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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, using a panel of about 130,000 firms, over the period 2003-2018. We consider the different dimensions of a Fund program, namely participation, loan size and number and scope of conditions, and we look at their effects on growth of firm sales, as well as on income redistribution within the firm. Our identification strategy exploits the differential effect of changes in IMF liquidity on program participation (Lang 2016). We find a positive impact of IMF programs on firms' sales growth, and the effect is persistent through time. What is more, we find that performance is improved through the alleviation of the firm financing constraint. More severe conditionality seems to worsen firm performance in the short run, but then turns beneficial over the years. Finally, we find that participating to an IMF program reduces the labor income share in the short term, but employment increases in the long run, suggesting that the increased income is reinvested into the firm.

Keywords: IMF conditionality, IMF, Firm growth, Labor Income Share

JEL Classification: F33, O19, E24

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1 Introduction

This paper takes a new approach to an old debate on the effects of IMF imposed conditionality schemes. 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 performance considering growth in the firm sales and how income is then redistributed within the firm. Our approach allows us to perform a more accurate assessment of the effect of loans at the firm level by exploiting both program and firm heterogeneity to investigate effectiveness. Most importantly, we are able to shed some light on whether IMF programs may crowd in or crowd out, at least to some extent, local economic activity.

More specifically, this study combines firm level data and an IMF conditionality dataset data to evaluate the effect of the IMF intervention on firm growth. Data on firm sales are extracted from the World Bank Enterprise Survey (WBES) which provides data on almost 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 arrangement dates, program type, commitment amount, condition type, and relevant policy areas, resulting in a dataset with over 32,000 unique conditions for any of the 189 countries potentially under IMF schemes, over the 1980-2014 period.

This methodology is part of a growing field of studies utilizing a macro-micro approach to revisit orthodox results from policy impact analyses (see for example the emerging strand of literature evaluating aid-effectiveness at the subnational level).[1] Looking at firm level outcomes not only allows us to make conclusions on the country level effects, but also allows us to exploit firm heterogeneity and identify potential channels of interest. Furthermore, the availability of detailed data on IMF conditionality schemes allows us to disaggregate IMF lending and potentially observe the channels through which IMF programs impact firms. In particular, we look at how conditions targeting policy areas have differential effects on firm sales and the labor income share. This paper then contributes to the literature on the IMF effectiveness contingent on the types of program and, to the best of our knowledge, this is the first study that evaluates the effect of the IMF programs on firms' sales growth and labor share at the firm level.

The scope of the paper, using the outlined methodology, is to highlight the 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 finan-

[1] The availability of geo-coded aid data has produced an emerging strand of literature evaluating aid-effectiveness at the subnational level (Bluhm et al. 2018; Chauvet and Ehrhart 2018; Del Prete et al. 2019; Gehring et al. 2019; Dreher and Lohman 2015; Dreher et al. 2021; Marchesi et al. 2021).

cial 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).[2] More generally, the literature on firm performance points up three main kinds of constraints on firm growth in developing countries: the financing constraint (Beck et al. 2005; Choudhary and Limodio 2021; Harrison et al. 2004), lack of infrastructure, such as transport, energy, telecommunications, and water (see among others Bluhm et al. 2020; Jedwab and Moradi 2016); 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, on the other hand, it is hard to reconcile the IMF intervention with increased government spending, given the IMF preference for austerity-oriented measures.

Considering supply factors, we investigate one main channel through which IMF loans may influence firm performance, which consists in releasing the firm financing constraints. We expect that IMF lending could affect firm borrowing capacity, as lending institutions in the home country are typically most exposed to the debt of their sovereign, and hence their balance sheets suffer from the deterioration of such assets, negatively impacting lending.[3] Therefore, consistently with the literature on catalytic finance (among others see Corsetti et al. 2006; Krahnke 2020; Marchesi and Thomas 1999, Marchesi 2003; Mody and Saravia 2006; Morris and Shin 2006), we start by considering the *signaling effect* of IMF programs, and then focus on the *extent of the IMF intervention* (that is *loan size* and *conditionality*). Hence, the main idea is that IMF lending could signal to the international markets renewed confidence in the country, which translates in easier access to finance at the firm level, in turn enhancing firm performance.[4]

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 Lang 2016). 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

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

[3] Related to this, one might expect that an export oriented firm is more likely to benefit from the IMF intervention through an improved access to trade credit. In fact, the deterioration in the credit quality of exporting firms after a financial crisis or a default could make trade credit less available and more expensive.

[4] Foreign-currency borrowing is important for many firms in developing countries, and therefore they may benefit from renewed sovereign credibility.

a difference-in-differences logic.

Using data on almost 130,000 firms, spread across 139 countries, over the period 2003-2018, we find a positive impact of participating in an IMF program on firms' sales growth, and, what is more, the effect is persistent over time. Specifically, controlling for firm fixed effects, sales could be, on averge, almost 30 percent higher for firms in countries benefitting from IMF lending.[5] We confirm our hypothesis regarding the firms financing constraints, namely that the main channel of transmission of an IMF program is though the alleviation of the firm financing constraint, while other dimensions do not seem to be important. As the loan conditionality is concerned, the time dimension seems to be an important factor to determine its effectiveness: the same set of conditions which negatively affect firm performance in the short run turn out to enhance firms sales in the longer term. Finally, when considering how the increased sales are redistributed within the firm, we find that participating to an IMF program reduces the labor income share in the short runs, but employment increases over the years, suggesting that the increased income is eventually reinvested into the firm.

Our contribution is then twofold. First, we contribute to the recent advances in the use of firm level data by considering IMF intervention. Second, to the best of our knowledge, this is the first study that evaluates the effect of the IMF intervention on the labor income share at the firm level.

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 Existing Literature

This paper is related to several strands of literature. The first one broadly looks at IMF effectiveness, by considering the 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). Beyond growth, monetary stability, debt management and the containment of external arrears are key goals of IMF programs (Kentikelenis, Stubbs, and King 2016). IMF programs are associated to reduced inflation and monetary growth, less risk of cur-

[5] When considering the loan amount, we find effects which are comparable to our baseline result.

rency crises and banking crises, and improved market performance of banks (Dreher and Walter 2010; Papi et al. 2015; Steinwand and Stone 2008).

The success of any IMF programme hinges largely on its catalytic effect, namely the propensity of private investors to finance a country under 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).

What is more, several contributions have considered in more details the varied conditions attached to IMF financing, finding that conditions are a key mechanism linking IMF lending to policy outcomes.[6] 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, 2019) have focused on public sector conditions.[7] 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 programme implementation horizons has emerged and that in the aftermath of an IMF programme, all relevant macroeconomic variables tend to improve compared with the pre-programme period.[8] 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.

In addition to these economic effects, IMF programs also appear to affect political outcomes.[9] Several scholars link IMF programs to political instability and suggest that they increase the risk of civil war onset (Hartzell et al. 2010), coup d'états (Casper 2017), and government crises (Dreher and Gassebner 2012). One explanation for these politically destabilizing effects of IMF programs is

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

[7] More specifically, Reinsberg et al. (2019), analyzing 70 developing countries from 1980 to 2014, find that IMF labor market policy reforms significantly reduce both individual and collective labor rights. Rickard and Caraway (2019), find that public sector conditionality is a key mechanism linking IMF lending to policy outcomes. In particular, they 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).

[8] One area in which the effectiveness of IMF programmes 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. 2007b).

[9] In turn, there is a vast literature that considers IMF decision-making, focusing instead on the geopolitical determinants of IMF programs. These contributions link a country's geopolitical proximity to the IMF major shareholders (especially to the U.S.) with a variety of types of preferential treatment (e.g., Copelovitch 2010; Dreher et al. 2008; Dreher et al. 2009; Dreher et al. 2018a, 2018b; Stone 2008; Lang and Presbitero 2018) For a recent survey, see Dreher and Lang (2019).

that the burdens of economic adjustments under IMF programs are often distributed unequally.[10] In particular, Vreeland (2002) examines the labor share of income from manufacturing finding that IMF programs have a negative effects on income distribution. 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.

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

More specifically, as we mainly focus on the alleviation of the financing constraint as a channel of transmission of an IMF programs, this paper also relates to the theoretical literature that explain how a financial crisis (or a sovereign default) may propagate to firms. For example, Mendoza and Yue (2012) show how a sovereign default impacts the economy through the costs that firms face when they rely on external finance to buy production input abroad. Under a financial crisis, these kinds of firms may be forced to shift to domestic inputs, leading to efficiency losses and reducing output and, in turn, GDP growth. On the other hand, Corsetti et al. (2012) identifies the role of the fiscal channel as the main element through which may firms are affected by a sovereign default. Gourinchas et al. (2017) suggest that firms may experience a decline in production because a debt crisis lead to a contractionary fiscal policy, which in turn increases the likelihood of corporate defaults and results in higher corporate borrowing costs, lower investments, and output losses. In summary, what emerges from this literature is that firms should see their performance worsened under a financial crisis.[11] More recently, Choudhary and Limodio (2021), based from evidence from Pakistan, show 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 long-term investments.

Finally, this paper also relates to the analysis of labor income share.[12] Between 1994 and 2014,

[10] More generally, Furceri et al. (2018) investigate the aggregate and distributional effects of policies to liberalize international capital flows, finding that the so called financial globalization has led, on average, to limited output gains while contributing to significant increases in inequality.

[11] As the empirical literature is concerned, Hébert and Schreger (2017), using data from Argentina, show that higher sovereign bond yields caused a decline in the stock price of Argentine firms. Ferrando et al. (2017), using micro-data at firm's level, find that SMEs in country experiencing a sovereign distress face price and quantity rationing by banks.

[12] At the firm level, the labor income share can be defined as the portion of the firm's sale that goes to the workers.

the labor income share dropped in 29 out of 50 countries, accounting for almost two thirds of the world's GDP (Dao et al. 2017). A decline in the labor income share indicates a slower growth rate of product wages than the growth in the average productivity of labor. To this extent, micro-level studies provide insightful knowledge on the drivers of the labor income share.[13].A study by Böckerman and Maliranta (2012) using longitudinal plant-level data on Finland show that micro-level restructuring could explain a significant part of the differences between the declining labor income share and increasing labor productivity. They also show that a growing level of international trade catalyzes this process. Aghion and Howitt (2006), in an earlier paper, argued that micro-level restructuring is an important factor in understanding the industrial productivity growth. A similar concern is echoed in the trade and international finance literature (Melitz 2003; Bernard and Jensen 2004, Furceri et al. 2018). It argues that in the presence of heightened competitiveness due to globalization, resources are reallocated from the less efficient to the more efficient firms.

3 Data Description

3.1 IMF Intervention

The primary measure of interest is IMF intervention, which we analyze in different ways. In its simplest form, 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 under the IMF Member Financial Data. The result is an indicator variable for *IMF Participation* for each country-year. 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. To instead get a measure for the extent of IMF involvement, we can directly use the disbursement amounts, measured as the ratio of SDR credits to GDP. In this case the estimated relationship with sales captures the varying degrees of intensity of IMF interventions. Figure 1 gives an idea of the magnitude of IMF loans, by showing average disbursements to GDP, for the different World Bank macro regions, between the first half of our sample period (2000-2009) and the second half (2010-2018).

INSERT FIGURE 1

[13]Studies at the firm or sectoral level could potentially explain the rising gap between the rate of growth in labor productivity and that of wages using financial aid, globalization, labor market regulations, and other institutional factors.

The third variable of interest regards the stringency of IMF programs as measured by the number of binding conditions per policy area. For this we rely on the dataset compiled by Kentikelenis, Stubbs, and King (2016). The authors exclusively use IMF executive board documents (Letters of Intent and Memorandum of Economic and Financial Policies), which are therefore of greater reliability and more comprehensive with respect to similar projects that publish similar data. The result is a dataset with disaggregated data on IMF conditionality, providing information on 32,261 unique conditions for 135 different countries over the period 1980-2014. These conditions are categorized in different groups, including types of arrangements (i.e., concessional versus. non-concessional), conditionality type, dimensions of conditionality such as quantitative or structural and "hard" versus "soft," as well as the relevant affected policy areas.

The data on programs from this dataset covers a broad universe of IMF lending type. Broadly, the general arrangement type 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).[14]

The primary focus for our analysis, however, are the differing policy areas affected by IMF program conditionality. Kentikelenis et al. (2016) group conditions for each program into one of 13 mutually exclusive affected policy areas; Fiscal reforms, Revenues and taxes 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. For the purpose of our analysis, we aggregate these policy areas into five distinct groups in order to reduce the granularity of the data and focus on the effects of the main policy-area related conditions. More specifically, we consider: *Financial, External, Fiscal, Institutional*, or *Labor* reforms.[15] Figure 2 below plots the evolution of the average number of conditions per policy area imposed by the IMF over years, as can be seen, the increase in total

[14] 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 or PLC or shock-specific arrangements like ESF or EAND. In the end however the main lending facilities comprise around 87% of the sample.

[15] 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 tested 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.

conditionality is driven primarily by conditions in the area of financial reform and financial reforms are the ones that command the most attention of IMF programs.[16] Table A2, in the online Appendix A, outlines our aggregation strategy, and shows the resulting heterogeneity among policy-area reforms, while Table A4 reports some summary statistics on conditionality conditions.

INSERT FIGURE 2

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 implies the possibility to construct a pseudo-panel for 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 A4, in the online Appendix A.

