in place. On the one hand, foreign-currency borrowing is important for many firms in developing countries, which could then benefit from a renewed sovereign credibility. Moreover one might expect that an export oriented firm is more likely to benefit from the IMF intervention through an improved access to trade credit (e.g., Petersen and Rajan (1997)). On the other hand, capital inflows may also adversely impact exporters to the extent that they induce Dutch disease, that is an appreciation of the real exchange rate detrimental to outward-looking firms (Rajan and Subramanian, 2011).[40] Focusing on sovereign inflows, Broner et al. (2021) also document that these lead to an appreciation of the domestic currency thereby benefiting firms operating in non-tradable industries as opposed to those operating in the tradeable sector.

In order to examine this heterogeneity, we re-estimate the baseline model presented in Eq. 4, splitting the firms into two groups based on the observed value of the characteristic in question. This method, while coming at the costs of reducing the number of observation for each sub-sample, has the advantage of allowing us to use the same identification method over the different sub-sample. Regressions are run using an IV estimator with country fixed effects only.[41] The results are presented in Table 2, in which columns are sorted according to the channel (i.e., firm characteristic). Odd columns show the results obtained from the subsample of firms without the characteristics under consideration, while even columns consider the sub-sample of firms with that characteristic. In particular, we distinguish between firms relying on a degree of *External* finance which is below or above the industry median value (columns 1–2), firms requesting (bank) *Loan*s smaller or biggerthan the median loan (columns 3–4), firms reporting lack or the presence of *Financial obstacles* (columns 5–6), firms that are smaller or bigger than the median *Size* (columns 7–8) and *Exporting* and non-exporting firms.[42]

We show the estimated 2SLS coefficient for IMF program for each sub-sample regression. What differs across the different specifications is the size of the coefficients, and hence the magnitude of the effect of IMF participation on firm performance

[35] This strategy was first implemented by Rajan and Zingales (1998), who investigate whether financial development facilitates economic growth by exploring whether it may reduce the costs of external finance to firms.

[36] Suppliers credit means that working capital is purchased on credit or advances from suppliers or customers.

[37] In particular, Broner et al. (2021) focus on domestic financial firms, that are directly connected to the government, firms that are more financially dependent, and firms operating in tradeables. Due to lack of data, we can only focus on firms that are more financially dependent and on exporting firms.

[38] More specifically, in our sample, around 71 percent of loans are made by private commercial banks, 17 percent are made by state-owned banks or by government agencies, while less than 3 percent are made by non-bank financial institutions (such as microfinance institutions, credit cooperatives, credit unions and finance companies). The remaining sources of finance are unspecified.

[39] Small firms are more likely, than big ones, to report larger financing obstacles, hence they are more likely to benefit from a credit injection due to the Fund intervention (Begeneu and Salomao, 2015; Bottero et al., 2020;Cooley and Quadrini, 2001).

[40] By Dutch disease we refer here to the apparent causal relationship between the increase of capital inflows and the decline of a country' export. The idea is that after the capital inflows the country's exchange rate appreciates, hence depressing its terms of trade. More generally, it can also refer to any intervention resulting in a large inflow of foreign currency, including a sharp surge in natural resource prices or foreign direct investment.

[41] The sample with firm fixed effects would be to small to allow us to split the data according to the firm characteristics under consideration.

[42] The *Size* of the firm could be interpreted as another measure of the firm's financing constraints.

Table 2
IMF participation and firm sales growth, financial channels.

