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Flowing under the radar: micro evidence of official lending

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Foreword

This volume is the result of a research carried out at the Department of Economics, Management, and Statistics (DEMS) at the University of Milan-Bicocca and is submitted as a the doctoral thesis of the candidate Pietro Bomprezzi.

The author has been entirely free to conduct and present his research in the manner of his choosing as an expression of his own ideas.

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The BBQ crowd, for everything I have learned from you. The researcher I am today is in large part thanks to all of you.

My Anthi, for being there every step of the way, for keeping me grounded, for your unwavering support in every form. I can only say s'agapo toso poli.

Milan, 10/01/2024

Declarations

Chapter 1 of this thesis has been published as joint work with Silvia Marchesi in 2023 in the *Journal of International Money and Finance*. The paper was presented at a series of conferences and workshops including the Political Economy of International Organizations (PEIO) conference (2021), the conference of The European Public Choice Society (2021), and the German Development Economics Conference (2021).

Chapter 2 is derived from joint work with Silvia Marchesi and Rima Turk-Ariss of the International Monetary Fund, from which this paper started as part of a research visit. The joint work and previous versions of it have been presented at the internal seminar series of the IMF (2021), the conference of The European Public Choice Society (University of Minho, Braga, 2022), and the PEIO conference (UC San Diego, 2023). A previous version of this work has been circulated as an IMF Working Paper (22/157) and as a Center for European Studies (CefES) Working Paper (N. 520). The paper is a revise and resubmit at the *IMF Economic Review*.

Chapter 3 is derived from joint work with Silvia Marchesi and Tania Masi of the Università degli Studi "G. d'Annunzio" Chieti. It has been presented at Debtcon (EUI, Fiesole, 2022) and is published in the *IMF Economic Review* (2023).

The proposed version of Chapter 4 in this thesis is single-author work by the candidate and derives from his work with proprietary methods for geocoding bilateral aid projects. Versions of this work have been presented at the conference of The European Public Choice Society (University of Hannover, 2023), the conference of the Center for European Studies (Zurich, 2023) and the Milan PhD workshop (Milan 2023).

Contents

Introduction

If asked to identify the major players in international finance at the turn of the decade, one could be forgiven for giving attention to the usual suspects. Volatility in equity markets, with the dot-com bubble, subprime mortgages, and private creditors in sovereign debt markets European debt crises, were the main drivers of trends in international finance. The focus has therefore been heavily skewed towards private cross-border capital flows, such as foreign direct investments, portfolio flows, remittances, and in general private sector borrowing. This thesis argues that behind these commonly studied actors, *official finance*, including both concessional and non-concessional flows from multilateral and bilateral lenders, has been taking an increasing role on the global stage.

* * *

The underlying motivation in this work builds on a series of now well-established facts. First, as recently documented by Horn et al. (2020), official lending is much larger than commonly known, often surpassing total private cross-border capital flows, especially in times of global turmoil when private flows generally shrink.[1] While much attention has rightfully gone to the rise of bilateral lenders such as China, the lending of large multilateral institutions such as the IMF has remained of second-interest. This thesis provides new evidence on the role that such institutions can play in the global economy, both in terms of the effects of their lending on the real economy (Chapter 1 and 2) as well as on sovereign debt markets where other creditors are also operating (Chapter 3). That the behavior of official creditors is so often subject to political (and geopolitical) distortions makes for a compelling area of study. Work by Dreher et al. (2019) has highlighted mechanisms through which official finance (Chinese aid) can be captured by political distortions, begging the question if similar mechanisms are at play across other creditor groups (Chapter 4).[2]
From a practical perspective, much of the recent interest in the different players in international finance has been driven by the dissemination of new data, both for tracking

[1] Horn, Sebastian, Reinhart, Carmen M., and Christoph Trebesch. 2020. Coping With Disasters: Two Centuries of International Official Lending. NBER Working Paper No. 27343

[2] Dreher, Axel, Fuchs, Andreas, Hodler, Roland, Parks, Bradley C., Raschky, Paul A., and Michael J. Tierney. 2019. African leaders and the geography of China's foreign assistance. Journal of Development Economics.

global capital flows as well as for studying their effects. This thesis builds on both these trends. Through the use of firm-level data, this thesis first provide new insights into the mechanisms through which official lending from multilateral institutions can impact local economic activity. As the shift goes from multilateral to official bilateral creditors, the thesis presents new evidence of the importance of official creditors for financial markets in developing countries. Within these countries, development assistance from bilateral donors plays an important role, and the allocation of these capital flows is subject to various distortions.

* * *

This collection of essays is organized in the following manner. First, Chapter 1 revisits the question of the effectiveness of multilateral lending by studying the effects of IMF lending at the local level through the use of firm level data. Chapter 2 is a companion paper, diving deeper into the mechanisms through which IMF lending may impact the local economy. This paper uses detailed balance sheet data to test the presence of a signaling effect on firm investments following an IMF program.

Chapter 3 moves from multilateral to bilateral. This chapter examines the link between sovereign defaults and credit risk by distinguishing between commercial and official debt through the use of dissagreggated data on borrowing costs and market measures of risk.

Chapter 4 serves two purposes. First, it introduces a new, geocoded dataset of bilateral aid projects by OECD donor countries. Then, with the availability of this new data, it studies the distortions in aid allocation as measured through a regional favoritism channel, as in other work on Chinese or World Bank allocation, and by considering the role of the aid implementing agencies.

Chapter 1

A firm level approach on the effects of IMF programs

Pietro Bomprezzi*, Silvia Marchesi*

Abstract

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

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1.1. Introduction

If at one point the era of the IMF as the steward of global capital markets seemed over, the global financial crisis of 2008 and more recently the Covid-19 pandemic have given it a new raison d'être. In particular, adverse global shocks such as the Covid 19 crisis or recent geopolitical tensions have triggered an unprecedented withdrawal of non-resident portfolio flows from emerging markets, increasing the relative importance of official flows. As recently documented by Horn et al. (2020), official lending is much larger than commonly known, often surpassing total private cross-border capital flows, especially in times of global turmoil when private flows generally shrink. In the wake of the recent Covid-19 pandemic for example, about one hundred countries approached the IMF for short-term emergency assistance (around double the number that requested the Fund's assistance in the aftermath of the 2008 financial crisis).[1] This resurgence of official flows and IMF lending in particular is motivation to re-investigate the effectiveness of IMF programs. There is surprisingly little agreement on the direction and magnitude of the effects of IMF lending.[2] Of even greater significance is the lack of evidence pointing to specific channels through which IMF programs impact the real economy. The extensive dissemination of firm level data, however, now gives the opportunity to pick up on previously immeasurable mechanisms.

This paper takes a new approach to an old debate on the effects of IMF conditionality lending. Rather than drawing conclusions at the country level, we take advantage of micro-level data to explore the effects of IMF conditional lending on firm performance, considering growth in firm sales and subsequently how redistribution occurs within the firm. Our approach allows us to perform a more accurate assessment of the effect of IMF lending at the firm level by exploiting both program and firm heterogeneity to investigate effectiveness. We argue that leveraging such heterogeneity to understand the channels through which IMF programs affect the economy is important.

Providing intuitions on micro-level outcomes for IMF programs is relevant both for recipient country policymakers and Fund objectives. A substantial (both theoretical and empirical) literature centered around firm financial frictions has highlighted how shocks to the financial system can affect the real economy differentially based on the composition of the private sector. Recent work by Broner et al (2021) find that sovereign debt inflows have

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

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

heterogeneous stock-market effects on domestic firms.[3] Li and Su (2022) show how capital inflows can lead to financial crises through a shortening of corporate debt maturity structure. On the other hand, Fan and Kalemli-Őzcan (2016) show the role financial sector reforms have in shaping aggregate corporate savings, and specifically how the effect depends on financing behavior of individual firms. In this paper, we provide an extended, cross-country analysis of these firm-level effects of global capital inflows. The ability of the IMF to impact the financial sector through these large financial flows as well as its reform programs makes it uniquely important.

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

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

The scope of the paper, using the outlined methodology, is also to highlight channels and transmission mechanisms through which IMF conditional lending may affect the real economy. As described by Chauvet and Ehrhart (2018), there are two ways through which concessional financial flows may influence firm performance: demand (for example increased demand, financed by IMF loans, is met by firms' production), and supply (IMF loans may affect the productive capacity of firms).[5] More generally, the literature on firm performance points to three main kinds of constraints on firm growth in developing countries: the financing constraint (Beck et al. 2005; Harrison et al. 2004; Choudhary and

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

[4] For example, the availability of geocoded data has produced an emerging strand of literature evaluating aid-effectiveness at the subnational level (Bluhm et al. 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).

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

Limodio 2022; Fonseca and Matray 2021), lack of infrastructure (see among others Bluhm et al. 2020; Jedwab and Moradi 2016), and the institutional environment (e.g., Fisman and Svensson 2007).

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

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

Our primary hypothesis is one of a *signaling effect* of IMF programs to investors, as IMF lending could indicate to the international markets renewed confidence in the country. This eventually which translates into easier access to finance at the firm level, as credit conditions in the home country are strongly tied to the degree of financial distress the sovereign is experiencing. The transmission of sovereign risk operates largely through the balance sheets of banks, which especially affects firms with large financing needs.[7]

Moreover, restoring a country's creditworthiness, may enhance private capital inflows into the recipient countries, through a *catalytic finance* effect. This could be either directly, when companies access foreign capital markets themselves, or indirectly, when their banks rely on foreign financing. Hence by improving the credit stance of a recipient country, IMF intervention may alleviate the firms' financing constraints and eventually enhance their performance.

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

Our main identification strategy is based on an instrumental variable (IV) that combines temporal variation in the IMF's liquidity with cross-sectional variation in a country's prior probability of participating in an IMF program (see Gehring and Lang 2020, Lang 2021).

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

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

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

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

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

1.2. Effectiveness of IMF lending

This paper is related to several strands of literature. The first one broadly looks at IMF effectiveness by considering a wide range of dimensions related to an IMF intervention. While some studies find a positive (Bas and Stone 2014) or insignificant (Atoyan and

Conway 2006) relationship between IMF programs and growth, the majority of empirical studies suggest immediate negative effects (Barro and Lee 2005; Dreher 2006; Easterly 2005; Marchesi and Sirtori 2011; Przeworkski and Vreeland 2000). Other studies consider monetary stability, debt management, the containment of external arrears as key goals of IMF programs (Kentikelenis, Stubbs, and King 2016) and even distributional consequences or socio-political consequences of IMF programs (Casper 2017; Dreher and Gassebner 2012; Garuda 2000; Hartzell et al. 2010; Lang 2021; Oberdabernig 2013, and Vreeland 2002).[8] IMF programs have also been associated to reduced inflation and monetary growth, lower risk of currency crises and banking crises, and improved market performance of banks (Dreher and Walter 2010; Papi et al. 2015; Steinwand and Stone 2008).

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

Several contributions have considered in more detail the varied conditions attached to IMF financing, finding that conditions are a key mechanism linking IMF lending to policy outcomes.[9] For example, Reinsberg et al. (2018) and Forster et al. (2019) have focused their attention to structural conditions, Reinsberg et al. (2019) focused on labor conditionality, while Rickard and Caraway (2014, 2019) have focused on public sector conditions.[10] A recent report (IRC 2019) on the effectiveness of the IMF conditionality shows that over about the last ten years a tendency towards more structural conditionality and longer program implementation horizons has emerged and that in the aftermath of an IMF program all relevant macroeconomic variables tend to improve compared with the pre-program period.[11] In sum, the existing evidence suggests some positive adjustment effects regarding financial, fiscal and monetary positions, but the improvement has

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

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

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

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

generally fallen short of expectations, especially in terms of GDP growth and debt reduction.[12]

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

As we mainly focus on the alleviation of the firms' financing constraint as a channel of transmission of an IMF programs, this paper also relates to the literature that explain how a financial crisis (including a sovereign debt default) may propagate to firms (e.g., Corsetti et al. 2012; Gourinchas et al. 2017; Mendoza and Yue 2012).[13] A number of papers then look explicitly at financial crises and their effects on the financial sector.[14] Gennaioli et al. (2014) and (2018) show that lending by the banking sector can sharply decline in case of a sovereign default, especially if banks hold large amounts of sovereign bonds. In the specific context of developing countries, due to limited information, low collateral value, and a large informal sector, firms primarily produce soft information and are dependent on a banking system that promotes lending relationships (Fonseca and Matray 2021; Choudhary and Limodio 2022).[15] In turn, these domestic banks increase credit supply when capital inflows are higher (Baskaya et al. 2017; Schnabl 2012).[16]

Finally, this paper also broadly relates to a vast international finance literature that studies the effects of capital flows on firms, focusing on sovereign debt inflows, FDI, bank, and equity portfolio flows (e.g., Broner et al. 2021; Baskaya et al. 2017, and Schnabl 2012). In particular, the closest contribution to ours is the paper by Broner et al (2021), who show

on reforms. As a result, only 33 percent of all IMF programs between 1980 and 2015 were fully completed (Reinsberg et al. 2022a, 2022b).

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

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

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

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

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

that sovereign debt inflows reduce the domestic interest rate by raising the price of government debt, which benefits banks directly. In turn, banks expand domestic credit benefiting domestic firms (especially those which rely more on external financing).
In summary, we contribute to the existing literature by showing that the gain in financial creditworthiness induced by IMF intervention is passed on to firms operating in the borrowing countries, in turn leading to an increase in sales. We document that the financing channel is central for the firm to exploit the gain in creditworthiness which occurs at the macrofinancial level following an IMF program. Finally, using firm level data allows us to investigate more carefully on the channels of transmission of IMF intervention on the recipient countries' economies, an important aspect that has so far been neglected in this literature.

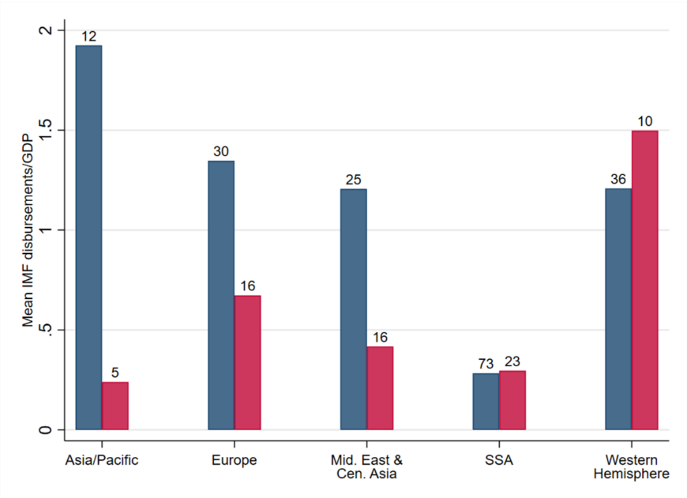
1.3. Data Description

1.3.1. IMF Intervention

Our primary variable of interest is IMF participation. Namely we consider a country to be under an IMF program for years where there are positive disbursements from the IMF to a member country, as reported by IMF Member Financial Data. Broadly, arrangements can be divided into concessional and non-concessional loans. Concessional loans are reserved for low-income countries and are those loans that carry very low interest rates (0–0.5 percent). Our sample period starts effectively in 2000, making the bulk of the programs considered Poverty Reduction and Growth Facility (PRFG), Extended Credit Facility (ECF) as concessional programs, and Stand-By Arrangements (SBA) or Extended Fund Facility (EFF).[17] It is important to note that since our IV is meant for selection into program but not into program type we do not identify the differential effects of concessional versus non-concessional programs. Section 4 explains our IV in greater detail.
In Figure 1 we plot these disbursements, alongside the number of IMF programs, for the different IMF regional departments, splitting our sample into the period 2000-2009 and the period 2010-2018. There has been a large relative drop in disbursements to Asia, driven by the East Asian financial crises at the turn of the millennia. In general, the size of interventions has diminished in last decade except in Sub-Saharan Africa and the Western Hemisphere, which includes Latin and South America. The number of programs signed per period also fell across regions. For our baseline model, the treatment is constructed at the country-year level. The result is an indicator variable for IMF participation. This

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

Figure 1: Average IMF disbursements and total programs by decade and regions



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

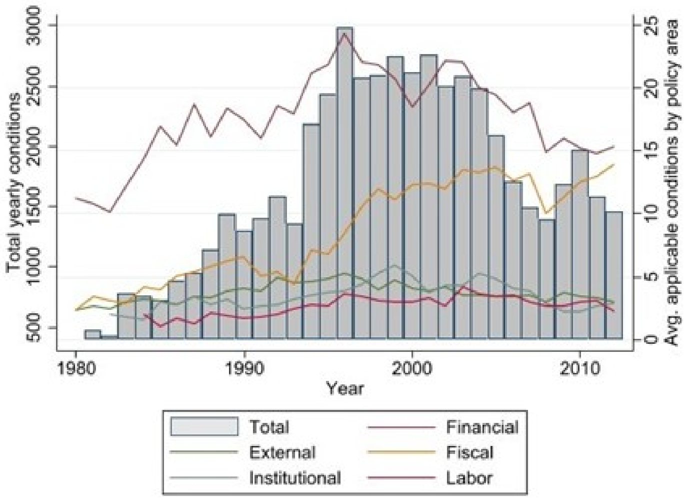
dichotomization is standard in the literature, and allows us to capture the effects of being under a program versus not being under a program.

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

We focus on the number of conditions as a measure of stringency of IMF programs. Kentikelenis et al. (2016) group conditions for each program into one of 13 mutually exclusive affected policy areas; fiscal reforms, revenues and tax reforms, financial sector and monetary policy reforms, state-owned enterprise reform, state-owned enterprise privatization, external debt reforms, trade and exchange systems reforms, public and private labor reforms, social policies, redistributive policies, institutional reforms, land and

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

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.

environmental reforms, and a residual category.[19] Figure 2 plots the yearly evolution of the average number of conditions per policy area imposed by the IMF. In the figure, we aggregate policy areas into five distinct groups in order to reduce the granularity of the data.[20]

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

1.3.2. 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

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

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

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

2003 and 2018, and provides information for approximately 130,000 unique firms over 4 iterations of the survey. One of the advantages of this updated version of the WBES is the availability of multiple questionnaire waves, which gives the possibility to track firms which participate in more than one wave. Within the survey, there are close to 15,000 firms which were recontacted at least once over the different iterations. Detailed information on the number of surveys per country and firms per survey can be found in Table A5, in the online Appendix A.

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

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

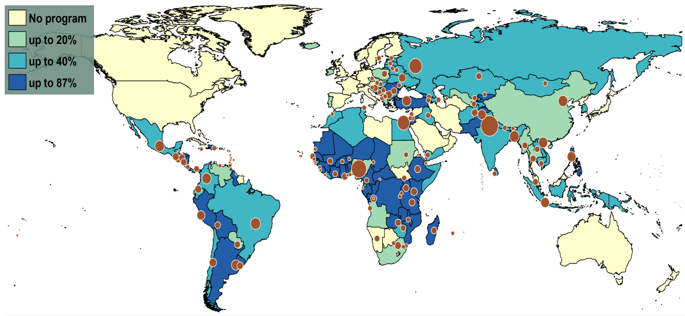
The final sample used in our analysis covers 135 developing and emerging market countries: 22 Asian, 52 African, 31 Latin America, and 30 Eastern European. We drop countries defined as being in conflict during survey years, since these countries tend to experience abnormally high growth rates in the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

We observe 38,870 unique firms in the whole of Africa, with about 18 percent of firms being recontacted. In Asia we instead observe 29,542 unique firms, of which 16 percent are recontacted, while Latin America has 28,688 unique firms which participated in the survey,

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

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

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.

but a larger percentage of these (33 percent) were successfully recontacted. Finally, across Eastern Europe we have data on 26,744 unique firms with 18 percent of them being recontacted. Within regions, the recontact rate varies by country, where smaller countries rarely participate in multiple waves of the survey.

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

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

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

$$LIS_{i,t} = \frac{Compensation\ of\ employee_{i,t}}{Total\ sales_{i,t}} \tag{1.1}$$

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

1.4. Endogenous Selection into IMF Programs

We want to test whether the presence of an IMF program in a given country may affect the growth of firm sales in that country. The fundamental methodological issue with this question is that selection into IMF programs is obviously not random. On the contrary, "treated" countries typically experience an economic crisis when entering into a program, which directly affects the performance of their firms. As a consequence, simple comparisons between treated and non-treated will not yield causal effects, but instead will capture the negative bias resulting from omitted variables and reverse causality. Following Gehring and Lang (2020) and Lang (2021), we take the Funds's liquidity ratio, defined as the share of liquid resources over liquid liabilities, as a proxy for the lenders budget constraint and interact it with the recipient-specific probability of receiving a loan from the IMF as an instrument for IMF intervention. This strategy follows a difference-in-differences logic and is similar to shift-share or Bartik instruments.[25] The IV equation is then the following:

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

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

$$IV_{j,t}^{IMF} = IMF\ liquidity\ ratio_t \text{ x } IMF\ probability_{j,t} \tag{1.2}$$

where *IMF probability* is the (time-varying) share of years between 1980 and 2018 that country *j* received an IMF loan, while IMF resources is the temporal variation of *IMF liquidity*, which is defined as the organization's liquid resources divided by its liquid liabilities. The data on IMF liquidity derives from the original dataset of Lang (2021), which we extend up to 2018. Figure B1, in the online Appendix B, plots the natural log of the liquidity ratio and its components. Liquid resources are composed primarily of usable currencies and SDR on the Funds' balance sheet, and at times include additional borrowings when complementary resources are raised to boost lending capacity. Liquid liabilities instead are the sum of reserve tranche positions and outstanding borrowing. We rely on IMF Annual Reports, published yearly in April, as well as the April update of the Fund Resource and Liquidity Position.[26]

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

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

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

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

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

1.4.1. Selection into conditionality

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

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

As more countries require assistance, Funds resources are stretched and therefore programs on average entail more conditions to balance demand with the available resources. On the other hand, the time-varying average number of conditions per policy area for a given country captures the government bargaining position with the IMF. More specifically, as shown by Dreher and Jensen (2007) and Dreher et al. (2009), countries receiving more

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

conditions by the Fund would have a lower bargaining power and hence would tend to obtain a greater number of conditions also in the future.[28] In other words, once in a program, the probability of having more conditions for countries that already received more conditions in the past is higher when Fund budget is tight, as the Fund needs to be more selective in its allocation of funds and more conditions are imposed for countries with a higher historical average (lower bargaining position).[29] The marginal effects displayed in Figure B5, in the online Appendix B, confirm the proposed mechanism, namely that the greater the number of countries under an IMF program per year, the greater the effect of past conditionality on the current number of conditions. Together, these two predict the variation in number of conditions per policy area.[30] Formally, we can write the instrument as:

$$IV^{IMF}_{j,p,t} = Countries\ under\ IMF\ program_t \text{ x } Average\ number\ of\ conditions_{j,p,t} \qquad (1.3)$$

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

1.5. Empirical strategy and Results

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

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

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

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

$$1^{st}Stage : IMF_{j,t} = \alpha_1 (IMFprobability_{j,t} * IMFliquidity_t) + \alpha_2 IMFprobability_{j,t} + \beta X_{i,k,j,t} + \nu_j + \tau_t + \varepsilon_{j,t} \tag{1.4}$$

$$2^{nd}Stage : g_{i,k,j,(t,t-2)} = \alpha\widehat{IMF_{j,t}} + \beta X_{i,k,j,t} + \gamma Z_{j,t} + \delta IMFprobability_{j,t} + \tau_{k,t} + \nu_{j/i} + \varepsilon_{i,k,j,t} \tag{1.5}$$

where g is our outcome variable for firm i, in industry k, and country j. $\widehat{IMF_{j,t}}$ is our variable of interest measuring IMF intervention. X is a set of time varying firm-level characteristics, while Z is a set of time varying country-level variables. We also add the time-varying share of years that country j was under an IMF program. This means that we control for the initial, pre-determined *IMF probability* in both stages while year fixed effects absorb the level effect of IMF liquidity. Hence, for identification we only need to assume the exogeneity of the interaction term conditional on its two constituent terms (as well as the fixed effects and the control vector X). We then include industry-year dummies $\tau_{k,t}$, in order to control for industry time-varying heterogeneity and $\varepsilon_{i,k,j,t}$ is the error term. We include either country or firm fixed effects according to the specification (country dummies when using a pooled model, otherwise firm fixed effects, and with standard errors clustered at the country level). Finally, to avoid extremely fast-growing firms driving the results, we exclude the top one percent of sales growth from the sample distribution.

Our main specification considers as the outcome variable the average firm sales growth, measured as the change in (log) firm sales between t and t-2.[31] We then control for a number of characteristics at the firm level, following the same specification of Chauvet and Ehrhart (2018). First we take *Sales*, in logarithm, measured at one lag with respect to the dependent. Firm *Size* takes the value one for firms with fewer than 20 employees, the value two for firms with between 20 and 100 employees, and three for firms with more than 100 employees. We also consider the characteristics of firm ownership using two variables, *State* and *Foreign*. *State* is a dummy variable which is equal to one when part of the firm is (partially) owned by the state, while *Foreign* is a dummy variable which is equal to one when part of (or all) the firm is owned by a foreign individual or company. Finally, we include information on whether the firm is outward looking using *Export*, which is a dummy variable equal to one when the firm exports part of or all its sales, either directly or indirectly (as a supplier to exporting firms). These firm-level characteristics are measured in year t since we do not have their pre-determined value at year t-2.

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

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

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

1.5.1. IMF participation

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

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

Table 1: IMF participation and firm sales growth

	Contemporaneous				Lagged			
	Pooled OLS	2SLS	FE OLS	2SLS FE	Pooled OLS	2SLS	FE OLS	2SLS FE
IMF Program	0.0437	0.297**	0.0780	0.267***	0.0581	1.083*	0.0380	0.717**
	(1.11)	(2.54)	(1.20)	(3.52)	(1.38)	(1.65)	(0.67)	(2.37)
ln(Sales(t-1))	-0.0857***	-0.0882***	-0.140***	-0.170***	-0.0858***	-0.0908***	-0.140***	-0.156***
	(-12.27)	(-10.53)	(-10.36)	(-8.69)	(-12.28)	(-11.89)	(-10.17)	(-10.88)
State	0.0229	0.0291*	0.0955*	0.147**	0.0227	0.0345**	0.1000*	0.132***
	(1.48)	(1.70)	(1.86)	(2.35)	(1.47)	(2.00)	(1.89)	(2.86)
Foreign	0.0570***	0.0603***	0.0459	0.0668**	0.0573***	0.0588***	0.0481	0.0630**
	(6.25)	(5.78)	(1.45)	(1.96)	(6.25)	(5.62)	(1.46)	(2.17)
Exports	0.0478***	0.0497***	0.0505***	0.0278	0.0480***	0.0478***	0.0502***	0.0608***
	(7.47)	(7.47)	(2.74)	(1.40)	(7.49)	(6.66)	(2.78)	(2.99)
Size	0.156***	0.159***	0.116***	0.120***	0.156***	0.163***	0.114***	0.126***
	(11.85)	(10.26)	(5.89)	(5.03)	(11.87)	(11.84)	(5.81)	(7.09)
(ln) GDP per Capita	-0.0142	-0.0319	0.241	0.231*	0.0209	0.431	0.272	0.555**
	(-0.10)	(-0.20)	(1.36)	(1.81)	(0.14)	(1.39)	(1.31)	(2.47)
GDP Growth	-0.00545	-0.00882	0.000684	-0.0102	-0.00799	-0.0612*	-0.000272	-0.0529*
	(-0.90)	(-1.04)	(0.12)	(-1.64)	(-1.19)	(-1.86)	(-0.03)	(-1.94)
Population	-0.0190	0.557	-0.161	0.919	0.0127	1.528	-0.102	1.503
	(-0.06)	(0.83)	(-0.29)	(1.24)	(0.04)	(1.08)	(-0.18)	(1.35)
Corruption	-0.0112	-0.165**	-0.0506	-0.137***	-0.00796	-0.295*	-0.0401	-0.287**
	(-0.30)	(-2.03)	(-1.14)	(-2.76)	(-0.25)	(-1.84)	(-1.09)	(-2.49)
Probability IMF program		-1.390*		0.199		-1.505		-1.153
		(-1.84)		(0.27)		(-1.33)		(-1.25)
First stage:								
IMF liquidity*Probability		-1.296***		-1.746***		-0.603*		-1.082***
		(-3.15)		(-5.53)		(-1.82)		(-2.94)
Observations	79666	66610	16902	6562	79666	75416	16902	9701
R2	0.182	0.119	0.279	0.267	0.182	0.00131	0.277	0.112
KleibergenPaap		0.0154		0.00172		0.111		0.0273
Panels			11080	3281			11080	4814
Controls	YES	YES	YES	YES	YES	YES	YES	YES
FirmFE	NO	NO	YES	YES	NO	NO	YES	YES
IndustryxYearFE	YES	YES	YES	YES	YES	YES	YES	YES
CountryFE	YES	YES	NO	NO	YES	YES	NO	NO

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Column 3 uses the within estimator with firm fixed effects. Column 4 uses an IV estimator with firm fixed effect. The coefficient Instrument in this case is the IV IMF liquidity x IMF probability for the first stage in our IV models. Columns 5-8 use the same estimators as in columns 1-4, but the variable of interest IMF participation is lagged by one period. All models include industry-year dummies and firm and country level controls. Kleibergen-Paap p-values are for the underidentification LM test. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p<0.01, **p<0.05, *p<0.1.