The survey is constructed to generate a representative sample of a countries manufacturing and service sectors, with the final aim of providing indicators for the investment climate in a country. This means the 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.

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 constructed 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

[16] A further breakdown of the number of conditions by policy area and conditionality type, by region, are presented in figures A1 and A2, in the online Appendix A.

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.[17] 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. Following this sample 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 only 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.[18]

The main data of interest from the survey are the values of firm sales, 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 panel 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 descrive 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 for each country and year. Figure 3 below 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

[17] 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% or more of a countries geographic area under conflict as well as at least 10% of the population affected.

[18] See Table A6, in the online Appendix A, for a detailed deconstruction of survey sample, with unique firms and total number of observations by country.

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.

INSERT FIGURE 3

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.[19] In this paper, we use the information on compensation at the firm level, which is less susceptible to problems related to the mixed income that arises from self-employment. The Enterprise Survey (ES) asks the same set of questions of enterprises that have employer–employee relationships, so we are not concerned about the comparison within our dataset. Following Zhou (2016), the labor income share (LIS) at the firm level is defined as:

$$LIS_{i,t} = \frac{Compensation\ of\ employee_{i,t}}{Total\ sales_{i,t}} \tag{1}$$

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 attempted to 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 then marked as outliers and turned into missing. We use these values to run separate regressions following our methodology 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, and the extent of an IMF program, in a given country and year, may affect the country's growth of firm sales. 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

[19] 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).

is likely to affect the performance of their firms As a consequence, simple comparisons between treated and non-treated country-year observations will not yield causal effects, but instead will capture the negative bias resulting from omitted variables and reverse causality. Strategies to deal with this type of endogeneity of loans at the macroeconomic level have evolved over time.

A new strand is currently emerging in the aid effectiveness literature based on quasi-experiments. Early 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. 2020) or on temporal variation in US wheat production to instrument US food aid (Nunn and Qian 2014).[20] More specifically, following Lang (2016), Gehring and Lang (2020) and Lang (2021), we use the interaction of the lender's budget, proxied by the Funds's liquidity ratio defined as the share of liquid resources over liquid liabilities, with the recipient-specific probability of receiving a loan from the IMF as an instrument for IMF intervention. The IV equation in then the following:

$$IV_{j,t}^{IMF} = IMF\ liquidity\ ratio_t \text{ x } IMF\ probability_{j,t} \tag{2}$$

where *IMF probability* is the 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.[21] Our main identification strategy is thus based on an instrumental variable (IV), which combines temporal variation in the *IMF liquidity* with cross-sectional variation in a country's prior probability of participating in an IMF program.

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 (as displayed in Figure B2, in the online Appendix B). Controlling for year fixed effects (which captures IMF liquidity) as well as for the individual time-varying, country-specific probability component of the interaction term, the identifying assumption underlying this approach thus 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 (i.e., IMF liquidity directly affects our firm-level indicators such as sales) if and

[20] This strategy is similar to Bartik shift-share instruments typically used in the labor and migration literature, respectively (e.g., Autor et al. 2013; Altonji and Card 1991).

[21] Since our data on *IMF liquidity* are available only up to 2014, our year sample also ends in 2014, for all the specifications in which we instrument IMF participation and loans, such as Table 1, Table 2, Table 5 and Table 6.

only if there was 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.[22] 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.

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.[23] The trends (which are shown in Figure B3, in the online Appendix B) are clearly parallel and not obviously correlated to *IMF liquidity*, but most importantly, the difference between *IMF liquidity* and the each group remains constant over time.

The same procedure outlined allows us to instrument for disbursements rather than a binary indicator for IMF program. As before, the amount of disbursements stemming from the Fund towards borrower countries is lower in times of low *IMF liquidity*, and countries with a stronger historical proximity to the Fund receive more disbursements (because they are more likely to have signed an agreement). Therefore the same diff-in-diff logic described for selection in a program holds here.

It is important to mention that the mechanisms underpinning this relationship are not as strong as in our baseline case. From a theoretical standpoint, as pointed out by Sturm et. al (2005), political variables are more closely connected to the conclusion of IMF agreements (selection into program) rather than the disbursements of IMF credits, which are instead determined more by economic factors. It is not therefore obvious that exogenous variations, such as those attributable to revisions of IMF member quotas would explain the size of loans. When we plot the first stage marginal effects for our instrument on disbursements to GDP, in Figure B4 of the online Appendix B, we find the same pattern as when instrumenting for participation.

[22] To a lesser extent, as some would argue, the *Fund liquidity* could vary because of large individual loan repayments or disbursements. Lang (2021) addresses this and finds that the cases that would fit this criteria are few and do not influence the identification.

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

4.1 Selection into program type

The same endogeneity concerns holding for the selection into an IMF program, apply when considering the type of program a country is assigned into. In our context, by type of program we intend 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 an intrusive conditionality. Furthermore, the total number of conditions depends on series of unobservable characteristics that introduce omitted variable bias.[24]

The identification strategy is similar to the one adopted by Forster et. al (2019), and also follows the same 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 other words, once in a program, the probability of having a high number of conditions for countries that received already high conditions in the past is higher when *Fund liquidity* is low as the Fund needs to be more selective into disbursements of consequent tranches.

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, the Funds resources are stretched and therefore programs on average entail more conditions to balance demand with the avaiable 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. Together, these two predict the variation in number of conditions per policy area.[25] Formally, we can write the instrument as:

$$IV^{IMF}_{j,p,t} = Countries\ under\ IMF\ program_t \text{ x } Average\ number\ of\ conditions_{j,p,t} \qquad (3)$$

where p stands for each policy area in a given country j. We therefore run separate regressions for each of the five policy areas considered, and we plot separately the first-stage effects on number of conditions by policy area (see Figure B5 reported in the online Appendix B). Because the instrument follows the same diff-in-diff logic as the original instrument, the same caveats apply. The exclusion restrictions apply so long as variables correlated to the number of countries under an

[24] For example, imposed conditionality may depends on the preferences and bargaining power of both the borrowing country as well as the IMF. To the extent that the country can select the number of conditions, these same preferences also determine policies in the country which explain firm-level sales growth.

[25] 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.

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

We investigate the impact of IMF intervention on firm performance using the following general specification:

$$g_{i,k,j,(t,t-2)} = \alpha + \beta X_{i,k,j,t} + \gamma Z_{j,t} + \tau_{k,t} + \mu_{j/i} + \varepsilon_{i,k,j,t} \tag{4}$$

where g is our outcome variable for firm i, in industry k, and country j. X is a set of time varying firm-level characteristics, while Z is a set of time varying country-level variables including our variables of interest such as *IMF Participation* and *IMF disbursements to GDP*.[26] 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 (with standard errors clustered at the country level). Finally, to avoid extremely fast-growing firms driving the results, we excluded the top one percent of the growth distribution from the sample.

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.[27] Following Chauvet and Ehrhart (2018), at the firm level, we control for the lagged value of *Sales*, in logarithm, which is measured at one lag with respect to the dependent. We also control for the following characteristics. Firm *Size*, which 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 control for the characteristics of firm ownership using two variables, *State* and *Foreign*. *State* is a dummy variable which is equal to one when part of (or all) the firm is 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

[26] They are averages taken over three years, that is between t and t-2. We also lag both variables (that is between t-2 and t-4) to investigate the persistent effect of IMF programs.

[27] More precisely, since in the WBES all data on sales are reported on 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.

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). The firm-level characteristics are measured in year t since we do not have their pre-determined 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*. The source of all these data are the World Development Indicators. 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.

We are working on a panel of rather large formal firms: around 22 percent are outward looking (exporting either directly or indirectly) and the average size is about 1.7 out of 3. Furthermore, the vast majority of firms rely on formal sources of financing (bank, self-financing, or state-tied financing) and about half of the sample is of firms that have an overdraft account, which is a measure of financial sophistication. Table A5, in the online Appendix A, presents a description of all the variables used in the analysis, while Table A3 shows some basic summary statistics.

5.1 IMF participation

In this Section, we provide 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 when fixed effects are used the dummies are dropped because of collinearity.

INSERT TABLE 1

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.[28] Interestingly, the coefficient of *State* is positive but not always significant. Moving to 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.[29] The first-stage results show the coefficient for our instrument, which is always negative and significant, and with the expected sign. Kleibergen Paap tests provide further evidence in support of identification.

In our simplest model, shown in column 1 of Table 1 (where we estimate a pooled OLS without instrumenting), we find that average firm sales growth is about 9 percent higher for firms in a program, although the coefficient is significant only at the ten percent level. When instrumenting for IMF participation, in column 2, we find instead that sales increase by 30 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 similar results, sales increase by 16 percent in the OLS specification (column 3) and close to 30 percent instrumenting for participation in a program (column 4). Both coefficients are now significant at the one percent level. Similarly, we find evidence for long-term effects in column 8, where we use an instrumented lagged value of IMF Program, and find that average firm sales growth more than doubles, the coefficient being significant at the five percent level.[30]

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 programme could work as a sort of signal of 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 the sovereign borrowers some fiscal space. We should emphasize that this effect can realize both at the firm level (e.g., as export-oriented firms that rely on trade credit see a pic in demand) as well as at the country level

[28] 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, Antràs and Helpman 2004; Helpman et al. (2004).

[29] The only exception is the IMF coefficient of column (6), which is positive but not significant at conventional levels.

[30] When instrumenting for IMF participation in the pooled sample, the coefficient remain positive but not significant at conventional levels.

as domestic (and foreign operating on domestic soil) credit institutions become boisterous about future economic prospects.

In the next Section we investigate whether, besides the signalling effect, the extent of the IMF intervention may also play a separate role in affecting firm performance. More specifically, in the next two sub-sections, we consider the loan size and program conditionality.

5.2 IMF disbursements

In this Section, using data from the IMF Member Financial Data, we match the corresponding disbursements to GDP to the Fund individual programs, which were considered in the previous analysis. Disbursement size is first and foremost a proxy for the scope of IMF reform objectives. Positive disbursements correspond to a successful implementation and review of IMF mandated reforms, indicating that on the whole IMF policy reforms are effective in stimulating private sector growth. In turn larger disbursements imply the successful implementation of more intrusive reforms. Table 2 present the results, where the structure of this table is similar to that of Table 1. For the reasons outlined before, this identification mechanism is weaker when trying to explain extent of IMF intervention, and for this reason differently from Table 1 we can only identify effects when considering short-run (contemporaneous).

Columns 1 to 4 of Table 2 shows our results for a simple pooled OLS, a 2SLS, a fixed-effects model, and a 2SLS with fixed-effects, respectively. Turning directly to tour variables of interest, we can see that the coefficients denoting the amount of *Fund disbursements to GDP* are all positive but not always significant.[31] The coefficient of the instrumented loan, in column 2, is positive and significant at the five percent level. Similar results are found when turning to the IV specification, which included firm fixed effects, in column 4.[32] In quantitative terms, we find that one standard deviation increase in disbursements to GDP boosts average firm sales growth by 14 percent.

INSERT TABLE 2

Our results then confirm the presence of a liquidity effect, where bigger loans correspond to a larger (positive) effect on sales. Most importantly, considering the loan size allows us to investigate on the channels of transmission of Fund disbursements Specifically, we can leverage the extensive

[31] In particular, the coefficient of the OLS specification, in column 1, is positive but not significant at conventional levels.

[32] The first-stage results show that the coefficient for our instrument are always positive and significant, and with the expected sign. Kleibergen Paap tests provide further evidence in support of identification.

heterogeneity of our data to explore some of the channels through which the liquidity effect may affect firm sales. On a theoretical ground, we identify several channels of interest through which IMF disbursements could influence firm sales growth. 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). In this Section we try to unravel this black box with the help of some firm level characteristics and indicators of firm operating environment.

As above-mentioned, from the demand side, the effects are theoretically ambiguous. On the one hand, IMF disbursements are expected to alleviate the government borrowing constraints, and hence increase the size and share of government contracts. 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 disbursements with a boost in government spending.

From a supply side perspective, loans can affect the productive capacity of the firms. We focus on this supply side dynamic, and test empirically if the presence of an IMF loan can have an impact on firm sales through some specific channel which is measured by a specific firm characteristic.[33].In particular, we consider two broad channels through which IMF loans may influence firm growth: (1) access to finance and financial reputation and (2) trade channel. The reason why we focus on these two channels has obviously to do with our working hypothesis, namely that the Fund is expected to release the financing constraint of the recipient country (both directly and through the catalytic-finance effect), which eventually would propagates its effects at the firm level. In turn, trade would also represent a channel of transmission, to the extent that improved access to credit (e.g., trade credit) may improve the performance of export oriented firms.

In line with the existing literature, we postulate that a firm achieves growth in sales with better access to finance as IMF disbursements flow in. To that hand we distinguish between firms with access to *Formal* channels of finance (such as through the banking system, the state and self-finance) and *Informal* ones. We also control for firms having access to *Overdraft* facility and those that have explicitly declared to have experienced *Financial obstacles*.

Similarly, one might expect that a firm with more *Trade obstacles* is less likely to be able to fully benefit from liquidity effects passed on from large IMF disbursements. We then directly interact

[33]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. In particular, they interact measures of financial development with industrial sectors that are relatively more in need of external finance. They find that such sectors develop disproportionately faster in countries with more developed financial markets.

the disbursements to GDP with the variable *Export*.[34] Finally we include *Size*, as it is a good proxy for many other characteristics (such as ownership status), which may affect the ability to benefit more from the IMF intervention (bigger firms, for example, are more able than small ones to access a credit injection in the financial markets). Table A7, in the online Appendix A, lists all the firm specific information we use both in this Section and in the next one on conditionality.