	Without External Finance (1)	With External Finance (2)	Loan size small (3)	Loan size big (4)	Without Fin. Obst. (5)	With Fin. Obst. (6)	Size small (7)	Size big (8)	Non Exporters (9)	Exporters (19)
IMF participation	0.247*	0.276**	0.083	0.314*	0.203*	0.307**	0.308*	0.199**	0.319*	0.195**
	(1.83)	(2.06)	(1.33)	(1.98)	(1.82)	(2.15)	(1.86)	(2.12)	(1.84)	(2.29)
Difference test (p-value)	0.00	0.00	0.00	0.00	0.00					
Observations	37224	39073	12201	12459	45666	29102	50232	27292	60270	17253
R2	0.052	0.041	0.082	0.053	0.046	0.037	0.028	0.025	0.026	0.053
Kleibergen Paap (p-value)	0.063	0.013	0.002	0.028	0.028	0.041	0.053	0.017	0.056	0.006
Firm FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF participation on average firm sales growth. Regressions are run using an IV estimator with country fixed effects. Columns are sorted according to the channel (i.e., firm characteristic). Specifically, different subsamples are obtained by splitting the data by whether the firm has the characteristic in question or not. Odd columns show the results for the subsample of firms without the characteristics under consideration, while even columns consider the sub-sample of firms with that characteristic. In particular, we distinguish between firms relying on External finance below or above the median value (columns 1–2), firms with a Loan smaller or bigger than the median loan (columns 3–4), firms reporting lack or the presence of Financial obstacles (columns 5–6),.firms that are smaller or bigger than the median size (columns 7–8), Exporting and non-exporting firms. To test the equality (or difference) of our coefficients of interest over the two specifications, we run a simple Chow test. The Chow statistic is computed by running the same model on two subsamples, splitting the data by whether the firm has the characteristic in question or not, and on the full sample. The statistic is distributed F(k, N1 + N2-2*k), with k degrees of freedom, and N1 and N2 observations on the subsamples. All specifications include industry-year dummies, country dummies, and firm and country level controls. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p < 0.01, **<p0.05, *p < 0.1.

through the different channels. We run a Chow test in order to test the equality (or difference) of our coefficients of interest over the two specifications, finding a significant difference between the coefficients in each column couple.[43]

We find that firms with their main financing for working capital coming from *External* sources experience relatively higher growth rates of sales after participating to an IMF program (columns 1–2 of Table 2). The positive effect on sales growth for firms in a program which rely relatively more on *External* financing is about 28 percent, compared with about 25 percent for those in the complementary sample. We then find that firms which have obtained bigger (mainly bank) *Loan* approvals see a relatively higher average growth rate of sales post IMF program (columns 3–4).[44] As previously discussed, the faster growth for those firms with bigger loans can be interpreted along the intensive margin, the effect on firms is increasing in the size of the credit opening from a bank. In particular, average firm sales growth is about 31 percent higher for firms with a loan higher then the median value.[45]

In columns 5–6, we find that firms reporting *Financial obstacles* experience faster growth relative to their counterparts. This evidence suggests that a program may be alleviating financial constraints, independently of the type of financing the firm relies on. Smaller firms also grow more rapidly as opposed to larger ones (columns 7–8). In particular, sales increase by about 31 percent both for firms experiencing financial obstacles as well as smaller firms, while they increase by about 20 percent under the alternative circumstances. Finally, in columns 9–10, we find that *Exporting* firms benefit less from the IMF intervention, as the average effect is below the effect on non-exporting firms.

In summary, we find that firms relying on *External* finance are more exposed to the positive effects of an IMF intervention, as this is the channel through which an IMF program is likely to be transmitted. On the other hand, we see that firms which are more likely to experience some *Financial obstacles* are also positively affected. All this evidence then is in support of our main hypothesis that the main channel of transmission of an IMF program is through the alleviation of firm financial distress. In the next section we will focus on the specific role of the IMF conditionality.

5.3. Conditionality

As a measure of the extent of IMF intervention, in this section we consider the number and scope of conditions. More specifically, as previously described in Section 3, we consider both the total number of binding conditions as a broad proxy for severity of an IMF program as well as financial conditions, because of its direct relation with our channels of interest. We therefore estimate the impact of an additional condition on firm sales growth.

[43] The Chow statistic is computed by running the same model on two sub-samples, splitting the data by whether the firm has the characteristic in question or not, and on the full sample. The statistic is distributed $F(k, N_1 + N_2 - 2 * k)$, with k degrees of freedom, and N_1 and N_2 observations on the subsamples.

[44] Of granted loans or credit lines to firms in our sample, 71 percent come from private commercial banks.

[45] While we find no significant effect in the case of firms with lower than median value Loans.