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.[33] Interestingly, the coefficient of *State* is positive but not always significant. Among the country-level controls, countries with greater *Corruption* experience lower firm sales growth, while the coefficients of both *Population* and *GDP Growth* are not significant. Both the coefficients of *GDP per capita* and *Sales* suggest a catching-up effect: countries with lower level of development and firms with lower initial sales tend to experience higher growth of sales.

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

Table 1 shows our baseline results. In columns 1 and 3, where we estimate a pooled OLS and a fixed-effects model without instrumenting, we do not find statistically significant results. Instead, when instrumenting for IMF participation, as in column 2, we find that sales increase by 26 percent for firms in countries under a Fund program and the coefficient is significant at the five percent level. When we control for firm fixed-effects we find comparable results; sales increase by about 24 percent when instrumenting for participation in a program (column 4), with the coefficient being significant at the one percent level. Similarly, we find evidence of positive, medium-term effects. In column 6, where we use an instrumented lagged value of IMF participation, we find a roughly 44 percent increase in average firm sales growth, with the coefficient being significant only at the ten percent level. Controlling for firm fixed-effects and instrumenting for participation in a program, we find that sales increase by about 38 percent (column 8), with the coefficient being significant at the one percent level. This result is in line with the previous literature, according to which the effects of IMF programs tend to be stronger when measured on longer horizons (see Balima and Sokolova 2021).

Our results would correspond to an average firm growth of about 3.6 percent (column 2) and 3.4 percent (column 4), which are comparable with the empirical evidence currently available in the literature regarding the effects of a country's IMF program participation on its output growth (e.g., Bas and Stone 2014; Binder and Bluhm 2017).[34] Such effects are notable, and we attribute them to a signaling effect of IMF participation. In a nutshell, the main intuition is that the adoption of an IMF program signals a country's "good intent" (as in Marchesi and Thomas 1999; Gehring and Lang 2020), which is then rewarded by

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

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

either some commercial debt restructuring (Marchesi 2003) or private capital inflows (e.g., Mody and Saravia 2006, Morris and Shin 2006; Krahnke 2020). In turn, such catalytic effect both improves the recipient's financial markets and gives sovereign borrowers some fiscal space. We should also emphasize that this effect can realize both at the firm level (e.g., as financially constrained firms are able to borrow more or more easily) as well as at the country level as domestic (and foreign operating on domestic soil) credit institutions become more able and more willing to lend. In the next section we provide some evidence on the channels through which IMF intervention is expected to affect firm performance.

1.5.2. Channels of transmission

In general, the literature points out that financial flows can have both demand and supply side effects on firms (e.g., Chauvet and Ehrhart 2018, Marchesi et al. 2021). On the demand side, the effects are theoretically ambiguous. On the one hand, IMF programs are expected to alleviate the government borrowing constraints, and hence increase the size of government budgets. This effect would be especially pronounced for firms which are large, state-owned, or operate almost exclusively in sectors directly affected by government expenditure. On the other hand, given its historical preference for austerity-oriented measures, it is hard to reconcile the IMF programs with a boost in government spending. From a supply-side perspective, we test empirically if the presence of an IMF program may have an impact on firm sales through some specific firm financial characteristic.[35] The reason we focus on this channel is related to our working hypothesis, namely that the Fund is expected to release the financial burden of firms operating in recipient countries, which would in turn lead to an increase in sales. Concretely, a reduction in financial frictions could occur through a *signaling effect.* This improves the balance sheets of domestic financial institutions holding sovereign bonds by reducing a country's sovereign risk, in turn spurring on increased lending. Moreover, such signaling effect, by restoring a country's creditworthiness, may enhance private capital inflows into the recipient countries, benefiting directly firms or financial institutions through *catalytic finance*. Considering both effects is important to our analysis. For example, in emerging economies, a typical catalytic finance mechanism is likely to be more relevant, given the importance of foreign capital flows in these countries. Instead, in low-income countries, given the importance of domestic bank lending to the financial system, the reduction of systemic risk has positive effects on financing for firms. Furthermore, in general more liquid capital markets also imply less problems with sourcing when the production process requires imports or inputs external to the firm.

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

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

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

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

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

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

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

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

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

In order to examine this heterogeneity, we re-estimate the baseline model presented in Equation 4, splitting the firms into two groups based on the observed value of the characteristic in question. This method, while coming at the costs of reducing the number of observation for each sub-sample, has the advantage of allowing us to use the same identification method over the different sub-sample. Regressions are run using an IV estimator with country fixed effects only.[41] The results are presented in Table 2, in which columns are sorted according to the channel (i.e., firm characteristic). Odd columns show the results obtained from the subsample of firms without the characteristics under consideration, while even columns consider the sub-sample of firms with that characteristic. In particular, we distinguish between firms relying on a degree of *External* finance which is below or above the industry median value (columns 1-2), firms requesting (bank) *Loan*s smaller or bigger than the median loan (columns 3-4), firms reporting lack or the presence of *Financial obstacles* (columns 5-6), firms that are smaller or bigger than the median *Size* (columns 7-8) and *Exporting* and non-exporting firms.[42]
We show the estimated 2SLS coefficient for IMF program for each sub-sample regression. What differs across the different specifications is the size of the coefficients, and hence the magnitude of the effect of IMF participation on firm performance through the different channels. We run a Chow test in order to test the equality (or difference) of our coefficients of interest over the two specifications, finding a significant difference between the coefficients in each column couple.[43]
We find that firms with their main financing for working capital coming from *External* sources experience relatively higher growth rates of sales after participating to an IMF program (columns 1-2 of Table 2). The positive effect on sales growth for firms in a program which rely relatively more on *External* financing is about 28 percent, compared with about 25 percent for those in the complementary sample. We then find that firms which have obtained bigger (mainly bank) *Loan* approvals see a relatively higher average growth rate of sales post IMF program (columns 3-4).[44] As previously discussed, the faster growth for those firms with bigger loans can be interpreted along the intensive margin, the effect on firms is increasing in the size of the credit opening from a bank. In particular, average firm sales growth is about 31 percent higher for firms with a loan higher then the median value.[45]
In columns 5-6, we find that firms reporting *Financial obstacles* experience faster growth relative to their counterparts. This evidence suggests that a program may be alleviating financial constraints, independently of the type of financing the firm relies on. Smaller

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

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

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

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

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

firms also grow more rapidly as opposed to larger ones (columns 7-8). In particular, sales increase by about 31 percent both for firms experiencing financial obstacles as well as smaller firms, while they increase by about 20 percent under the alternative circumstances. Finally, in columns 9-10, we find that *Exporting* firms benefit less from the IMF intervention, as the average effect is below the effect on non-exporting firms.
In summary, we find that firms relying on *External* finance are more exposed to the positive effects of an IMF intervention, as this is the channel through which an IMF program is likely to be transmitted. On the other hand, we see that firms which are more likely to experience some *Financial obstacles* are also positively affected. All this evidence then is in support of our main hypothesis that the main channel of transmission of an IMF program is through the alleviation of firm financial distress. In the next section we will focus on the specific role of the IMF conditionality.

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

	W/o external finance	External finance	Small loan	Big loan	W/o fin. obst.	Fin. obst.	Small firm	Big firm	Non-exporter	Exporter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
IMF participation	0.247*	0.276**	0.083	0.314*	0.203*	0.307**	0.308*	0.199**	0.319*	0.195**
	(1.83)	(2.06)	(1.33)	(1.98)	(1.82)	(2.15)	(1.86)	(2.12)	(1.84)	(2.29)
Difference test (p-value)	0.00		0.00		0.00		0.00		0.00	
Observations	37224	39073	12201	12459	45666	29102	50232	27292	60270	17253
R2	0.052	0.041	0.082	0.053	0.046	0.037	0.028	0.025	0.026	0.053
Kleibergen Paap (p-value)	0.063	0.013	0.002	0.028	0.028	0.041	0.053	0.017	0.056	0.006
Firm FE	NO	NO	NO	NO	NO	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

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

1.5.3. Conditionality

As a measure of the extent of IMF intervention, in this section we consider the number and scope of conditions. More specifically, as previously described in Section 3, we consider both the total number of binding conditions as a broad proxy for severity of an IMF program as well as financial conditions, because of its direct relation with our channels of interest. We therefore estimate the impact of an additional condition on firm sales growth. In order to comment on the causal effects of the number of IMF conditions, one has to take into account the endogeneity of conditionality. Our baseline identification strategy, however, works for selection into an IMF program but not for selection into the number and type of conditions. For this reason, we run our regressions on the sample of countries already under an IMF program, in order to determine the degree of intrusiveness of the IMF on the recipient countries. We then apply the same type of shift-share style instruments described in Section 4.1. Specifically, following Forster et al. (2019), we take the interaction between the number of countries under an IMF program in a given year (a proxy for how tight would be the Funds budget constraint) and the average number of conditions implemented by the Fund in the country. Once a country is in a program, the number of applicable conditions is increasing with the number of countries under a Fund program, but differentially based on the bargaining power of countries (see Section 4.1). The impact of IMF conditionality on firm performance is given by the following model:

$$g_{i,k,j,(t,t-2)} = \alpha_2 + \beta_2 X_{i,k,j,t} + \gamma_2 F_{j,t} + \lambda N_{j,t} + \tau_{k,t} + \nu_i + {}_{i,k,j,t} \tag{1.6}$$

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

Table 3 shows our estimates for an increase in conditionality on firm sales. The panel on the left shows the results of the short term analysis, while the panel on the right presents the results up to the medium term. For either financial or total, we show the coefficients of a fixed-effects model when not correcting for the endogeneity bias as well as the instrumented

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

coefficient.[47] The coefficients of our variables of interest, in the IV specification, generally show the adverse effects of increasingly severe IMF programs. For example, an additional condition in the Financial policy area leads to an 8 percent drop in average firm sales growth. In the medium-term, the effect becomes positive when considering all conditions but remains negative yet insignificant for financial conditionality.[48]

Table 3: Number of conditions and firm sales growth

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

Notes: Differential effects of IMF conditionality on average firm sales growth. Regressions are run on subsample of countries under an IMF program using an OLS estimator. Columns are sorted according to the policy area reform. All specifications include industry-year dummies, firm fixed effects, and firm and country level controls. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p<0.01, **p<0.05, *p<0.1.

Finally, as in the previous section, we focus on the financing channel. To that end, in Table 4, we consider only financial conditions, and we look at the differential effects of the firm characteristics outlined in Section 5.2. The main difference with respect to the results obtained in Table 2 is that we now consider only countries under a Fund program.[49] Due to the reduced number of observations in the treated sample, we are unable to identify the effects of financial conditions in all sub-samples. The Kleibergen Paap tests provide evidence in support of identification only when considering *External finance* (columns 1-2) and *Financial obstacles* (columns 5-6). We find that financial conditions have a positive effect on the performance of those firms that in a given industry rely more on *External* finance. We also find that firms with *Financial obstacles* see their performance decline

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

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

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

(slightly) more with financial conditionality than firms declaring no obstacles.[50] Hence, a more intrusive financial conditionality seems more burdensome for firms that are financially constrained.

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

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

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

	W/o external finance	External finance	Small loan	Big loan	W/o fin. obst.	Fin. obst	Small firm	Big firm	Non-exporter	Exporter
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Financial conditions	-0.066***	0.081***	-0.035	-0.017	-0.044***	-0.053***	-0.073**	-0.064***	-0.074**	-0.149
	(-2.90)	(25.47)	(-1.31)	(-1.04)	(-3.44)	(-2.78)	(-2.08)	(-2.70)	(-2.52)	(-1.60)
Difference (p-value):	0.00		0.00		0.00		0.00		0.00	
Observations	10551	9037	2796	3003	10554	8903	13490	6363	15348	4504
R2	0.131	0.157	0.228	0.174	0.140	0.137	0.0891	0.110	0.109	0.0994
Kleibergen Paap (p-value)	0.092	0.055	0.273	0.109	0.085	0.083	0.139	0.146	0.105	0.188

Notes: Differential effects of IMF financial conditionality on average firm sales growth. Regressions are run using an IV estimator with country fixed effects. Columns are sorted according to the channel (i.e., firm characteristic). Specifically, different subsamples are obtained by splitting the data by whether the firm has the characteristic in question or not. Odd columns show the results for the subsample of firms without the characteristics under consideration, while even columns consider the sub-sample of firms with that characteristic. In particular, we distinguish between firms relying on External finance below or above the median value (columns 1-2), firms with a Loan smaller or bigger than the median loan (columns 3-4), firms reporting lack or the presence of Financial obstacles (columns 5-6), firms that are smaller or bigger than the median size (columns 7-8), Exporting and non-exporting firms. To test the equality (or difference) of our coefficients of interest over the two specifications, we run a simple Chow test. The Chow statistic is computed by running the same model on two subsamples, splitting the data by whether the firm has the characteristic in question or not, and on the full sample. The statistic is distributed F(k, N1+N2-2*k), with k degrees of freedom, and N1 and N2 observations on the subsamples. All specifications include industry-year dummies, country dummies, and firm and country level controls. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p<0.01, **p<0.05, *p<0.1.

1.6. Redistribution within the firm

Given the evidence of increased sales for firms following a program, a natural question at this stage would be to wonder how the increased sales are redistributed within the firm.[51] More specifically, we look at the share of sales going to the workers (or labor income share), as this information is available in the dataset. More generally, an increase in sales could be redistributed either to the workers, to the owners of the firms, or re-invested.
Unfortunately, we do not have data either on firm investments or on profits and we only have information on the compensation of employee, which explains why we test whether IMF participation, through an increase in firms' sales, may have an impact on the labor income share.[52]
We then re-estimate Equation 5 considering as our dependent variable the labor income share described in Section 3.2. As in the baseline specification, in columns 1-2 of Table 5 we estimate both a pooled OLS and IV considering the full sample of firms, while in columns 3-4 we use a fixed effects estimator. The first stage results show that the coefficient for our instrument are always negative and significant as expected.[53] The main result of Table 5 is that we find no effect on labor income share neither in the contemporaneous (columns 1-4) nor lagged specifications (columns 5-8). More specifically, IMF participation significantly reduces the labor income share only in the OLS specifications but not in the IV ones, and only in the contemporaneous setting.
This result could also depend on the circumstance that most salaries have little variable component and are fixed in the short term. Hence, if sales increase (for example after the Fund intervention) profits should also increase, while labor expenses remain unchanged at first. That might change when contracts are renegotiated, but this would then realistically take a while. However, since we find no evidence in support of this, we conclude that the increased sales may be used either to increase the stock of capital, or to increase the profits of the firms' owners.
Given the difficulty in measuring firm investment decisions in our survey data, we also consider the firm employment decisions, testing whether IMF participation may affect the growth of permanent employers at the firm level. The intuition is the following: if owners divert revenues away from redistributive goals in the short term for investment decisions, employment will most likely increase subsequently to match human capital to physical.[54]
Hence, by constructing an indicator of firm employment growth in the same way that firm sales growth is constructed, we should be able to measure the effect of IMF participation on firm employment growth (jobs). As shown in Table C1, in the online Appendix C, we

[51] For example, Vreeland (2002) focuses on redistributive effects in favor of the workers, while Lang (2021) considers the impact of IMF programs on inequality.
[52] Bomprezzi et al. (2022), in a different setting, actually find that IMF participation increases firms' investments.
[53] Kleibergen-Paap tests provide further evidence in support of identification.
[54] This is the reason why our measure of employment incorporates only full-time, permanent worker.

Table 5: IMF participation and labor income share

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

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Column 3 uses the within estimator with firm fixed effects. Column 4 uses an IV estimator with firm fixed effect. The coefficient Instrument is the IV IMF liquidity x IMF probability for the first stage in our IV models. Columns 5-8 use the same estimators as in columns 1-4, but the variable of interest IMF participation is lagged by one period. All models include industry-year dummies and firm and country level controls. Kleibergen-Paap p-values are for the underidentification LM test. Standard errors are clustered at the country level. t-statistics in parenthesis, ***p<0.01, **p<0.05, *p<0.1.

can find that IMF programs do induce an increase in employment but only in the lagged specification and only using a fixed effects estimator.
To sum up, the increase in firm sales we observe after a country participates in an IMF program seem to have no effect on the workers' compensation, but is found, at least to some extent, to increase the number of permanent workers in the medium-term.

1.7. Robustness

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

1.7.1. Identification

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

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

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

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

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

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

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

1.7.2. Sample dependence

An equally important issue to address is the sensitivity of results to the composition of the sample. While our country sample is vast and therefore unlikely that a given country is driving the results, issues of sample dependence could arise from the firm sample within countries. The stratified random sampling methodology for the WBES explained in Section 3.2, at least theoretically, guarantees that the patterns for firm sales growth are not being driven by a particular set of firms more exposed to IMF lending.[59]
Table C2 in online Appendix C tackles these issues of sample dependence in the firm dimension more rigorously. For example, in columns 3 and 4, we show that results are robust when limiting the sample to only domestic firms. Hypothetically, results could be driven by foreign firms operating in a given country, which are typically larger and more sophisticated than solely domestic ones. However, we find no evidence for such results. Similarly, we show in columns 5-8 that firm characteristics (*Size* and *Export*) closely tied to firm performance are not endogenous to program participation. Finally, we also show that the results are robust to a model with no firm controls. In a similar vein, in columns 9-12 we also show that IMF participation has no effect on each of the different firm characteristics that we use as "channels" of transmission of IMF intervention.[60] Table C3 instead shows the effects of IMF participation on firm sales growth when the sample is split along four broad industry groups. The results show that effects are strongest in the *Manufacturing*

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

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

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

sector, which is more represented in the sample, and in the *Retail* sector. We find no effects in *Services*, where the identification in the first stage is not significant at conventional levels, or in the *Food* sector.[61]
Another limitation to survey data is the problem of recontacting firms. Beside promising best practices and efforts to create a panel structure in their survey, the WBES can provide no guarantee that firms which can be recontacted will be. And there is no way to know why some firms don't appear in future waves of the survey. The biggest limitation which would affect our results on firm sales growth is firms dropping out because they go bust, what we call the survivor bias. If this were the case however, we would expect that the distribution of firms with repeated interviews versus the distribution of single-presence (no repeated interviews) firms would be significantly different. Figure C1 in the Appendix shows that the two distributions are rather similar.
To test formally the robustness of our results to firm sample dependence, as a final step we run a randomization of the sample of firms per country. We consider different strategies of randomization, where each one has unique implications on the final sample. We begin by considering the simplest case of random sampling without replacement of 200 observations per country.[62] As a second test, we randomly draw without replacement a share (50 percent) of observations per country. A final more sophisticated method is to weight each country in the sample by its economic size, and randomly sample without replacement a number of observations proportional to this weight. For each method we run 100 simulations and compute the average of the estimated (second stage) coefficient alongside the standard error and the percent of simulations where the coefficient is insignificant.[63]
We then apply the same methods to a panel sample of firms. In this case, we consider 70 randomly drawn (without replacement) unique firms per country and their corresponding recontacts, if such recontact occurred in later waves of the survey.[64] We run 100 simulations and find the average coefficient using a panel model with firm fixed effects. All results are reported in Table C4, in the online Appendix C; they are consistent across all the methods and we find an average effect very similar to our baseline results.

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

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

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

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

1.8. Conclusions

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

We argue that IMF intervention could signal to international markets renewed confidence in the country, which in turn translates into easier access to finance at the firm level. Our results show a positive impact of participating in an IMF program on firms' sales growth, and the effect is persistent through time. Controlling for firm fixed effects, sales can be up to 24 percent higher for firms in countries benefiting from IMF lending. More specifically, we find that IMF intervention is associated to a greater increase in sales for firms relying relatively more on external finance or reporting more financial obstacles. These results suggest that the Fund improves firm performance by relaxing the financial constraints faced by domestic firms in recipient countries. Furthermore, we find that the time dimension is an influences program effectiveness. While more severe conditionality worsens firm performance in the short run, the effect turns beneficial in the medium-term. Our findings shed light on the channels through which IMF programs affect domestic firms. A related question is then whether IMF programs, as well as improving a country's creditworthiness for external investors, may also make "domestic " investors more willing to invest in the country, by reducing the degree of uncertainty over the recipient country's future economic prospects. We leave this question for future research.

Appendix

Appendix A: Descriptives

A1: Distribution of firms across industries

Industry	Observations	Percent
Chemicals	5975	4.37%
Electronics	1338	0.98%
Metals & Minerals	6757	4.95%
Food	10821	7.92%
Garments	10910	7.98%
Manufacturing	31812	23.28%
Retail	21807	15.96%
Services	33666	24.64%
Not reported	13535	9.91%
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`.

A2: Description of policy area reforms

Policy Area	Description
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 sector, trade and exchange systems	Trade liberalization, tariffs, quotas; Exchange system (foreign exchange rate regime, exchange rate policy); Capital account liberalization; FDI
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 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 policies, public and private	Wage, employment limits; Pensions

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

A3: Summary statistics

	Observations	Mean	Sd	Max	Min
Dependent variables					
Sales growth	102807	0.140	0.505	10.50	-8.531
Labor income share	59498	0.224	0.239	7	0.003
Employment growth	125017	0.058	0.211	3.719	-4.736
Firm variables					
Sales last fiscal year	123597	16.640	3.288	37.20	0
State owned	137154	0.017	0.130	1	0
Foreign owned	137108	0.105	0.307	1	0
Exporting	138119	0.221	0.415	1	0
Size	132786	1.718	0.764	3	1
External financing	135340	26.65	33.56	100	0
Loan size (log)	38449	14.89	3.44	36.1	0
Financial obstacles	133877	1.546	1.354	4	0
Country variables					
IMF participation	142454	0.330	0.470	1	0
Log per capita GDP	142194	7.893	1.082	12.086	4.765
GDP growth	141902	4.759	3.164	47.213	-19.282
Log population	142448	17.163	1.939	21.044	9.144
Corruption index	125307	2.208	0.658	6	0

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

A4: Variable Description

	Description	Source
FIRM		
Sales growth	Average annual growth rate of sales, percent	World Bank Enterprise Survey (2018)
Labor Income Share	Share of employee compensation over total sales	World Bank Enterprise Survey (2018)
Jobs	Average annual growth rate of permanent full-time employees, percent	World Bank Enterprise Survey (2018)
Log Sales (base year)	Establishment Sales 3 Years Ago, in log	World Bank Enterprise Survey (2018)
State	Dummy ==1 if state ownership > 0	World Bank Enterprise Survey (2018)
Foreign	Dummy=1 if owned by private foreign individuals, companies or organizations	World Bank Enterprise Survey (2018)
Export	Dummy=1 if sales from indirect exports > 0	World Bank Enterprise Survey (2018)
Size	Firm category Based On No. Of employees: 1 Small (< 20), 2 Medium (20-99), 3 Large (> 100)	World Bank Enterprise Survey (2018)
External finance	Dummy==1 if main financing from external channels (banks, suppliers' credit, other fin. inst.)	World Bank Enterprise Survey (2018)
Loan size	Size of the loan from commercial banks, state owned banks, or other financial institutions	World Bank Enterprise Survey (2018)
Firm has financial obstacles	How much an obstacle is access to finance, categorical	World Bank Enterprise Survey (2018)
COUNTRY		
GDP Growth	GDP (constant 2015 US$), Annual rate of change	WDI, World Bank (2019)
GDP per capita (log)	GDP (constant 2015 US$), per capita (in log)	WDI, World Bank (2019)
Population (log)	Log of total population	WDI, World Bank (2019)
Corruption	International Country Risk Guide - Corruption	ICRG PRS Group (2019)
Policy area	Number of applicable conditions per policy area	Kentikelenis et. al (2016)
IMF participation	Dummy ==1 if country under program	IMF financial data

A5: Survey Sample Description

African and Middle East			Eastern Europe and Central Asia			Asia			Latin America and the Caribbean		
Country	**Years**	**Obs.**	**Country**	**Years**	**Obs.**	**Country**	**Years**	**Obs.**	**Country**	**Years**	**Obs.**
Angola	2006, 2010	785	Albania	2007, 2009, 2013	839	Bangladesh	2011, 2013	1692	Antigua & Barbuda	2010	151
Benin	2004, 2016	497	Armenia	2009, 2013	734	Bhutan	2009, 2015	503	Argentina	2006, 2010, 2017	3108
Botswana	2006, 2010	610	Azerbaijan	2009, 2013	770	Cambodia	2013, 2016	845	Bahamas	2010	150
Burkina Faso	2006, 2009	533	Belarus	2008, 2013	633	China	2012	2700	Barbados	2010	150
Burundi	2014	157	Bosnia & Herz.	2009, 2013	721	Fiji	2009	164	Belize	2010	150
Cabo Verde	2006, 2009	254	Bulgaria	2007, 2009, 2013	1596	India	2014	9281	Bolivia	2006, 2010, 2017	1339
Cameroon	2006, 2009, 2016	931	Croatia	2007, 2009, 2013	1152	Indonesia	2009, 2015	2764	Brazil	2003, 2009	3444
Central African Rep	2011	150	Czech Republic	2009, 2013	504	Lao PDR	2009, 2012, 2016	1107	Chile	2006, 2010	2050
Chad	2009, 2018	303	Estonia	2009, 2013	546	Malaysia	2015	1000	Colombia	2010, 2017	1935
Congo, Rep	2009	151	Georgia	2008, 2013	733	Micronesia	2009	68	Costa Rica	2010	538
Cote d'Ivoire	2009, 2016	887	Hungary	2009, 2013	601	Mongolia	2009, 2013	722	Dominica	2010	150
Dem. Rep. Congo	200,620,102,013	1228	Kazakhstan	2009, 2013	1144	Mynamar	2014, 2016	1239	Dominican Rep	2010, 2016	719
Djibouti	2013	266	Kosovo	2009, 2013	472	Nepal	2013	482	Ecuador	2003, 2006, 2010, 2017	1838
Egypt, Arab Rep	2013, 2016	4711	Kyrgyz Rep	2009, 2013	505	Pakistan	2007, 2013	2182	El Salvador	2006, 2010, 2016	1772
Eritrea	2009	179	Latvia	2009, 2013	607	Papua New Guinea	2015	65	Grenada	2010	153
Eswatini	2006, 2016	457	Lithuania	2009, 2013	546	Samoa	2009	109	Guatemala	2006, 2010, 2017	1457
Ethiopia	2011, 2015	1492	Moldova	2009, 2013	723	Solomon Islands	2015	151	Guyana	2010	165
Gabon	2009	179	Montenegro	20,092,013	266	Thailand	2016	1000	Honduras	2003, 2006, 2010, 2016	1578
Gambia	2006, 2018	325	North Macedonia	20,092,013	726	Timor-Leste	2009, 2015	276	Jamaica	2010	376
Ghana	2007, 2013	1214	Poland	2009, 2013	997	Tonga	2009	150	Mexico	2006, 2010	2960
Guinea	2006, 2016	373	Romania	2009, 2013	1081	Vanuatu	2009	128	Nicaragua	2003, 2006, 2010, 2016	1599
Guinea-Bissau	2006	159	Russian Federation	2009, 2012	5224	Vietnam	2005, 2009, 2015	3199	Panama	2006, 2009	969
Jordan	2013	573	Serbia	2009, 2013	748						
Kenya	2007, 2013	1438	Slovak Republic	2009, 2013	543						
Lebanon	2013	561	Slovenia	2009, 2013	546						
Lesotho	2009, 2016	301	Sweden	2014	600						
Liberia	2009, 2017	301	Tajikistan	2008, 2013	719						
Madagascar	2009, 2013	977	Turkey	2008, 2013	2496						
Malawi	2009, 2014	673	Ukraine	2008, 2013	1853						
Mali	2003, 2007, 2010, 2016	1190	Uzbekistan	2008, 2013	756						
Mauritania	2006, 2014	387									
Mauritius	2009	398									
Morocco	2013	407									
Mozambique	2007	479									
Namibia	2005, 2014	909									
Niger	2005, 2009, 2017	426									
Nigeria	2007, 2009	5048									
Rwanda	2006, 2011	453									
Senegal	2007, 2014	1107									
Sierra Leone	2009, 2017	302									
South Africa	2003, 2007	1540									
South Sudan	2014	738									
Sudan	2014	662									
Tanzania	2006, 2013	1232									
Togo	2009, 2016	305									
Tunisia	2013	592									
Uganda	2013	762									
West Banks and Gaza	2013	434									
Yemen, Rep.	2010, 2013	830									
Zambia	2007, 2013	1204									
Zimbabwe	2011, 2016	1199									

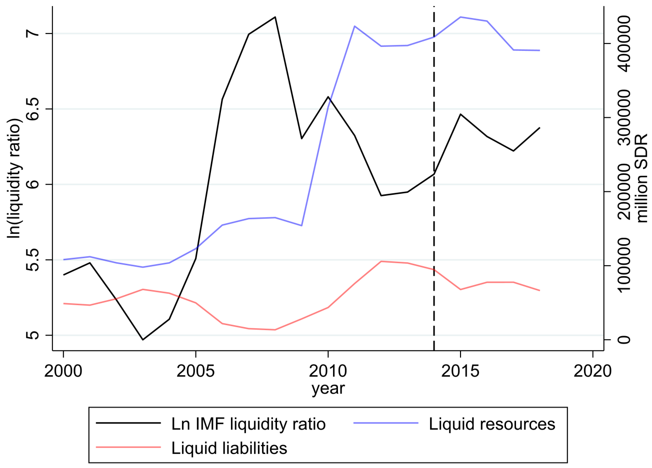
Appendix B: Identification

B1: Time-varying heterogeneous trends

	(1)	(2)	(3)	(4)	(5)
IMF Participation	0.265**	0.338*	0.368*	0.128	-0.010
	(2.00)	(1.89)	(1.74)	(1.34)	(-0.08)
Bank Crises × IMF Probability	-0.048		-0.096		
	(-0.74)		(-0.93)		
Currency Crises × IMF Probability		0.079		0.102	
		(1.03)		(1.06)	
Observations	77385	77385	77385	77385	77385
R^2	0.039	0.018	0.012	0.063	0.069
Kleibergen-Paap (p-value)	0.037	0.050	0.068	0.123	0.119
Firm FE	NO	NO	NO	NO	NO
Country FE	YES	YES	YES	YES	YES
Industry × Year FE	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES

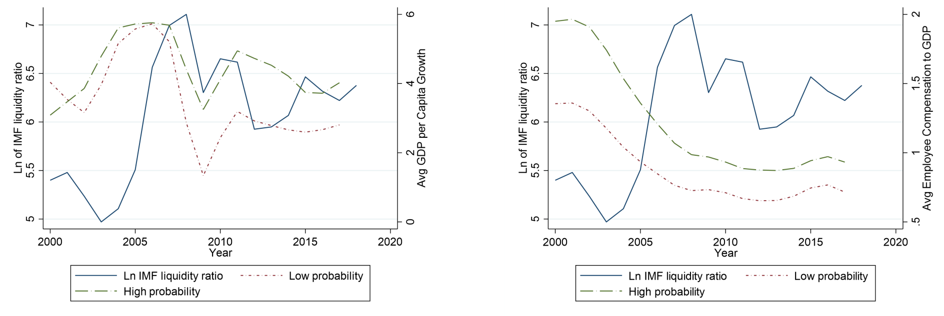
Notes: Effect of IMF participation on average firm sales growth when controlling for heterogeneous global trends. Columns 1 and 2 control for the global number of Bank crises while columns 3 and 4 control for global Currency crises. Columns 5 and 6 control for both simultaneously. Coefficients shown 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.