In order to examine this heterogeneity, we re-estimate the baseline models interacting *IMF disbursements to GDP* with the above mentioned firm characteristics. Equation 5 includes the same country and firm level controls as in the baseline model, except for the interaction term of IMF loans with a set of relevant firm-level characteristics. Because *Fund disbursements to GDP* are included in the model as an interaction, we do not instrument for disbursements to GDP with our IV, which is in turn an interaction. Instead, in order to limit selection bias, we estimate each regression on a reduced sample, considering only countries with IMF programs.[35] The regression equation is then the following:

$$g_{i,k,j,(t,t-2)} = \alpha_1 + \beta_1 X_{i,k,j,t} + \gamma_1 F_{j,t} + \delta C_{i,k,j,t} + \theta D_{j,t} + \lambda C_{i,k,j,t} \text{ x } D_{j,t} + \tau_{k,t} + \mu_j + \varepsilon_{i,k,j,t} \quad (5)$$

where X and F stand for the firm and country level controls, respectively, C stands for the firm-specific characteristic we test as a potential channel and D denotes the *disbursement amount to GDP* for the country j the firm belongs to. Standard errors are clustered at the country level, as before.

Table 3 presents the results. In each of the columns, we show the outcome on the interaction between IMF disbursements to GDP and a channel. The interaction terms with various measures of financial sophistication are shown in columns 1-4. The sum of these effects point to the presence of a liquidity channel through which IMF loans can influence firm sales. The effects associated to the principle sources of financing are complementary; firms with their main financing for working capital coming from formal sources experience higher growth rates of sales, while firms relying on informal source of finance are associated to a contraction in sales.[36] On a similar note, firms with an overdraft account do better with respect to their counterparts for a given level of Fund disbursements to GDP, while firms experiencing financial obstacles perform worse. On the other

[34] This interaction could also capture the so called Dutch disease effect. From a macroeconomic point of view, loans might also adversely impact firm growth if they induce Dutch disease, that is an appreciation of the real exchange rate detrimental to outward-looking firms (Rajan and Subramanian 2011).

[35] As we show in Table C1, in the online Appendix C, the results are robust to using the Woolridge control function approach (Woolridge 2015), which allows us to correct for the potential endogeneity bias.

[36] Such negative effect on performance could be explained by the stronger competition exerted by the firms benefitting from increased formal financing.

hand, the interaction with size of the firm is not significant. Finally, the interaction with our export identifier shows no evidence of a privileged effects for esport oriented firms (nor evidence of the Dutch disease) and, similarly, we find no evidenc of a differential effect for firms experiencing trade obstacles.

INSERT TABLE 3

Figure 4 show the marginal effects for the interactions with the categorical indicators, *Size*, *Financial obstacles*, and *Trade obstacle.* As can be seen, the effect on average firm sales growth is increasing across levels of firm size and is always positive. For financial or trade obstacles, where higher values indicate greater obstacles, the effect is (slightly) decreasing. In all cases, at the average value, the effect is positive.

INSERT FIGURE 4

In summary, we confirm our main hypothesis that the main channel of transmission of an IMF program is though the alleviation of the firm financing constraint, while other dimensions do not seem to be important. In the next Section we will focus on the specific role of the IMF conditionality.

5.3 Conditionality

As a further measure of the extent of the IMF intervention, in this Section we consider the various dimensions of conditionality. More specifically, we consider the differential impact of IMF area-specific reforms associated to each program. As previously described in Section 3, we take five broad policy areas: *Financial, External, Fiscal, Institutional, and Labor* sector reforms. We therefore measure the impact of an additional condition for each policy area on firm sales growth.

As discussed in Section 4, in order to comment on the effects of the IMF conditions, one has to take into account the endogeneity of a program type. The identification strategy works for selection into program type, not selection in program, therefore we run our regressions on the sample of countries under an IMF program, in order to determine the degree of intrusiveness of the IMF in these countries. We apply the same type of shift-share style instruments used in our previous analysis. Specifically, following Forster et al. (2019), we take the interaction between the number of countries under IMF program in a given year (a proxy for the Funds budget constraint) and the average number of conditions for a given policy area implemented by the Fund in the country.

The intuition is that, once in a program, the probability of having a high number of conditions, for countries that received already high conditions in the past, is higher when *Fund liquidity* is low, as the Fund would need to be more selective into the disbursement of consequent tranches.[37] We then investigate the impact of IMF conditionality on firm performance using the following specification:

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

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 IMF conditions, which vary by type of policy area p.[38] We take both contemporaneous and lagged values of N, to test for the persistency of conditionality. As above standard errors are clustered at the country level.

Tables 4a and 4b show the effects for our variable of interest, which is the number of conditions per specific policy area on firm sales growth. While Table 4a shows the results of the short term analysis, Table 4b presents the results up to the long term. For each set of columns (corresponding to each policy area) the first column shows the coefficient when we do not correct for the endogeneity bias, while the second one shows the instrumented coefficient, as well as the coefficient for the IV in the first stage.[39]

The coefficients of our variables of interest, in the IV specification, generally show the adverse effects of increasingly severe IMF programs.[40] For example, we see that an additional condition in the *Financial* policy area can lead to an 8 percent drop in average firm sales growth. The magnitude of the effect associated to *Fiscal* reforms is explained by understanding what these reforms entail. Restrictive fiscal policies have a direct and strong influence on the profitability of sales, as consumer spending power is reduced. Overall, such negative short-term effects seem to be limited to the areas of *Financial* and *Fiscal* policies, while we find no statistically significant effects when considering the *External* or *Institutional* reforms.[41]

[37] 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 (i.e., budget constraint is binding), the greater the effect of past conditionality on the current number of conditions.

[38] We run separate regressions for each policy area. The strong collinearity between number of conditions across policy areas makes it impossible to control for conditions in other policy areas.

[39] 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, except for *Fiscal* and *Labor*, where in fact the effect is weaker.

[40] The coefficient of the *External* sector conditionality is positive, but not significant at conventional levels.

[41] In Table C2, in the online Appendix C, using the Woolridge control function approach (Woolridge 2015), we interact the number of conditions per policy area with firm-level characteristics. We find that bigger firms benefit from *Financial* conditionality, firm experiencing trade obstacles are negatively affected by *External* sector

Table 4b shows the long run effects of Fund conditionality. In this case, (almost) all coefficients of the IV specifications turn positive.[42] *External, Fiscal* and *Institutional* reforms all seem to improve firm sales growth. Intuitively, both *Fiscal* and *Institutional* reforms are slow to implement by design, and therefore effects can be expected further down the line. In the same light, conditions under the category of *External* reforms are also slow to implement, as exchange systems and trade rules are negotiated not only at the country level, but when they do come into play it is at the benefits of firms. Across the time structure we surprisingly find no effects associated to *Labor* reforms.[43]

INSERT TABLES 4a, 4b

In a nutshell, the temporal dimension seems to be important in order to determine the effectiveness of the Fund conditionality: the same conditions which negatively affect firm performance in the short run, turn out to enhance firms sales in the longer term. In the next Section we examine how the increased sales are redistributed within the firm.

6 Redistribution within the firm

Given the evidence of increased income for firms following a program, a natural question is to wonder how the increased sales are redistributed within the firm. More specifically, sales can be redistributed either to the worker or the owner of firms. Both these effects have important distributional consequences which the IMF is sensitive to.[44]

Therefore, in this Section we estimate Equation 4 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 positive and significant, and with the expected sign.[45] The main result of Table 5 is that any effect on labor income share seem to be in the short run. That is, the increase in firm sales we observe induces firms to divert away compensation from the employees, but the effect is

conditionality and that *Labor* conditionality improves firm performance under greater political stability.

[42] The coefficient of the *Financial* sector conditionality is negative but not significant at conventional levels.

[43] This is likely due to the low number of this type of reforms in our data.

[44] In theory, income could either be reinvested or redistributed, but since our survey data does not contain information on investment decisions, we are unable to test directly for this choice.

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

confined to the short term. More specifically, IMF participation reduces the labor income share by about 6 percentage points.

INSERT TABLE 5

Given the impossibility to measure firm investment decisions, we consider the firm employment decisions, for which we have data from the WBES, testing whether IMF participation may affect the growth of permanent employers at the firm level. The intuition would be 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.[46] 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 6, we find that, IMF programs seem to induce an increase in employment but only in the long run, as effects are not statistically significant before. In particular, after a country participates to an IMF program, the number of permanent workers increase by 13 percent, which is a sizeable economic effect.

INSERT TABLE 6

As we did in Section 5.3 above, we also look at the specific effect of IMF conditions taking the labor income share as the dependent variable. Tables 7a and 7b show the OLS and 2SLS results when our variable of interest is the number of conditions for a given policy area. Again the IV strategy is the one by Forster et al. (2019). In the same way that participation in a program adversely affects the labor income share, we also find that increasing degrees of intervention by the IMF negatively affect the labor income share, at least in the short term.

In Table 7a, across the board for different policy areas, we find that a greater number of conditions is associated to falling levels of the labor income share when we instrument for the number of conditions. Looking at the IV specification, this negative effect is significant in the case of *Financial, Fiscal and Institutional* conditionality. Unsurprisingly, the effect is particularly strong when we consider *Fiscal* reforms, where one average the labor income share falls by 11 percentage points following an increase in the number of conditions. Because these reforms are redistributive by nature, we find that the redistribution occurs at the detriment of the worker.[47] Thus, in general,

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

[47] This result is also in line with the austerity narrative of Fund structural reforms.

it would seem that not only are workers of countries in programs worse off than those in countries which are not, but the intrusiveness of the program matters as well.[48]

As shown in Table 7b, however, the effects from program participation on the labor income share disappear in the long run, as none of these coefficients are statistically significant at conventional levels. As with Tables 4a and 4b (where we measured the effects of the number of conditions on sales growth), the negative effects of more severe IMF conditionality is a temporary phenomenon, attributable to adjustment costs for the country as it implements these reforms, but seems to dissipate over the years.

INSERT TABLE 7a, 7b

To sum up, the increase in firm sales we observe after a country participates to an IMF program lead to a temporary reduction in the workers' compensation, but it is found to increase the number of permanent workers in the long-run. The next Section contains some robustness analysis.

7 Robustness

This Section contains an in-depth discussion of potential robustness tests for our main results. Tabular results and figures related to this Section are presented in the online Appendices B-D. We begin with issues regarding the identification strategy, in particular to address the exclusion restrictions, to then turning to a series of alternative specifications for our main models. Finally we discuss issues related to our survey data, specifically the topic of 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 may affect firm sales differentially, conditional on the share of years spent under an IMF program.

Following 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

[48] When we interact the firm characteristics with the number of conditions per policy area (see Table C4, in the online Appendix C), using the Woolridge control function approach, we find that firms in a setting with greater *Labor* obstacles (or larger firms) suffer more in the case of *Labor* market reforms. On the other hand, *Institutional* reforms lead to an increase in the labor income share for firms operating in a setting with lower *Corruption.*

an IMF program versus those with high share of years (*IMF probability).* Similarly, we plot the log of *IMF liquidity* over time alongside the trend of (i) employee's compensation, measured as the labor share of national income from the World Inequality Database, or (ii) a Gini index from the Standardized World Income Inequality Database (2020), over two sets of countries, those with a high and low *IMF probability.*[49] Figure B2 and B2, in the online Appendix B, plot these graphs. 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).[50] 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 the one of alternative trends driving 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.[51] Because these global 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. Table B1, in the online Appendix B, confirms our main results are robust to controlling for the differential effect of these alternative trends interacted with country-specific *IMF probability.* Our results are robust to controlling for these most obvious confounde, however, we cannot definitively rule out the presence of omitted variable bias.

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, such as in the original paper by Nunn and Qian (2014). In this case, our interaction component *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. That is: $\frac{1}{39}\sum_{y=1}^{39} A_{j,y}^{IMF}$, where A is a binary indicator that switches to one if country j received an IMF program in year y. Table B2, in the online Appendix B, 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 long run. This method, however, is also less intuitive as it captures future relationships between the Fund and a given country and uses it to explain present and past

[49] 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.

[50] As 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. We see this is not the case, as there is no evident correlation and difference between the groups and *IMF liquidity* remains constant over time.

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

relations. The time-varying version therefore remains our preferred IV.

7.2 Alternative specifications

As discussed above, in this Section of the Appendix, we present the results of alternative specifications. In Table C1 we show the results obtained applying the control function approach developed by Woolridge (2015) to the interaction between *IMF disbursements to GDP* and firm characteristics, to investigate the channels of transmission of IMF loans. We find that the results are in line with those presented in Table 3 above. Finally, Table C2 and C3 shows the results obtained interacting the number of IMF conditions and the available firm characteristics, to examine the channels of transmission of IMF conditionality on firm sales and labor income share, respectively.

7.3 Sample dependence

An equally important issue to address in our model is the role of sample dependence, such as the sensitivity of the results to the inclusion of certain countries in 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.[52]

There are a series of subsample analysis we can run as a sensitivity analysis. Table D1, in the online Appendix D, for example, shows the effects of IMF Participation on firm sales growth when the sample is split along five broad industry groups.[53] The results are robust across industries, with the exception of Services, where the identification in the first stage is not significant at conventional levels.[54]

Another quite evident limitation to survey data is the problem of recontacting firms. Beside promising best practices and efforts to create panel data in their survey, the WBES provides 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

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

[53] See Table A1, in the online Appendix A, for a description of the aggregation of industries.