In order to comment on the causal effects of the number of IMF conditions, one has to take into account the endogeneity of conditionality. Our baseline identification strategy, however, works for selection into an IMF program but not for selection into the number and type of conditions. For this reason, we run our regressions on the sample of countries already under an IMF program, in order to determine the degree of intrusiveness of the IMF on the recipient countries. We then apply the same type of shift-share style instruments described in Section 4.1. Specifically, following Forster et al. (2019), we take the interaction between the number of countries under an IMF program in a given year (a proxy for how tight would be the Funds budget constraint) and the average number of conditions implemented by the Fund in the country. Once a country is in a program, the number of applicable conditions is increasing with the number of countries under a Fund program, but differentially based on the bargaining power of countries (see Section 4.1). The impact of IMF conditionality on firm performance is given by the following model:

$$g_{i,k,j,(t,t-2)} = \alpha_2 + \beta_2 X_{i,k,j,t} + \gamma_2 F_{j,t} + \lambda N_{j,t} + \tau_{k,t} + \mu_i + \epsilon_{i,k,j,t} \tag{6}$$

where g is our outcome variable for firm i, in industry k, and country j. As above, X and F denote our standard set of controls, while N stands for the number of IMF conditions, which can be either total or financial. The strong collinearity between number of conditions across policy areas makes it impossible to control for conditions in other policy areas. When considering total conditions, this is not an issue. Financial conditions, as shown earlier, make up the vast majority across all programs and so the issue of confounding effects is mitigated. We take both contemporaneous and lagged values of N to test for the persistence of conditionality. As before, standard errors are clustered at the country level.[46]

Table 3 shows our estimates for an increase in conditionality on firm sales. The panel on the left shows the results of the short term analysis, while the panel on the right presents the results up to the medium term. For either financial or total, we show the coefficients of a fixed-effects model when not correcting for the endogeneity bias as well as the instrumented coefficient.[47] The coefficients of our variables of interest, in the IV specification, generally show the adverse effects of increasingly severe IMF programs. For example, an additional condition in the Financial policy area leads to an 8 percent drop in average firm sales growth. In the medium-term, the effect becomes positive when considering all conditions but remains negative yet insignificant for financial conditionality.[48]

Finally, as in the previous section, we focus on the financing channel. To that end, in Table 4, we consider only financial conditions, and we look at the differential effects of the firm characteristics outlined in Section 5.2. The main difference with respect to the results obtained in Table 2 is that we now consider only countries under a Fund program. [49]

Due to the reduced number of observations in the treated sample, we are unable to identify the effects of financial conditions in all sub-samples. The Kleibergen Paap tests provide evidence in support of identification only when considering *External finance* (columns 1–2) and *Financial obstacles*(columns 5–6). We find that financial conditions have a positive effect on the performance of those firms that in a given industry rely more on *External* finance. We also find that firms with *Financial obstacles* see their performance decline (slightly) more with financial conditionality than firms declaring no obstacles.[50] Hence, a more intrusive financial conditionality seems more burdensome for firms that are financially constrained.

In conclusion, when considering a broad measure of program severity (total conditions) the temporal dimension seems to be important in order to determine effectiveness: increasing conditionality, which negatively affects firm performance in the short run, turns out to enhance firm sales in the medium-term. Focusing on the channels of financial conditionality, we find that financial conditions, while having on average a negative effect on firm sales, turn out to positively affect firms relying on external finance.

6. Redistribution within the firm

Given the evidence of increased sales for firms following a program, a natural question at this stage would be to wonder how the increased sales are redistributed within the firm.[51] More specifically, we look at the share of sales going to the workers (or labor income share), as this information is available in the dataset. More generally, an increase in sales could be redistributed either to the workers, to the owners of the firms, or re-invested. Unfortunately, we do not have data either on firm investments or on profits and we only have information on the compensation of employee, which explains why we test whether IMF participation, through an increase in firms' sales, may have an impact on the labor income share.[52]

[46] It is important to note here that taking into account compliance with conditionality would likely affect the results (e.g., see Dreher 2006, Reinsberg et al., 2022a,b). Nevertheless, incorporating the degree of implementation of conditions into the empirical analysis remains outside the scope of this paper.

[47] In general, the first stage results are as expected, since the coefficients of the IV are all positive. The Kleibergen Paap statistics also provide evidence in support of this relevance.

[48] The latter is not identified in the lagged specification.

[49] Since the coefficient of the lagged *Financial* conditionality is not found to be statistically significant in the medium-term, we focus here only on the contemporaneous effects.