Figure B1: IMF liquidity data



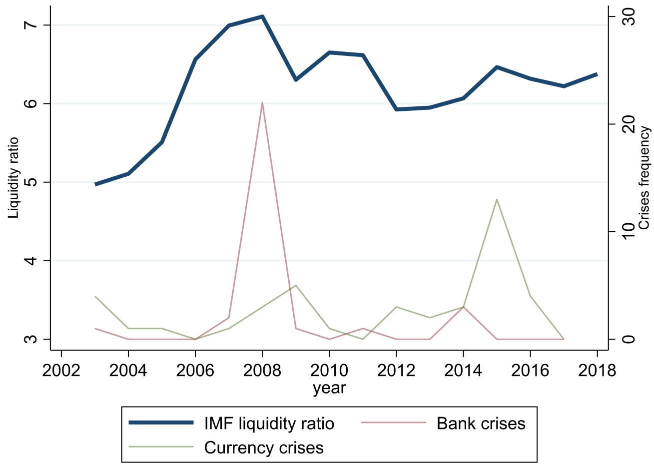
Notes: Figure shows the evolution over the study period of the IMF liquidity ratio alongside its components, IMF liquid resources and liabilities. Liquid resources are composed primarily of usable currencies and SDR on the Funds' balance sheet, and at times include additional borrowings when complementary resources are raised to boost lending capacity. Liquid liabilities instead are the sum of the reserve tranche positions and outstanding borrowing. Data is from the IMF Annual Reports, published in April of every year, as well as the Fund Resource and Liquidity positions. Data from the latter source is also taken in the April update. Data before 2014 (black dotted line) is original Lang (2020) data, while after are author calculations.

Figure B2: Pre-test parallel trends



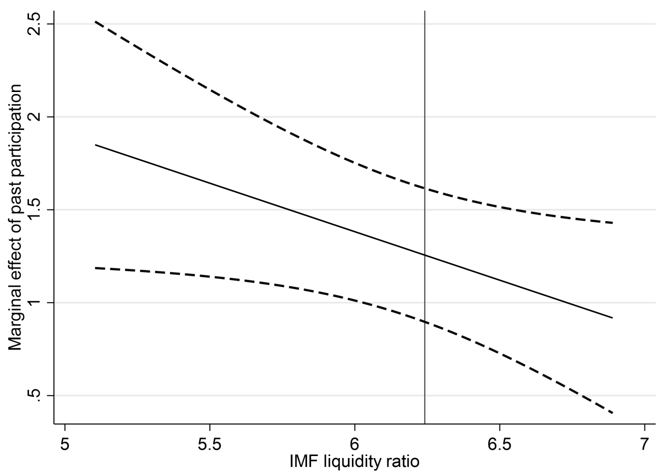
Notes: Pre-test parallel trends of firm sales growth and the exogenous component of our IV. Plot on left is of the log of IMF liquidity over time alongside the trend of GDP per capita growth over two sets of countries, those with a low share of years under an IMF program versus those with high share of years (IMF probability). Plot on the right is log of IMF liquidity over time alongside the trend of employee's compensation, measured as the labor share of national income from the World Inequality Database, over two sets of countries, those with an average share of years under an IMF program versus those with low share of years (IMF probability).

Figure B3: IV time-varying component and global trends



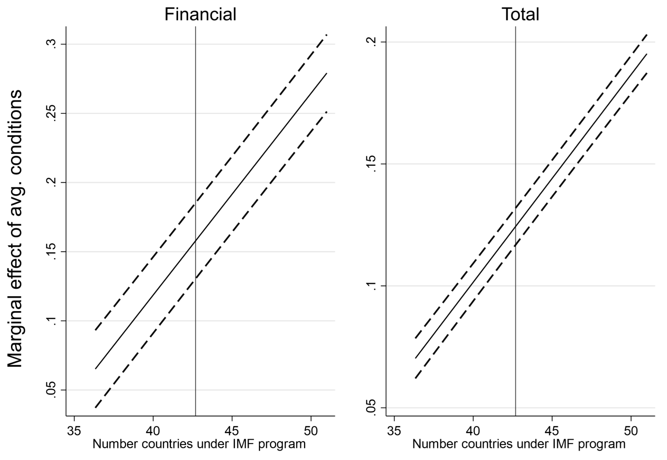
Notes: Plots of yearly trends in banking crises or currency crises as compared to the time component of the shift-share instrument, the natural log of the IMF liquidity ratio. Plot shows presence and co-movements with respect to the liquidity ratio and underlying global trends. Left axis shows value of ln(liquidity ratio), right axis number of yearly banking and currency crisis.

Figure B4: First stage marginal effects, IMF participation



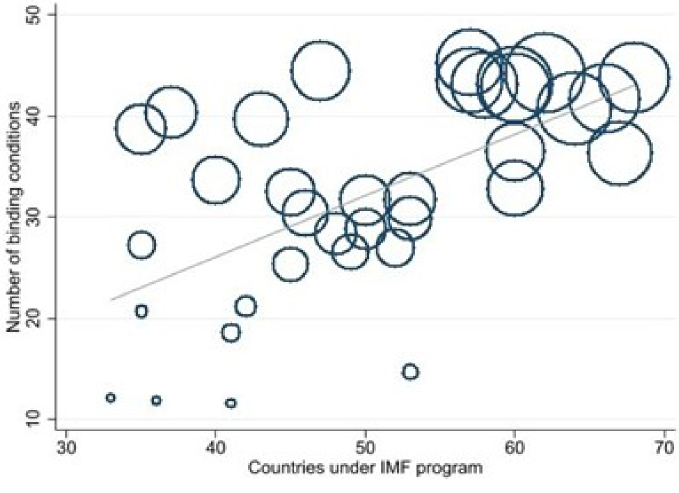
Notes: Marginal effects of share of past years under IMF programs on probability of a new IMF program, for differing levels of IMF liquidity in a given year. Based on specification in Column 1 of Table 1. Dotted lines show 95% confidence interval.

Figure B5: First stage marginal effects, IMF conditionality



Notes: Marginal effects of average number of financial or total binding conditions 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.

B2: Time invariant share of years

	Contemporaneous				Lagged			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
IMF participation	0.014	0.314	0.025	0.234***	0.057**	1.187	0.037	0.604**
	(0.66)	(1.61)	(0.91)	(2.70)	(2.29)	(0.92)	(1.31)	(2.04)
First stage:								
Instrument		-0.592**		-0.992***		-0.188		-0.534**
		(-1.97)		(-3.58)		(-0.90)		(-2.16)
Observations	77524	77524	10586	10586	77524	77524	10586	10586
R2	0.152	0.024	0.225	0.068	0.153	-0.369	0.226	-0.213
Kleibergen Paap (p-value)		0.089		0.010		0.387		0.057
Panels			5114	5114			5114	5114
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	NO	NO	YES	YES	NO	NO	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	NO	NO	YES	YES	NO	NO

Notes: Column 1 uses an OLS estimator with country dummies. Column 2 uses an IV estimator with country dummies. Column 3 uses the within estimator with firm fixed effects. Column 4 uses an IV estimator with firm fixed effect. The coefficient Instrument 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, ***$p<0.01$, **$p<0.05$, *$p<0.1$.

Appendix C: Additional specifications and sample dependence

C1: IMF participation and jobs

	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	0.009	0.029	0.009	0.041	0.003	0.138*	0.009	0.131**
	(0.89)	(0.65)	(0.69)	(1.19)	(0.22)	(1.67)	(0.78)	(2.35)
IMF probability		-0.107		0.197		-0.182		-0.147
		(-0.83)		(1.37)		(-1.17)		(-0.79)
First stage:								
Instrument		-0.949***		-1.316***		-0.705**		-1.208***
		(-2.74)		(-4.97)		(-2.14)		(-3.06)
Observations	85841	85841	13086	13086	85841	85841	13086	13086
R2	0.0633	0.0118	0.0883	0.0210	0.0632	0.000388	0.0883	-0.00728
Kleibergen Paap (p-value)		0.046		0.007		0.053		0.012
Panels			6308	6308			6308	6308
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. 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, **p<0.05, *p<0.1.

C2: Endogenous controls and channels

	Baseline without controls		Non-foreign firms		Size as dependent		Exporting as dependent		External finance as dependent		Financial obstacles as dependent	
	2SLS	2SLS FE	2SLS	2SLS FE	2SLS	2SLS FE	2SLS	2SLS FE	2SLS	2SLS FE	2SLS	2SLS FE
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
IMF participation	0.277*	0.311***	0.269**	0.217***	-0.134	0.008	-0.050	-0.023	-13.96	-4.662	-0.095	0.038
	(1.81)	(2.73)	(2.04)	(3.75)	(-0.69)	(0.19)	(-1.01)	(-0.92)	(-1.52)	(-0.99)	(-0.47)	(0.23)
Observations	93135	14714	69817	8675	93224	14741	93224	14741	91458	14438	89844	91458
R2	0.034	0.089	0.039	0.087	0.436	0.078	0.117	0.019	0.011	0.013	0.015	0.011
Kleibergen Paap (p-value)	0.144	0.083	0.038	0.007	0.045	0.008	0.046	0.008	0.046	0.008	0.044	0.046
Firm FE	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES
Country FE	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES	NO
Controls	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

Notes: Role of potentially endogenous controls in baseline results. Odd columns use an IV estimator with country dummies, even columns use an IV estimator with firm fixed effects. In columns 1-2 the baseline specification is estimated excluding control variables; in columns 3-4 the baseline specification is estimated excluding foreign firms; in columns 5-6 the dependent variable is firm size; in columns 7-8 the dependent variable is a dummy equal to one in the case of exporting firms; in columns 9-10 the dependent variable is external finance, while in columns 11-12 is financial obstacles. 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, **p<0.05, *p<0.1.

C3: IMF participation and firm sales growth, by industries

	Food		Manufacturing		Retail		Services	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
IMF participation	-0.084***	0.069	0.004	0.401*	0.033	0.218***	0.006	0.675
	(-4.71)	(1.05)	(0.13)	(1.70)	(1.09)	(2.81)	(0.22)	(0.96)
Observations	7045	7045	36218	36218	11581	11581	17022	17022
R2	0.175	0.077	0.155	0.008	0.159	0.048	0.157	0.153
Kleibergen Paap (p-value)		0.029		0.099		0.004		0.411
Country FE	YES	YES	YES	YES	YES	YES	YES	YES
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Industry x Year FE	YES	YES	YES	YES	YES	YES	YES	YES

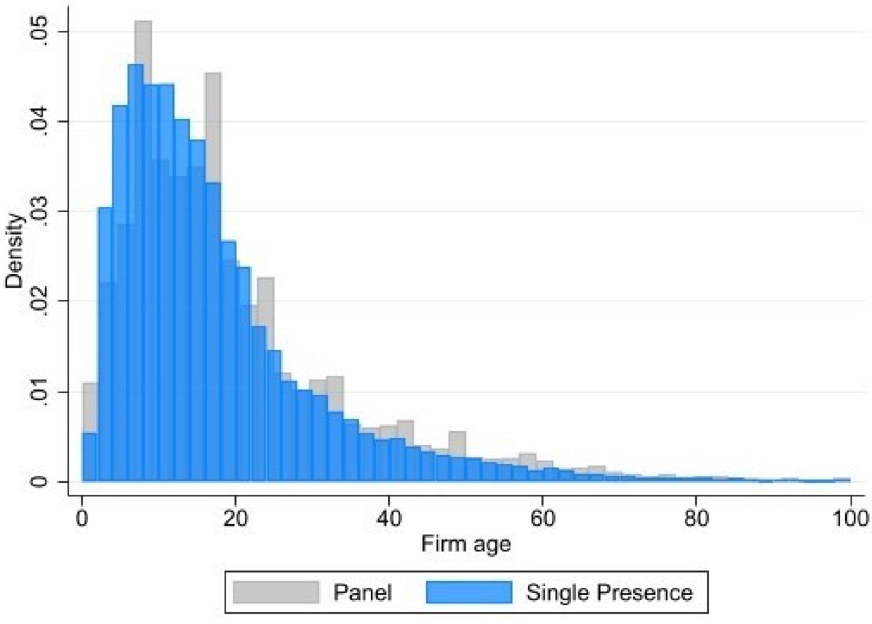
Notes: Effects of IMF participation on average firm sales growth, by industry. Aggregation of industries based on WBES stratification defined in Table A1. For each industry we show the estimators from a simple OLS and IV model. All models include firm and country level controls as well as country and 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, **p<0.05, *p<0.1.

C4: Randomization of firm

	OLS count		FE count			FE (percent)
	(1)	(2)	(3)	(4)	(5)	(6)
Beta	0.355	0.258	0.349	0.338	0.218	0.333
SE	0.062	0.022	0.058	0.046	0.047	0.05
Percent insignificant	14%	0	20%	0	2%	0
Observations	11694	38775	11452	3794	2792	3628

Notes: Randomization strategies for the 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 C1: Survivor bias: distribution of pooled v. FE sample



Notes: Distributions of firm age in the 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.

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Chapter 2

Do IMF Programs Stimulate Private Sector Investment?

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Abstract

This paper investigates the dynamic aggregate response of firm investments to the approval of an IMF arrangement. Using a local projection methodology, we find that distinguishing between General Resource Account (GRA) and Poverty Reduction and Growth Trust (PRGT) financing matters for the path of investment. Following a GRA arrangement, investments start to increase after two years, while the effect is quite limited after a PRGT. Adopting a stacked difference-in-differences estimator and exploiting firm-level characteristics, we find that firms having a domestic ownership, relying more on external finance, or which are more subject to uncertainty, invest more following a GRA agreement.

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2.1. Introduction

Economic headwinds, from the pandemic, to supply crises, to geopolitical tensions faced by countries have reinvigorated the role of the International Monetary Fund (IMF). Fund resources have been tapped over the past decade to deal with systemic debt crises in advanced economies such as in the Euro area, as well as reviving its role among developing and fragile economies. Traditionally, financial support by the IMF aims to create breathing room for countries hit by crises as they implement adjustment policies to restore macroeconomic stability and growth. While policies depend on country circumstances, the set of corrective actions provide a seal of approval that appropriate policies are adopted, helping mitigate crises and boosting future prospects during periods of heightened risks.[1]
At the macro level, the effects of IMF programs have been investigated focusing on two main channels. One strand of literature considers the liquidity effects of IMF credit injections, which can reduce the probability of self-fulfilling runs arising from illiquidity problems (Boockmann and Dreher 2003; Dreher 2006; Dreher and Vaubel 2004; Zettelmeyer 2000). More recently, the signaling argument is typically used to explain a catalytic finance effect, namely the propensity of private capital to flow into the country following the approval of an IMF program (among others Corsetti et al. 2006; Gehring and Lang 2020; Marchesi and Thomas 1999; Marchesi 2003; Mody and Saravia 2006; Morris and Shin 2006).
This paper links IMF participation to firm investment decisions, which are contingent also on future macroeconomic and policy prospects. While previous work documents the relationship between the IMF and firm performance in the short term (Bomprezzi and Marchesi 2023), this paper, using detailed balance sheet data, provides evidence on the interplay between the IMF and the firm's decision to invest, which represents a medium to long-term effect.[2]
IMF programs may have a positive investment effect through different channels. They may provide the recipient governments with additional money to spend, as well as reducing uncertainty about future economic policies and improving expectations of domestic and foreign investors by serving as a seal of approval. Beyond specific financial or legal factors, economic policy uncertainty is an important determinant of investment decisions. Providing additional resources represents a necessary but not sufficient condition to boost investments, if expectations on the country's future prospects are not affected.

[1] In the presence of policy uncertainty and hence lack of economic stability, misallocation of resources leads to lower aggregate productivity and investments, which are leading explanations for economic disparities across countries (Hsieh and Klenow 2009).

[2] Bomprezzi and Marchesi (2023) find that IMF intervention has a positive impact on firms' sales growth and that firm performance improves through the alleviation of the financing constraint. Recent studies have re-investigated economic outcomes following official capital flows at a more disaggregated level with respect to broad macroeconomic aggregates (Bluhm et al. 2020; Chauvet and Ehrhart 2018; Dreher and Lohman 2015; Dreher et al. 2021; Marchesi et al. 2021).

This paper focuses on tangible fixed asset investments, which tend to be non-reversible. Hence firms would favor precautionary delays in long-term decisions until future expectations improve. We propose a signaling mechanism under which firms, when undertaking these investment decisions, are sensitive to the expected economic environment. Under this hypothesis, the reduction of uncertainty that accompanies IMF programs ultimately triggers the firms' decision to increase tangible investments, even if no real macroeconomic effects have had time to materialize.

A growing strand of literature considers the adverse impact of uncertainty on firm investment. A common strategy is to proxy exposure to uncertainty through the volatility of returns of stock prices (Leahy and Whited 1996; Bloom et al. 2007; Bloom 2009; Panousi and Papanikolaou 2012; Alfaro, Bloom, and Lin 2021). In particular, Alfaro et al. (2021) provide two different proxies of firm uncertainty at the micro level: realized stock return volatility and implied volatility. In section 5, we employ the first of these indicators as our preferred measure of firm-level uncertainty.

We consider the difference between Poverty Reduction and Growth Trust (PRGT) and General Resource Account (GRA) IMF arrangements. This distinction is relevant, as IMF programs are not "one size fits all". Under GRA financing, a member's balance of payment needs should be resolved by the end of the program period and no follow-up arrangement would be anticipated. In contrast, financing under the PRGT is tied to achieving or making progress towards a stable and sustainable macroeconomic position consistent with strong and durable poverty reduction and growth. We first estimate the dynamic aggregate response of firm investments following the approval of an IMF program, financed either through GRA or PRGT, through a local projection methodology. We find that following the approval of a GRA, investments start to increase after two years, while after the beginning of a PRGT there is a mild effect that vanishes after two years.

While the main advantage of the local projection is to give a broad picture of the evolution of investments over time, it comes at the cost of assessing more in detail the role of firm-level indicators. For this reason, we adopt a stacked difference-in-differences approach to exploit firm-level information. We focus on three main firm characteristics: firm external financial dependence (Rajan and Zingales 1998), the role of sectoral uncertainty (Alfaro et al. 2021) and whether the firm operates within the country. These represent the various channels through which the IMF "seal of approval" may play a role in determining investments. Specifically, a reduction in the recipient country's level of uncertainty improves future economic prospects, and for this reason influences the decision of lenders to finance firm investments as well as of firms to invest, especially for those firms relying more on external finance or more exposed to firm-level uncertainty. Moreover, firms with domestic ownership are also more constrained by the future prospects of their own country when making an investment plan, while foreign owned firms gain a sort of natural hedge by being part of a foreign group and hence less sensitive to what happens in a country. Our results show that firms relying more on external finance, more subject to uncertainty, or

having domestic ownership invest relatively more following a program approval.
This paper contributes to the empirical literature on IMF effectiveness, and in particular to the strand of macro-micro work studying the channels through which IMF programs influence local economic activity. Given the importance of private sector activity to the success of an IMF program, the evaluation of different lending facilities and their outcomes has practical relevance for program stakeholders. To the best of our knowledge, this is the first paper that investigates whether different types of IMF programs, as well as improving a country's creditworthiness for external investors, may also make internal ones more willing to invest.
The remainder of the paper is organized as follows. Section 2 illustrates the related literature and Section 3 describes the data. Section 4 shows the results obtained using a local projection methodology, while section 5 presents the results of a stacked difference-in-differences estimator. Section 6 contains some robustness analysis. The final section 7 concludes.

2.2. Literature Review

2.2.1. Effects of IMF Programs

Traditionally the literature on IMF effectiveness focuses on broad country-level outcomes (Przeworkski and Vreeland 2000; Barro and Lee 2005; Easterly 2005; Dreher 2006; Marchesi and Sirtori 2011; Bas and Stone 2014). Among these studies, more recent ones have focused on the specific objectives of IMF policy conditions in pursuing macroeconomic stability. For example, some argue monetary stability, debt management, and the containment of external arrears as key goals of IMF programs (Kentikelenis, Stubbs, and King 2016). Also, IMF programs have been associated with reduced inflation and monetary growth, lower risk of currency crises and banking crises, and improved market performance of banks (Dreher and Walter 2010; Papi et al. 2015; Steinwand and Stone 2008).[3] In sum, the evidence suggests some positive adjustment effects regarding financial, fiscal, and monetary positions, though the benefits have generally fallen short of expectations, especially in terms of GDP growth and debt reduction (IEO 2021).
The success of IMF programs, however, largely hinges on its catalytic effect, namely the propensity of private capital to flow into the country following the approval of an IMF program. The signaling role of an IMF-supported adjustment program and its catalytic effects have both been extensively analyzed in the literature with mixed findings (e.g., 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; Zwart 2007). While conditionality can reassure international investors that adequate

[3] In addition, moral hazard incentives by borrowing countries expecting a bail-out could also be a concern (Dreher 2006).

policies are being implemented to resolve the balance of payments needs (Tirole 2002), the preferred creditor status of the IMF could make foreign investors fear penalization in case of a debt restructuring (Mody and Saravia 2006).
This paper belongs more generally to the growing body of literature focusing on the effects of official intervention at the subnational level. Recent studies have re-investigated economic outcomes following official capital flows at a more disaggregated level with respect to broad macroeconomic aggregates (Bluhm et al. 2020; Bomprezzi and Marchesi 2023; Chauvet and Ehrhart 2018; Dreher and Lohman 2015; Dreher et al. 2021; Marchesi et al. 2021).[4] The paper which most closely relates to ours is Bomprezzi and Marchesi (2023), who evaluate the effects of IMF programs on firm-level outcomes by considering two dimensions: participation in a program and scope of conditionality. They find that IMF intervention has a positive impact on firms' sales growth and that firm performance improves through the alleviation of the financing constraint.

2.2.2. Firm Investment Under Uncertainty

The literature on determinants of investment dynamics emphasizes the role of firm and sector-specific factors such as size, profitability, asset tangibility, and industry median leverage (Myers, 1984; Myers and Majluf, 1984; Titman and Wessels, 1988; Harris and Raviv, 1991; Booth and et al. 2001; Baker and Wurgler, 2002; Lemmon et al. 2008; Graham et al. 2015). Another strand of literature instead emphasizes the role of country-specific macroeconomic and institutional factors in determining firm outcomes (Borio, 1990; Rajan and Zingales, 1998; Kayo and Kimura, 2011; Cevik and Miryugin, 2018), as well the role of political instability (Herrala and Turk-Ariss, 2016).
Recent work underlines the importance in distinguishing between different sources of uncertainty as determinants of firm investments. For example, Baum et al. (2010) distinguish between own uncertainty (based on a firm's stock returns), market uncertainty (derived from the returns on a stock index), and a measure of covariance between the two. They find that an increase in market uncertainty inhibits investments, while finding that the effects of firm-level uncertainty are contingent on other firm specific factors such as cash flow. Similarly, Kang et al. (2014) find that economic policy (i.e., macro) uncertainty depresses firms' investment decisions, and the effect is greater for firms with higher firm-level uncertainty (proxied by stock price volatility). Recent methodological advances

[4] Dreher and Lohman (2015) were among the first to apply a macro-micro approach to evaluate the effectiveness of official capital flows. Using night-time light intensity, evaluate the effects of World Bank aid on development. Similarly, Marchesi et al. (2021) use survey data confront Chinese and World Bank project aid effects on firm sales. Bluhm et al. (2020) explore the equality inducing effects of Chinse infrastructure investments. Chauvet and Ehrhart (2018) use survey data to evaluate the effects of multilateral and bilateral aid flows on firm sales, finding a positive effect which manifest through the alleviation of an infrastructural constraint as well as a financing constraint.

focus on providing improved proxies of uncertainty (Jurado et al. 2015; Baker et al. 2013; Gulen and Ion 2016).[5]

More closely related to our paper is the growing strand of literature that considers the adverse impact of uncertainty on firm investment. A common strategy is to proxy exposure to uncertainty through the volatility of returns of stock prices (Leahy and Whited 1996; Bloom et al. 2007; Baum et al. 2010; Bloom 2009; Panousi and Papanikolaou 2012; Alfaro et al. 2021). Bloom et al. (2007) present a model in which uncertainty reduces firms' irreversible long-term investments in response to shocks to sales, arguing that firms become more cautious during times of heightened stock price volatility. Using data on U.S. firms over 1970–2005, Panousi and Papanikolaou (2012) show that firm-level idiosyncratic risk (or the volatility of stock price returns that is not explained by market or industry returns) associates negatively with corporate investment. In particular, and directly relevant for our paper, Alfaro et al. (2021) construct a firm-level dataset of uncertainty measures as well as firm-level instruments to address endogeneity concerns.