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

survivor bias. If this were the case however, we would expect that the distribution of firms with repeated interviews versus the distribution of single-presence (no repeated interviews) firms would be significantly different. Figure D1 in the Appendix shows that the two distributions are rather similar.

Finally, we run a randomization of the sample of firms per country, as a formal test to the sensitivity of our results to sample dependence. 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.[55] In a similar vein, we then choose to 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.[56]

We then applied 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.[57] We can run 100 simulations and find the average coefficient using a panel model with firm fixed effects. All results are reported in Table D2, in the online Appendix D; 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 panel of 130,000 firms in 139 developing countries, over the period 2003-2018. We consider the different dimensions of a Fund program, namely participation, loan size and number and scope of conditions, and we look at their effects on growth of firm sales, as well as on income redistribution within the firm. Our identification strategy exploits the differential effect of changes in *IMF liquidity* on program participation (Lang 2016).

[55] 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.

[56] 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.

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

Our results show a positive impact of participating in an IMF program on firms' sales growth, and, what is more, the effect is persistent through time. Controlling for firm fixed effects, sales could be almost 30 percent higher for firms in countries benefitting from IMF lending and the results is confirmed when considering the loan size.

As the channels of transmission are concerned, we find that IMF intervention is associated to a sales' increase for firms that are financially constrained, which suggests that loans could improve firm performance through the alleviation of financing constraints in developing countries. Moreover, it favours bank-financed firms over those operating in the informal sector. Using a detailed information on conditionality, we find that the time dimension seems to be an important factor in determining programs' effectiveness. More severe conditionality seems to worsen firm performance in the short run, while turning beneficial in the long term.

Finally, when taking labor income share as the dependent variable, we find that participating to an IMF program reduces the labor income accruing to the workers by about 6 percentage points, but this negative effect seems to be confined to the short run. Since firm-level employment increases in the long term, for countries participating in a programs, this evidence seems to suggest that the increased income is reinvested into the firm.

This paper contributes to the literature on the IMF effectiveness, it is the first study that evaluates the effect of IMF participation on firm performance providing (we believe) important insights for the underlying mechanisms behind IMF intervention. Since in the wake of the pandemic over one hundred countries have already approached the IMF for assistance, following a period of relative calm, the IMF activity is likely to be again under scrutiny (e.g, see Archibong et al. 2021; Chari et al. 2021; Goldfajn et al. 2021; Spence 2021).

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Tables

Table 1: IMF participation and firm sales growth

	Contemporaneous				Lagged			
	Pooled OLS	2SLS	FE	2SLS FE	Pooled OLS	2SLS	FE	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Participation	0.097*	0.306**	0.163***	0.274***	0.130**	1.044	0.151**	0.636**
	(1.96)	(2.52)	(2.95)	(3.75)	(2.43)	(1.46)	(2.50)	(2.08)
(ln) Sales$_{(t-1)}$	-0.095***	-0.095***	-0.175***	-0.176***	-0.095***	-0.098***	-0.177***	-0.182***
	(-12.90)	(-13.15)	(-8.49)	(-8.69)	(-13.01)	(-13.55)	(-8.53)	(-9.60)
State	0.024	0.029*	0.159**	0.145**	0.023	0.029	0.168**	0.141**
	(1.37)	(1.65)	(2.40)	(2.22)	(1.32)	(1.58)	(2.51)	(2.12)
Foreign	0.067***	0.065***	0.063*	0.057*	0.068***	0.067***	0.068*	0.055
	(6.71)	(6.15)	(1.78)	(1.72)	(6.78)	(6.04)	(1.78)	(1.56)
Exports	0.051***	0.045***	0.029	0.029	0.051***	0.048***	0.028	0.026
	(6.84)	(6.57)	(1.42)	(1.44)	(6.86)	(5.55)	(1.40)	(1.24)
Size	0.171***	0.170***	0.109***	0.110***	0.171***	0.176***	0.112***	0.117***
	(13.04)	(13.02)	(4.64)	(4.85)	(13.15)	(13.85)	(4.70)	(5.31)
(ln) GDP per Capita	-0.029	-0.039	0.248*	0.238*	0.033	0.388	0.304**	0.480**
	(-0.18)	(-0.23)	(1.95)	(1.88)	(0.19)	(1.32)	(2.09)	(2.47)
GDP Growth	-0.013	-0.010	-0.008	-0.011*	-0.019**	-0.062*	-0.015**	-0.049**
	(-1.39)	(-1.13)	(-1.43)	(-1.67)	(-2.01)	(-1.69)	(-2.21)	(-1.98)
Population	0.575	0.556	1.527*	1.189	0.777	1.460	2.202**	2.362*
	(1.17)	(0.80)	(1.88)	(1.45)	(1.46)	(1.02)	(2.16)	(1.78)
Corruption	-0.079	-0.168**	-0.102**	-0.129***	-0.052	-0.147	-0.083**	-0.181**
	(-1.61)	(-2.10)	(-2.51)	(-2.78)	(-1.15)	(-1.26)	(-2.12)	(-1.96)
IMF Probability		-1.307		0.465		-0.242		-0.179
		(-1.61)		(0.58)		(-0.15)		(-0.13)
First stage:								
Instrument		-1.341***		-1.717***		-0.714		-1.261**
		(-3.57)		(-6.30)		(-1.58)		(-2.35)
Observations	57469	57469	6264	6264	57469	57469	6264	6264
R2	0.200	0.127	0.368	0.276	0.200	0.0380	0.364	0.191
Kleibergen Paap (p-value)		0.017		0.002		0.132		0.041
Panels			3132	3132			3132	3132
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. Since our data on IMF liquidity are available up to 2014, our year sample also ends in 2014. 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.

Table 2: IMF disbursements and firm sales growth

	Pooled OLS	2SLS	FE	2SLS FE
	(1)	(2)	(3)	(4)
IMF disbursements	6.616	35.90**	7.689 *	31.53**
	(1.29)	(2.13)	(1.95)	(2.35)
IMF Probability		-0.482		0.0368
		(-0.44)		(0.04)
First stage:				
Instrument		-1.142**		-1.492***
		(-2.54)		(-3.42)
Observations	57469	57469	6264	6264
R2	0.199	0.118	0.360	0.233
Kleibergen Paap (p-value)		0.061		0.015
Panels			9907	9907
Controls	YES	YES	YES	YES
Firm FE	NO	NO	YES	YES
Industry x Year FE	YES	YES	YES	YES
Country FE	YES	YES	NO	NO

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Since our data on IMF liquidity are available up to 2014, our year sample also ends in 2014. 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 disbursements* 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.

Table 3: IMF disbursements and firm sales growth, channels

	Formal Finance	Informal Finance	Overdraft	Financial Obstacles	Size	Trade Obstacles	Exporting Firms
	(1)	(2)	(3)	(5)	(6)	(7)	(8)
IMF x Channel	4.593***	-5.117**	5.297***	-0.787**	0.365	-0.181	-1.155
	(2.86)	(-2.46)	(8.48)	(-2.43)	(0.4)	(-1.01)	(-0.52)
Channel	-0.027**	0.037**	-0.081***	-0.006**	0.182***	0.007*	0.057***
	(-2.20)	(2.52)	(-7.71)	(-2.19)	(15.6)	(1.7)	(3.71)
IMF disbursements	3.562	8.008***	2.955	9.755***	7.330***	8.054***	7.817***
	(1.19)	(3.4)	(1.2)	(3.44)	(3.33)	(3.41)	(3.35)
Observations	21129	21129	20815	20713	21129	19859	21129
R2	0.236	0.236	0.237	0.235	0.236	0.23	0.236
Controls	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF disbursements 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 channel considered, through interactions with firm-specific characteristics. 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.

Table 4a: Number of conditions and firm sales growth, contemporaneous

	Financial		External		Fiscal		Institutional		Labor	
	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of conditions	-0.0251	-0.087***	-0.061	0.103	-0.003	-0.499***	-0.209*	-0.221	0.187	-0.761
	(-1.27)	(-9.06)	(-0.68)	(1.27)	(-0.10)	(-4.38)	(-1.89)	(-1.58)	(1.27)	(-1.40)
First stage:										
Instrument		0.0145***		0.0166***		0.0022***		0.018*		-0.010
		(43.08)		(32.91)		(3.74)		(12.02)		(-1.58)
Observations	1590	1590	1590	1590	1590	1590	1590	1590	1590	1590
R2	0.372	0.242	0.367	0.187	0.366	0.206	0.378	0.206	0.371	0.130
Panels	795	795	795	795	795	795	795	795	795	795
Kleibergen Paap (p-value)		0.037		0.023		0.113		0.010		0.220
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF disbursements 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 channel considered, through interactions with firm-specific characteristics. 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.

Table 4a: Number of conditions and firm sales growth, lagged

	Financial		External		Fiscal		Institutional		Labor	
	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of conditions	0.015***	-0.002	0.079***	0.124***	0.016***	0.027***	0.058***	0.046*	0.034***	0.035
	(3.40)	(-0.06)	(4.62)	(5.93)	(2.90)	(2.85)	(4.78)	(1.90)	(2.88)	(0.84)
First stage:										
Instrument		0.002		0.004**		-0.001***		-0.066***		-0.008**
		(0.85)		(4.07)		(-3.75)		(-3.20)		(-2.43)
Observations	2798	2798	2798	2798	2798	2798	2798	2798	2798	2798
R2	0.345	0.201	0.344	0.225	0.345	0.217	0.339	0.215	0.336	0.214
Panels	1382	1382	1382	1382	1382	1382	1382	1382	1382	1382
Kleibergen Paap (p-value)		0.503		0.003		0.020		0.039		0.023
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF disbursements 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 channel considered, through interactions with firm-specific characteristics. 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.

Table 5: IMF participation and labor income share

	Contemporaneous				Lagged			
	Pooled OLS	2SLS	FE OLS	2SLS FE	Pooled OLS	2SLS	FE OLS	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Participation	-0.049**	0.002	-0.058***	-0.068**	-0.035	0.244	-0.0286	-0.080
	(-2.19)	(0.02)	(-2.76)	(-2.14)	(-1.24)	(0.7)	(-0.98)	(-0.92)
IMF Probability		0.273		0.237		1.092		0.435
		(0.68)		(0.82)		(1.36)		(1.35)
First stage:								
Instrument		-1.292***		-1.751***		-0.599		-1.040**
		(-3.10)		(-5.56)		(-1.52)		(-2.15)
Observations	33861	33861	3358	3358	33861	33861	3358	3358
R2	0.121	0.0821	0.0819	0.0411	0.12	0.063	0.0741	0.0309
Kleibergen Paap (p-value)		0.023		0.003		0.145		0.053
Panels			1679	1679			1679	1679
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. Since our data on IMF liquidity are available up to 2014, our year sample also ends in 2014. 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.

Table 6: IMF participation and job

	Contemporaneous				Lagged			
	Pooled OLS	2SLS	FE OLS	2SLS FE	Pooled OLS	2SLS	FE OLS	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Participation	0.014	0.052	-0.0001	0.027	0.018	0.161	0.011	0.134*
	(1.27)	(1.48)	(-0.01)	(1.25)	(1.19)	(1.31)	(0.66)	(1.79)
IMF Probability		-0.341		0.249		-0.125		0.338
		(-1.63)		(1.13)		(-0.41)		(0.99)
First stage:								
Instrument		-1.297***		-1.680***		-0.682*		-1.155***
		(-3.57)		(-6.27)		(-1.50)		(-2.24)
Observations	64160	64160	7758	7758	64160	64160	7758	7758
R2	0.064	0.013	0.131	0.040	0.064	0.004	0.131	0.014
Kleibergen Paap (p-value)		0.018		0.002		0.155		0.048
Panels			3879	3879			3879	3879
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 and industry-year dummies. Column 2 uses an IV estimator with country dummies. Since our data on IMF liquidity are available up to 2014, our year sample also ends in 2014. Column 3 uses the within estimator with fixed effects at the firm level. Column 4 uses an IV estimator with firm fixed effect. The coefficient *Instrument* is the IV *IMF liquidity*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.

Table 7a: Number of conditions and labor income share, contemporaneous

	Financial		External		Fiscal		Institutional		Labor	
	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of conditions	-0.009*	-0.027***	-0.010	-0.024	-0.014*	-0.118***	-0.086***	-0.104***	0.049	-0.517
	(-1.88)	(-6.38)	(-0.43)	(-0.92)	(-1.86)	(-4.74)	(-4.69)	(-5.84)	(1.31)	(-1.09)
First stage:										
Instrument		0.014***		0.016**		0.002**		0.019***		-0.007
		(23.90)		(33.35)		(2.98)		(16.65)		(-1.23)
Observations	862	862	862	862	862	862	862	862	862	862
R2	0.159	0.0461	0.151	0.0409	0.158	0.0773	0.181	0.0641	0.155	-0.507
Panels	431	431	431	431	431	431	431	431	431	431
Kleibergen Paap (p-value)		0.0209		0.0143		0.105		0.0105		0.268
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Effects of additional conditions per policy area on the labor income share. Regressions are run on subsample of countries under an IMF program. Columns are sorted according to policy area considered, using both a standard within estimator and an IV estimator with firm fixed effects. The coefficient *Instrument* is the IV *Number of countries under IMF program*Average number of conditions in policy area p for country j* for the first stage in our IV models. 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.