[50] Within the sub-sample of firms that are relatively more reliant on *External finance,* we find that an additional condition corresponds to an 8 percent increase in average firm sales, while those firms that report relatively more financial obstacles face a drop in average firm sales of 5 percent for each additional condition (instead of 4 percent).

[51] For example, Vreeland (2002) focuses on redistributive effects in favor of the workers, while Lang (2021) considers the impact of IMF programs on inequality.

[52] Bomprezzi et al. (2022), in a different setting, actually find that IMF participation increases firms' investments.

Table 3
Number of conditions and firm sales growth.

	Contemporaneous Financial FE (1)	2SLS FE (2)	Total FE (3)	2SLS FE (4)	Lagged Financial FE (5)	2SLS FE (6)	Total FE (7)	2SLS FE (8)
Number of conditions	-0.025	-0.087***	-0.011	-0.060***	0.015***	-0.002	0.006***	0.011*
	(-1.27)	(-9.06)	(-0.88)	(-9.06)	(3.40)	(-0.06)	(3.58)	(1.78)
First stage: Instrument		0.014***		0.008***		0.001		-0.007
		(43.08)		(49.62)		(0.85)		(-1.56)
Observations	1590	1590	1590	1590	2798	2798	2798	2798
R2	0.372	0.242	0.369	0.242	0.345	0.201	0.345	0.215
Panels	795	795	795	795	1382	1382	1382	1382
Kleibergen Paap (p-value)		0.037		0.043		0.503		0.094
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF conditionality on average firm sales growth. Regressions are run on subsample of countries under an IMF program using an OLS estimator. Columns are sorted according to the policy area reform. All specifications include industry-year dummies, firm fixed effects, and firm and country level controls. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p < 0.01, **<p0.05, *p < 0.1.

Table 4
IMF conditionality and firm sales growth, financial channels.

	Without External Finance (1)	With External Finance (2)	Loan size Small (3)	Loan size big (4)	Without Fin. Obst. (5)	With Fin. Obst. (6)	Size Small (7)	Size Big (8)	Non-Exporters (9)	Exporters (10)
Financial conditions	-0.066***	0.081***	-0.035	-0.017	-0.044***	-0.053***	-0.073**	-0.064***	-0.074**	-0.149
	(-2.90)	(25.47)	(-1.31)	(-1.04)	(-3.44)	(-2.78)	(-2.08)	(-2.70)	(-2.52)	(-1.60)
Difference (p-value)	0.00	0.00	0.00	0.00	0.00					
Observations	10551	9037	2796	3003	10554	8903	13490	6363	15348	4504
R2	0.131	0.157	0.228	0.174	0.140	0.137	0.0891	0.110	0.109	0.0994
Kleibergen Paap (p-value)	0.092	0.055	0.273	0.109	0.085	0.083	0.139	0.146	0.105	0.188
Firm FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF financial conditionality on average firm sales growth. Regressions are run using an IV estimator with country fixed effects. Columns are sorted according to the channel (i.e., firm characteristic). Specifically, different subsamples are obtained by splitting the data by whether the firm has the characteristic in question or not. Odd columns show the results for the subsample of firms without the characteristics under consideration, while even columns consider the sub-sample of firms with that characteristic. In particular, we distinguish between firms relying on External finance below or above the median value (columns 1–2), firms with a Loan smaller or bigger than the median loan (columns 3–4), firms reporting lack or the presence of Financial obstacles (columns 5–6),.firms that are smaller or bigger than the median size (columns 7–8), Exporting and non-exporting firms. To test the equality (or difference) of our coefficients of interest over the two specifications, we run a simple Chow test. The Chow statistic is computed by running the same model on two subsamples, splitting the data by whether the firm has the characteristic in question or not, and on the full sample. The statistic is distributed F(k, N1 + N2-2*k), with k degrees of freedom, and N1 and N2 observations on the subsamples. All specifications include industry-year dummies, country dummies, and firm and country level controls. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p < 0.01, **<p0.05, *p < 0.1.

We then re-estimate Eq. 5 considering as our dependent variable the labor income share described in Section 3.2. As in the baseline specification, in columns 1–2 of Table 5 we estimate both a pooled OLS and IV considering the full sample of firms, while in columns 3–4 we use a fixed effects estimator. The first stage results show that the coefficient for our instrument are always negative and significant as expected.[53] The main result of Table 5 is that we find no effect on labor income share neither in the contemporaneous (columns 1–4) nor lagged specifications (columns 5–8). More specifically, IMF participation significantly reduces the labor income share only in the OLS specifications but not in the IV ones, and only in the contemporaneous setting.