In summary, this paper contributes to the empirical literature on IMF effectiveness by exploring the impact of IMF programs on private sector investments. It proposes a domestic signaling effect, under which firms when undertaking non-reversible long-term investment decisions are sensitive to the current and expected policy environment. Under this hypothesis, the reduction of domestic policy uncertainty that accompanies IMF programs induces firms to increase tangible investments. The paper also contributes to the empirical literature on economic uncertainty by incorporating micro-level indicators in the context of international capital flows. The next section describes the data.

2.3. Data

2.3.1. Identifying IMF Programs

We focus on the pre COVID-19 period, drawing data on programs from the IMF's Monitoring of Fund Arrangements (MONA) database between 2002 and 2019. We consider the main lending instruments in the IMF's toolkit, which are tailored to different types of balance of payments needs as well as other specific country circumstances. Unlike previous work, we distinguish between GRA and PRGT.[6] Whereas GRA financial

[5]Jurado et al. (2015) provide econometric estimates of aggregate uncertainty, showing that popular uncertainty proxies overestimate the number of quantitatively important uncertainty episodes. Baker et al. (2013) deviate from traditional proxies by constructing a three-part index containing news-based, future tax provisions, and economic forecast components. Gulen and Ion (2016) use this index to show that the news-based component is the most relevant in explaining the negative relationship between aggregate uncertainty and capital investments and highlighting how the magnitude of the effect varies by the degree of investment irreversibility.

[6]Lending instruments under the GRA include the Stand-By Arrangement (SBA) for short-term or potential balance of payments problems, the Extended Fund Facility (EFF) for medium-term support to address protracted balance of payments problems, the Flexible Credit Line (FCL) and the Precautionary and Liquidity

support is available to all member countries on non-concessional terms, the IMF also provides concessional financing through the PRGT to cater to the diversity and needs of low-income countries.

We make the distinction between GRA and PRGT lending facilities because the policy ramifications differ between the two. Under GRA, policy measures must be taken within the program period and the macroeconomic adjustment be completed by the time repurchases (or repayment) to the IMF begin.[7] Balance of payment needs should also be resolved by the end of the program period and no follow-up arrangement would in principle be expected. In contrast, financing under the PRGT is tied to achieving, or making progress towards, a stable and sustainable macroeconomic position consistent with poverty reduction and growth.[8] The distinction between GRA and PRGT is important because it implies that, unlike for the GRA, repeated programs financed under the PRGT can be expected for sustained engagement to deliver progress towards macroeconomic stability. For expositional simplicity, from here on we label lending facilities under GRA financing as "GRA programs" and likewise PRGT financed lending facilities as "PRGT programs".

Our treatment variable of interest is an indicator that takes the value one if a country approved an IMF program during the year but no later than October. Otherwise, the subsequent year is coded as the program approval year.[9] As such, we account only for announcement effects which occur sufficiently early in the calendar year as to determine investments. Our sample contains only countries that with an IMF program over the sample years. This setup helps to mitigate problems of endogeneity, whereby estimates of the effects of an IMF program approval on investment dynamics could be biased by selection into the sample. Secondly, with a sample of treated countries, the focus can shift to the heterogeneity among arrangements.

Figure 1 plots the number of unique programs recorded per year in the MONA database for the two types of arrangements considered (GRA and PRGT). GRA make up the bulk of programs over the full sample, while PRGT programs represent a smaller share, generally not surpassing 5 per year. On average, the overall number of programs per year increased in the latter half of our sample. The following sub-section introduces the various firm-level data.

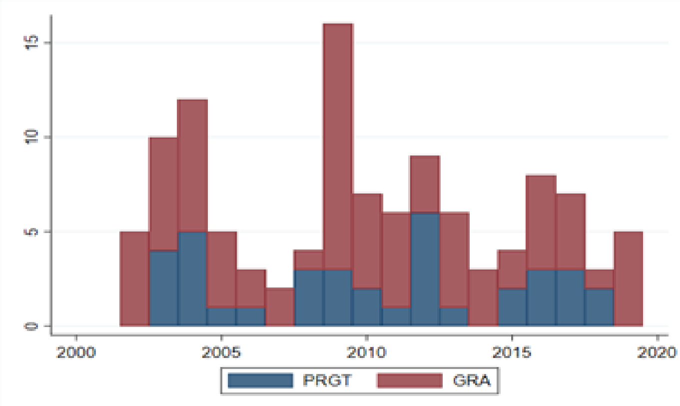
Line (PLL) to help prevent or mitigate crises and boost market confidence during periods of heightened risks. For PRGT, two lending facilities are considered; (i) the Extended Credit Facility (ECF) for sustained medium- to long-term engagement in case of protracted balance of payments problems and (ii) the Standby Credit Facility (SCF) to address short-term balance of payments and adjustment needs caused by domestic or external shocks, or policy slippages.

[7] Amounts drawn under a SBA are repaid over $3\frac{1}{4}$–5 years, whereas credit provided under an EFF are to be repaid over $4\frac{1}{2}$–10 years in 12 equal semiannual installments.

[8] Repayments under the ECF carries a grace period of $5\frac{1}{2}$ years and a final maturity of 10 years, whereas the SCF has a grace period of four years and a final maturity of 8 years.

[9] We follow the IMF Independent Evaluation Office (IEO, 2021) strategy for coding program start years.

Figure 1: Distribution of IMF programs per year



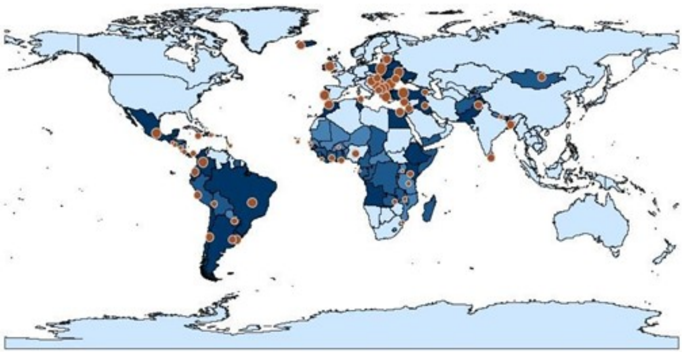
Notes: Number of unique IMF programs signed per year, by program type. Blue bars are for the PRGT category, red bars are for the GRA category.

2.3.2. Firm Tangible Fixed Asset Investment and Balance Sheet Data

We retrieve balance sheet data from the Orbis database provided by Bureau Van Dijk. To assess the influence of expectations on firm investment decisions, it is important to focus on tangible investments because of their non-reversible nature. Generically, tangible investment refers to investments in physical assets (e.g., property, plants, and equipment) acquired by a firm for long-term use and which have tangible value. We scale tangible fixed assets by total assets as a preferred investment measure. As opposed to other more generic categories (financial or intangible), this allows us to capture how firms react to changes or potential changes in the macroeconomic environment.

The Orbis database provides balance sheet data for firm-level controls. We follow the specification of Kalemli-Özcan et al. (2022) in identifying our main Orbis firm-level controls. These include a set of balance sheet variables and ratios that are standard in the corporate finance literature as determinants on firm investment. First, we use the log of total assets to proxy for firm size. Leverage is measured as the ratio of total debt to total assets, where total debt is in turn the sum of all long-term debt, loans, credits, and other current liabilities. Debt maturity is proxied by the ratio of long-term debt to total debt in order to capture the rollover risk of firms. Companies with a longer debt maturity structure are more “locked-in” in their investment paths and have lower rollover risk, namely they are less likely to rollover their debt in the short-term to finance new investments. To capture the drag that past debt has on current finances, we include the ratio of interest expense to earnings before taxes (EBIT). Sales growth captures growth opportunities for the firm. Finally, we control for cash flows scaled by total assets as is

Figure 2: IMF Programs and Firms



Notes: The figure plots average SDR access (MONA) over sample years and number of unique firms in Orbis sample for a given country. Light blue indicates no programs between 2002 and 2020, darker color indicates greater average access, larger bubbles indicate larger panel of firms.

standard in the literature. Table A1, in Appendix A, presents variable descriptions and sources, while Table A2 provides the full summary statistics.

Finally, Orbis provides information on firm ownership, incorporation dates, and sectors of operation. From these we construct sector-year fixed effects to account for time-varying, sector specific heterogeneity.[10] There are several data issues with Orbis however that deserve discussion. Most significantly, firm coverage varies by region and by country (see Table A5, in Appendix A). For countries where the filing of financial information is mandatory for all, the Orbis sample is more comprehensive (Kalemli-Özcan et al. 2015). By nature of funding needs, countries in the sample that have had an IMF program are for the most part middle and lower income, and highly concentrated in Africa, Latin America, the Caribbean, Eastern Europe, and Southeast or Central Asia. Orbis has typically more limited data in these countries compared with firms from other parts of the world, particularly with respect to Western Europe and the Americas. Figure 2 gives a graphical representation of countries having had an IMF program, showing a clear concentration in Africa. The size of the dots indicates the number of unique firms for which we have balance sheet data in the country. The Orbis coverage of this data in Africa is provided for half of the MONA sample. Nonetheless, there is strong overlap between Orbis and MONA coverage in Eastern Europe, Latin America, and Central Asia.

We follow the procedure outlined by Kalemli-Özcan et al. (2015) in order to mitigate remaining data quality issues related to Orbis and rely on historical Orbis data, downloading year-specific vintages and then matching firms over time with Orbis' unique firm identifiers. This produces firm samples which are more nationally representative and mitigates the need to re-weigh the data. We adopt some simple data cleaning to our sample

[10] Table A4, in Appendix A, reports the average tangible investment by NACE main sector for each year across all firms.

and our main variables and drop financial firms, government sector firms, and other firms which operate primarily in service activities.[11] We avoid double counting by considering only consolidated financial statements when available and clean the data by removing cases of erroneously reported balance sheet items, such as negative costs. Finally, all balance sheet variables are winsorized so that their kurtosis falls to a value around 10.

Our final firm sample is an unbalanced panel of 43,949 firms for 69 countries from 2000 to 2019.[12] In the next section we explain our identification strategy and baseline model.

2.4. Local projections

We are interested in the dynamic response of firm investments to the approval of an IMF program. As a baseline method, we estimate impulse response functions using local projections (LP), which have become a popular because of their flexibility and simplicity.[13] We not only aim to track the evolution of firm investment dynamics over time following the approval of an IMF program, but also estimate the average treatment effect (ATE) of such programs on investments.

To account for the endogeneity of an IMF program approval, we exploit a methodology developed by Jordà and Taylor (2016) that uses a propensity-score based method, combined with local projections (Jordà, 2005) to find the ATE of an IMF program on the firm tangible fixed assets investment rate.[14] Therefore we accept the endogeneity of entering an IMF program and attempt to explicitly model for it. If the probability is modeled correctly, we can re-balance the sample as if random. In a second stage we use as the outcome variable the cumulative change in the ratio of tangible fixed assets to total assets. The final estimator gives an average treatment effect known as the Adjusted Inverse Propensity Weighted (AIPW) estimator (Jordà and Taylor, 2016). The AIPW estimator incorporates the flexibility of local projections with a method for reducing endogeneity bias. The two-stage method described above is doubly robust, in that the estimator will be unbiased if either of the two stages is correctly specified. The underlying idea is that the predictor set in the first

[11] We drop firms with a main NACE Rev. 2 category of Financial, Public administration and defense, Real estate activities, Administrative and support services, Human health and social work, Other service activities, Activities of the household, and Extraterritorial. We drop these sectors either because they follow different accounting standards or have core activities which do not require tangible assets.

[12] Some descriptive statistics are presented in Figure A1, in Appendix A, where we categorize firms by age according to their age. As would be expected investments for younger firms generally grow faster than for other firms.

[13] As described by Jordà (2005), local projections can be estimated by simple regression models and are in general more robust to misspecification errors than other related methods.

[14] Dealing with the endogeneity of IMF programs is an issue that is tackled in several different ways in the literature (e.g., Barro and Lee 2005; Gehring and Lang 2020; Lang 2021). Crucially for our empirical strategy, this IV is suitable for the identification into an IMF program but not into program type (GRA vs. PRGT).

stage, and then the control set in the second stage, should be expansive enough to capture as much of the variation in program approval as possible.[15]

In our first stage we model the probability of being under a specific program type by estimating a propensity score for each observation in our sample. Our dependent variable is the dummy variable identifying IMF program years as indicated in the MONA dataset. The propensity score for being under a program is predicted by the multinomial logit model:

$$P_{i,t,p} = \lambda(\beta, Z_{t-1,i}) \tag{2.1}$$

Where λ is the multinomial logistic distribution function and Z is a vector of country-specific controls including macro and political variables as well as macro-region fixed effects.[16] We estimate then the probabilities of either a) having no program, b) having a GRA program, c) having a PRGT program. In the model, the base values are the non-program years, and we estimate the propensity scores for each outcome. This allows us to capture the heterogeneity of program type as well as the types of country typically associated to one of the two.

This first stage specification follows Dreher et al. (2009) and includes a dummy if a country was under a program in the past, a measure of autocracy, the country's investment to GDP ratio, the log of real GDP per capita measured in PPP, total debt service, the budget balance, ratio of reserves to imports, real GDP growth, changes in reserves, the current account balance to GDP, and two measures of political quality including a dummy for election years and the log of checks-and-balances. Table A2 in the Appendix describes the predictor variables in detail.

The estimated $P_{i,t,p}$ is then the predicted probability of being under program type p, for country i at time t given our set of predictor variables. From this, the second stage re-balances to create a synthetic sample where the decision to be under an IMF arrangement is as good as random. Using our logit estimates, we can estimate the extent of the non-randomness in our sample. Specifically, a highly endogenous event would be predictable based on observables and have a high $P_{i,t,p}$, while a control would have a low $P_{i,t,p}$. We assign the weights $\frac{1}{P_{i,t,p}}$ to the treatment group and $\frac{1}{(1-P_{i,t,p})}$ to the control group. The average treatment effect, given the re-balanced sample, will then be the difference of the average weighted potential outcomes of the two groups across our sample.

Table B1 in Appendix B reports the estimated coefficients for the first stage. The results are in line with the literature. There is strong evidence of path dependency, where countries

[15] With this approach, we do not need to rely on exclusion restrictions. Even if all our variables were endogenous, if there is no unexplained deviation from the conditional forecasted change in ratings, the ATE will be unbiased (Jordà and Taylor 2016).

[16] Since our outcome is based on program type, as opposed to considering all programs together, including country fixed effects would produce collinearity with the outcome in certain groups that only had one type of program. For this reason, we use macro-region fixed effects.

that have participated in programs in the past are more likely to enter a new program. GDP per capita and GDP growth are both negatively associated with the likelihood of being under a PRGT arrangement, as more well-off countries typically have less of a need for these programs. The positive coefficient on GDP per capita when treatment is GRA is justified by the fact that among our sample of always-taker IMF countries, the richer ones are eligible for GRA arrangements only. An increase in reserves is also negatively correlated with IMF arrangements, indicating the importance of reserves in staving off balance of payment crises which can lead to an IMF program. It may be surprising that variables such as current account to GDP are not significant in some cases, given the Fund's mandate to help countries in a balance of payment crises, but this result is in line with previous work (Conway, 1994). Finally, we find some evidence of the role of political variables in our sample. The literature speaks to different reasons as to why these variables might influence the probability of being under a program.[17] For example, combative elections might make the stigma of a program unappealing for incumbent politicians, which reflects the negative sign on our legislative election dummy.
The outcome variable, which is modeled in the second stage, is the cumulative change in the firm tangible fixed assets scaled by total assets, which captures investment throughout the years. Our baseline model models the outcome variable as measured with a local projection (Jordà 2005) according to the following baseline specification:

$$\Delta y_{i,j,k,t+h} = \alpha + \beta Z_{i,j,k,t-1} + \delta X_{j,t-1} + \gamma D_{j,t} + \square_i + \tau_{k,t} + {}_{i,j,k,t} \quad h = 1, 2...5 \tag{2.2}$$

Where $\Delta y_{i,j,k,t+h}$ is thus the conditional forecast of the dependent variable from time t to $t+h$, where h denotes the forecast time horizon of up to five years. The outcome is measured for firm i, in country j, and sector k. $Z_{i,j,k,t-1}$ is a vector of firm control variables as described in Section 3, and also contains the lagged difference in investment $\Delta y_{i,j,k,t+h}$ to account for serial correlation. $X_{j,t-1}$ is a vector of country-level controls and lagged treatment variables. These country-level variables fall into three broad categories of economic, financial, and political factors. We consider both the growth rate of real GDP and the log of real GDP per capita, which capture growth opportunities for the firm. We proxy for the size of the banking sector and financial development using the log of claims by depository institutions on the private sector. The real interest rate captures both the representative lending rate offered in the economy as well as inflation risk to investments. Finally, we use the International Country Risk Guide (2021) index of political risk to control for the broad perception of investment risk within the country. Table A2, in Appendix A, presents the description, and sources of all variables.
$D_{j,t}$ is our country-level treatment variable, which is equal to one for the year when the country enters an IMF program as described in Section 3.1. We also control for the remaining program years. Finally, we include firm fixed effects $\square_i$ and sector-year

[17] See for example Przeworski and Vreeland (2000) and Dreher and Vaubel (2004).

time-varying heterogeneity $\tau_{k,t}$. This way, we account for both global factors determining investment dynamics as well as industry-specific unobservable characteristics tied to investment choices. Standard errors are clustered at the country level. $_{i,j,k,t}$ is the error term. Regression equation (2) is run for each point in horizon h on the rebalanced sample to obtain the desired average treatment effect, ATE:

$$ATE_h = \frac{1}{n}\sum_{i}^{I}\sum_{t}^{T}\{[\frac{(\Delta y_{i,j,k,t+h})(D_{j,t})}{P_{i,t,p}} - \frac{(\Delta y_{i,j,k,t+h})(1- D_{j,t})}{1-P_{i,t,p}}]$$
$$- \frac{D_{j,t} - P_{i,t,p}}{P_{i,t,p}\,(1- P_{i,t,p})}[(1-P_{i,t,p})m_1^h(Z_{i,t-1},\ X_{i,j,k,t-1}) + (P_{i,t,p})m_0^h\,(Z_{i,t-1},\ X_{i,j,k,t-1})]\} \tag{2.3}$$

Where $\Delta y_{i,j,k,t+h}$ are the estimated conditional forecasts for the local projections (Equation 2), and $D_{j,t}$ is the dummy variable to indicate treatment, in our case program approval. $P_{i,t,p}$ are the estimated propensity scores from Equation 1. The first part of Equation 3 is a standard inverse propensity-score weighted ATE. Intuitively, this is like a group-means comparison between countries that have signed a program and those that have not, with the additional step that we correct for allocation bias of the treatment by modeling it in Equation 1, reducing it to a unidimensional element, which is the estimated propensity score, and inverting to achieve a random distribution. The second part is an adjustment term consisting of the weighted average of the two independent regression estimates. The purpose of the adjustment term is to stabilize the estimator as the propensity scores get close to the extremes (0 or 1) and therefore alleviates the need to truncate weights.[18]

In conclusion, the use of local projections for our estimation strategy is motivated by several factors. First, local projections are free of structural constraints that would otherwise be imposed on a parallel VAR model, thereby allowing for the response of investments to an IMF program approval to vary non-linearly over the forecast horizon, making them useful for computing dynamic effects. Local projections are also easier to compute and can be estimated using ordinary least squares (OLS).[19] However, local projections are not without drawbacks. Since the estimation does not impose any direct link between impulse responses at times h and h+1, estimates can sometimes display erratic behavior (Ramey and Zubairy, 2014). Furthermore, as the horizon increases, observations are lost on both sides, which can lead to loss of efficiency. Therefore, local projections are optimal for short to medium term projections, and the efficiency of the estimator is a function of forecast horizon over the total size of the time dimension T. Because we forecast

[18]Jordà and Taylor (2016) show that their AIPW estimator has properties such that extreme values of the propensity scores are offset by the adjustment term, in contrast to a standard IPW estimator.

[19]In evaluating the properties of local projections, Montiel Olea and Plagborg-Møller (2021) and Plagborg-Møller and Wolf (2021) argue for the use of lag-augmented local projections as a requirement for robustness.

the impulse response of investments up to a max of 5 years over a 20-year period, our choice of method remains safe. In the robustness tests (Section 5), we test the sensitivity of results by restricting estimates to groups of firms with data over a full forecast and lag horizon. Table 1 presents the local projection baseline results, with the impulse response functions plotted in Figure 3. The ATE is computed at each time t+h for programs approved at time t. We find that the effect of GRA programs is increasing over time, peaking at four years after program approval. On average, tangible assets grow over four years by a cumulative amount of almost four percentage points with respect to the approval year. For PRGT programs, on the other hand, we find only a weak temporary effect. In the first year after program approval, tangibles accumulate marginally, with a value around one percentage point above the reference level, with no significant effects afterwards.

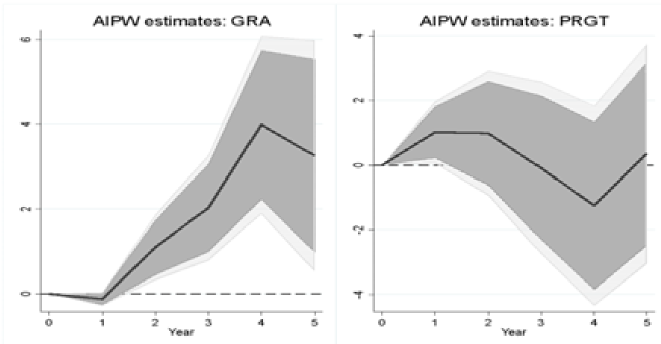
Table 1: Program Approval and Firm Investment Response, AIPW

			GRA		
	1	2	3	4	5
AIPW	-0.123	1.096*	2.036**	3.986**	3.260*
	(-1.48)	(2.78)	(3.21)	(3.72)	(2.34)
N	21643	19002	16516	14337	12608
			PRGT		
	1	2	3	4	5
AIPW	1.019*	0.989	-0.079	-1.254	0.359
	(2.09)	(1.00)	(-0.06)	(-0.79)	(0.21)
N	21643	19002	16516	14337	12608

Notes: Average treatment effect of a Fund program approval estimators for each h-step ahead forecast on the cumulative change in firm tangibles/TA, with $h = 1, 2, 3, 4, 5$. Standard errors clustered at the country level, T-statistics in parenthesis. $^{*} p < 0.1$, $^{**} p < 0.05$, $^{***} p < 0.01$.

The positive effect of GRA approval suggests that in GRA countries an IMF intervention is enough to trigger an increase in investments. On the other hand, in PRGT-eligible countries multiple confounders inhibit firm investments. The positive effect of a Fund program is not enough to offset the drag on private sector investments due to poorer access to credit, lower quality of institutions, and fewer cash generating opportunities that are associated with the markets in which these firms likely operate. The differential effects of the type of IMF financing on investments can also be explained by the nature of these programs. Under PRGT, it is likely that repeated IMF engagement, which our treatment does not capture, would provide firms with the kind of confidence boost needed to match GRA effects. Finally, PRGT arrangements target mostly social programs and safety nets, therefore dimensions that wouldn't impact firms' decisions to invest. In the next section,

Figure 3: Program approval and firm investment response, AIPW estimates



Notes: Panel A shows AIPW average treatment effects for each h-step ahead cumulative change in tangible fixed asset investment rate with respect to base year (yt+h – yt) following the signing of the respective IMF program (GRA or PRGT). Shaded areas show 90 and 95% confidence intervals, standard errors clustered at the country level.

we focus on GRA agreements and present the results using a stacked difference-in-differences estimator.

2.5. Stacked difference-in-differences

While our baseline result provides estimates of dynamic effects of an IMF program, it does not allow us to evaluate how firm-specific heterogeneity influences the outcome. In this section, we take a more granular approach to capture the differential effects of a GRA program approval. Specifically, using a difference-in-differences (DiD) approach, we consider a firms' external financial dependence, level of uncertainty, and whether it operates within the recipient country.

Our sample consists of countries that have an IMF program at different points in time and switch in and out of treatment.[20] The analysis presented in the previous section has therefore the flavor of a staggered difference-in-differences. As recent developments in the applied econometrics literature suggest (Goodman-Bacon 2021, De Chaisemartin and D'Haltfoeuille 2020, Callaway and Sant'Anna 2021, Borusyak and Jaravel 2021), two-way fixed effects estimates may produce inconsistent estimates in this setting. One of the reasons is that countries treated at the beginning of the sample may enter in the control group for countries that experience a crisis toward the end of the sample. To address this potential concern, we carry out an alternative estimation strategy based on a "stacked difference-in-differences" similar in spirit to Cenzig et al. (2019) and Deshpande and Li (2019). The objective of the procedure is to ensure that every country experiencing an IMF

[20] Figure A2, in Appendix A, plots the treatment status by country and program type for each year in the sample, showing the dynamic nature of treatment is evident in our sample.

program (treated) is compared only to clean controls, i.e., countries that did not experience a program.

The method consists in splitting the data into n sub-experiments, where each sub-experiment represents a unique calendar year where treatment (program approval) occurred for any cross-sectional group (country). A treatment window is defined, such that only observations with treatment outside a k-years are considered as controls. As a result, all observations within a sub-experiment will have the same program adoption year, and a clean control group without confounding effects from other program adoptions. These sub-experiments are then stacked to create a dataset which consists of n independent panel event studies.

The model contains the same country and firm controls as in our baseline specifications, fixed effects, and sector-year fixed effects to account for time-varying heterogeneity. A further advantage of a stacked DiD setup is the ability to compute dynamic effects. As in our baseline local projection specifications, we are interested in the time-varying effects of the adoption of an IMF program conditional on the firm characteristics (FC). We specify a model, as shown in Equation 7, where we identify the two years before and the five years after program approval (with year 0 as the reference year) with a set of indicator variables YSE (years since event):

$$\begin{aligned} Tan/TA_{i,j,k,t} =& \alpha + \beta Z_{i,j,k,t-1} + \delta X_{j,t-1} + \sum_{j=-k\alpha}^{kb} \gamma_j \left[FC_{i/i,t} * 1(YSE_t = j) \right] \\ & + \sigma FC_{i/i,t} + \sum_{j=-k\alpha}^{kb} \rho_j + 1(YSE_t = j) + \square_{j/i} + \tau_{k,t} + + \varepsilon_{i,j,k,t} \end{aligned} \tag{2.4}$$

Our parameter of interest is γ_i, representing the interaction between the indicator for the jth year before/after the program approval and the firm characteristics.

We start by considering the importance of firm financial frictions, which is the typical obstacle to a firm investment. For evaluating the role of financial frictions, we first rely on the seminal work by Rajan and Zingales (1998) (henceforth RZ) on external financial dependence. The underlying idea is that the role of financial markets is to reduce problems of moral hazard and adverse selection, thereby reducing the costs of the firm in raising funds. Financial development, or any structural shock to the financial system of a country, should disproportionately help firms which are more dependent on external financing. In our context, we consider the adoption of an IMF program as a comparable positive shock to the financial market. The RZ index is a sector-specific, time-invariant measure of the share of investment that is not financed by internal cash flow in the median listed U.S firm over the 1980s. The guiding assumption to this approach is that the U.S capital market is a good proxy for a frictionless market, and credit demand is driven by industry-specific technological fundamentals. In a cross-country framework such as ours, the second assumption is that these industry fundamentals are constant across countries.[21] We use the indices computed by Eppinger and Neugabauer (2022) following the RZ methodology. From Compustat, the authors define the index of external financial dependence for U.S firms over the years 1990-2005. Being closer in time to our sample, it is a better proxy of technological demands of an industry. External financial dependence is then defined as capital expenditures minus cash flow from operations for each firm, then divided by the sum of capital expenditure over the period, and finally using the median value by industry as a measure.[22] We then merge these industry values reported as NACE sectors with our Orbis data.[23]

We then turn to our second measure of firm heterogeneity, which is a proxy for firm-level uncertainty. Alfaro et al. (2021) provide two different proxies of uncertainty at the micro level: (i) realized stock return volatility of daily returns from the Center for Research in Security Prices (CRSP) and (ii) implied volatility, as constructed from a mix of put and call-at-the-money options. We employ the first of these indicators as our preferred measure of firm-level uncertainty.[24] The data spans from 1992 to 2019 and provides the year-by-year two digit SIC industry codes. We therefore aggregate these measures by taking the median sector-year value and match them with our firm data also at the sector-year level. By matching U.S data with our sample at the sector level we are constructing a measure of uncertainty that is not firm varying. This measure should then be interpreted as an industry-specific characteristic, which is comparable across countries, à la Rajan and Zingales (1998).[25]

Finally, we try to capture the extent to which a domestic firm could be differentially exposed to policy uncertainty within a country, as opposed to a foreign owned firm. Using Orbis historical vintages, we take ownership data for firms beginning in 2007. We retrieve information on the global ultimate owner (GUO) and the global ultimate consolidated owner where it exists. These are the ultimate owners, net of all intermediate ownership connections, with at least 50% of direct or indirect ownership in the firm. We classify a firm as having a foreign vs. a domestic owner each year. However, simply comparing

[21]While a small literature compares the original index with a few country-specific measures (Eppinger and Neugabauer, 2022), the RZ index has been widely used and shown to be consistent across countries (Cetorelli and Gambera 2001; Beck and Levine 2002; Fisman and Love 2003, 2007; Kroszner et al. 2007; Pagano and Pica 2012).