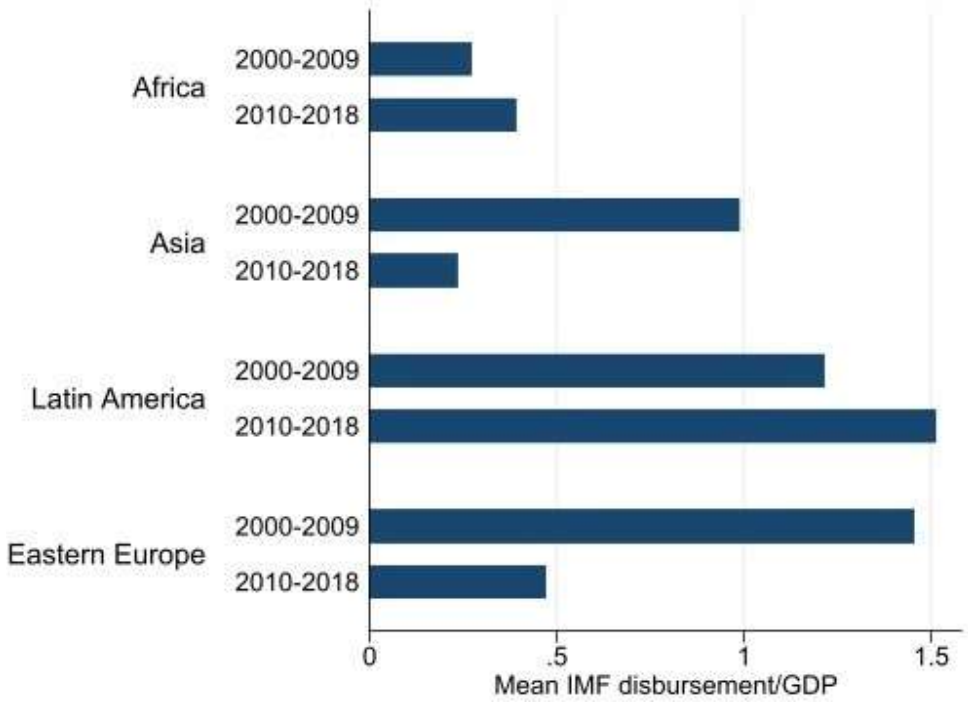
Table 7b: Number of conditions and labor income share, lagged

	Financial		External		Fiscal		Institutional		Labor	
	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE	FE	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Number of conditions	0.003	0.020	0.008	-0.027	0.002	0.001	-0.011	0.005	0.002	-0.037
	(1.25)	(0.72)	(0.63)	(-0.93)	(0.64)	(0.26)	(-1.19)	(0.42)	(0.16)	(-1.05)
First stage:										
Instrument		0.001		0.003**		-0.01**		-0.008***		-0.004**
		(0.44)		(3.50)		(-4.68)		(-5.86)		(-2.22)
Observations	1476	1476	1476	1476	1476	1476	1476	1476	1476	1476
R2	0.105	-0.030	0.103	0.017	0.104	0.029	0.104	0.033	0.103	0.011
Panels	726	726	726	726	726	726	726	726	726	726
Kleibergen Paap (p-value)		0.680		0.005		0.008		0.010		0.074
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Effects of additional conditions per policy area on the labor income share. Regressions are run on subsample of countries under an IMF program. Columns are sorted according to policy area considered, using both a standard within estimator and an IV estimator with firm fixed effects. The coefficient *Instrument* is the IV *Number of countries under IMF program*Average number of conditions in policy area p for country j* for the first stage in our IV models. 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.

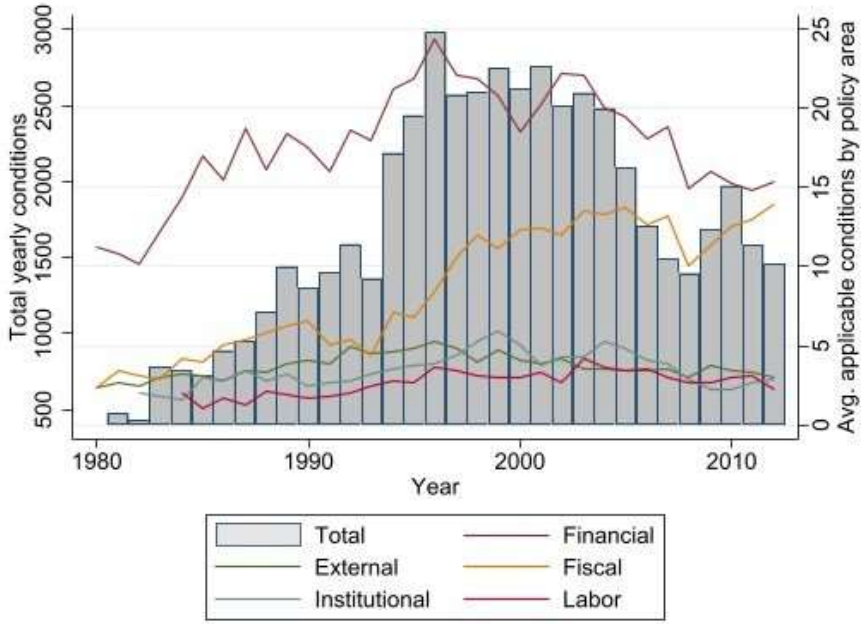
Figures

Figure 1: Average IMF disbursements by decade for WB regions



Notes: Average IMF disbursements for set of countries under IMF program by World Bank defined macro regions, comparing first and second half of sample period.

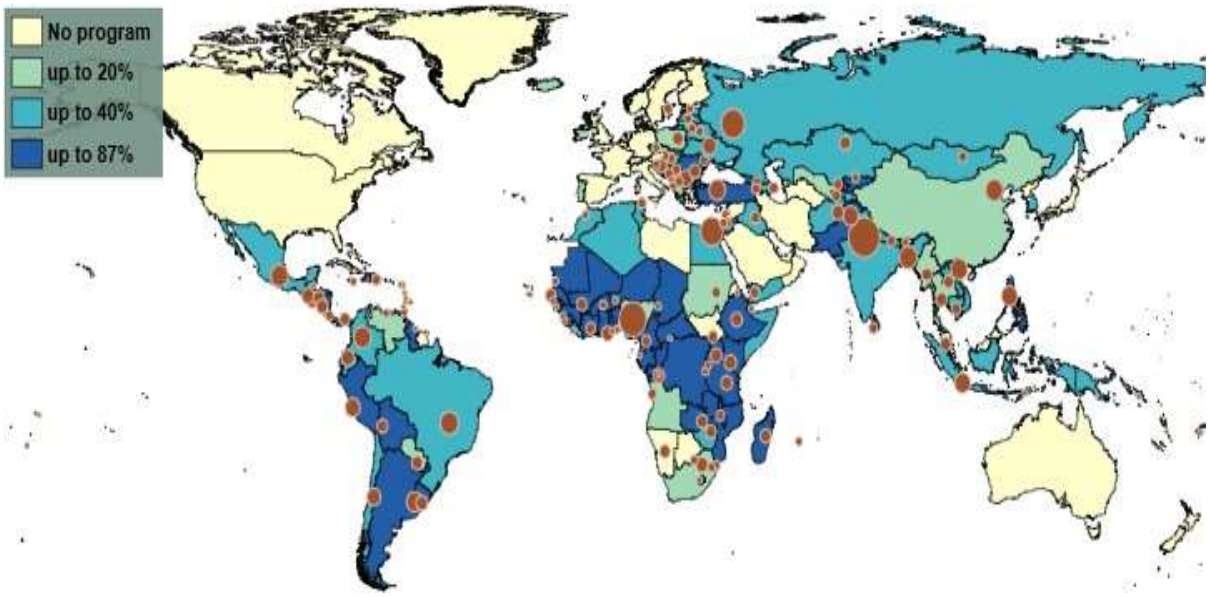
Figure 2: Total IMF conditions and average conditions by policy area



Notes: Evolution of number of conditions by policy area. Left axis shows total number of binding conditions

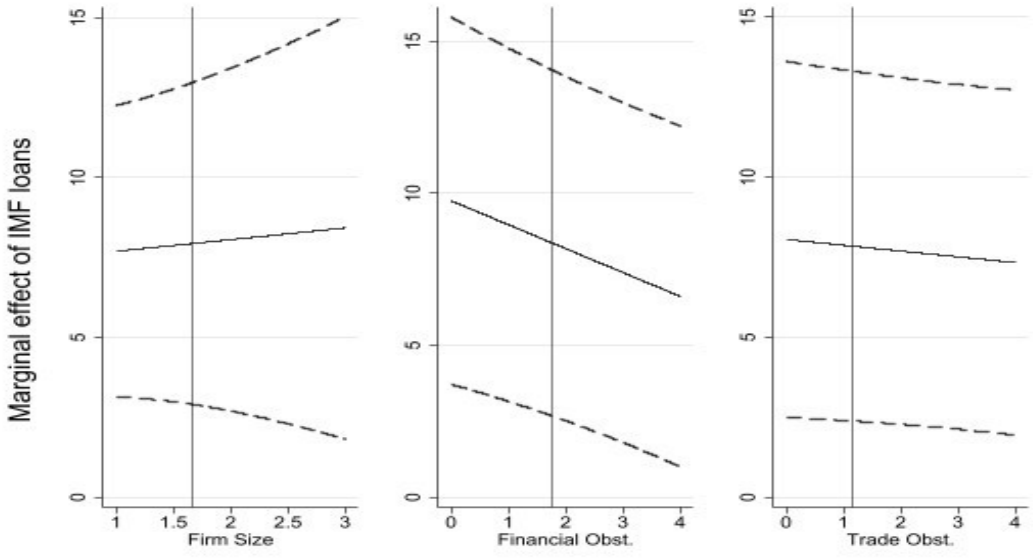
imposed as part of IMF programs, right axis shows average number of conditions by policy area.

Figure 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.

Figure 4: Marginal effects of IMF disbursements by levels of firm characteristics



Notes: Marginal effects of an additional IMF disbursement, measured as SDR credits to GDP, on average firm sales growth by different values of firm characteristics. First plot shows effects for levels of firm size, with 3 representing firms with more than 100 employees. Second and third plots shows effects for varying degrees of financial and trade obstacles, respectively, from low to high number of perceived obstacles. Dotted lines show 90% confidence interval.

Online Appendices

Online Appendix A: Descriptives

Table A1: Distribution of firms across industries

	Observations	Percent	Aggregations
Chemicals	5975	4.37	Chemicals and Electronics
Electronics	1338	.98	
Metals & Minerals	6757	4.95	
Food	10821	7.92	Food
Garments	10910	7.98	Manufacturing
Manufacturing	31812	23.28	
Retail	21807	15.96	Retail
Services	33666	24.64	Services
Not reported	13535	9.91	Not reported
Total	136621	100.00	

Notes: Number of observations from full sample (2000-2018) excluding conflict countries. Author's calculations based on World Bank Enterprise Survey classification of firms by industry. For further information see WBES methodological notes at https://www.enterprisesurveys.org/en/methodology.

Table A2: Description of policy area reforms

Financial	Financial sector/monetary policy	Monetary policy (Reserve money, interest rates, base money); Government securities, issuance and auctions; Audit, privatization, bankruptcy of financial institutions
	External debt	Debt management, arrears
External	External sector, trade and exchange systems	Trade liberalization, tariffs, quotas; Exchange system (foreign exchange rate regime, exchange rate policy); Capital account liberalization; FDI
Fiscal	Fiscal policies	Expenditure policy, audits, budget issues; Fiscal transparency
	Revenues and taxes	Tax policy, legislation and administration
	Redistributive and social policies	All measures of a clearly redistributive nature, incl. poverty reduction measures
Institutional	Institutional policies	Legal reforms, bankruptcy laws, judicial system reforms; Competition policy, private sector development; Anti-corruption measures.
	SOE privatization	Privatization of non-financial SOEs
	SOE reforms	Audits of SOEs, restructurings; Regulatory reforms in utilities, price controls and marketing restrictions
Labor	Labor policies, public and private	Wage, employment limits; Pensions

Notes: Author's aggregations based on the original classifications by Kentikelenis et al (2016).

Table A3: Summary statistics

	Observations	Mean	Sd	Max	Min
Dependent variables					
Sales growth	102807	.1405	.5045	10.5	-8.531
Labor income share	59498	.2242	.2389	7	.0025
Employment growth	125017	0.0578	0.211	3.719	-4.736
Firm variables					
Sales last fiscal year	123597	16.64	3.288	37.2	0
State owned	137154	.0171	.1298	1	0
Foreign owned	137108	.1051	.3067	1	0
Exporting	138119	.2205	.4146	1	0
Size	132786	1.718	.7644	3	1
Formal financing	138913	.7618	.426	1	0
Informal financing	138913	.0958	.2943	1	0
Overdraft account	128017	.5681	.4953	1	0
Financial obstacles	133877	1.546	1.354	4	0
Trade obstacles	125764	1.018	1.235	4	0
Tax obstacles	137110	1.805	1.331	4	0
Administrative obstacles	138218	1.413	1.286	4	0
(Lack of) Political instability	90131	3.356	1.486	5	1
(Lack of) Corruption	92128	3.243	1.501	5	1
Country variables					
IMF disbursement/GDP	142194	0.00126	0.0045	0.1112	0
IMF Participation	142454	0.3301	0.4702	1	0
Log per capita GDP	142194	7.8932	1.0823	12.0856	4.765
GDP growth	141902	4.7585	3.1644	47.213	-19.282
Log population	142448	17.1628	1.9385	21.044	9.144
Corruption index	125307	2.2078	0.6584	6	0

Notes: Summary statistics for main variables on full sample (2000-2018), excluding conflict countries.