This result could also depend on the circumstance that most salaries have little variable component and are fixed in the short term. Hence, if sales increase (for example after the Fund intervention) profits should also increase, while labor expenses remain unchanged at first. That might change when contracts are renegotiated, but this would then realistically

[53] Kleibergen-Paap tests provide further evidence in support of identification.

Table 5
IMF participation and labor income share.

	Contemporaneous				Lagged			
	Pooled OLS (1)	2SLS (2)	FE OLS (3)	2SLS FE (4)	Pooled OLS (5)	2SLS (6)	FE OLS (7)	2SLS FE (8)
IMF participation	-0.039**	0.032	-0.034**	-0.033	-0.030	0.117	-0.012	-0.077
	(-2.59)	(0.37)	(-2.10)	(-0.82)	(-1.42)	(0.53)	(-0.50)	(-1.12)
IMF probability		-0.003		-0.136		0.245		0.210
		(-0.01)		(-0.52)		(0.93)		(0.94)
First stage:								
Instrument		-0.916**		-1.279***		-0.737**		-1.240***
		(-2.54)		(-4.20)		(-2.34)		(-3.37)
Observations	47396	47396	5968	5968	47396	47396	5968	5968
R2	0.180	0.119	0.085	0.053	0.180	0.113	0.083	0.044
Kleibergen Paap (p-value)		0.053		0.012		0.035		0.006
Panels			2906	2906			2906	2906
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	NO	NO	YES	YES	NO	NO	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	NO	NO	YES	YES	NO	NO

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Column 3 uses the within estimator with firm fixed effects. Column 4 uses an IV estimator with firm fixed effect. The coefficient Instrument is the IV IMF liquidity x IMF probability for the first stage in our IV models. Columns 5–8 use the same estimators as in columns 1–4, but the variable of interest IMF participation is lagged by one period. All models include industry-year dummies and firm and country level controls. Kleibergen-Paap p-values are for the underidentification LM test. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p < 0.01, **<p0.05, *p < 0.1.

take a while. However, since we find no evidence in support of this, we conclude that the increased sales may be used either to increase the stock of capital, or to increase the profits of the firms' owners.

Given the difficulty in measuring firm investment decisions in our survey data, we also consider the firm employment decisions, testing whether IMF participation may affect the growth of permanent employers at the firm level. The intuition is the following: if owners divert revenues away from redistributive goals in the short term for investment decisions, employment will most likely increase subsequently to match human capital to physical. [54] Hence, by constructing an indicator of firm employment growth in the same way that firm sales growth is constructed, we should be able to measure the effect of IMF participation on firm employment growth (jobs). As shown in Table C1, in the online Appendix C, we can find that IMF programs do induce an increase in employment but only in the lagged specification and only using a fixed effects estimator.

To sum up, the increase in firm sales we observe after a country participates in an IMF program seem to have no effect on the workers' compensation, but is found, at least to some extent, to increase the number of permanent workers in the medium-term.

7. Robustness

This section contains an in-depth discussion of different robustness tests for our main results. Tabular results and figures related to this section are presented in the online Appendices B and C. We begin with issues regarding the identification strategy, in particular to address the exclusion restrictions. Then we discuss issues related to our survey data, specifically related to sample dependence.

7.1. Identification

The biggest threat to identification regards the presence of underlying, time-varying heterogeneous trends which are correlated to IMF liquidity and affect firm sales differentially conditional on the share of years under an IMF program. This critique of shift-share style instruments was pointed out by Christian and Barrett (2017), who showed that the original findings by Nunn and Qian (2014) could be explained by spurious correlation between the time varying component of their IV and particular time trends in their outcome variable. This issue does not arise with respect to IMF liquidity for a number of reasons. First, as previously mentioned, IMF liquidity is determined primarily by an institutional rule where every five years countries review their quotas with the IMF, making it orthogonal with respect to potential trends in firm sales.[55]

Because the identification strategy follows a diff-in-diff logic, a problem would arise if this parallel trends assumption failed, i.e., the correlation between IMF liquidity and the two groups did not remain constant over time. Following

[54] This is the reason why our measure of employment incorporates only full-time, permanent worker.