[22]See Eppinger and Neugabauer (2021), in Appendix A, for a detailed methodology on the construction of the index.

[23]Table A6, in Appendix A, reports the values of the EFD indices. As in RZ, the indices are only computed for a set of firms in manufacturing-oriented industries.

[24]This is constructed as the annualized 12-month standard deviation of daily CRSP returns of a sample of U.S firms. Furthermore, the authors provide firm level measures of 12-month compounded stock returns and Tobin's Q as additional controls to tease out first-moment effects.

[25]We also aggregate at the sector level as uncertainty is an industry-specific process that is driven by elements such as supply chain networks and product-specific demand elasticities.

domestically owned versus foreign-owned firms could be misleading. Foreign-owned firms are likely to be larger and more successful, for example when part of a multinational corporate group. Furthermore, their ownership changes occur quite frequently, and are likely driven by economic expansions or recessions. Yet, we want to identify a set of firms, which are tied to the country and whose activity is strongly contingent on the domestic country's economic performance. To that end, we take as treatment firms which do not switch ownership in the immediate years before and after program approval, labeling them as "never-leavers."

Table 2 presents the results of the DiD specification. Panel A of Table 2 presents the results when considering external financial dependence, the results in panel B consider firm's uncertainty, while those reported in panel C reports the dynamic stacked DiD estimates for "never-leavers". In all panels, the first two columns, which indicate the two years leading up to the program approval, show no evidence of an anticipation effect. As shown in panel A, we find that tangible assets grow disproportionately more relative to the base year for firms operating in sectors that are characterized by a high degree of external financial dependence. The effect is persistent over time. For example, for the industry which is at the bottom 5th percentile of external financial dependence (publishing and printing), the expected effect after one year is small and negative, at around -0.17 percentage points. For the firms in the industry at the top 95th percentile (communication equipment) the effect is 0.5 percentage points.

As shown in panel B, there is evidence that after the adoption of an IMF program firm investments increase in those sectors with higher volatility. Specifically, three years after the program adoption, greater sector-wide volatility leads to a 3-percentage point increase in tangible assets. Finally, the results presented in panel C, show that firms which remain exposed to the Fund program throughout the treatment period increase their tangible assets by around one percentage point as opposed to firms that change in ownership.

In conclusion, we find that firm characteristics are important to assess the effect of an IMF program. Consistently with our initial hypothesis, we find that firms relying more on external finance, more subject to sectoral uncertainty, or more tied to the domestic economy, all increase their investments after the adoption of an IMF program. The next section presents some robustness analysis and alternative specifications.

Table 2: Firm Frictions and Dynamic Stacked DiD Estimates

	-2	-1	1	2	3	4	5
Panel A: External Financial Dependence							
IMF participation	0.186	0.275	0.427*	0.501**	0.543**	0.591**	0.630**
	(0.87)	(1.41)	(2.08)	(2.73)	(2.98)	(3.19)	(3.29)
N	34,416	34,416	34,416	34,416	34,416	34,416	34,416
Panel B: Realized Volatility							
IMF participation	1.756	1.548	1.571	1.744*	1.835	1.961*	2.334*
	(1.37)	(1.53)	(1.55)	(1.71)	(1.63)	(1.75)	(1.89)
N	77,554	77,554	77,554	77,554	77,554	77,554	77,554
Panel C: Ownership switches							
IMF participation	0.341	0.509	1.341**	1.332**	1.091*	1.075*	0.086
	(0.41)	(0.68)	(2.41)	(2.38)	(1.83)	(1.73)	(0.35)
N	66,845	66,845	66,845	66,845	66,845	66,845	181,005

Notes: Year-specific DiD effect of a treatment *d* on tangibles/TA in a stacked event study setup. Panel A considers the interaction between the degree of external finance dependence and a dummy equal to 1 for the year *t* before/after the program approval. Panel B considers as the interacting term the measure of realized volatility. Panel C considers the interaction between a dummy identifying "never-leavers" and a dummy equal to 1 for the year *t* before/after the program approval. All specifications include full controls and sector-year fixed effects. Panel A uses country fixed effects, Panel B and C use firm fixed effects. IMF participation refers to GRA agreements. Standard errors clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

2.6. Robustness

This section provides a series of robustness tests. We start by testing pre-treatment trends. Then, we test for compliance with an IMF program and for the persistence of the effects. Finally, we run a series of tests on sample dependence.

2.6.1. Identification

Our identification strategy captures primarily the systemic differences between countries selecting into a program type, namely GRA or PRGT. We want to rule out the possibility that investments were already growing before program approval, for example due to an anticipation effect. As sensitivity check, we then estimate a simple fixed effects model, regressing investment at time t, on dummies for a program approval occurring at t+h. As shown in Table C1, in Appendix C, we find no evidence of systematic anticipation effects. As is well documented in the literature on the IMF, program interruption is common and compliance with Fund conditionality can be low (see among others, Dreher 2003, Dreher 2006 and, Reinsberg et al. 2022a, 2022b). Since the main assumption in this paper is that entry into a program signals a reduction in policy uncertainty, we test if program interruption interferes with this mechanism. Based on the number of reviews, a program can be either classified as completed or off-track. We take as treatment, rather than the adoption year of a program, the final program year, whether this is the originally scheduled end of the program or the effective end if the program went off-track. Table 3 presents the results using this alternative treatment for the AIPW estimator. Consistently with Table 1, we find different long-term effects for GRA and PRGT. In the case of GRA, we find that the effects are positive and significant regardless of whether the program goes off track or not. In the case of PRGT, we find that investments drop following the end of a program and this drop persists if the program goes off-track. Furthermore, we run our baseline estimates dropping programs classified as off-track, finding that the results are robust to this change.

Table 3: End-of-Program Effects by Completion Status

	1	2	3	4	5
GRA					
Completed programs	-0.23***	0.56	1.77**	3.46***	2.57*
	(-3.32)	(1.06)	(2.46)	(3.16)	(1.85)
N	21,643	19,002	16,516	14,337	12,608
Offtrack programs	0.27	1.15**	2.01***	3.91***	3.16**
	(1.18)	(2.49)	(3.32)	(4.03)	(2.78)
N	21,643	19,002	16,516	14,337	12,608
PRGT					
Completed programs	-1.05*	-0.99	-1.65	-0.27	2.72
	(-1.81)	(-0.91)	(-1.14)	(-0.17)	(1.62)
N	21,643	19,002	16,516	14,337	12,608
Offtrack programs	-2.58***	-1.17	-4.21**	-2.04	-0.76
	(-3.88)	(-1.07)	(-2.74)	(-1.27)	(-0.44)
N	21,643	19,002	16,516	14,337	12,608

Notes: AIPW average treatment effect of a program end, by completion status, for each time horizon $h = 1, 2, 3, 4, 5$. Standard errors clustered at the country level, T-statistics in parenthesis. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

2.6.2. Sample dependence

An important issue to address is the sensitivity of results to the composition of the sample. While our country sample is vast and therefore unlikely that a given country is driving the results, issues of sample dependence could arise from the firm sample within countries. We start by considering whether the results might be affected by specific country groups. We systematically drop countries belonging to the different IMF regional departments. Table C2 shows that the baseline results are robust to these sensitivity checks only

considering GRA arrangements, except when dropping Europe.[26] On the other hand, in the case of PRGT, the results are weaker when dropping regions like Sub-Saharan Africa or Middle East and Central Asia, since PRGT-programs are more common in these regions. Additionally, the local projection results could be driven by firms which are fast growing, but only for a specific period in time. Similarly, given differences in cross-country coverage in Orbis, our local projection estimates could be driven by firms subject to differing reporting standards or covering more years. In order to capture the average effect on investments over the full 5-year horizon, we reduce the sample by only including firms for which we have balance sheet data on investments over the full period. In the case of IMF arrangements, given the prevalence of programs that go offtrack, we also consider a subsample where the we drop offtrack programs. As Table C3, in Appendix C, shows, we find that the results are consistent with the baseline estimates reported in Table 1.
Lastly, we consider an alternative specification. In particular, we estimate the response of firm investments to Fund program approval considering firm age as a proxy for financial frictions, as indicated by the literature (Gertler 1988; Hadlock and Pierce 2010; Cloyne et al. 2018; Bahaj et al. 2019).[27] Especially in developing countries, firm age has been shown to be an appropriate proxy, where given less developed financial markets, younger firms are more leveraged, less liquid, and smaller in size.[28] More specifically, we split the sample into firms which are above vs. below the median age of firms. The rest of the specification follows the baseline model. Figure C1, in Appendix C, shows the AIPW average treatment effect for the two groups. Consistently with the baseline results, for both PRGT and GRA programs, younger, more financially constrained firms benefit more from an IMF arrangement. In the case of GRA, five years after the program approval, there is also a positive but much smaller effect for mature firms, while in the case of PRGT programs we find no such effects.

2.7. Conclusions

This paper provides new evidence on the role of IMF programs in stimulating firm investments. Using detailed firm-level data on tangible fixed assets, we estimate the dynamic response of firm investments to the approval of an IMF program. We find that distinguishing between GRA and PRGT financing matters for the path of investments,

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[26] Since, because our sample includes the European debt crises, it is unsurprising that removing this event attenuates the effect that the Fund may have on investments.

[27] There is an obvious disadvantage to using direct measures of financial frictions such as size, leverage, or liquidity because they respond endogenously to shocks, such as the approval of IMF arrangements, making it difficult to interpret ex-post effects as driven by ex-ante heterogeneity.

[28] It can also be argued that age is not fully exogeneous because of a survivorship bias or changes in ownership– younger firms tend to be more likely to go bust because of those same characteristics just defined or, when they do survive, they are more likely to be absorbed by older, larger firms in M&A operations.

and that GRA programs seem to induce a stronger investor reaction. Moreover, leveraging a DiD methodology, we document the presence of three potential channels through the reduction of policy uncertainty associated with an IMF program conditionality may affect firm investment choices. Specifically, focusing on GRA agreements, we examine the degree of firms' external financial dependence, firms' sectoral uncertainty, and the degree to which a firm is tied to the local economy. We find evidence that private investments are higher for firms relying more on external finance, or those which are exposed to greater uncertainty or for domestic firms.

To sum up, this is the first paper, to the best of our knowledge, documenting the effects of IMF participation on firms' tangible assets. The presence of a private-investment transmission channel helps improve our understanding of the factors determining effectiveness of IMF programs. Future research could focus on alternative mechanisms behind our results, in particular through specific conditionality, and of the implications for public investments.

Appendix

Appendix A: Sample and Descriptive Statistics

Table A1: Variable Descriptions and Sources

Dependent variables (first and second stage)		
GRA	First stage (logit) dependent. Dummy = 1 if country signs GRA program within the first 9 months of the year.	Monitoring of Fund Arrangements (MONA)
PRGT	First stage (logit) dependent. Dummy = 1 if country signs PRGT program within the first 9 months of the year.	Monitoring of Fund Arrangements (MONA)
Investment	Second stage (local projections) outcome. Annual percentage change of tangible fixed assets	BvD Orbis (2021)
Predictors and controls (first and second stage)		
Real GDP growth	GDP in constant prices, annual percent change	World Economic Outlook (October 2021)
Log real GDPPC	Log of GDP per capita in 2017 PPP dollars	World Economic Outlook (October 2021)
Predictors in the first stage only		
Past program	Dummy = 1 for program years when the country has been in a program in the past	MONA; Authors' calculations
Autocracy	Constraints on executive and competitiveness of electoral process: lower indicates less autocratic.	Polity 5 - CSP/INSCR

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	Definition	Sources
GFCF to GDP	Gross fixed capital formation to GDP	World Economic Outlook (October 2021)
Total debt service to GNI	Total debt service as a percent of GNI	World Development Indicators (2021)
Budget surplus	General govt. revenues – general govt. expenditures as a percent of GDP	World Development Indicators (2021); Authors' calculations
Total reserves/imports	Total international reserves in months of imports	World Development Indicators (2021)
Inflation	Annual percentage change in consumer price inflation	World Economic Outlook (October 2021)
Change in reserves	Change in international reserves	World Development Indicators (2021); Authors' calculations
Current account/GDP	Current account balance to GDP	World Economic Outlook (October 2021)
Legislative election	Dummy = 1 if the country had a legislative election in the previous year	Database of Political Institutions (2020)
Log legislative checks	Checks on the executive branch	Database of Political Institutions (2020)
Predictors in the second stage only		
Log claims	Log of claims by depository institutions on the private sector	International Financial Statistics (2021)
Real interest rate	Representative interest rates offered by banks to resident customers adjusted for inflation	World Development Indicators (2021)
Political risk rating	Captures government stability, socioeconomic conditions, ethno-religious tensions, and investment profile of the country: higher values, lower risk.	International Country Risk guide (2021)

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	Definition	Sources
Program years	Dummy = 1 if the country was under a program in a given year (excluding the year of signing)	MONA
Log total assets	Log of total assets	BvD Orbis (2021)
Debt maturity	Ratio of long-term debt to total debt	BvD Orbis (2021)
Leverage	Total debt to total assets	BvD Orbis (2021)
Interest/EBIT	Interest payments over EBIT (earnings before interest and taxes)	BvD Orbis (2021)
Cash flows/TA	Cash flows scaled by total assets	BvD Orbis (2021)
Sales growth	Annual percentage change in sales	BvD Orbis (2021)

Table A2: Summary Statistics

	Observations	Mean	S.d	Max	Min
Dependent					
Tangibles over total assets	277,572	31.08	27.25	100	0
Country controls					
Real PC GDP growth	277,818	3.25	4.28	81.79	-29
Log real PC GDP	277,780	9.91	0.51	11.37	6.63
Log claims by depository institutions	263,772	12.90	2.18	20.12	6.11
Real lending rate	147,993	5.44	9.05	93.92	-25.7
Political Risk Rating	264,879	67.22	10.18	92.50	31
Firm controls					
Log Total Assets	277,816	15.89	1.94	35.73	0.693
Long-term to total debt	231,150	39.26	40.02	100.00	0
Leverage	277,816	19.53	21.91	100.00	0
Interest expense to EBIT	169,973	27.47	437.62	10000.00	0
Cash flow to TA	192,994	8.09	11.15	60.96	-28.2
Sales growth	169,952	14.29	54.29	582.72	-92

Notes: Summary statistics on winsorized sample.

Table A3: Program Completion Status

Program Type	Completed	Off track	Ongoing	Partially completed	Total
PRGT	23	6	-	8	37
GRA	23	10	4	42	79
Others	29	2	4	4	39
Total	75	18	8	54	155

Notes: Tabulation of programs and their final review status as of 2020. For each program type, indicates the number of programs that were completed, offtrack, partially completed, or ongoing, as well as the total number of unique programs. Offtrack is defined as programs that failed to complete more than two reviews, partially entails the completion of more than two but less than the total number of expected final reviews (IMF 2018 Review of Conditionality, 2019). Others refers to precautionary and non-disbursing programs which are not considered in the sample.

Table A4: Yearly Average Firm Investment by Primary NACE Sector

	Year									
	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009
Agriculture, forestry, fishing	-0.37	-0.241	0.196	0.2	0.257	0.046	0.285	0.293	0.033	0.11
Mining and quarrying	-0.127	-0.027	0.215	0.192	0.203	0.162	0.36	0.398	0.21	0.232
Manufacturing	-0.137	0.009	0.188	0.215	0.265	0.073	0.26	0.308	0.013	0.088
Electricity, gas, steam	0.114	0.135	0.184	0.458	0.254	0.204	0.489	0.167	0.03	0.398
Water supply, waste management	-0.106	0.05	0.209	0.228	0.345	0.076	0.304	0.349	0.09	0.117
Construction	-0.156	-0.001	0.265	0.282	0.337	0.111	0.361	0.429	0.113	0.142
Wholesale and retail trade – repair	-0.079	0.141	0.331	0.376	0.419	0.167	0.373	0.412	0.092	0.133
Transport and storage	-0.133	0.077	0.264	0.286	0.306	0.088	0.318	0.362	0.079	0.078
Accommodation and food services	-0.062	-0.012	0.25	0.239	0.25	0.022	0.265	0.271	0.068	0.072
ICT	0.097	0.101	0.254	0.287	0.366	0.126	0.366	0.365	0.065	0.124
Professional, scientific, technical activities	0.007	-0.032	0.274	0.238	0.286	0.083	0.291	0.385	0.053	0.039
Education	-0.019	0.105	0.33	0.373	0.488	0.047	0.412	0.431	0.087	0.087
Arts	-0.094	0.208	0.499	0.415	0.301	0.219	0.385	0.42	0.11	0.109
	2010	2011	2012	2013	2014	2015	2016	2017	2018	2019
Agriculture, forestry, fishing	0.068	0.054	0.132	0.089	-0.005	-0.072	0.137	0.207	0.002	0.038
Mining and quarrying	0.209	0.208	0.316	0.102	0.005	0.005	0.055	0.08	-0.006	0.043
Manufacturing	0.044	0.013	0.135	0.074	-0.004	-0.018	0.041	0.188	-0.003	0.042
Electricity, gas, steam	0.207	0.109	0.254	0.281	0.02	-0.06	0.094	0.293	0.073	0.284
Water supply, waste management	0.022	0.055	0.124	0.103	-0.067	-0.04	0.009	0.172	-0.021	0.023
Construction	0.034	0.047	0.101	0.09	-0.028	-0.017	0.069	0.205	0.01	0.049
Wholesale and retail trade – repair	0.091	0.072	0.167	0.105	0.021	0.005	0.07	0.208	0.031	0.091
Transport and storage	0.028	0.067	0.108	0.079	-0.007	-0.011	0.051	0.151	0.038	0.061
Accommodation and food services	-0.013	0.01	0.047	0.052	-0.045	-0.058	0.007	0.166	-0.006	0.029
ICT	0.084	0.057	0.15	0.105	0	0.001	0.078	0.237	0.029	0.124
Professional, scientific, technical activities	0.043	0.013	0.114	0.061	-0.063	-0.067	0.032	0.194	0.019	0.066
Education	0.102	-0.004	0.069	0.012	-0.122	-0.14	0.262	0.168	0.15	0.014
Arts	0.016	0.037	0.138	0.09	-0.026	-0.006	0.122	0.232	0.102	0.093

Notes: Table shows year-sector firm average for investment. Sectors are NACE Rev. 2 main sections, excluding: Financial, Public administratiod/defense, Real estate, Administrative services, Health and social work, Other service activities, Household activities, and Extraterritorial

Table A5: Panel Summary

Country	Num. Obs.	Unique firms	Country	Num. Obs.	Unique firms
AF	6	1	JO	1,078	90
AL	240	81	KE	390	33
AM	72	24	KN	10	1
AO	4	1	LK	1,062	143
AR	982	149	LR	34	4
BA	9,391	806	LV	5,028	530
BB	29	5	MA	3,672	727
BD	1,550	191	MD	2,251	272
BF	8	3	MK	3,449	526
BG	10,056	1,084	ML	5	1
BO	143	25	MN	1,263	180
BR	6,336	949	MW	46	6
CD	1	1	MX	6,508	1,837
CI	138	21	MZ	23	4
CL	2,200	227	NG	1,161	104
CM	5	1	NI	32	6
CO	16,831	1,801	NP	48	7
CR	47	9	PA	150	21
CV	26	3	PE	683	128
CY	1,401	263	PK	1,385	313
DM	1	1	PL	100,859	9,919
DO	16	5	PT	40,587	3,198
EC	644	142	PY	215	37
EG	2,612	449	RO	45,738	3,614
GA	23	2	RS	23,605	1,783
GH	229	25	RW	7	1
GM	5	3	SN	18	2
GR	17,522	1,501	SV	24	6
GT	35	3	TN	349	40
HR	9,022	762	TR	44,411	7,944
HU	971	171	TZ	57	7
IE	12,024	1,364	UA	11,779	1,761
IQ	509	49	UG	21	2
IS	2,276	241	UY	1,079	296
JM	231	35	ZM	72	8

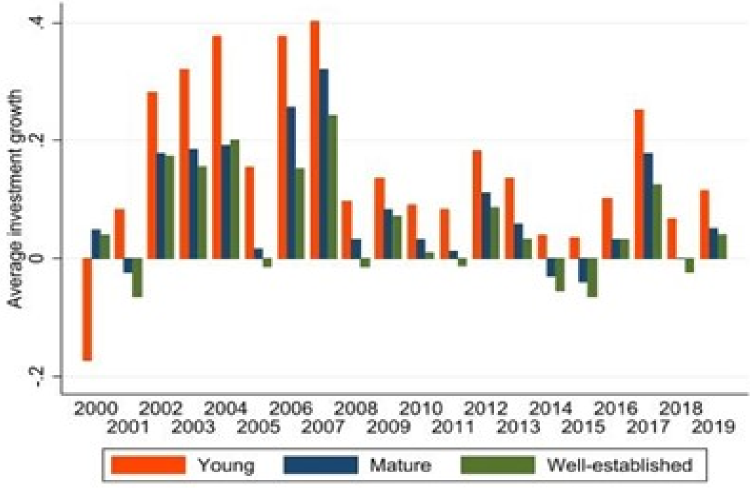
Notes: Panel summary showing the number of observations and unique firms for each 2-digit country ISO code.

Table A6: EFD by Sector

NACE Rev 1.1	Sector	EFD
16	Tobacco	-3.4462
19	Leather and footwear	-1.3422
361	Furniture	-0.5680
22	Publishing and printing	-0.4268
28	Fabricated metal products	-0.3272
35	Other transport equipment	-0.3057
150	Food (excl. beverages)	-0.1454
21	Pulp, paper and paper products	-0.1343
23	Coke and refined petroleum products	-0.1114
26	Non-metallic mineral products	-0.0884
20	Wood products, except furniture	-0.0627
17	Textiles	-0.0427
240	Chemicals (excl. pharamaceuticals)	0.0047
34	Motor vehicles	0.0759
27	Basic metals	0.0870
18	Wearing apparel and fur	0.1021
25	Rubber and plastic products	0.1205
29	Machinery and equipment	0.1255
31	Electrical machinery and apparatus	0.3269
360	Other manufacturing (excl. furniture)	0.3719
159	Beverages	0.3992
30	Office machinery and computers	0.6565
33	Medical/ precision/ optical instruments	1.0336
32	Radio/ TV/ communication equipment	1.1559
244	Pharmaceuticals	8.6029

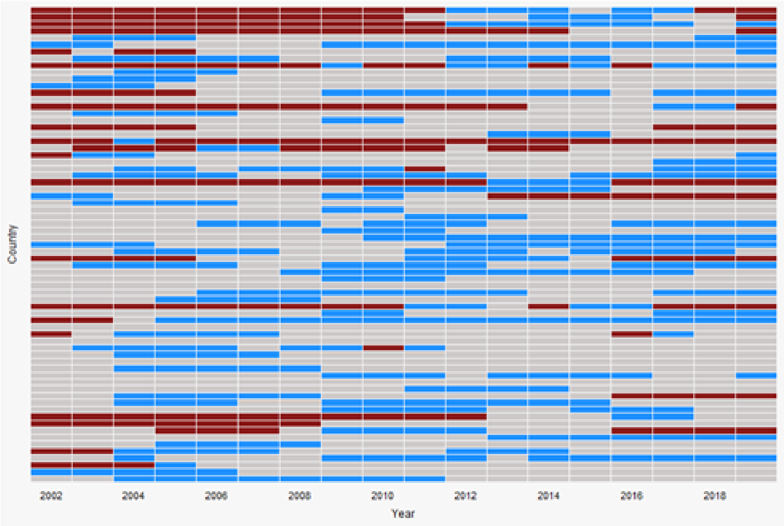
Notes: Eppinger and Neugabauer (2022) EFD indices computed from Compustat according to RZ (1998) methodology.

Figure A1: Average investment by firm age



Notes: Average firm investment growth by firm age. Young firms are between 0 and 14 years old, mature between 15 and 34, well-established are 35 and above. Investment growth is measured as the average per firm-age-category across countries and sectors each year.

Figure A2: Treatment status, by program type



Notes: Treatment status by year for countries in sample. Shaded bars indicate a country is under a given program for a specific year; red for GRA, blue for PRGT. Grey bars indicate no program, while white bars indicate missing years for the dependent variable (tangible fixed assets investments) due to Orbis missing data. Effective treatment status of observations therefore defined by years for which there exists Orbis data for at least one firm for a given country.

Appendix B: Augmented Inverse Propensity Score Weighted Estimator

Table B1: AIPW First Stages

Predictors	GRA	PRGT
Past program	2.195***	2.053***
	(8.041)	(7.343)
Log real GDPPC	0.575*	-0.813*
	(1.831)	(-1.841)
Autocracy	0.119	-0.154
	(0.814)	(-1.122)
GFCF/GDP	-0.097***	0.025
	(-3.431)	(0.867)
Total debt service to GNI	0.027	-0.145**
	(1.469)	(-2.460)
Budget surplus	-0.040	0.138***
	(-0.629)	(3.844)
Total reserves/imports	-0.112*	-0.175*
	(-1.773)	(-1.750)
Real GDP growth	-0.042	-0.097***
	(-0.802)	(-3.309)
Inflation (consumer price)	0.009	0.014
	(0.699)	(0.672)
Change in reserves	-0.006**	-0.000
	(-2.294)	(-0.001)
Current account/GDP	0.088**	-0.023
	(2.166)	(-0.872)
Legislative election	-0.387	-0.990
	(-1.379)	(-1.457)
Log(legislative checks)	-0.092	0.030
	(-0.143)	(0.055)
Observations	806	806

Notes: The model uses predictors listed in Table A2 in the first stage and region dummies as fixed effect. T-statistics in parenthesis, standard errors clustered at the country level. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B2a: AIPW Estimates, Second Stage (GRA)

	1	2	3	4	5
GRA dummy	0.122*	0.119	0.147*	0.115*	0.102**
	(2.91)	(1.98)	(3.01)	(2.27)	(3.58)
Lagged investments	-0.024*	-0.028*	-0.012	-0.016	-0.005
	(-2.35)	(-2.90)	(-1.12)	(-1.39)	(-0.62)
GRA years	0.122**	0.121*	0.093**	-0.022	-0.006
	(3.52)	(2.84)	(3.25)	(-0.59)	(-0.10)
Log (total assets)	-0.037*	-0.042	0.021	0.083**	0.121**
	(-2.54)	(-1.36)	(0.82)	(3.11)	(3.60)
Long term debt/total	0.001	0.022	0.011	0.012	0.035
	(0.05)	(0.94)	(0.31)	(0.29)	(0.50)
Leverage	0.068**	0.077	0.073*	0.171**	0.163***
	(3.31)	(1.53)	(2.72)	(3.50)	(11.01)
Interest coverage	0.002	0.002**	0.001	0.002	0.004
	(0.74)	(3.80)	(0.18)	(1.67)	(1.85)
Cash flows/TA	-0.048	-0.089	-0.121*	-0.124*	-0.089
	(-0.70)	(-1.33)	(-2.42)	(-2.57)	(-1.44)
Sales growth	-0.045**	-0.074***	-0.057***	-0.077**	-0.105***
	(-4.25)	(-5.03)	(-4.97)	(-3.67)	(-4.65)
Real GDP growth	0.001	-0.001	0.001	0.005	-0.002
	(0.17)	(-1.39)	(0.02)	(1.05)	(-0.29)
Real GDPPC	0.528**	0.139	0.388	0.475	1.015
	(3.08)	(0.97)	(1.08)	(0.94)	(2.11)
Bank claims	-0.079***	0.009	0.131	0.049	-0.004
	(-4.73)	(0.14)	(1.72)	(0.40)	(-0.04)
Real interest rate	0.004	-0.001	0.003	0.005	0.009
	(1.41)	(-0.07)	(1.22)	(1.45)	(1.80)
Political risk rating	-0.008*	-0.001	0.001	0.013	0.009
	(-2.56)	(-0.10)	(0.16)	(1.07)	(0.74)
R-squared	0.425	0.454	0.468	0.562	0.598
N	21817	18560	15900	13685	11899

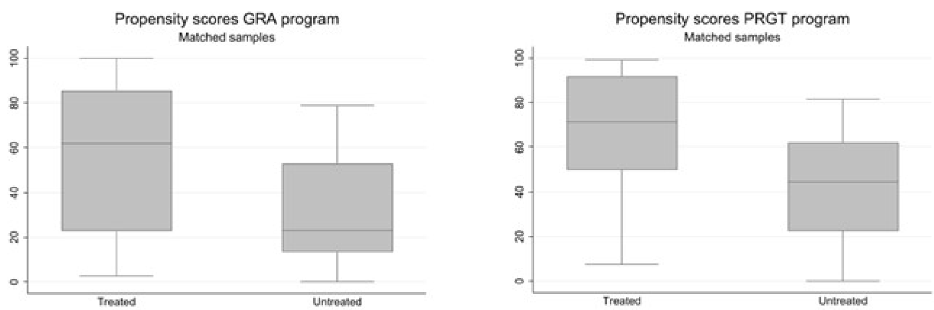
Notes: Control coefficient estimates for second stage regression in AIPW estimates, baseline model. Standard errors clustered at the country-sector level, T-statistics in parenthesis. * p < 0.1, ** p < 0.05, *** p < 0.01.