Table A4: Conditionality summary statistics

	Observations	Mean	S.d.	Max	Min
Number of Conditions					
Financial	123139	3.9	8	61	0
External	123139	.82	1.9	13	0
Fiscal	123139	3.02	6.5	49	0
Institutional	123139	.62	1.7	63	0
Labor	123139	.22	.8	13	0
Policy dummy					
Financial	142454	.37	.48	1	0
External	142454	.34	.47	1	0
Fiscal	142454	.38	.48	1	0
Institutional	142454	.31	.46	1	0
Labor	142454	.22	.42	1	0

Notes: Number of observations from full sample (2000-2018) excluding conflict countries.

Table A5: Variable description

Variable	Description	Source
FIRM		
Sales growth	Average annual growth rate of sales, percent	
Labor Income Share	Share of employee compensation over total sales	
Jobs	Average annual growth rate of permanent full-time employees, percent	
Log Sales (base year)	Establishment Sales 3 Years Ago, in log	
State	Dummy ==1 if state ownership > 0	
Foreign	Dummy=1 if owned by private foreign individuals, companies or organizations	
Export	Dummy=1 if sales from indirect exports > 0	
Size	Firm category Based On No. Of employees: 1 Small (< 20), 2 Medium (20-99), 3 Large (> 100)	
Finance formal	Dummy==1 if firm principal financing from formal channels (bank, state, self)	World Bank Enterprise Survey
Finance informal	Dummy==1 if firm principal financing from informal channels	
Firm has an overdraft facility	Dummy==1 if firms have an overdraft facility	
Financial obstacles	How much an obstacle is access to finance, categorical	
Trade obstacles	How much an obstacle are custom and trade regulations, categorical	
Labor obstacles	How much an obstacle are labor regulations, categorical	
Tax obstacles	How much an obstacle are tax rates, categorical	
Administrative obstacles	How much an obstacle are administrative tax regimes, categorical	
(Lack of) political instability	No obstacle with political instability, categorical variable (1-5)	
(Lack of) corruption	No obstacle with corruption, categorical variable (1-5)	
COUNTRY		
GDP Growth	GDP (constant 2015 US$), Annual rate of change	WDI, World Bank (2018)
GDP per capita (log)	GDP (constant 2015 US$), per capita (in log)	WDI, World Bank (2018)
Population (log)	Log of total population	WDI, World Bank (2018)
Corruption	International Country Risk Guide - Corruption Index	ICRG PRS Group (2018)
Policy area	Number of applicable conditions per policy area	Kentikelenis et. al (2016)
IMF participation	Dummy ==1 if country under program for given year	IMF Member financial data
IMF disbursements (to GDP)	IMF disbursements to GDP	IMF Member financial data

Table A6: Survey sample description

African and Middle East				Eastern Europe and Central Asia				Asia				Latin America and the Caribbean			
Country	Years	Obs.	Unique firms	Country	Years	Obs.	Unique firms	Country	Years	Obs.	Unique firms	Country	Years	Obs.	Unique firms
Angola	2006, 2010	785	602	Albania	2007, 2009, 2013	839	719	Bangladesh	2011, 2013	1692	1570	Antigua & Barbuda	2010	151	151
Benin	2004, 2016	497	366	Armenia	2009, 2013	734	566	Bhutan	2009, 2015	503	390	Argentina	2006, 2010, 2017	3108	
Botswana	2006, 2010	610	491	Azerbaijan	2009, 2013	770	701	Cambodia	2013, 2016	845	714	Bahamas	2010	150	150
Burkina Faso	2006, 2009	533	445	Belarus	2008, 2013	633	512	China	2012	2700	2700	Barbados	2010	150	150
Burundi	2014	157	157	Bosnia & Herz.	2009, 2013	721	606	Fiji	2009	164	164	Belize	2010	150	150
Cabo Verde	2006, 2009	254	201	Bulgaria	2007, 2009, 2013	1596	1526	India	2014	9281	9281	Bolivia	2006, 2010, 2017	1339	992
Cameroon	2006, 2009, 2016	931	676	Croatia	2007, 2009, 2013	1152	1115	Indonesia	2009, 2015	2764	2273	Brazil	2003, 2009	3444	2992
Central African Rep	2011	150	150	Czech Republic	2009, 2013	504	486	Lao PDR	2009, 2012, 2016	1107	813	Chile	2006, 2010	2050	1620
Chad	2009, 2018	303	303	Estonia	2009, 2013	546	473	Malaysia	2015	1000	1000	Colombia	2010, 2017	1935	1935
Congo, Rep	2009	151	151	Georgia	2008, 2013	733	652	Micronesia	2009	68	68	Costa Rica	2010	538	538
Cote d'Ivoire	2009, 2016	887	742	Hungary	2009, 2013	601	538	Mongolia	2009, 2013	722	591	Dominica	2010	150	150
Dem. Rep. Congo	200,620,102,013	1228	1136	Kazakhstan	2009, 2013	1144	1061	Mynamar	2014, 2016	1239	961	Dominican Rep	2010, 2016	719	616
Djibouti	2013	266	266	Kosovo	2009, 2013	472	461	Nepal	2013	482	482	Ecuador	2003, 2006, 2010, 2017	1838	1394
Egypt, Arab Rep	2013, 2016	4711	4711	Kyrgyz Rep	2009, 2013	505	461	Pakistan	2007, 2013	2182	2182	El Salvador	2006, 2010, 2016	1772	1433
Eritrea	2009	179	179	Latvia	2009, 2013	607	515	Papua New Guinea	2015	65	65	Grenada	2010	153	153
Eswatini	2006, 2016	457	457	Lithuania	2009, 2013	546	501	Samoa	2009	109	109	Guatemala	2006, 2010, 2017	1457	1317
Ethiopia	2011, 2015	1492	1120	Moldova	2009, 2013	723	540	Solomon Islands	2015	151	151	Guyana	2010	165	165
Gabon	2009	179	179	Montenegro	20,092,013	266	212	Thailand	2016	1000	1000	Honduras	2003, 2006, 2010, 2016	1578	1171
Gambia	2006, 2018	325	325	North Macedonia	20,092,013	726	548	Timor-Leste	2009, 2015	276	190	Jamaica	2010	376	376
Ghana	2007, 2013	1214	1183	Poland	2009, 2013	997	980	Tonga	2009	150	150	Mexico	2006, 2010	2960	2750
Guinea	2006, 2016	373	373	Romania	2009, 2013	1081	984	Vanuatu	2009	128	128	Nicaragua	2003, 2006, 2010, 2016	1599	1094
Guinea-Bissau	2006	159	159	Russian Federation	2009, 2012	5224	5087	Vietnam	2005, 2009, 2015	3199	2548	Panama	2006, 2009	969	845

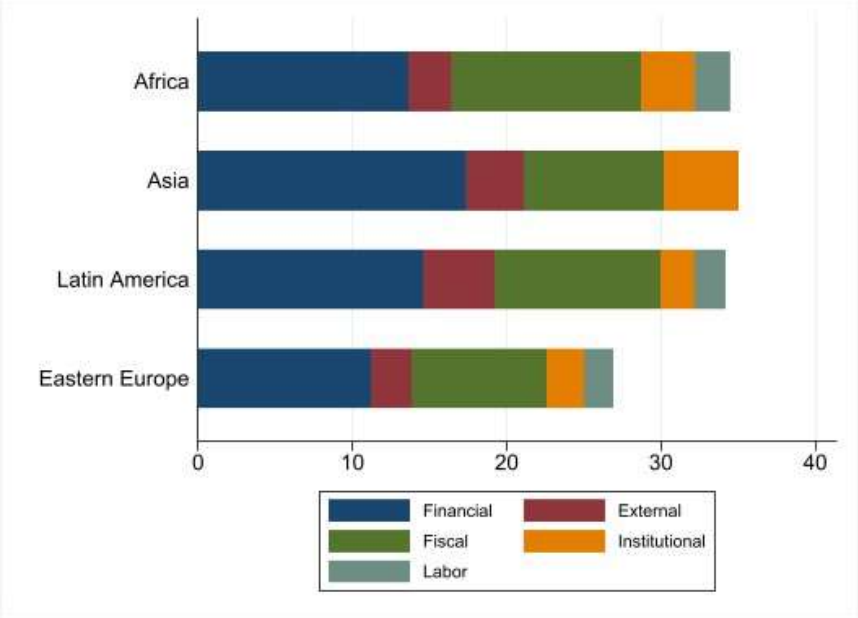
Jordan	2013	573	573	Serbia	2009, 2013	748	628			Paraguay	2006, 2010, 2017	1338	1067
Kenya	2007, 2013	1438	1287	Slovak Republic	2009, 2013	543	531			Peru	2006, 2010, 2017	2635	1974
Lebanon	2013	561	561	Slovenia	2009, 2013	546	451			St. Kitts & Nevis	2010	150	150
Lesotho	2009, 2016	301	240	Sweden	2014	600	600			St. Lucia	2010	150	150
Liberia	2009, 2017	301	220	Tajikistan	2008, 2013	719	684			St. Vincent & Grenadines	2010	154	154
Madagascar	2009, 2013	977	977	Turkey	2008, 2013	2496	2358			Suriname	2010	152	152
				Ukraine	2008, 2013	1853	1661			Trinidad & Tobago	2010	370	370
Malawi	2009, 2014	673	586										
				Uzbekistan	2008, 2013	756	617			Uruguay	2006, 2010, 2017	1575	1176
Mali	2003, 2007, 2010, 2016	1190	872							Venezuela	2006, 2010	440	387
Mauritania	2006, 2014	387	387										
Mauritius	2009	398	398										
Morocco	2013	407	407										
Mozambique	2007	479	479										
Namibia	2005, 2014	909	909										
Niger	2005, 2009, 2017	426	297										
Nigeria	2007, 2009	5048	5048										
Rwanda	2006, 2011	453	383										
Senegal	2007, 2014	1107	869										
Sierra Leone	2009, 2017	302	227										
South Africa	2003, 2007	1540	1349										
South Sudan	2014	738	738										
Sudan	2014	662	662										
Tanzania	2006, 2013	1232	1117										
Togo	2009, 2016	305	245										
Tunisia	2013	592	592										
Uganda	2013	762	762										
West Banks and Gaza	2013	434	434										
Yemen, Rep.	2010, 2013	830	691										
Zambia	2007, 2013	1204	1052										
Zimbabwe	2011, 2016	1199	897										

Notes: Description of country sample in WBES, excluding conflict countries. Shows years surveys are carried out, with total number of observations in our sample per country (pooled sample) and total number of unique firms per country. The total number of unique firms per country cannot be more than the number of observations in sample. When the two are equal, this can happen either because countries participate only in one wave of survey, or in the extreme case where there are multiple survey years and the total number of observations equals unique firms (see Nigeria), this implies a re-contact rate of 0%.

Table A7. Description of the channels

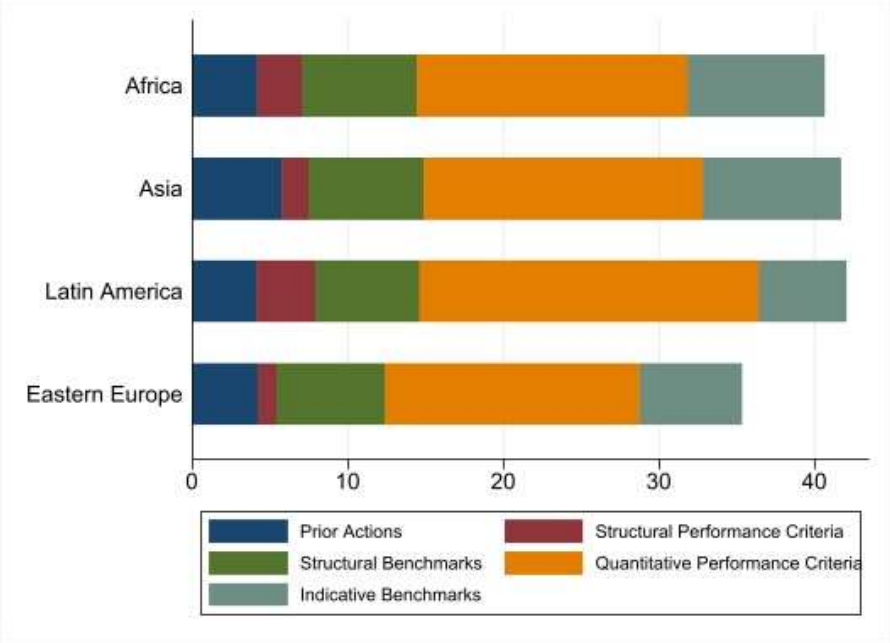
CREDIT (Reputations and actual constraint)
Firm has an overdraft facility (Yes / No)
Financial obstacles (None, minor, moderate, high)
Formal (bank, state, self) (Yes / No)
Informal (Yes / No)
TRADE (and Dutch Disease)
Trade obstacles (None, minor, moderate, high)
Exports goods (Yes/No)
LABOR,
Labor obstacles (Yes / No)
FISCAL
Fiscal (Yes / No)
Tax obstacles (Yes / No)
Administrative obstacles (Yes / No)
INSTITUTIONS
(perceived) Political instability (very high, high, moderate, minor, none)
(perceived) Corruption (very high, high, moderate, minor, none)

Figure A1: Average number of conditions for policy area by region



Notes: Average number of conditions by policy area in different World Bank defined macro regions, computed for sample of countries under a program.