[55] Following Borusyak et al. (2022) and Goldsmith-Pinkham et al. (2020), our research design reflects differential exogenous exposure to a common shock, which should be enough for the shift-share instrument to be valid.

Christian and Barrett (2017), we plot the log of IMF liquidity over time alongside the trend of GDP per capita growth over two sets of countries, those with a low share of years under an IMF program versus those with high share of years (IMF probability).[56] Similarly, we plot the log of IMF liquidity over time alongside the trend of employee's compensation, measured as the labor share of national income from the World Inequality Database, over two sets of countries, those with a high and low IMF probability. Figure B2, in the online Appendix B, shows these plots. The results give little reason to believe that the parallel trends assumption is violated in our case (both for a proxy of firms sales and labor income share). More precisely, the probability-specific trends in IMF liquidity and growth seem rather parallel across countries that regularly participate to an IMF program with respect to those which do not.

A similar issue is one of correlated global trends in the first stage. Specifically, there could exist global variables correlated with IMF liquidity driving the first stage. We explore some of these potential confounders as we consider the presence of global bank and currency crises. [57] Because these crises are direct determinants of global demand for IMF programs, if they are correlated to Fund liquidity it could also in turn determine the first stage effects. To start, Figure B3 plots the yearly variation in the two main potentially problematic trends: number of banking crisis and currency crisis, alongside the logarithm of the IMF liquidity ratio. This descriptive evidence shows weak correlation between the time-varying component of our IV and these global trends, with the exception of the global financial crises of 2008 where the IMF played a relevant role. As a more formal test, we control directly for the differential effects of such trends, as the exclusion restrictions require that the effects of the confounders are not contingent on IMF probability. Table B1 in the online Appendix B shows the 2SLS coefficients and the first stages: our baseline results are robust to controlling for these alternative trends interacted with country-specific IMF probability. However, we cannot definitively rule out the presence of omitted variable bias stemming from other time-varying trends.

A separate issue for the IV strategy lies with the second component of the interaction term, which is the time-varying share of years under an IMF program. Different iterations of this IV strategy rely on a time-invariant share component (see for example Nunn and Qian, 2014). In this case, IMF probability would be constructed as the total number of years country *i* is under an IMF program, over the total number of years in the sample *T*. This ratio would now be a constant number in every period and not time variant as described in Eq. (2).[58] Table B2 shows a replication of our baseline results where we look at firm sales growth but using this modified instrument. Results in the short run are consistent, but weaker in the medium-term. This method, however, is less intuitive as it also captures future relationships between the Fund and a given country as a predictor of present and past relations. The time-varying version therefore remains our preferred IV.

7.2. Sample dependence

An equally important issue to address is the sensitivity of results to the composition of the sample. While our country sample is vast and therefore unlikely that a given country is driving the results, issues of sample dependence could arise from the firm sample within countries. The stratified random sampling methodology for the WBES explained in Section 3.2, at least theoretically, guarantees that the patterns for firm sales growth are not being driven by a particular set of firms more exposed to IMF lending. [59]

Table C2 in online Appendix C tackles these issues of sample dependence in the firm dimension more rigorously. For example, in columns 3 and 4, we show that results are robust when limiting the sample to only domestic firms. Hypothetically, results could be driven by foreign firms operating in a given country, which are typically larger and more sophisticated than solely domestic ones. However, we find no evidence for such results. Similarly, we show in columns 5–8 that firm characteristics (*Size* and *Export*) closely tied to firm performance are not endogenous to program participation. Finally, we also show that the results are robust to a model with no firm controls. In a similar vein, in columns 9–12 we also show that IMF participation has no effect on each of the different firm characteristics that we use as "channels" of transmission of IMF intervention.[60] Table C3 instead shows the effects of IMF participation on firm sales growth when the sample is split along four broad industry groups. The results show that effects are strongest in the *Manufacturing* sector, which is more represented in the sample, and in the *Retail* sector. We find no effects in *Services*, where the identification in the first stage is not significant at conventional levels, or in the *Food* sector.[61]

Another limitation to survey data is the problem of recontacting firms. Beside promising best practices and efforts to create a panel structure in their survey, the WBES can provide no guarantee that firms which can be recontacted will be. And there is no way to know why some firms don't appear in future waves of the survey. The biggest limitation which would affect our results on firm sales growth is firms dropping out because they go bust, what we call the survivor bias. If this were the case however, we would expect that the distribution of firms with repeated interviews versus the distribution of single-

[56] We use country-level proxies to look for parallel trends between country groups because of the survey nature of the data. Aggregating firm sales or labor income share to the country level results in distorted representation of the trends.