Table B2b: AIPW Estimates, Second Stage (PRGT)

	1	2	3	4	5
PRGT dummy	0.123	0.112	0.026	-0.061	0.021
	(2.10)	(1.61)	(0.36)	(-0.61)	(0.19)
Lagged investments	-0.012	-0.025**	-0.015	-0.009	-0.004
	(-1.54)	(-3.07)	(-1.79)	(-1.06)	(-0.80)
PRGT years	0.037	-0.071	-0.098	-0.087	-0.015
	(1.56)	(-1.63)	(-1.71)	(-0.93)	(-0.17)
Log (total assets)	-0.017	-0.011	0.056**	0.085***	0.137***
	(-2.08)	(-0.46)	(3.82)	(4.47)	(5.17)
Long term debt/total	0.012	0.023	0.006	0.014	0.009
	(0.64)	(1.57)	(0.30)	(0.41)	(0.17)
Leverage	-0.018	0.004	0.044	0.091*	0.098**
	(-0.52)	(0.07)	(0.91)	(2.61)	(3.77)
Interest coverage	0.001	0.001	-0.001	0.001	0.003
	(0.46)	(0.31)	(-0.46)	(0.55)	(1.46)
Cash flows/TA	-0.055	-0.191***	-0.211**	-0.228*	-0.159*
	(-1.01)	(-7.23)	(-3.16)	(-2.82)	(-2.86)
Sales growth	-0.041**	-0.054***	-0.059**	-0.073***	-0.093**
	(-4.13)	(-4.47)	(-3.72)	(-4.73)	(-4.17)
Real GDP growth	-0.002	-0.002	-0.003	-0.002	-0.007
	(-1.34)	(-0.61)	(-0.49)	(-0.62)	(-1.88)
Real GDPPC	0.079	0.054	0.179	0.488	0.667
	(0.61)	(0.27)	(0.44)	(0.89)	(1.24)
Bank claims	-0.009	0.087	0.077	0.041	0.011
	(-0.36)	(1.21)	(0.92)	(0.42)	(0.11)
Real interest rate	0.002	0.002	0.004	0.001	0.004
	(0.69)	(0.72)	(1.53)	(0.74)	(0.89)
Political risk rating	-0.001	-0.004	0.001	0.004	0.005
	(-0.00)	(-0.57)	(0.17)	(0.45)	(0.68)
R-squared	0.127	0.196	0.228	0.266	0.295
N	21817	18560	15900	13685	11899

Notes: Control coefficient estimates for second stage regression in AIPW estimates, baseline model. Standard errors clustered at the country-sector level, T-statistics in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure B1: Balance tests for propensity scores



*Notes:*Plots show the estimated propensity scores for different outcome levels in the first stage multinomial logit model, where untreated is the base value "no program" and treated is either GRA or PRGT.

Appendix C: Alternative Specifications

Table C1: Anticipation Effects

			GRA		
Years to program	-5	-4	-3	-2	-1
Effect on investment growth	0.03	0.01*	-0.03***	0.01	0.01
	(1.56)	(1.75)	(-2.99)	(1.17)	(1.02)
N	27,585	27,585	27,585	27,585	27,585
			PRGT		
Years to program	-5	-4	-3	-2	-1
Effect on investment growth	0.01	0.02	-0.02	-0.03***	0.04
	(1.00)	(0.91)	(-0.67)	(-2.85)	(1.17)
N	27,585	27,585	27,585	27,585	27,585

Notes: Change in firm tangibles/TA investment rate in the h years leading up to program approval, with h=1,2,3,4,5. Model is a fixed effects regression with baseline controls, firm and sector-year fixed effects. Standard errors clustered at the country level, T-statistics in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: AIPW Dropping Regions

Region	*GRA* (1)	(2)	(3)	(4)	(5)
Asia Pacific	-0.09	1.24**	2.20***	4.44***	3.63**
	(-0.96)	(2.78)	(3.10)	(3.67)	(2.65)
N	20839	18284	15938	13908	12305
Europe	-0.02	0.29	-1.88	-5.13**	0.05
	(-0.06)	(0.19)	(-0.99)	(-2.98)	(0.02)
N	2228	1836	1483	1211	982
Mid. East & Cent. Asia	-0.04	1.21**	2.30***	4.61***	3.93**
	(-0.53)	(2.46)	(3.31)	(3.76)	(2.43)
N	21072	18579	16187	14054	12360
SSA	-0.13	0.99***	1.81***	4.10***	3.25**
	(-0.92)	(3.03)	(3.03)	(3.56)	(2.24)
N	21450	18840	16375	14213	12502
West. Hemisphere	-0.23***	1.46***	2.45***	3.61***	2.57**
	(-4.42)	(3.62)	(3.89)	(4.44)	(2.30)
N	20943	18438	16059	13933	12252
Region	***PRGT*** (1)	(2)	(3)	(4)	(5)
Asia Pacific	3.17***	2.48*	2.94	-2.30	1.58
	(5.74)	(1.76)	(1.73)	(-1.13)	(0.97)
N	20839	18284	15938	13908	12305
Europe	0.98*	-2.91	-4.60*	-4.91**	-13.27*
	(1.88)	(-1.74)	(-2.05)	(-2.25)	(-2.09)
N	2228	1836	1483	1211	982
Mid. East & Cent. Asia	0.72	0.61	-0.45	-1.55	0.51
	(1.42)	(0.60)	(-0.31)	(-0.88)	(0.26)
N	21072	18579	16187	14054	12360
SSA	-0.01	0.17	-0.97	-0.91	0.01
	(-0.03)	(0.19)	(-0.81)	(-0.52)	(0.01)
N	21450	18840	16375	14213	12502
West. Hemisphere	1.10**	0.95	-0.36	-0.97	0.34
	(2.52)	(0.90)	(-0.24)	(-0.70)	(0.21)
N	20943	18438	16059	13933	12252

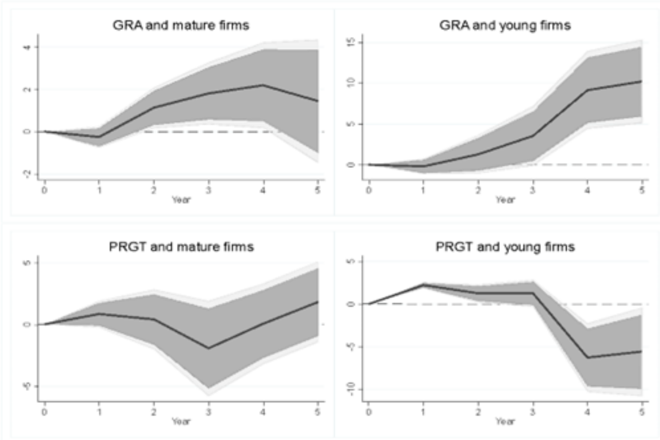
Notes: AIPW average treatment effects for each time horizon h=1,2,3,4,5 when region m is dropped. Regions correspond to IMF Regional Department groups. Standard errors clustered at the country-sector level, T-statistics in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C3: AIPW Robustness Tests

	GRA				
	1	2	3	4	5
Spell length	-0.131	1.094**	2.175**	4.173**	3.144*
	(-1.69)	(2.99)	(3.35)	(3.81)	(2.27)
N	21642	18941	16448	14281	12551
No offtrack	-0.113	1.073*	2.151**	4.075**	3.718*
	(-1.40)	(2.18)	(3.18)	(3.94)	(2.86)
N	21643	19002	16516	14337	12608
	PRGT				
	(1)	(2)	(3)	(4)	(5)
Spell length	1.072*	1.002	-0.296	-1.450	0.198
	(2.30)	(1.01)	(-0.21)	(-0.86)	(0.12)
N	21642	18941	16448	14281	12551
No offtrack	0.824	0.637	1.220	0.349	2.548
	(1.62)	(0.63)	(0.90)	(0.23)	(1.50)
N	21643	19002	16516	14337	12608

Notes: AIPW estimators for each time horizon h=1,2,3,4,5 under different conditions. Spell length restricts the sample to firms with a series of yearly observations spanning at least 5 years to cover the full projection horizon. No offtrack drops programs from the treatment dummy that were classified as off track. No advanced drops countries from the 2010 European Union sovereign debt crisis that required IMF intervention. Standard errors clustered at the country-sector level, T-statistics in parenthesis. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure C1: AIPW and firm age



Notes: AIPW average treatment effects of program signing on firm tangible fixed assets investment rate for groups of firms based on age. Firms are divided into two groups: mature firms are those with above-median age, young firms below-median age. Areas indicate 90 and 95% confidence intervals, standard errors are clustered at the country level.

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Chapter 3

Is to forgive to forget? Sovereign risk in the aftermath of private or official debt restructurings

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Abstract

We examine the link between sovereign defaults and credit risk by distinguishing between commercial and official debt and by taking into account the extent of the final restructuring events, which take place at the end of a default spell. We use a local projection based approach, combined with propensity score weighting (Jordà and Taylor 2016), to estimate the average treatment effect of the final restructuring on our outcome variables of agency ratings and bond yield spreads. Our results show that the average treatment effect on ratings is negative (and positive for bond spreads) up to seven years following the final restructuring with private creditors, while the opposite holds for official creditors. Furthermore, our results are robust to using a panel analysis, which allows us to investigate the importance of the final haircut size. Specifically, we find that the rating (spread) variation (increase) is larger for cases with deeper haircuts. Therefore, we find evidence that official and private defaults have different costs and then may induce selective defaults.

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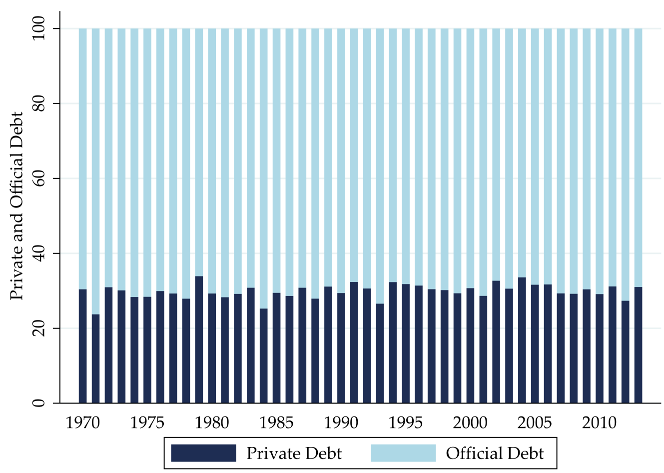
3.1. Introduction

In the wake of adverse global shocks such as the Covid-19 pandemic or recent geopolitical crises, a prolonged economic slowdown looms (particularly for developing countries) and hence a series of sovereign debt restructurings are foreseeable in the coming years, including those with official creditors. As recently shown by Horn *et al.* (2020), official lending is much larger than generally assumed (see Figure 1), often surpassing total private cross-border capital flows, especially in times of global turmoil when these flows generally shrink.[1] Although official debt accounts for a substantial share of total sovereign debt (especially in developing countries) and is expected to rise in the future, there is still relatively little research on the relative treatment of official versus private defaults. Given the historical evidence on the interplay between the two, it is of interest to evaluate how financial markets, in particular bond markets and credit rating agencies, react to different restructuring episodes by distinguishing between private and official events.

Figure 1: Share of private and official debt



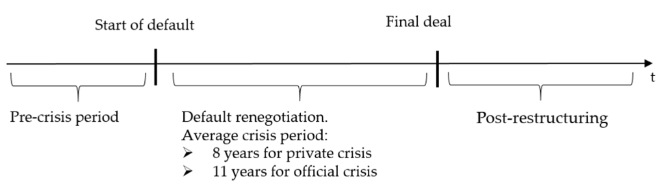
Despite a renewed interest in the role of official creditors (e.g., Lang *et al.* 2021; Mitchener and Trebesch 2021; Schlegl *et al.* 2019), not enough is yet known about the implications of debt restructurings involving official creditors. In particular, Lang *et al.* (2021) find that the official debt service suspension guaranteed by the Debt Service Suspension Initiative (DSSI) in May 2020 induced a larger decline in borrowing costs for countries eligible for

[1] For example, during the Eurozone crisis (2010-2012), private international lending was replaced by official international loans and the governments of Cyprus, Greece, Ireland Portugal and Spain received official funds from both the International Monetary Fund and the newly created European Financial Stability Facility (now the European Stability Mechanism).

this initiative, compared to similar but ineligible countries.[2] Instead, what happens when countries exit a debt crisis and whether the related net present value variation leaves residual stigma in financial markets is a separate issue.

This paper aims to fill this gap by documenting the relationship between sovereign debt restructurings and a country's credit risk, looking at how borrowing costs vary in the aftermath of a default.[3] In particular, we take both an indirect and a direct measure of borrowing costs, namely agency ratings and bond yield spreads. Compared to bond spreads, credit ratings are available for a larger set of countries and are a reliable measure in times of crisis. Moreover, as a consequence of the Covid-19 crisis, credit-rating agencies are likely to be put under the spotlight, as is normally the case during downturns.[4] We take restructurings - and not default - as our main explanatory variable. Restructurings typically take place at the end of a renegotiation spell, which may take years after the default occurs.[5] Figure 2 describes the timeline we consider for our analysis. Given the scope of the paper, we distinguish between *official* and *private restructurings.* More specifically, *official restructuring* stands for agreements reached with official creditors (in the Paris Club of official creditors).[6] In contrast, *private restructuring* denotes a restructuring deal with external private creditors (foreign banks and bondholders).

Figure 2: Crisis timeline



[2]The World Bank and the International Monetary Fund urged G20 countries to establish the DSSI, which took effect on 1st May 2020, and has been extended until the end of 2021. It has so far delivered about $5 billion in relief to more than 40 countries (out of 73 eligible countries). The DSSI is a form of debt relief that eases financing constraints through liquidity provision by deferring debt service repayments without affecting the NPV of public debt.

[3]While this paper speaks to the potential benefits from the side of the debtor in strategic interaction between actors of debt restructuring arrangements, work by Andritzky and Schumacher (2019) also highlights how private debt restructurings may not always be a net loss for creditors.

[4]During the last financial crisis, they were accused of accelerating the euro-zone sovereign-debt crisis by downgrading some of the bloc's big economies, including France (The Economist 9th May 2020). Recent papers have examined the reaction of credit agencies and bond markets to the last financial crisis (e.g., Born *et al.* 2020, Daehler *et al.* 2020, Hale *et al.* 2020, Kempf and Tsoutsoura 2020).

[5]In a recent paper, Meyer *et al*. (2020) show that default episodes take, on average, seven years to resolve and that they typically involve multiple restructurings.

[6]The Paris Club is an informal forum of the most important official creditor countries, which was designed in 1956 to deal with the payment difficulties of debtors.

We should emphasize that this paper does not provide an evaluation of the effects of an official restructuring on a country's overall (both official and private) financing conditions. Instead, our aim is to assess the ability of a defaulting country to tap into private capital markets in the aftermath of a debt crisis. For this reason, we consider two commonly used and relevant variables as outcomes following a default with either private or official creditors, acknowledging that they are both measures that are naturally "skewed" towards capturing the reactions of private creditors. Focusing on private market financing conditions allows us to compare more fairly the effects of private and official restructurings, at the same time, some of the differential effects of official vs private restructurings might arise from the way costs are being measured.

We add to previous works by comparing the rating outcome of official and private restructurings, hence primarily contributing to the empirical literature on official debt. To the best of our knowledge, it is the first time in this literature that the distinction between private and official deals, as well as the occurrence and magnitude of a default, are taken into account in the context of agency ratings and bond spreads. Our results may then provide some insight for the debate on the consequences of debt heterogeneity, which introduces the possibility for governments to operate selective defaults discriminating across investors (e.g., Erce and Mallucci 2018; D'Erasmo and Mendoza 2021).[7]

Sovereign credit ratings can be interpreted as a forward-looking summary indicator of macroeconomic and (often) political conditions, as these affect repayment prospects and tend to be highly correlated with borrowing costs.[8] These measures explicitly pertain to a sovereign's ability (and willingness) to service financial obligations to non-official (commercial) creditors. Hence, they are "biased" in favor of measuring the probability of default on debt owed to private creditors. Understanding how rating agencies and institutional investors evaluate the repayment ability towards official creditors is not straightforward. This depends on how visible official debt risk is and how rating agencies incorporate it into their rating models.

From official documentation, rating agencies seem to evaluate official risk only to the extent to which it can also affect the repayment prospects of government obligations to the private sector, due to the preferred creditor status associated with many official claims (e.g.,

[7] Erce and Mallucci (2018) assume that a government issues debt both domestically and abroad and can operate selective defaults between the two types of debt. Using new data on the legal jurisdiction of sovereign defaults (hence distinguishing between defaults under domestic law and defaults under foreign law), they show that selectiveness is the norm. D'Erasmo and Mendoza (2021) take a novel approach by building a model in which the government chooses optimal debt and default on domestic and foreign creditors by taking the distributional consequences of a default into account.

[8] Cantor and Packer (1996) were among the first to focus on the relationship between default history and credit ratings, finding that countries that defaulted after 1970 are associated with a significant drop in a country's credit rating.

DBRS 2018).[9] In other words, official debt seems to be generally perceived as problematic, and hence adversely affecting sovereign rating, only to the extent to which arrears to official creditors may indicate growing financial distress and/or lack of willingness to pay, which eventually impacts private repayments as well. Moreover, the Paris Club includes a *comparability of treatment* clause, which aims to ensure a balanced treatment of the debtor country's debt by all external creditors.[10] Despite this caveat, we still believe that showing the heterogeneous treatment of creditors in the event of default is important as it could help to shed light on what precisely are the costs of default to a sovereign country.
Analyzing 130 final restructurings episodes over the 1990-2018 period, we use both the Adjusted Inverse Propensity-score Weighted (hereafter AIPW) estimator, which consists in a propensity-score based method combined with local projections (Jordà andTaylor 2016), and standard panel data analysis. Since the choice to enter into a restructuring is contingent on the country's economic conditions, this methodology allows us to explicitly model and account for the endogeneity of a default episode. The AIPW estimator proceeds in two stages: the first stage estimates a propensity score for each observation in the sample, while the second stage rebalances the sample and estimates the Average Treatment Effect (hereafter ATE) using a conditional local projection. Our results show that commercial and official defaults are associated with different outcomes: while the average treatment effect on ratings is negative (and positive for bond spreads) over the seven years following the final restructuring with private creditors, the opposite holds with official creditors.
In the second part of the paper, we use a panel analysis to explicitly take into account a measure of the severity of the debt crisis, such as the creditors' loss (or haircut), as in Cruces and Trebesch (2013a).[11] On the one hand, default involving larger haircuts may entail more severe reputational costs. On the other hand, a channel of debt relief operates in the opposite direction (Krugman 1988). The overall impact is then theoretically ambiguous and remains an empirical question. Using monthly data on average ratings of eight rating

[9] Such preferred status, however, is not supported by a recent paper of Schlegl *et al.* (2019). While confirming that multilateral institutions are senior creditors, they show that official bilateral debt is junior, or at least not senior, to bank loans and bonds.

[10] More specifically, in accordance with this clause, debtor countries should seek from other official bilateral creditors (that are not members of the Paris Club) and private creditors a treatment on comparable terms to those granted in the Paris club. Debtors are also required to share with the Paris Club the results of their negotiations with other creditors. Seeking comparable terms with the Paris club, however, does not necessarily imply being able to obtain them. Timing is also crucial as rating agencies may consider an agreement with the Paris Club a negative (or positive) event depending on whether it is (or not) followed by a private deal. In a similar vein, they may positively evaluate a private agreement, which is directly followed by an official one that may contribute to reducing the overall debt burden.

[11] Recent papers (e.g., Arellano et al. 2019; Amador and Phelan 2021) present theories of sovereign default able to rationalize the large heterogeneity in debt crises, which are typically partial and vary in their duration. Yue (2010) theoretically investigates sovereign default and the role of debt renegotiation in sovereign debt markets. Consistent with the empirical evidence, the model predicts that interest rates and haircuts increase with the level of debt.

agencies and 130 countries, we find that private defaults seem to involve some reputational costs up to seven years since the last agreement, while official defaulters may even benefit from the present value reduction. Using bond spreads as a dependent variable, we confirm the results of Cruces and Trebesch (2013a) in the case of private haircuts, while we find that spreads fall for up to seven years after final official deals.

Thus, our main result is that private credit events are more costly than official ones when it comes to sovereign risk. Moreover, the rating (spread) variation (increase) is larger for cases with deeper haircuts, which are both new results.

Even if our results may depend on how rating agencies incorporate official risk into their rating models, they are important because they document that the costs of default vary with the amounts of debt and the type of creditors affected.[12] In particular, the higher cost of private defaults is most likely driven by a less creditor-friendly negotiation process, which in turn results in higher economic uncertainty and more severe punishment from the creditors.[13] On the other hand, official restructurings arranged within the "Paris Club umbrella" should guarantee a smoother approach in how deals are orchestrated, hence lowering the collateral damage of such an event.[14] Moreover, while an official default often occurs without much media coverage, defaulting on private debt is more visible and hence more likely to result in a rating downgrade. Finally, new evidence (Horn *et al.* 2020; Schlegl *et al*. 2019) suggests that official lenders typically shoulder the burden for private creditors, which could explain why we find evidence of positive market sentiment in the aftermath of an official restructuring.

Sovereigns, being aware that the consequences of a default depend in important ways on who the defaulted creditors are and what bargaining power each creditor group has, may then decide to prioritize their repayments accordingly. These results are consistent with Schlegl *et al.* (2019), who find evidence of seniority for multilateral institutions but not for official (Paris club) bilateral debt.[15] What is more, private creditors seem to "free ride" on official ones: they are typically paid first and lose less than bilateral official creditors. Thus,

[12]The importance of the way in which restructurings are actually arranged, at least for private defaulters, is also confirmed by the results of Asonuma and Trebesch (2016), Trebesch and Zabel (2017) and Asonuma *et al.* (2019), who find that less confrontational (or preemptive) restructurings are associated with a lower output loss as compared to soft (non-preemptive) defaults.

[13]The literature on sovereign debt has recently investigated different dimensions of default costs, including legal aspects. In particular, Schumacher *et al.* (2021) show that legal developments have strengthened the hands of private external creditors and raised the cost of default for debtors.

[14]As argued by Tomz (2007), concerns about reputation sustain international lending and repayments. Hence, any measure that would help to reinforce such reputational mechanism between debtors and creditors is particularly important as it would help investors distinguish between excusable defaults and inexcusable ones (e.g., Grossman and Van Huyck 1988).

[15]Paris Club restructurings cover just one form of official sector financing. Non-Paris Club debt (e.g., by China) has become much more prominent in recent years, and the European sovereign debt crisis has also brought about large-scale restructurings of official debt that did not go through a Paris Club procedure. Hence, we should highlight here that our results do not necessarily extend to these other kinds of official creditors.

the increase in borrowing costs we detect after private restructurings may explain why private creditors are typically paid first. To summarize, documenting this difference can help shed light on why countries default with certain creditors. As official lending is likely to increase and official debt sustainability is becoming again a topic of concern, understanding the difference between private and official deals will be even more important.[16]

The empirical literature on sovereign defaults has generally found that default costs are difficult to quantify and short lived.[17] Only recently, thanks to novel measurement strategies of a country's repayment record, persistent effects of default can be precisely detected, bringing the empirical results in line with the effects of a default according to the theory. This paper then contributes to the (empirical) literature on default costs. In particular, to the emerging literature on the characteristics and the economic relevance of debt restructurings, from both a private sector perspective (Asonuma and Trebesch 2016, Asonuma *et al.* 2021, Asonuma and Joo 2021; Forni *et al.* 2016; Kuvshinov and Zimmermann 2019; Meyer *et al*. 2019, Reinhart and Trebesch 2016; Schlegl *et al.* 2019; Schumacher *et al.* 2021; Trebesch and Zabel 2017) and an official sector perspective (Cheng *et al*. 2017, 2018a, 2018b; Corsetti and Erce 2018; Lang *et al*. 2021; Marchesi and Masi 2020, 2021; Reinhart and Trebesch 2016).[18]

The rest of this paper is organized as follows. Section 2 describes the data. Section 3 describes the inverted-propensity score local projection approach, and section 4 provides the AIPW results. Section 5 presents the results of the panel analysis, while section 6 contains a discussion of the findings, as well as some additional evidence on the importance of litigation costs. Section 7 concludes.

3.2. Data

In the case of private creditors, we rely on the original data by Asonuma and Trebesch (2016), which provide information on the start and end date of defaults with private

[16] Despite our data end only in 2018, and hence we cannot give specific answers as the current crisis is concerned, nevertheless our results may provide some insights derived from the most recent historical evidence.

[17] This literature has mainly looked at the effects of sovereign defaults on international trade (e.g., Rose 2005, Borensztein and Panizza 2010, Broner *et al*. 2010), international credit markets (e.g., Borensztein and Panizza 2009, Gelos *et al*. 2011 and Panizza *et al*. 2009), and GDP growth (Sturzenegger and Zettelmeyer 2008, Borensztein and Panizza 2009, De Paoli *et al.* 2009, Levy Yeyati and Panizza 2011), finding, overall, short lived effects of sovereign defaults. For a survey of this literature, see Panizza *et al.* (2009) and Tomz and Wright (2013).

[18] In a companion paper, Marchesi and Masi (2020) find similar results using the Institutional Investor's index as the dependent variable and a synthetic control method (SCM). Due to data limitations, they could only apply this method to the ratings provided by the Institutional Investor Magazine, but not to agency ratings, which are only available since the '90s. What is more, while the SCM allows to contrast the rating outcome of either private or official defaulters, the local projection analysis allows us to enlarge the sample by considering countries defaulting with both types of creditors, as well as to take the severity of the default into account.

creditors. As a proxy for the severity of the debt restructuring, we consider the corresponding present value reduction, or "haircut." We rely on the original dataset by Cruces and Trebesch (2013b) for the data on debt restructurings with foreign private creditors (i.e., commercial banks and bondholders). These data exclude debt restructurings that mainly affected domestic creditors. Focusing on foreign creditors makes sense for different reasons, one of which being that we wish to measure a financial market effect that is not heavily influenced by domestic events. This dataset provides a list of 187 distressed sovereign debt restructurings with external banks and bondholders that occurred between 1970 and 2013. It includes information on the amount of debt restructured, the face value reduction, and a measure of debt relief (Preferred Haircut HSZ) computed by the authors considering the present value of both old and new debt instruments.

For official debt restructurings, we rely on the Paris Club data (collected by Cheng *et al*. 2017), which contains 429 sovereign debt restructurings with the Paris Club between 1956 and 2015.[19] Paris Club creditors may provide (official) debt treatments to debtor countries in the form of rescheduling (i.e., debt relief by postponement of debt service payments) or, in the case of concessional rescheduling, reduction in debt service obligations during a defined period (flow treatment) or as of a set date (stock treatment). The restructuring approach of the Paris Club has evolved over time. In the 1980s, negotiations took place on a case-by-case basis and focused on short-term liquidity problems, mostly implementing maturity extensions without nominal debt reduction. During the 1990s and 2000s, especially for low-income countries, restructurings became increasingly concessional, including debt stock cancellations. Specifically, as low-income countries are concerned, the possibility of a partial debt stock cancellation of non-ODA debt was gradually extended from 33 percent of the eligible debt in 1988 (Toronto Terms) to 50 percent in 1991 (London Terms) and 66 percent in 1994 (Naples Terms). In 1996, the World Bank and the IMF implemented the Heavily Indebted Poor Countries (HIPC) Debt Initiative, which was first strengthened in 1999, and, more recently, in 2005, when, under the Multilateral Debt Relief Initiative (MDRI) multilateral institutions were encouraged to increase their specific contribution to debt reduction. Debt relief at the completion point under the HIPC Initiative is provided within the HIPC Exit Terms.