Figure A2: Average number of conditions for conditionality type by region



Notes: Average number of conditions by conditionality type in different World Bank defined macro regions, computed for sample of countries under a program.

Online Appendix B: Identification

Table B1: Time-varying heterogeneous trends

	2SLS FE	2SLS	2SLS FE	2SLS	2SLS FE	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
IMF Participation	0.552**	0.504**	0.189*	0.535	0.473**	0.577*
	(2.45)	(2.33)	(1.85)	(1.39)	(2.36)	(1.91)
Bank crises x IMF probability	0.089	0.048			0.139*	0.010
	(1.37)	(1.14)			(1.86)	(0.17)
Currency crises x IMF probability			-0.239	0.493	-0.656*	0.494
			(-1.10)	(0.68)	(-1.68)	(0.63)
Observations	6264	57469	6264	57469	6264	57469
R2	0.222	0.172	0.283	0.168	0.246	0.161
Firm FE	YES	NO	YES	NO	YES	NO
Country FE	NO	YES	NO	YES	NO	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES
Kleibergen Paap (p-value)	0.0149	0.0126	0.0481	0.287	0.0313	0.101
Panels	3132		3132		3132	

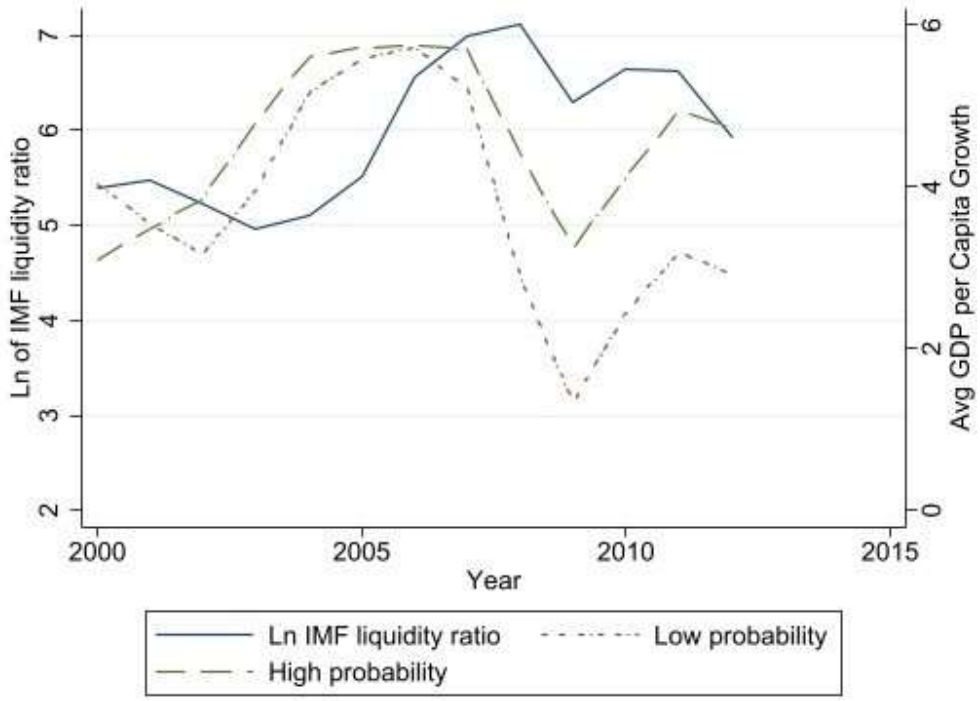
Notes: Effect of *IMF participation* on average firm sales growth when controlling for heterogenous global trends. Columns 1 and 2 control for the global number of *Bank crises* while columns 3 and 4 control for global *Currency crises*. Columns 5 and 6 control for both simultaneously. Coefficients shown are the second stage estimates of the IV estimator, both with firm FE and without. All specifications control for industry-year dummies and country and firm 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.

Table B2: Time invariant share of years

	Contemporaneous				Lagged			
	Pooled OLS	2SLS	FE OLS	2SLS FE	Pooled OLS	2SLS	FE OLS	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF Participation	0.097*	0.357**	0.163***	0.202**	0.130**	2.302	0.151**	0.686**
	(1.96)	(2.02)	(2.95)	(2.56)	(2.43)	(0.92)	(2.50)	(2.18)
First stage:								
Instrument		-0.731**		-1.197***		-0.192		-0.631**
		(-2.25)		(-4.39)		(-0.86)		(-2.48)
Observations	57469	57469	6264	6264	57469	57469	6264	6264
R2	0.200	0.121	0.368	0.281	0.200	-0.411	0.364	0.172
Kleibergen Paap (p-value)		0.053		0.003		0.408		0.031
Panels			3132	3132			3132	3132
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*IMF probability* for the first stage in our IV models, where *IMF probability* is now the number of years over the sample that a country is under a program. 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.

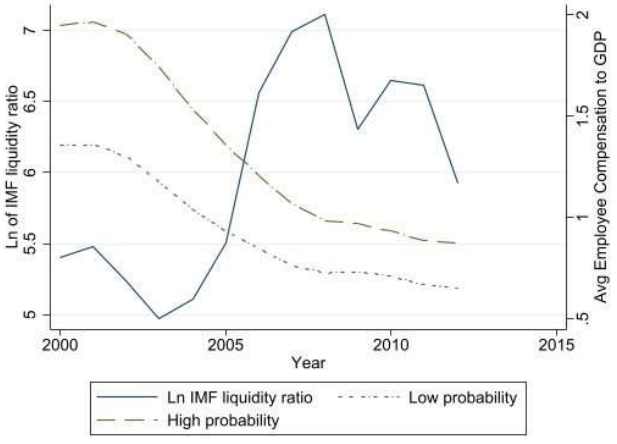
Figure B1: Pre-test parallel trends, average GDP growth as proxy for firm sales



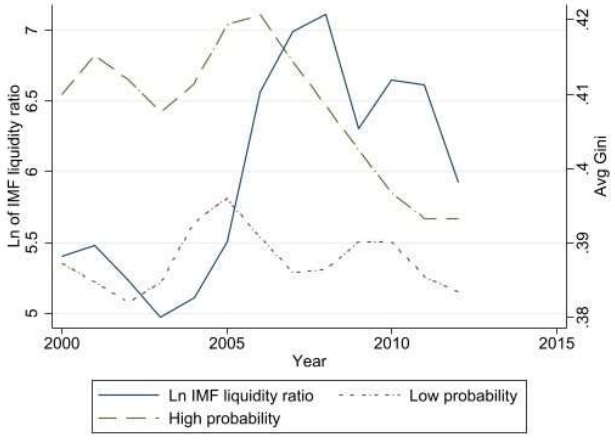
Notes: Pre-test parallel trends of firm sales growth and the exogenous component of our IV. Plot of the log of *IMF liquidity* over time alongside the trend of GDP per capita growth over two sets of countries, those with a low share of years under an IMF program versus those with high share of years (*IMF probability*).

Figure B2: Pre-test parallel trends for labor income share

(a) Employee's compensation

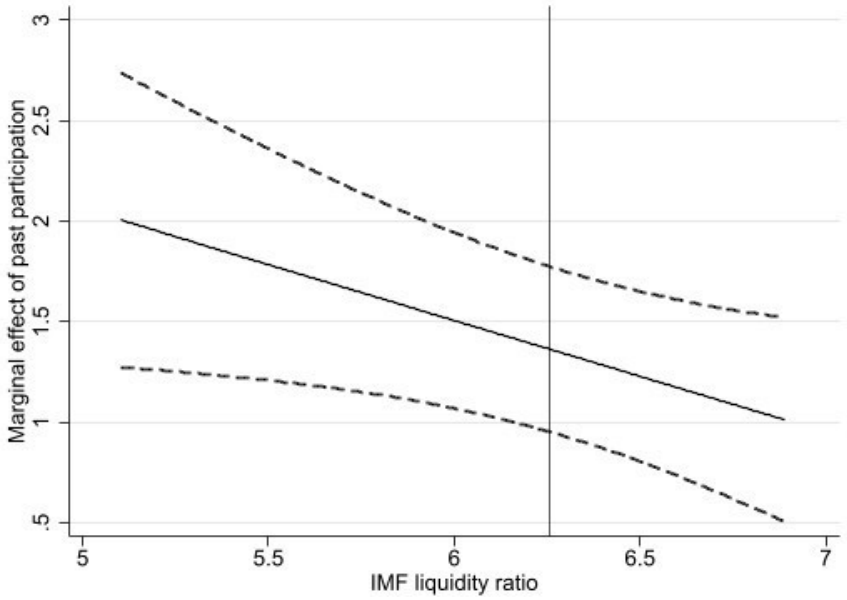


(b) Gini



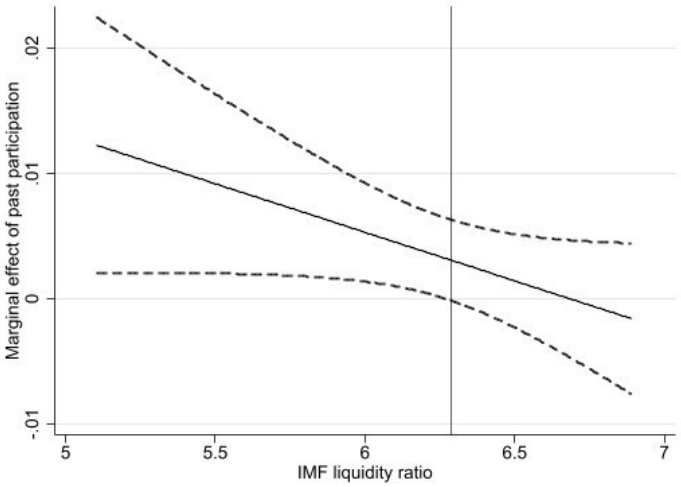
Notes: Pre-test parallel trends of the labor income share and the exogenous component of our IV. Plots the log of *IMF liquidity* over time alongside the trend of (a) employee's compensation, measured as the labor share of national income from the World Inequality Database, or (b) a Gini index from the Standardized World Income Inequality Database (2020), over two sets of countries, those with an average share of years under an IMF program versus those with low share of years (*IMF probability).*

Figure B3: First stage marginal effects on current participation



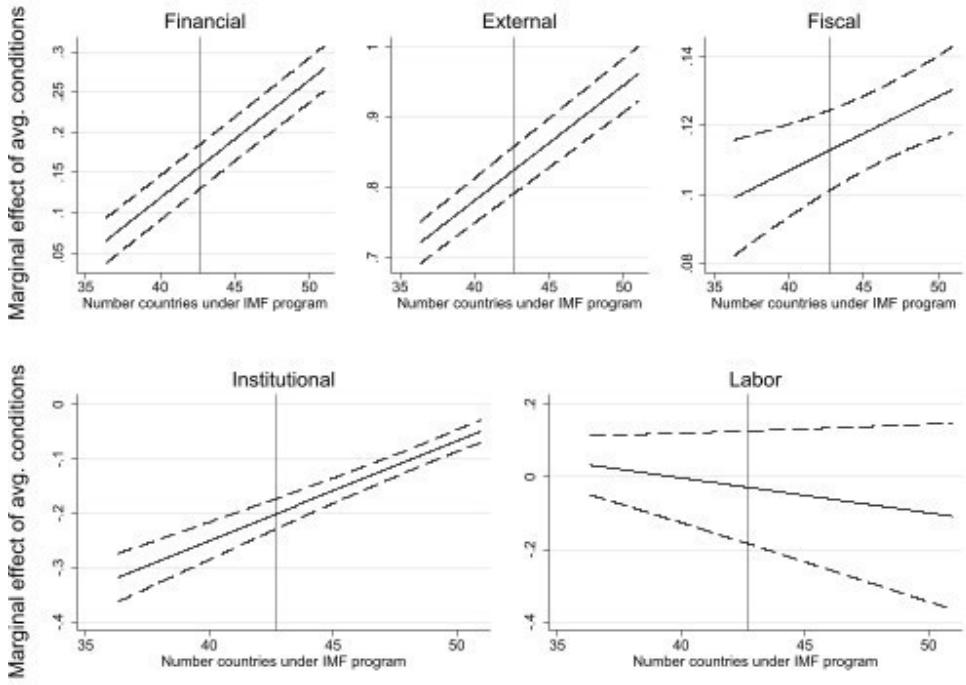
Notes: Marginal effects of share of past years under IMF program on current participation in IMF program for differing levels of IMF liquidity in a given year. Based on specification in Column 1 of Table 1. Dotted lines show 95% confidence interval.