[57] Data on global banking and currency crises are from Laeven and Valencia (2013).

[58] Hence, Eq. (2) would become the following: $IV_{j,t}^{IMF} = IMF\ liquidity\ ratio_t \times IMF\ probability_j$.

[59] Besides, firm-level controls should also control for these potential channels.

[60] In unreported regressions we document that IMF participation has a positive and significant effects on lending from banks and other financial institutions. These results are available upon request. We also tried to exploit alternative firms' indicators as alternative channels of transmission of IMF programs but they were found to be endogenous to the IMF intervention.

[61] Splitting the sample, however, reduces the number of observations too drastically to be able to use the panel specification.

presence (no repeated interviews) firms would be significantly different. Figure C1 in the Appendix shows that the two distributions are rather similar.

To test formally the robustness of our results to firm sample dependence, as a final step we run a randomization of the sample of firms per country. We consider different strategies of randomization, where each one has unique implications on the final sample. We begin by considering the simplest case of random sampling without replacement of 200 observations per country.[62] As a second test, we randomly draw without replacement a share (50 percent) of observations per country. A final more sophisticated method is to weight each country in the sample by its economic size, and randomly sample without replacement a number of observations proportional to this weight. For each method we run 100 simulations and compute the average of the estimated (second stage) coefficient alongside the standard error and the percent of simulations where the coefficient is insignificant.[63]

We then apply the same methods to a panel sample of firms. In this case, we consider 70 randomly drawn (without replacement) unique firms per country and their corresponding recontacts, if such recontact occurred in later waves of the survey.[64] We run 100 simulations and find the average coefficient using a panel model with firm fixed effects. All results are reported in Table C4, in the online Appendix C; they are consistent across all the methods and we find an average effect very similar to our baseline results.

8. Conclusions

This paper studies the effects of IMF programs on firm performance, by using a sample of 130,000 firms in 139 developing countries, over the period 2003–2018. We consider two dimensions of a Fund program, namely participation and number of conditions, and we look at their effects on growth of firm sales. Our identification strategy exploits the differential effect of changes in IMF liquidity on program participation (Lang 2021, Gehring and Lang 2020).

We argue that IMF intervention could signal to international markets renewed confidence in the country, which in turn translates into easier access to finance at the firm level. Our results show a positive impact of participating in an IMF program on firms' sales growth, and the effect is persistent through time. Controlling for firm fixed effects, sales can be up to 24 percent higher for firms in countries benefiting from IMF lending. More specifically, we find that IMF intervention is associated to a greater increase in sales for firms relying relatively more on external finance or reporting more financial obstacles. These results suggest that the Fund improves firm performance by relaxing the financial constraints faced by domestic firms in recipient countries. Furthermore, we find that the time dimension is an influences program effectiveness. While more severe conditionality worsens firm performance in the short run, the effect turns beneficial in the medium-term.

Our findings shed light on the channels through which IMF programs affect domestic firms. A related question is then whether IMF programs, as well as improving a country's creditworthiness for external investors, may also make "domestic" investors more willing to invest in the country, by reducing the degree of uncertainty over the recipient country's future economic prospects. We leave this question for future research.

Data availability

Data will be made available on request.

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jimonfin.2023.102819.

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[62] In the case a country has less than 200 observations, all of them are taken. This occurs in the case of some small countries such as Antigua and Barbuda (151), Republic of Congo (151), Suriname (152), and Papua New Guinea (65) among others.

[63] Clearly here the second stage coefficient is computed on a model with country but not firm fixed effects, because by randomly drawing observations instead of firms we would disrupt the panel structure. See Chong and Gradstein (2009) for detail on this methodology.

[64] Doing this, we can be sure not to involuntarily disrupt the panel structure of the data by creating singletons.

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