Following Cheng *et al.* (2017), by looking at the terms of treatment (reported in Table 3 of their paper), we were able to compute proxies of the present value reduction for official deals, and to compare this value with the corresponding haircut measure in the case of private agreements (or *Preferred Haircut HSZ)* used by Cruces and Trebesch (2013a).[20] We should emphasize that due to important differences in the way in which private and official

[19]To supplement information on the start/end of the debt crisis, we also rely on Beers and Mavalwalla (2018).

[20]Cheng *et al.* (2017) provide an overview of the different terms and report the net present value relief associated with the different Paris Club Terms of Treatment over the years. In some cases (i.e., for some of the *ad hoc* agreements), we had to calculate the net present value relief by directly looking at the Paris Club documentation (http://www.clubdeparis.org/en/traitements).

haircuts are computed, the comparison between the two can only be indicative and should be taken with caution.[21]

Our sample includes a maximum of 130 developing countries. Since the data on private debt restructurings are available only up to 2013, in the panel analysis, our year sample ends then. It includes 68 defaulting countries that experienced at least one debt crisis during the sample period and 62 non-defaulters. Among defaulters, 47 countries had both private and official debt restructurings, 14 countries had only official restructurings (through the Paris Club) while 10 countries experienced only private defaults.[22] Table A1a, in the online Appendix A, shows all countries and years, including a list of debt crisis episodes studied here.

Table 1: Haircuts over time (in percent)

	Observations	Mean	SD	Min	Max
Private haircut					
1970-1988	81	23	53	-10	93
1989-2001	57	43	26	-8	92
2002-2013	20	53	31	5	96
Official haircut					
1970-1988	1	33	0	33	33
1989-2001	71	58	20	12	100
2002-2013	34	77	28	4	100

Table 2: Haircuts by country's income (in percent)

		High Income	Middle Income	Low Income
Haircut %				
	Private	27	33	53
	Official	100	65	62
# of restructuring countries				
	Private	7	42	5
	Official	1	22	9

[21] Most importantly, while Cruces and Trebesch (2013b), following Sturzenegger and Zettelmeyer (2006, 2008), use a specific discount rate for each restructuring, Cheng *et al.* (2017) consider the different Paris Club treatment terms, such as Toronto (33%), London (50%), Naples (67%), Lyon (80%) and Cologne (90%), without using a discount factor.

[22] The group of official defaulters includes Angola, Benin, Burkina Faso, Cambodia, Comoros, Egypt, El Salvador, Georgia, Ghana, Guatemala, Indonesia, Kyrgyz Republic, Myanmar, Sri Lanka. Only private defaulters are Argentina, Belize, Greece, Iraq, Paraguay, Serbia, Slovenia, South Africa, Uruguay and Venezuela.

Table 1 shows summary statistics on haircuts for different subperiods in the full sample of 264 restructurings.[23] While the average haircut is about 34 percent over the full sample mean, looking at the three different subperiods, we detect a sizeable increase in this amount over time. Average haircut size is more than double during the last subperiod (2002-2013), as compared to the initial period (1970-1988), and about 20 percent higher with respect to the intermediate one (1989-2001).
As official restructurings are concerned, we find that the average haircut over the entire period is about 64 percent, much higher than the corresponding average for private.[24] Looking at the three different subperiods, we also find an increase in their size over time. The average haircut size during the last subperiod (2002-2013) is more than two times the average haircut implemented during the initial period (1970-1988) and almost double with respect to the average size of the intermediate period (1989-2001).
Figure 3 shows the evolution over time of both private and official haircuts (in percent). As can be seen, while private agreements were more common up to the mid-nineties, Paris club deals have prevailed in more recent years. Moreover, the haircut size tends to be much higher under official deals.
Table 2 presents summary statistics of private/official haircuts according to a country's income. As the number of countries is concerned, we find that middle-income countries tend to default more with both types of creditors, while low-income countries tend to benefit from the highest average haircuts.[25] Finally, Figure 4 reports the frequency by size of private and official haircuts (in percent). As can be seen, the distribution of the private haircuts is generally smoother, peaking around 90 percent during the HIPC initiative. On the other hand, official haircuts follow a multimodal distribution, where the multiple peaks correspond to the different Paris Club treatment terms.

3.2.1. Dependent variables

Our main proxy to measure a country's creditworthiness is its sovereign long-term foreign-currency rating. As shown by Reinhart (2002), ratings predict defaults. Hence, this makes them an informative measure of creditworthiness for countries with severe payment problems. Moreover, ratings may also represent a ceiling for the credit rating of private companies from the respective country (Borensztein *et al*. 2013). Finally, they may also capture the private sector's ease of access to foreign capital (Gehring and Lang 2020) as well as representing a good proxy for a country's access to international financial markets.[26]

[23]Among those, 158 episodes involved restructuring with private creditors, while 106 involved deals with official creditors.
[24]As said, in order to compare the two types of defaulters, we only consider official restructurings that were agreed until 2013, which is the last year for which we have information about the size of private restructurings.
[25]The only high-income country which receives an official haircut of 100 percent was Seychelles in 2009.
[26]Afonso *et al.* (2012) related ratings to changes in government bond spreads.

Figure 3: Private and official haircuts over time (in percent)

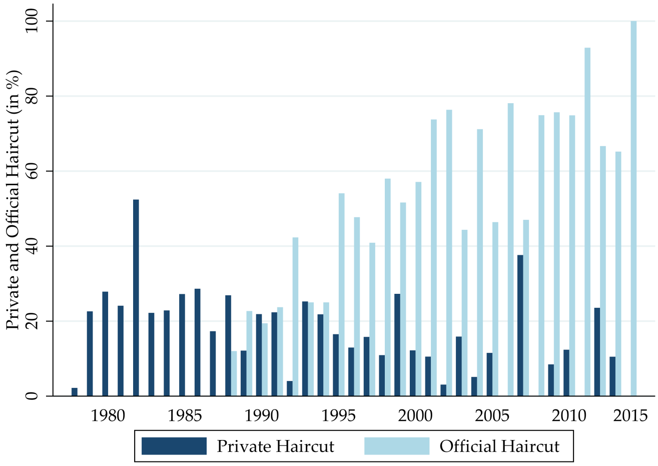
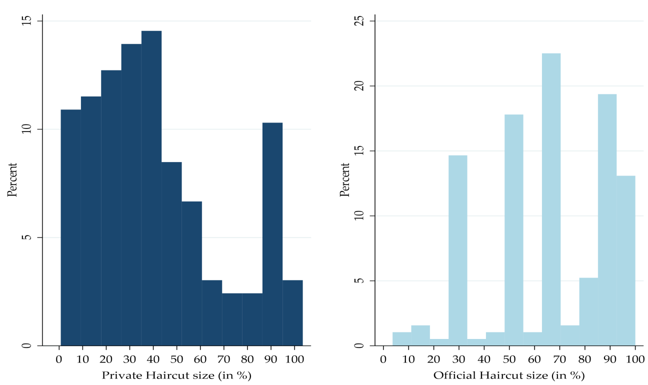


Figure 4: Frequency by size of private and official haircuts (in percent)



We retrieve monthly information via Bloomberg on eight rating agencies: CI, Dagong, DBRS, Fitch, JCR, Moody's, R&I, and S&P. To analyze the dynamics around default times, we use data at a monthly frequency. We obtained an unbalanced panel, as each agency assigns ratings to a different set of countries over varying time periods. Table A2a, in the online Appendix A, shows the number of observations, countries, and years for each agency. The pair-wise correlation between sovereign ratings from the eight credit rating agencies under analysis ranges from 0.869 (between Standard and Poor's and Dagong Global) and 0.992 (between Fitch and Japan Credit Rating Agency) (see Table A2b). For our empirical analysis, all ratings have been translated to a 21-point scale. This means that we assign the highest value of 21 for an "AAA" rating. "C" and "D" in turn are translated into a value of one. We follow the translation described by Fuchs and Gehring (2017), which is reported in Table A3, in the online Appendix A.
Since the data on credit agencies are available for the full sample of countries only since 1990, our monthly data are organized in an unbalanced panel, including a maximum of 130 developing countries, over the years 1990-2018.[27] We take the average rating of all eight agencies as the dependent variable. As a robustness check, in order to account for the possible influence of agency-country time-invariant characteristics (what is called the "home bias" in sovereign rating, see Fuchs and Gehring 2017), we replicate the analysis using the agency-country pairs of ratings (dyadic data).[28] As a further robustness check, we also estimate the effects on the average of only North American agencies (i.e., Moody's, Fitch, Standard & Poor's, Dominion Bond Rating Services), and the rating provided by Moody's, that is the agency that supplies information for the highest number of countries. Finally, we take the monthly average secondary market bond stripped yield spread from J.P. Morgan's EMBI Global (EMBIG) for each country as a secondary dependent variable.[29] EMBIG spreads have been used to proxy foreign currency borrowing costs of both governments and the private sector in emerging market economies. Due to data availability, this sample is restricted to 47 countries from 1993 to 2018. Among the 47 countries covered by the EMBIG, 23 are countries that restructured their debt, while the other 24 are non-defaulters.[30] Table A2c, in the online Appendix A, shows the correlation between the

[27] In contrast to the full period for which haircuts are available, from 1970-2013.

[28] Recent studies document the existence of incentives for ratings agencies to distort ratings in favor of their respective home countries, as well as economically and culturally aligned countries (Fuchs and Gehring 2017), or for issuers in the market for commercial mortgage-backed securities (Sean et al. 2019). More recently, Kempf and Tsoutsoura (2020) find that partisan perception affects the actions of professionals in the financial sector.

[29] The stripped yield spread is the difference between the weighted average yield to maturity of a given country's bonds included in the index and the yield of a US Treasury bond of similar maturity.

[30] The 23 defaulters are Algeria, Argentina, Belize, Brazil, Bulgaria, Cote d'Ivoire, Croatia, Dominican Republic, Ecuador, Iraq, Mexico, Nigeria, Pakistan, Panama, Peru, Philippines, Poland, Russia, Serbia and Montenegro, South Africa, Ukraine, Uruguay, and Venezuela. The 24 non-defaulters are Chile, China, Colombia, Egypt, El Salvador, Gabon, Georgia, Ghana, Greece, Hungary, Indonesia, Jamaica, Kazakhstan, Lebanon, Lithuania,

(average) agency rating and bond spread in the reduced sample, while Table A4 presents some summary statistics.

3.3. AIPW estimation

Our baseline estimates rely on a local projection methodology developed by Jordà and Taylor (2016) to account for the endogeneity of a sovereign default. Using a propensity-score based method, combined with local projections (Jordà 2005), we find the average treatment effect of the end of a debt crisis on our outcome variables over a seven-year period.

Calculating the average, unbiased, effect of a sovereign default on ratings would require comparing two contrasting scenarios: one where we can measure the change in ratings following a debt restructuring event, and one where we measure the change in ratings when no such event has occurred, ceteris paribus. If the decision was fully exogenous, we could simply compare the average change in ratings of defaulters versus non-defaulters. However, the choice to enter into a restructuring with either private or official creditors is endogenous to a number of observable and non-observable factors influencing ratings. Furthermore, it is difficult to pinpoint the direction of the effect, as falling ratings are just as likely to signal a default as they are to be a consequence of defaults. With this methodology, we accept the endogeneity of default, and instead attempt to explicitly model and account for it.

The technique was applied to the area of sovereign debt distress by Kushinov and Zimmerman (2019), who estimate the effect of defaults on GDP. Following Asonuma *et al.* (2016) and Rho and Saenz (2021), we first estimate the probability of being under a debt restructuring with private or official creditors. Then, if the decision is modeled correctly, we can re-balance the sample as if the decisions were taken at random (Jordà and Taylor 2016; Kushinov and Zimmerman 2019).

In the second stage, we use the average rating and the monthly bond spread as the potential outcome variables, as described in the previous section. The AIPW estimator gives us an unbiased estimate for the average treatment effect of a final restructuring on sovereign credit ratings. We define as final restructurings those that were not followed by another restructuring vis-a-vis private or official creditors within (at least) the subsequent four years. Local projections have the attractive property of being free of structural constraints that would instead be imposed on a parallel VAR model. Therefore our ATE response varies non-linearly over the forecast horizon. In the scope of this paper, we apply this

Malaysia, Morocco, South Korea, Sri Lanka, Thailand, Trinidad and Tobago, Tunisia, Turkey, and Vietnam. This list includes countries with no external sovereign debt restructuring in the chosen period, as well as countries that entered the EMBIG more than seven years after their restructuring. For more information, see Cruces and Trebesch (2013a).

methodology to cases of defaults with private and official creditors in order to compare the differential effects on sovereign credit ratings and bond spreads.

3.3.1. Identification

The methodology is divided into two stages. First, we model the probability of being under a debt restructuring by estimating a propensity score for each element in our sample. We evaluate the use of different discrete choice models. First, a "one-type" logit model with outcome equal to one for either private or official restructuring years. A "two-type" or multinomial logit, where the outcomes are private restructuring years in one case and official ones in the other. Finally a "three-type" model which considers an additional outcome when restructuring years are for both private and officials. As in Asonuma et al. (2016), we screen between the models selecting the one reporting the lowest Akaike Information Criterion (as reported in Table B3, in the online Appendix B). This leads us to select the first binary model. The propensity score is then the likelihood of a default as predicted by the logit model:

$$PD_{i,t} = \Lambda(\beta, Z_{i,t-1}) \tag{3.1}$$

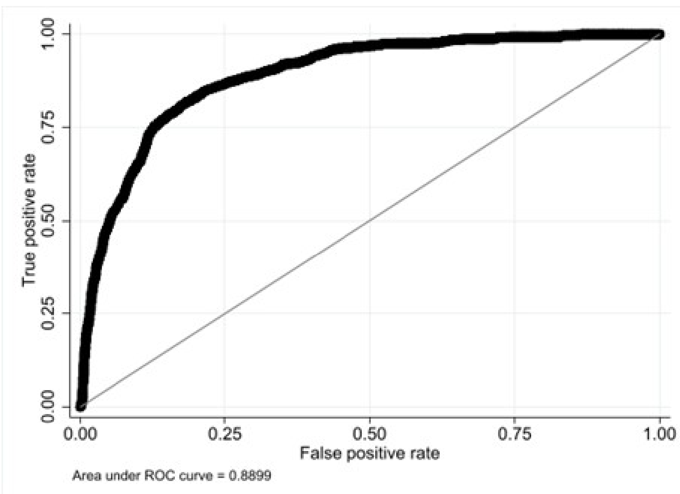
where Λ is the logistic distribution function and Z is a vector of macro and political control variables lagged by one year. Our predictor variable set is based on Asonuma *et al.* (2016) and Rho and Saenz (2021). In particular, our predictors of choice include bank credit to GDP, a banking crises dummy, general gov. gross debt to GDP, a dummy for high inflation, openness, general gov. primary balance, real GDP growth, reserves to GDP, the U.S effective federal funds rate, and the share of past months under default for a given country as well as income group dummies. The standard errors are clustered at the country level (Jordà and Taylor 2016; Kushinov and Zimmerman 2019). The estimated $\widehat{PD}_{i,t}$ is then the predicted default probability for country i at time t conditional on our set of predictor variables.

Then, the second stage re-balances in order to create a synthetic sample where the default decision is as good as random. Using our logit estimates, we can estimate the extent of the non-randomness in our sample. Specifically, a highly endogenous default would be predictable based on observables and have a high $\widehat{PD}_{i,t}$, while a highly endogenous control country would have a low $\widehat{PD}_{i,t}$. We assign the weights $1/\widehat{PD}_{i,t}$ to the defaulter (treatment) group and $1/(1-\widehat{PD}_{i,t})$ to the non defaulter (control) group. Given the re-balanced sample, the average treatment effect will then be the difference between the average weighted potential outcomes of defaulters and non-defaulters across our sample.

Table B2, in the online Appendix B, reports the estimated coefficients from the first stage for our baseline model in column 1. Levels of debt are important for predicting the probability of being under a debt restructuring, while reserves to GDP are negatively correlated with a debt crisis but not significant, there is strong evidence of path

dependency, and measures of systemic financial risk increase the probability of being under a debt restructuring.
Figure 5 shows the Receiver Operating Characteristic (ROC) curve for the first stage. The ROC curve plots the true positive rates against the false positive rates, and we can interpret the area under the curve (AUC statistic) as the predictive ability of the model. An AUC statistic equal to 0.50 means that the covariates have no predictive ability, while a value equal to 1 corresponds to perfect predictive ability. Our model returns a AUC of 0.88, confirming its predictive ability. Figure 6 shows that the matching procedure generates a control group that is similar enough to the treated group. Indeed, most observations of treated and control groups range in the same level of the estimated probability of being under a debt restructuring.

Figure 5: AIPW first stage ROC curve



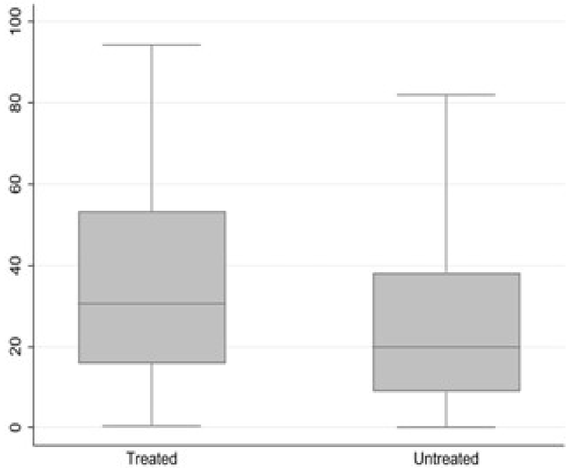
Notes: : The Receiver Operating Characteristic (ROC) curve plots the true positive rates against the false positive rates, such that the area under the curve (AUC statistic) indicates the predictive ability of the model. Under the null that the covariates have no predictive ability, the AUC is equal to 0.50, and perfect predictive ability corresponds to an AUC statistic of 1. Our first stage for estimating the probability of being in a debt crisis under our baseline sample of monthly data returns an AUC of 0.88.

The potential outcome, which is modeled in the second stage, is the change in the outcome following the end of a restructuring as measured with a local projection (Jordà, 2005):

$$\Delta y_{i,t+h} = \alpha + \beta Z_{i,t-1} + \gamma_j FC_{i,t} + \delta_j FR_{i,t} + \eta_{i,h} + \tau_t + u_{i,t+h}, \quad h = 1, ..., 7. \tag{3.2}$$

Here $\Delta y_{i,t+h}$ is the cumulative conditional forecast of the change in outcome from time t to $t + h$, and h is our forecast horizon spanning up to seven years. We consider as outcome both the credit rating and bond spread measured in country i. We take as time t for the

Figure 6: Post-matching estimated propensity score



Notes: Boxplots of the estimated propensity scores, after the matching procedure, between countries that are under a debt crisis (treated countries) and countries that are not under a debt crisis (untreated countries), for our baseline monthly dataset.

treatment the period belonging to the year of the final private (or official) restructuring, therefore estimating the changes in ratings/bond spreads following the end of a debt crisis. Furthermore, because we are interested in evaluating stigma effects, and not just the mechanical co-movements of ratings over the restructuring period, as in Cruces and Trebesch (2013a), we exclude the years of the crisis.

$FC_{i,t}$ is a dummy equal to one when a country has finalized its final private (or official) restructuring and $FR_{i,t}$ denotes the corresponding amount of final private (or official) haircut. We include both private and official restructurings in the same specification, therefore accounting for both types of events given the overlap between the two. $Z_{i,t-1}$ is a vector containing macroeconomic and political control variables, lagged by one year as in the first stage. η_i indicates country fixed effects and τ_t denotes time fixed effects, which allows us to control for common trends. This way, we can also account for global factors that might have influenced the simultaneous dating choice of debt restructuring events (e.g., Baker or Brady plan in the two periods, 1985-88, or 1989-94). Finally, $u_{i,t+h}$ is the error term. As in the first stage, we cluster the standard errors at the country level, as the treatment occurs at the country level.

As control variables are concerned, we mainly rely on the specification by Cruces and Trebesch (2013a). Therefore, in order to capture the sovereign's domestic economic performance, we include public debt to GDP, the general government net lending/borrowing, GDP real growth, reserves to GDP, inflation rate (based on consumer prices), current account, and the ICRG political risk index. Furthermore, we include the amount of IMF net lending to control for the possibility that the different results, between private and official agreements, may depend on additional financing from the IMF that are associated with official restructurings. While an IMF programme is a *sine qua non* condition for Paris Club creditors to provide relief, not all private restructurings were associated with IMF programmes. Table B1, in the online Appendix B, provides a detailed description of each variable and its source.

We run the above regression for each point in horizon h on the re-balanced sample and reach the desired ATE:

$$ATE_h = \frac{1}{n}\sum_i \sum_t \left\{ \left[\frac{(y_{i,t+h} - y_{i,t})(FC_{i,t})}{PD_{i,t}} - \frac{(y_{i,t+h} - y_{i,t})(1 - FC_{i,t})}{1 - PD_{i,t}} \right] - \frac{(FC_{i,t} - PD_{i,t})}{PD_{i,t}(1 - PD_{i,t})} \left[(1 - PD_{i,t}) m_1^h(Z_{i,t-1}, FR_{i,t}, \theta_1^h) + PD_{i,t} m_0^h(Z_{i,t-1}, FR_{i,t}, \theta_{0h}) \right] \right\} \tag{3.3}$$

Here $y_{i,t+h} - y_{i,t}$ is the estimated cumulative conditional forecast from our local projections, and $FC_{i,t}$ is the dummy used to distinguish between defaulters and non-defaulters and $PD_{i,t}$ are the estimated propensity scores from the first stage. The first part is a standard inverse score weighted estimator of the ATE. Intuitively, this is like a group-means comparison between defaulters and non-defaulters, with the difference that we correct for allocation bias of the treatment by modeling for it with the propensity score, afterwards inverting it to achieve a random distribution. The second part is an adjustment term consisting of the weighted average of the two regression estimators. The purpose of the adjustment term is to stabilize the estimator as the propensity score gets close to the extremes (0 or 1) and therefore alleviates the need to truncate the weights. Hence ATE_h is the average treatment effect of final restructuring computed over the seven-year horizon.

The AIPW estimator has a number of features that make it suitable for calculating the dynamic effects and for the estimation under endogeneity issues. The combination of local projections and propensity score weighting is doubly-robust, in that the estimator will be unbiased as long as either of the stages is specified correctly. The underlying idea is that the predictor set in the first stage, and the control set in the second stage, should be expansive enough to explain as much variation in sovereign default decisions as possible.[31]

[31] With this, we do not need to rely on exclusion restrictions. Even if all our variables were endogenous, as long as there is no unexplained deviation from the conditional forecasted change in ratings, the ATE will be unbiased (Jordà and Taylor 2016).

3.4. Results

We now present the ATE estimates, starting with credit rating as the dependent variable, while in the next section, we use bond spread as the dependent variable.

3.4.1. Credit ratings

To identify post-crisis episodes, we focus on final restructurings only and we exclude observations during crisis years. This will allow us to compare our outcomes with respect to "normal times," as agency rating would mechanically improve the assessment of a country, once it exits from default status. Table 3 shows the ATE in the case of private and official restructurings. In the case of private agreements, the estimates indicate a persistent negative effect of a final restructuring on agency ratings. While in the first years, there is an average drop by less than one notch in our scale of agency ratings, by the third year, the drop in agency ratings increases to more than one notch. The effect peaks after four years with a 2.25 drop in agency ratings. Notably, the ATEs are negative and significant for all the seven years in the analysis.

The dynamics of ratings in a post-crisis setting suggest that a private restructuring likely implies a long-lasting, reputational effect on the sovereign defaulter. Clearly, this is influenced by the size of haircuts imposed on creditors, which is the reason why we control for severity of default. The second stage local projection used in the estimation of this ATE does well in forecasting the change in agency ratings both in the short and long term, with the R-squared going from 18 to 60 percent.[32]

As official deals are concerned, we find, on average, an increase in ratings, as the ATE is always positive after the final restructuring. As before, the results are significant for every year considered, and the effect peaks after 3 years, when the expected change in ratings is of 1.12 notch with respect to the base year.

Positive spillover effects seem to dominate following a restructuring with official creditors. The ATEs from Table 3 are plotted in Figure 7. As can be seen, the dynamic response of agency ratings following the final restructurings for both event types is persistent for all the years of our estimates. Moreover, the differences between private and official defaults persist when we consider dyadic data as opposed to the average rating as well as the average of only North American agencies (i.e., Moody's, Fitch, Standard & Poor's, Dominion Bond Rating Services), and the rating provided by Moody's. Table B6 reports the estimated ATE for the dyadic setting and the alternative outcome variables.[33]

[32] The coefficients from the second stage local projection are reported in Table B4, in the online Appendix B.

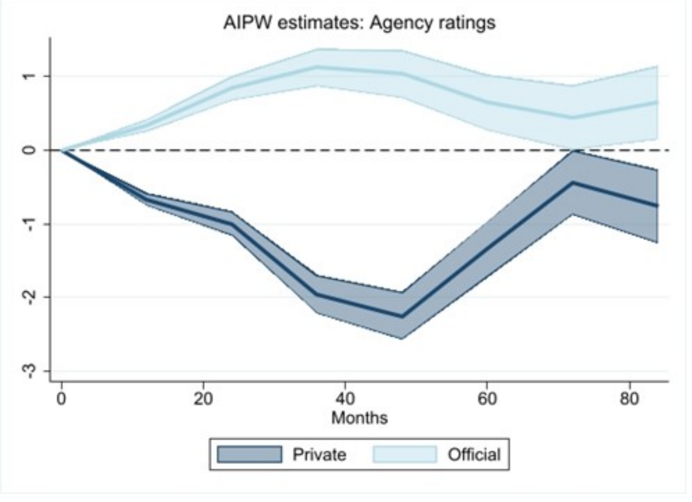
[33] Figure B1 shows the Receiver Operating Characteristic (ROC) curve for the first stage in the dyadic setting, and Figure B2 proves that the matching procedure generates a control group that is similar enough to the treated group.

Table 3: ATE on change in average ratings, private and official restructurings

Panel A: Private restructurings							
Year	1	2	3	4	5	6	7
AIPW	-0.67***	-1.00***	-1.96***	-2.25***	-1.35***	-0.44*	-0.76***
	(-15.43)	(-11.67)	(-14.77)	(-13.51)	(-6.89)	(-1.94)	(-2.98)
Observations	14605	14605	13633	12656	11673	10697	9750
Panel B: Official restructurings							
Year	1	2	3	4	5	6	7
AIPW	0.33***	0.84***	1.12***	1.03***	0.64***	0.44*	0.65**
	(7.71)	(9.84)	(8.45)	(6.19)	(3.29)	(1.95)	(2.52)
Observations	14605	14605	13633	12656	11673	10697	9750

Notes: Table shows average treatment effect of private (top panel) and official restructurings (bottom panel) on change in average agency ratings. Standard errors are clustered at the country level. T-statistics in parenthesis. The model uses predictors and controls for first and second stage listed in the Online Appendix B and controls for country fixed effects and time-varying heterogeneity. Significance levels: *0.10, **0.05, ***0.01.

Figure 7: Year-by-year ATE, ratings



Notes: Graphs show AIPW average treatment effect estimates for each h-step ahead forecast of change in average agency ratings following the end of a private restructuring in one case, and official restructuring in another.

3.4.2. Bond spread

This section now presents the results of the AIPW methodology applied to the monthly average secondary market bond stripped yield spread (EMBIG). Given the direct connection between ratings and spread, we expect our results to mirror those on ratings. Table 4 shows the computed ATEs in the case of both private and official agreements, while Table B5, in the online Appendix B, reports the coefficients from the second stage. As above, the estimated local projection includes controls, country, and time fixed effects, thereby estimating the ATE of the conditional forecast of bond spreads for h-steps ahead. Following the final private restructuring, the ATE is large. One year after the event, we find an increase of about 204 basis points in the spread with respect to the base year. The effect peaks after three years, when the spread is 1393 bp with respect to the base period, after which this change in spread falls. Finding such results for secondary market yields reveals that the aforementioned reputational effects are felt on markets as well as being perceived by credit rating agencies. Even after the end of a debt crisis involving private creditors, investors' sentiment remains sour for sovereign debt instruments.