Figure B4: First stage marginal effects on disbursements



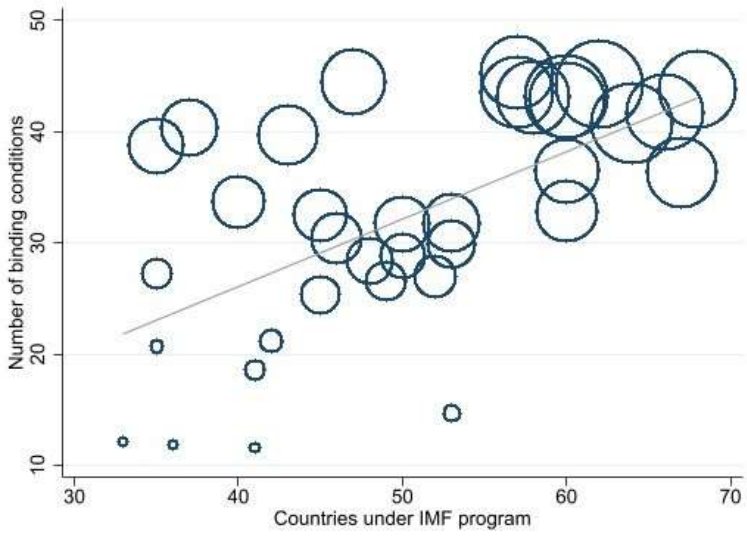
Notes: Marginal effects of share of past years under IMF program IMF disbursements for differing levels of IMF liquidity in a given year. Dotted lines show 95% confidence interval.

Figure B 5 First stage marginal effects on number of conditions by policy area



Notes: Marginal effects of average number of binding conditions per policy area p for country j on current number of conditions for a given number of countries under an IMF program in a given year. Dotted lines show 90% confidence interval.

Figure B6: IMF average yearly conditions imposed and countries under program



Notes: Plot of yearly average number of binding conditions imposed globally by the IMF for a given number of countries under an IMF program in that given year. Bubbles represent specific years, with size of bubbles accentuating the relationship between number of countries and number of conditions. Line of best fit shows that as IMF constraint becomes binding, i.e., there are more countries under a program, the number of conditions imposed increases.

Online Appendix C: Alternative specification

Table C1: IMF disbursements and firm sales growth, channels (control function approach)

	Finance Formal	Finance Informal	Overdraft	Financial Obstacles	Size	Trade Obstacles	Exporting firms
	(1)	(2)	(3)	(5)	(6)	(7)	(8)
IMF x Channel	0.040***	-0.050**	0.049***	-0.008**	-0.003*	0.006	-0.006
	(2.96)	(-2.24)	(9.75)	(-2.42)	(-1.83)	(0.76)	(-0.25)
Channel	-0.018	0.042**	-0.084***	-0.006**	0.008*	0.190***	0.054***
	(-1.60)	(2.68)	(-8.28)	(-2.03)	(1.77)	(18.85)	(3.32)
Disbursements	-0.985	-0.94	-1.054	-1.462***	-0.954	1.426	-0.933
	(-1.38)	(-1.32)	(-1.55)	(-9.00)	(-1.33)	(0.7)	(-1.31)
1st Stage Residuals	0.651	0.635	0.712	0.809***	0.662	-1.746	0.623
	(0.91)	(0.89)	(1.05)	(5.13)	(0.92)	(-0.87)	(0.87)
Observations	19853	19853	19556	19457	18645	19853	19853
R2	0.246	0.246	0.246	0.256	0.239	0.245	0.245
Controls	YES	YES	YES	YES	YE	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of IMF disbursements on average firm sales growth, correcting for endogeneity bias with control function approach (Woolridge, 2015). Regressions are run on subsample of countries under an IMF program using an OLS estimator. Columns are sorted according to channel considered, through interactions with firm-specific characteristics. All specifications include first stage residuals, 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.

Table C2: Number of conditions and firm sales growth, channels (control function approach)

	Financial			External			Fiscal			Institutional			Labor		
	Overdeaft	Fin. Obst.	Size	Trade Obst.	Export	Size	Tax Obst	Admin. Obst	Size	(Lack of) Pol. Inst.	(Lack of) Corruption	Size	Labor Obst.	(Lack of) Pol. Inst.	Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Conditions x Channel	0.0003	-0.0001	0.002*	-0.004*	0.004	0.007	-0.0001	-0.0001	0.0008	0.0007	0.001	-0.0002	-0.0008	0.002**	-0.003
	(0.25)	(-0.27)	(1.84)	(-1.96)	(0.46)	(1.29)	(-0.31)	(-0.60)	(1.01)	(0.74)	(0.77)	(-0.08)	(-0.49)	(2.13)	(-0.95)
Channel	-0.067***	-0.009*	0.167***	0.015**	0.043*	0.177***	0.004	0.004	0.185***	-0.009**	-0.008**	0.19***	0.006	-0.009***	0.207***
	(-4.17)	(-1.70)	(10.30)	(2.47)	(1.98)	(13.70)	(0.58)	(1.29)	(15.39)	(-2.45)	(-2.13)	(18.81)	(1.27)	(-2.76)	(18.12)
Conditions	-0.009	0.010	-0.009	0.037	-0.014	-0.223	0.013	0.014	0.019	-0.105	-0.100	-0.230	-1.034***	-1.188***	-0.971***
	(-0.52)	(0.59)	(-0.37)	(0.16)	(-0.06)	(-0.27)	(0.48)	(0.53)	(0.86)	(-0.85)	(-0.81)	(-1.35)	(-9.99)	(-12.83)	(-9.96)
1st Stage Residuals	0.009	-0.021	0.006	0.041	0.082	0.281	0.032	0.031	0.023	-0.026	-0.033	0.115	0.994***	1.118***	0.936***
	(0.44)	(-1.02)	(0.20)	(0.17)	(0.35)	(0.33)	(1.39)	(1.34)	(1.30)	(-0.15)	(-0.19)	(0.60)	(9.21)	(14.20)	(9.14)
Observations	19556	19457	19853	18645	19853	19853	19632	19814	19853	14837	14846	19853	19630	14837	19853
R2	0.241	0.245	0.241	0.236	0.242	0.242	0.244	0.247	0.246	0.236	0.235	0.245	0.254	0.239	0.255
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: Differential effects of additional conditions per policy area on average firm sales growth, correcting for endogeneity bias with control function approach (Woolridge, 2015). Regressions are run on subsample of countries under an IMF program. Columns are sorted according to policy area considered, as well as the firm-specific characteristic considered for the interaction with number of conditions per policy area. All specifications use a standard OLS estimator including first stage residuals, with 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.

Table C3: Number of conditions and labor income share, channels

	Financial			External			Fiscal			Institutional			Labor		
	Overdeaft	Fin. Obst.	Size	Trade Obst.	Export	Size	Tax Obst	Admin. Obst	Size	(Lack of) Pol. Inst.	(Lack of) Corruption	Size	Labor Obst.	(Lack of) Pol. Inst.	Size
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Conditions x Channel	0.0004	0.0002	-0.0005	0.001	-0.0006	-0.001	0.0003	0.0002	-0.0003	-0.0006	0.001*	0.001	-0.002**	0.0003	-0.004*
	(0.44)	(0.72)	(-0.81)	(0.77)	(-0.09)	(-0.52)	(0.99)	(1.20)	(-0.32)	(-0.45)	(2.01)	(0.98)	(-2.11)	(0.23)	(-1.82)
Channel	0.002	-0.0004	0.057***	-0.005	0.017	0.054***	-0.004	-0.002	0.051***	0.003	-0.004	0.052***	0.009***	0.002	0.055***
	(0.11)	(-0.12)	(4.81)	(-1.13)	(1.59)	(5.05)	(-1.46)	(-1.04)	(4.87)	(0.65)	(-1.63)	(7.02)	(2.84)	(0.39)	(6.15)
Conditions	-0.022*	-0.023*	-0.029	0.224*	0.252*	0.905*	-0.021	-0.019	-0.014	0.100***	0.02***	-0.019	-0.246**	-0.018	-0.230**
	(-1.81)	(-1.97)	(-1.67)	(1.85)	(2.01)	(2.00)	(-1.48)	(-1.37)	(-1.38)	(2.85)	(2.89)	(-0.28)	(-2.12)	(-1.08)	(-2.09)
1st Stage Residuals	0.013	0.014	0.021	-0.231*	-0.254*	-0.905*	0.0194	0.0176	0.014	-0.181***	-0.183***	-0.101	0.255**	-0.057***	0.243**
	(1.20)	(1.20)	(1.23)	(-1.85)	(-1.97)	(-1.97)	(1.04)	(0.97)	(0.97)	(-4.67)	(-4.66)	(-1.45)	(2.09)	(-6.65)	(2.09)
Observations	11790	11753	11975	11212	11975	11975	11841	11943	11975	10086	10093	11975	11838	10086	11975
R2	0.118	0.116	0.117	0.115	0.115	0.115	0.116	0.115	0.115	0.120	0.120	0.122	0.117	0.120	0.116
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Notes: differential effects of additional conditions per policy area on LIS, correcting for endogeneity bias with control function approach (Woolridge, 2015). Regressions are run on subsample of countries under an IMF program. Columns are sorted according to policy area considered, as well as the firm-specific characteristic considered for the interaction with number of conditions per policy area. All specifications use a standard OLS estimator including first stage residuals, with 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.

Online Appendix D: Sample dependence

Tables D1: IMF participation and firm sales growth, by industries

	Chemicals & electronics		Food		Manufacturing		Retail		Services	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IMF Participation	-0.155*	0.148***	-0.132***	0.236	0.058	0.246*	0.153**	0.362***	0.110*	0.743**
	(-1.92)	(21.59)	(-3.32)	(0.94)	(1.24)	(1.72)	(2.36)	(3.24)	(1.77)	(2.22)
Observations	6110	6110	5301	5301	18898	18898	9393	9393	12012	12012
R2	0.188	0.136	0.195	0.124	0.219	0.144	0.217	0.129	0.209	0.0574
Kleibergen Paap (p-value)		0.066		0.076		0.038		0.0036		0.163
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

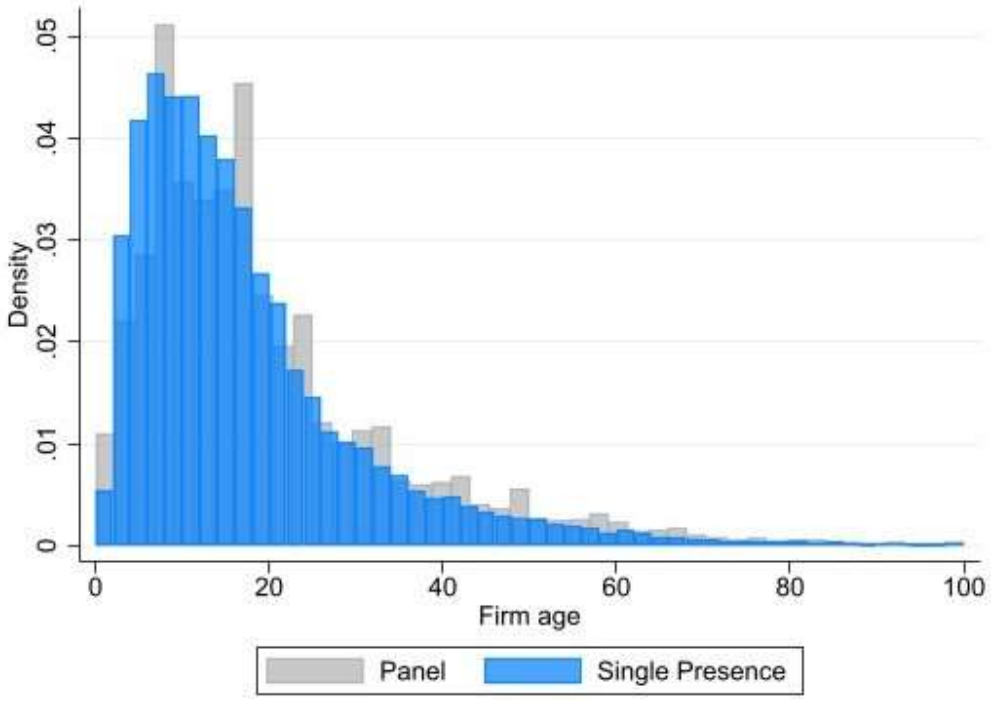
Notes: Effects of IMF participation on average firm sales growth, by industry. Aggregation of industries based on WBES stratification defined in Table A1. For each industry we show the estimators from a simple OLS and IV model. All models include firm and country level controls as well as country and industry-year dummies. 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.

Table D2: Randomization of firm

	OLS count	OLS (percent)	OLS (weighted)	FE count	FE (percent)	FE (weighted)
	(1)	(2)	(3)	(4)	(5)	(6)
Beta	0.363	0.298	0.365	0.359	0.263	0.365
SE	0.064	0.026	0.069	0.054	0.064	0.062
Percent insignificant	6%	0	4%	0	4%	0
Observations	9882	33397	9587	2702	1642	2582

Notes: Randomization strategies for firm sample in regressions. Beta represents the average second stage coefficient for *IMF participation* on firm sales growth for 100 regressions with random sampling (without replacement). Columns 1-3 show the results for a 2SLS estimator in a sample of pooled firms, whereas columns 4-6 show the results for a 2SLS estimator with firm fixed effects. Different columns represent different randomization strategies. Percent significant states the share of estimated coefficients in the simulations that were statistically insignificant with a p-value < 0.1. SE is the standard error of Beta over the 100 simulations.

Figure D1: Survivor bias: distribution of pooled v. FE sample



Notes: Distributions of firm age in full sample (excluding conflict countries) for firms which only appear in one wave of the survey (single presence) versus firms that are recontacted at least once over different waves.