On the contrary, following an official restructuring, the change in spread with respect to the base year is constantly falling, where in the first period, the spread falls by a little more than 257 basis points, and then falls consistently over the forecast horizon. The ATEs from Table 4 are plotted in Figure 8. As we can see, the dynamic response of bond spread following the end of a restructuring episode for both event types is persistent for all the years in our estimates.

These results mirror those obtained when considering the Institutional Investor's index as the dependent variable and using the Synthetic Control Method instead of the AIPW (see Marchesi and Masi 2020). On the other hand, our results contrast with those of Reinhart and Trebesch (2016), who document a strong increase in average ratings (for emerging markets) when private agreements follow a debt relief. Reinhart and Trebesch also find that despite the substantial relief obtained, ratings in advanced economies did not recover after the war official debt forgiveness of 1934.

In summary, consistently with Schlegl *et al.* (2019), we find that defaulting on private debt is highly visible and hence more likely (than official crisis) to result in a rating downgrade. On the other hand, official lenders may shoulder the burden for private creditors, which is one explanation for why following official restructurings we find evidence of positive market reaction. We return to these points in greater detail in Section 6.

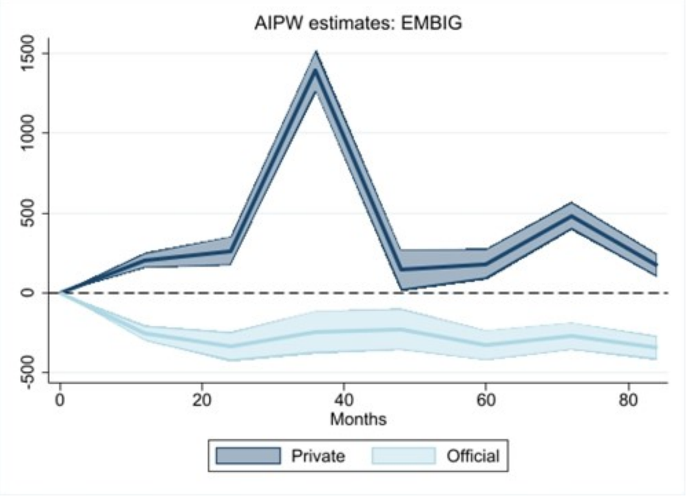
As previously mentioned, different reactions are likely to depend on the different terms of the restructurings with private with respect to official creditors. In particular, the higher cost of large defaults is most likely driven by a less creditor-friendly negotiation process, which in turn results in higher economic uncertainty and more severe punishment from the creditors. On the other hand, official restructurings that are arranged within the "Paris Club umbrella" are supposed to guarantee a relatively smoother approach to the way in which deals are actually orchestrated with respect to private ones, hence lowering even further the collateral damage of a default. In the next two sections, we provide some additional evidence on the importance of restructuring size, as well as on the role of litigation costs in the case of private agreements.

Table 4: ATE on change in bond spread, private and official restructurings

Panel A: Private restructurings							
Year	1	2	3	4	5	6	7
AIPW	203.66***	260.76***	1393.04***	140.84**	181.06***	482.68***	168.79***
	(8.14)	(5.49)	(20.11)	(2.11)	(3.71)	(10.77)	(4.42)
Observations	5382	4833	4307	3785	3337	2923	2528
Panel B: Official restructurings							
Year	1	2	3	4	5	6	7
AIPW	-257.33***	-337.88***	-247.44***	-231.94***	-329.48***	-273.35***	-346.96***
	(-10.33)	(-7.13)	(-3.60)	(-3.48)	(-6.76)	(-6.12)	(-9.11)
Observations	5382	4833	4307	3785	3337	2923	2528

Notes: Table shows average treatment effect of private (top panel) and official restructurings (bottom panel) on change in monthly average country yield spread over US Treasury bonds (EMBIG stripped spread) measured in basis points (bp). Standard errors are clustered at the country level. T-statistics in parenthesis. The model uses predictors and controls for first and second stage listed in the Online Appendix B and controls for country fixed effects and time-varying heterogeneity. Significance levels: *0.10, **0.05, ***0.01.

Figure 8: Year-by-year ATE, bond spreads



Notes: Graphs show AIPW average treatment effect estimates for each h-step ahead forecast of change in average agency ratings following the end of a private restructuring in one case, and official restructuring in another.

3.5. Restructuring size

The AIPW methodology comes with the advantages of overcoming endogeneity and being free of structural constraints. However, it does not allow us to evaluate whether the effect changes at different levels of restructuring size. As we mentioned in the Introduction, restructurings involving higher haircuts may entail more severe reputational costs. On the other hand, the channel of debt relief operates in the opposite direction. Since higher haircuts reduce government's debt substantially, such debt reduction may allow countries to exit a situation of debt overhang and improve economic prospects, as described by Krugman (1988). To evaluate such an effect, we use ordinary least squares to distinguish the rating variation associated with the default *per se* from that associated with the amount of the debt affected, i.e., "occurrence" versus "magnitude." Sections 5.1 and 5.2 present the results obtained by using the dependent variables credit ratings and bond spreads, respectively.

3.5.1. Credit ratings

As mentioned in section 4.1, in order to consider post-crisis outcomes, we exclude observations during crisis years, and take up to seven years after the final haircut, to capture the existence of persistent effects.[34] We estimate a model that includes country fixed effects, period-fixed effects, and cluster the standard errors at the country-level. We, therefore, control for unobserved effects that vary at the country and period level, substantially reducing concerns over endogeneity. Ordinary least squares treats the dependent variable as cardinal. This implies that the difference between an "AA" and an "AA+" rating, for example, is the same as between "BB" and "BB+."[35] The regression equation then is:

$$c_{i,t} = \alpha + \beta Z_{i,t-1} + \gamma_j FC_{i,t-j} + \delta_j FR_{i,t-j} + \eta_i + \tau_t + u_{i,t}, \quad j = 1, \ldots 3,\ 4\&5,\ 6\&7 \tag{3.4}$$

where $c_{i,t}$ represents the credit rating in country i at time t. $FC_{i,t-j}$ is a dummy equal to one when a country has finalized its final private (official) restructuring and $FR_{i,t-j}$ denotes the corresponding amount of private (official) haircut, and Z is a vector containing the control variables (lagged by one year). η_i and τ_t denote agency-country pair and time fixed effects, respectively. Finally, $u_{i,t}$ is the error term.

As explained above, the advantage of including both official and private restructurings in the same specification is that it allows us to detect their effects and avoid an omitted

[34] As in Cruces and Trebesch (2013a), we add together the years 4&5, and 6&7, after a restructuring to give more weight to events that are further back in time.

[35] We should emphasize, however, that the economic consequences of the rating contraction may not be linear, as losing the two notches from junk territory is clearly different from switching, for example, from AAA to AA (in S&P's rating).

variable bias. Moreover, we are also able to distinguish the rating variation associated with the default per se from that associated with the haircut size ("occurrence" versus "magnitude"). The list of control variables is the same described in section 3.1. Table C1, in the online Appendix C, provides a detailed description of all our variables.

Table 5 presents the results obtained by considering the size of the final private and official haircuts. In columns 1-2 of Table 5, we include haircut size, expressed in percentage points, up to seven years after the final restructuring (with and without control variables, respectively). Column 2 shows that a one percentage point increase in the private haircut size is associated with a decrease of about 0.04 notch in the credit rating in year one after the final haircut. This implies that a final haircut of about 40 percent, roughly corresponding to the mean for our sample, can be associated with a decrease of about 1.6 notches in year one.

In the case of an official agreement, a one percentage point increase in an official haircut is associated with an increase of about 0.02 notch in the credit rating, in year one after the restructuring. Hence, a haircut of about 60 percent (the mean for our sample) can be associated with an increase of about 1.2 notch, in year one. These results are economically relevant both in the case of private and official restructurings. In turn, in columns 3-4, we include only the dummy indicating the occurrence of the private and official restructuring, while the last two columns contain the full specification (with and without control variables). While all these results are reported for comparison, we mostly base the discussion on the fully specified model of column 6.

To be able to comment these results, however, it should be kept in mind that the coefficients shown in the fully specified model have to be interpreted conditionally, as in any interaction model. The best way to interpret the findings of Table 5 is to look at Figures 9a and 9b, which show the expected variation in agency ratings conditional on the private and official haircut size. In other words, we plot the marginal effect $\delta_j R_{i,t-j} + \gamma_j$ from equation 4 above. The different panels correspond to the number of years after the restructuring, and the dotted lines show 90 percent confidence bands. The effects are calculated from the complete specification (column 6). Aside from an easier interpretation, this joint estimate and the resulting graphs are important because the high correlation between C and R makes it complicated to draw inference about individual effects, but facilitates inference about their sum (see Cruces and Trebesch 2013a).[36]

The bottom line of Figure 9a is that private haircuts are negative and statistically significant for years one to seven after the final agreement. This can be seen because the upper confidence band is always below the zero horizontal line for every haircut size greater than 20 percent (the mean of this sample being around 40 percent). The reduction in credit

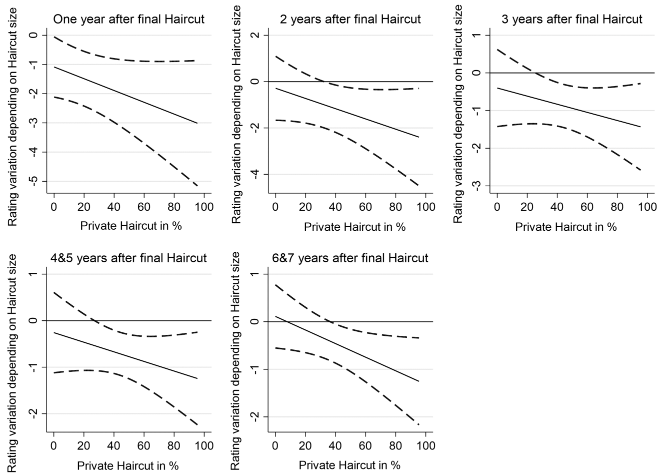
[36] As pointed out by Cruces and Trebesch (2013a), multicollinearity does not bias least squares estimates, but the high correlation between C and R will tend to increase the estimated standard errors. The high correlation between C and R (about 0.7 in our sample) lowers the variance of the estimated effect of interest, $\gamma + \delta R$.

Table 5: Private and official haircut and average rating, OLS

	(1)	(2)	(3)	(4)	(5)	(6)
Final Private Haircut (-1)	-0.046***	-0.039***			-0.013	-0.021
	(-2.919)	(-3.185)			(-0.548)	(-1.504)
Final Private Haircut (-2)	-0.035***	-0.029***			-0.004	-0.020
	(-2.841)	(-2.726)			(-0.218)	(-1.398)
Final Private Haircut (-3)	-0.025***	-0.018***			-0.002	-0.008
	(-2.985)	(-2.985)			(-0.110)	(-0.751)
Final Private Haircut (-4 & 5)	-0.020***	-0.017***			-0.003	-0.012
	(-2.661)	(-2.921)			(-0.224)	(-1.229)
Final Private Haircut (-6 & 7)	-0.015**	-0.013***			-0.008	-0.013
	(-2.274)	(-2.936)			(-0.639)	(-1.529)
Final Official Haircut (-1)	-0.002	0.017***			0.016	0.019*
	(-0.410)	(3.746)			(0.908)	(1.786)
Final Official Haircut (-2)	-0.003	0.010**			0.021	0.018
	(-0.615)	(2.328)			(1.387)	(1.558)
Final Official Haircut (-3)	-0.005	0.004			0.017	0.014
	(-1.034)	(0.912)			(1.373)	(1.354)
Final Official Haircut (-4 & 5)	-0.002	0.007			0.016*	0.016*
	(-0.596)	(1.468)			(1.800)	(1.878)
Final Official Haircut (-6 & 7)	0.001	0.007**			0.015***	0.018***
	(0.318)	(2.341)			(3.503)	(3.447)
Final Priv. Haircut Dummy (-1)			-2.392***	-2.324***	-1.868**	-1.301**
			(-3.733)	(-3.445)	(-2.007)	(-2.060)
Final Priv. Haircut Dummy (-2)			-1.801***	-1.432**	-1.650**	-0.528
			(-3.576)	(-2.524)	(-2.182)	(-0.715)
Final Priv. Haircut Dummy (-3)			-1.230***	-0.868**	-1.184*	-0.570
			(-3.252)	(-2.565)	(-1.753)	(-0.953)
Final Priv. Haircut Dummy (-4 & -5)			-0.928***	-0.721**	-0.802	-0.215
			(-2.853)	(-2.369)	(-1.298)	(-0.409)
Final Priv. Haircut Dummy (-6 & -7)			-0.574**	-0.449*	-0.273	0.020
			(-2.077)	(-1.901)	(-0.514)	(0.049)
Final Off. Haircut Dummy (-1)			-0.274	1.170**	-1.191	-0.087
			(-0.485)	(2.414)	(-0.910)	(-0.099)
Final Off. Haircut Dummy (-2)			-0.555	0.545	-1.705	-0.548
			(-1.038)	(1.100)	(-1.549)	(-0.609)
Final Off. Haircut Dummy (-3)			-0.626	0.163	-1.522*	-0.714
			(-1.448)	(0.378)	(-1.932)	(-1.005)
Final Off. Haircut Dummy (-4 & -5)			-0.501	0.152	-1.359**	-0.788
			(-1.426)	(0.378)	(-2.469)	(-1.463)
Final Off. Haircut Dummy (-6 & 7)			-0.248	0.175	-1.050***	-0.832***
			(-1.118)	(0.654)	(-3.698)	(-3.220)
GDP real growth (-1)	0.025		0.028		0.028	
	(1.152)		(1.247)		(1.267)	
Primary balance to GDP (-1)	0.006		0.005		0.005	
	(0.246)		(0.225)		(0.245)	
Current Account to GDP (-1)	-0.018		-0.018		-0.017	
	(-1.416)		(-1.393)		(-1.325)	
Reserves to GDP (-1)	0.003		0.004		0.003	
	(0.314)		(0.367)		(0.262)	
Public debt to GDP (-1)	-0.041***		-0.041***		-0.041***	
	(-3.522)		(-3.366)		(-3.445)	
Inflation (-1)	1.352		1.411		1.375	
	(0.505)		(0.534)		(0.517)	
Political risk (-1)	0.147***		0.141***		0.144***	
	(5.352)		(5.102)		(5.149)	
IMF Net Loans (-1)	-0.125		-0.114		-0.126	
	(-1.135)		(-1.039)		(-1.142)	
Constant	12.370***	5.350**	12.441***	5.613**	12.435***	5.415**
	(17.358)	(2.211)	(17.341)	(2.323)	(17.331)	(2.233)
Observations	20,409	13,296	20,409	13,296	20,409	13,296
R-squared	0.121	0.415	0.135	0.410	0.140	0.420
Number of id	124	85	124	85	124	85
Period FE	YES	YES	YES	YES	YES	YES

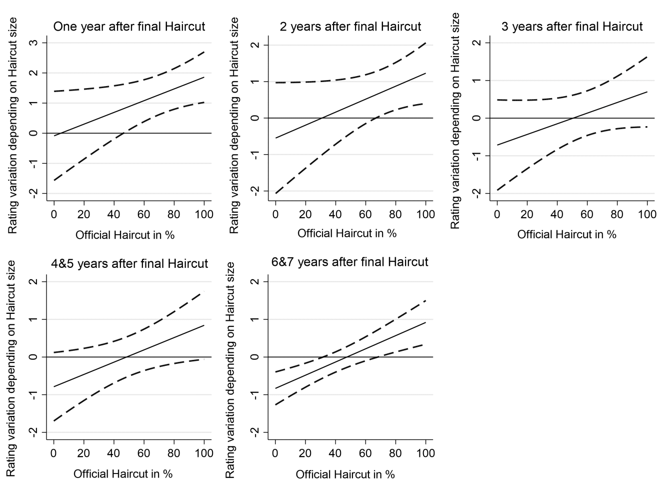
Notes: This table shows coefficients of an OLS fixed effects model at the country-period level. The dependent variable is the monthly average agency rating. Country and year fixed effects are included. S.E at the country level, t-stats in parentheses. Significance levels: *0.10, **0.05, ***0.01.

Figure 9a: Year-by-year ATE, bond spreads



Notes: Each graph shows the marginal effect of private haircut on average agency ratings, for different haircut sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 5, column 6. The rating contraction after a restructuring is statistically significant for levels of haircut at which the upper confidence band is below the zero horizontal line.

Figure 9b: Year-by-year ATE, bond spreads



Notes: Each graph shows the marginal effect of official haircut on agency rating, for different haircut sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 5, column 6. The rating increase after a restructuring is statistically significant for levels of haircut at which the lower confidence band is above the zero horizontal line.

rating associated with haircut size is also economically substantial, especially for years one and two after the final agreement.

In the case of official deals, as in Figure 9b, the rating increase of a restructuring is statistically significant for levels of haircut at which the lower confidence band is above the zero horizontal line. In years one to three after the final agreements, haircuts greater than 60 percent (corresponding to the mean of this sample) can be associated with significantly higher ratings. At lag 4&5, the rating increase can be significant only for haircuts greater than 80 percent, while at lag 6&7 the effect is never significant.

The results are also robust to a dyadic set-up in which we take into account the dyadic relationship between agency-country pairs, at least as time-invariant factors are concerned (as in column 1 of Table C2, in the online Appendix C).[37] The results are also robust to the inclusion of further variables to control for the presence of omitted variable bias, such as the number of years the chief executive has been in office, total population (in log), and per capita GDP (as in column 3 of Table C2).[38] The results also hold when using an ordered logit model for the discrete 21-step end-of-month rating, which accounts for the bounded nature of the dependent variable (as in columns 2 and 4 of Table C2). Finally, they are also robust to using as the dependent variable the average of only North American agencies (i.e., Moody's, Fitch, Standard & Poor's, Dominion Bond Rating Services) as opposed to only Moodys ' (columns 5-6 of Table C2). Taken together, this is strong evidence pointing to a significant difference between the effects on credit ratings from private and official restructuring events.

In conclusion, the (private) haircut size seems to involve some reputational costs and the correlation between private restructuring and agency credit rating is negative for years one to seven after the final restructuring episode. These results are consistent with Meyer *et al*. (2019), who document that the decline in investor returns is much smaller for low-haircut cases (i.e., lower than the median value) and with Asonuma *et al.* (2021), who find that post-default restructurings are associated with a decline in bank credit, an increase in lending interest rates, and a higher likelihood of triggering a banking crisis (especially in the case of preemptive agreements). Finally, they are also in line with Gennaioli *et al.* (2014), who show that the spillovers of a default, on domestic and foreign banks, are larger the higher the haircut.

The opposite holds in the case of official agreements, where agency ratings generally improve, and the more so, the larger the haircut.[39] As previously mentioned, there is a trade-off concerning the effect of debt reduction: a positive debt relief effect and a negative

[37] Figures C1a ad C1b, in the online Appendix C, report the marginal effects of the dyadic set-up.

[38] Our estimation results could still be biased due to the omission of time-varying country-specific variables correlated with both the government negotiation behavior and rating (e.g., the haircut size may vary when new governments take over).

[39] Hence, the positive growth prospect observed for official defaulters after the end of the default might be due to the absence of a negative stigma in the credit markets (see, for example, Marchesi and Masi 2021).

reputational effect. This evidence then suggests that while for private defaulters the negative reputational effects dominate, for official defaulters positive (debt relief) spillovers prevail (as in Arslanalp and Henry 2005).[40] However, the results in this section should be taken with caution, as identification is difficult and we cannot claim any causal effect but only strong conditional correlations. In the next section, we will consider a more direct measure of borrowing costs (bond spreads) as in Cruces and Trebesch (2013a).

3.5.2. Bond spread

In this section, we estimate equation 4 by taking the bond (EMBIG stripped) spread for each country as the dependent variable. The results are presented in Table 6. As in the previous section, in columns 1-2 of Table 6, we include the final haircut size, expressed in percentage points, up to seven years after the final agreement (with and without control variables). Column 2 shows that a one percentage point increase in haircut is associated with bond spreads that are about 4 bp higher in year one after the final restructuring. Thus, a restructuring with a final haircut size of about 40 percent (the mean in our sample of private haircuts) can be associated with 160 bp higher in year one. In the case of an official agreement, a one percentage point increase in the final official haircut is associated with a decrease of about 5 bp in the first year after the restructuring. This implies that a restructuring with a final haircut of about 60 percent (mean in our sample of official haircuts) can be associated with a reduction of almost 240 bp in the first year after the official agreement. These results are hence sizable both in the case of private and official deals.

In columns 3-4, as before, we include only the dummy indicating the occurrence of the final private/official default, while the last two columns contain the full specification, which confirm the relationship between private haircut and subsequent spreads for years four to seven after the final restructuring. In particular, Figures 10a and 10b, which are based on the full specification, show the mean increase in bond spreads associated with the final private restructuring for different levels of haircut and at different lag lengths. The main message of Figure 10a is that final restructurings with haircuts above 40 percent can be associated with significantly higher spreads from one to seven years after the restructuring.[41] For further illustration, suppose that haircuts increase by one standard deviation (21 pt in this sample); this implies spreads that are 109 bp higher in years 4 and 5 after the final restructuring, and 107 bp higher in years 6 and 7. These results are

[40] Since quite a few cases of official haircut concern countries which were eligible for the HIPC Initiative, these results are somehow in line with Raddaz (2011), who finds that the stock prices of companies having subsidiaries in countries benefited by multilateral debt relief (through the HIPC and the MDRI, increase significantly above those of other firms, especially around the launching of these initiatives.

[41] At lag 3 and 6&7, the statistical significance level is actually reached for values of haircut greater than 50 percent.

economically relevant and quite similar to those obtained by Cruces and Trebesch (2013a) in the case of private deals.

Finally, as official restructurings are concerned, Figure 10b shows that official haircuts above 30 percent (the mean of this sample being around 60 percent) can be associated with significantly lower spreads from one to seven years after the final official restructuring (at lag 6&7 the effect is statistically significant only for smaller haircuts, that between 10 and 50 percent). More specifically, an increase of official haircut by one standard deviation (39 pt in this sample) implies spreads that are 172 bp higher in years 4 and 5 after the final restructuring (while the coefficient is not significant at lag 6&7). This result is consistent with the recent findings of Lang *et al*. (2021), who show that countries benefiting from the Debt Service Suspension Initiative (DSSI) experience a larger decline in bond spread compared to similar but ineligible countries. As the DSSI is a NPV-neutral debt service suspension, we actually find that an official debt relief does not generate stigma, even when it is associated with an NPV reduction.[42]

In summary, as in Cruces and Trebesch (2013a), we find that controlling for both the occurrence and the magnitude of default is crucial to detecting a more lasting link between debt default and borrowing costs. Most importantly, private (official) restructurings are generally associated with lower (higher) ratings and higher (lower) spreads up to seven years after the final restructuring. What is more, the rating (spread) decline (increase) is larger for cases with deeper haircuts. Hence, the trade-off concerning the effects of sovereign debt restructurings seems to be associated with opposite outcomes for private and official defaulters. For the former, negative (reputational) spillovers seem to prevail, while for official defaulters, the positive spillovers of a debt reduction are more important (as in Arslanalp and Henry 2005).

As rating and spread represent indirect and direct measures for borrowing costs, our results suggest that default costs may vary with the restructuring terms and the relative treatment of official versus private creditors. Our results point to the importance of the way in which debt restructurings are orchestrated, in line with the distinction between "excusable and inexcusable" (Grossman and van Huyck 1988) and "hard" and "soft" defaults (Trebesch and Zabel 2017). In the next section, we provide some further discussion on this issue, as well as some additional results pointing to the importance of the restructuring terms.

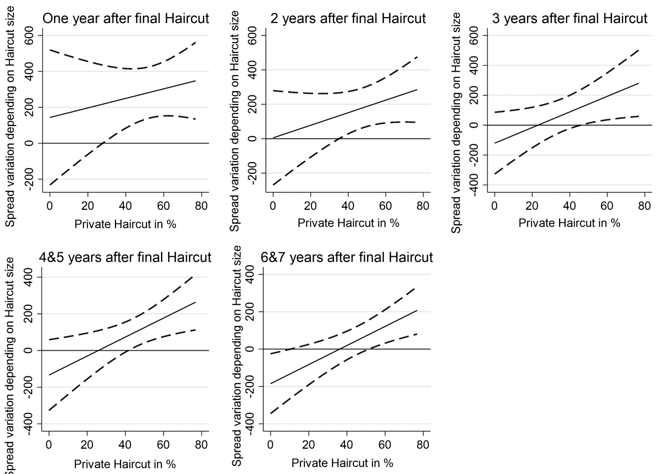
[42] Moreover, while they focus on the beginning of the debt crisis spell, we consider what happens in the aftermath of the default, by taking into account the final haircut size.

Table 6: Private and official haircut and bond spread, OLS

	(1)	(2)	(3)	(4)	(5)	(6)
Final Private Haircut (-1)	7.213***	5.483***		6.420**	2.658	
	(4.397)	(4.469)		(2.021)	(0.793)	
Final Private Haircut (-2)	4.745***	3.811***		6.186**	3.648	
	(3.975)	(3.468)		(2.544)	(1.382)	
Final Private Haircut (-3)	4.088***	2.852**		5.354**	5.220**	
	(3.467)	(2.439)		(2.359)	(2.148)	
Final Private Haircut (-4 & 5)	3.186***	2.335***		7.012***	5.175**	
	(3.704)	(2.672)		(4.039)	(2.575)	
Final Private Haircut (-6 & 7)	0.714	0.782		7.787***	5.104***	
	(0.791)	(0.873)		(5.066)	(3.170)	
Final Official Haircut (-1)	-6.099***	-3.864***		-7.074***	-3.964***	
	(-6.917)	(-4.101)		(-5.864)	(-2.812)	
Final Official Haircut (-2)	-7.726***	-5.110***		-8.974***	-6.177***	
	(-6.539)	(-4.959)		(-5.102)	(-3.415)	
Final Official Haircut (-3)	-5.824***	-4.433***		-4.889**	-3.252	
	(-3.891)	(-3.302)		(-2.414)	(-1.483)	
Final Official Haircut (-4 & 5)	-5.537***	-4.772***		-5.235***	-4.364**	
	(-4.596)	(-3.973)		(-3.075)	(-2.172)	
Final Official Haircut (-6 & 7)	-4.320**	-1.984		-3.434	-1.277	
	(-2.461)	(-0.990)		(-1.562)	(-0.473)	
Final Priv. Haircut Dummy (-1)			282.817***	249.980***	16.249	143.651
			(3.449)	(2.958)	(0.107)	(0.752)
Final Priv. Haircut Dummy (-2)			165.635***	151.506**	-84.230	5.254
			(2.616)	(2.225)	(-0.702)	(0.038)
Final Priv. Haircut Dummy (-3)			132.791**	70.706	-72.835	-120.083
			(2.153)	(1.230)	(-0.654)	(-1.144)
Final Priv. Haircut Dummy (-4 & -5)			69.092	49.256	-192.207**	-133.768
			(1.463)	(1.030)	(-2.174)	(-1.366)
Final Priv. Haircut Dummy (-6 & -7)			-47.559	-19.131	-313.644***	-184.746**
			(-1.179)	(-0.461)	(-4.109)	(-2.274)
Final Off. Haircut Dummy (-1)			-402.231***	-235.900***	95.154*	43.102
			(-3.239)	(-2.871)	(1.759)	(0.671)
Final Off. Haircut Dummy (-2)			-237.451*	-195.862**	209.638**	93.816
			(-1.686)	(-2.194)	(2.170)	(0.912)
Final Off. Haircut Dummy (-3)			-236.661**	-206.217**	12.591	-23.764
			(-2.287)	(-2.543)	(0.151)	(-0.221)
Final Off. Haircut Dummy (-4 & -5)			-213.239***	-162.998**	47.605	40.920
			(-2.895)	(-2.476)	(0.696)	(0.459)
Final Off. Haircut Dummy (-6 & 7)			-200.692**	-106.753	-86.382	-69.284
			(-2.226)	(-1.207)	(-1.317)	(-0.836)
GDP real growth (-1)	10.664***		11.582***		10.081***	
	(5.659)		(5.772)		(5.064)	
Primary balance to GDP (-1)	-2.332		-0.959		-0.564	
	(-0.665)		(-0.269)		(-0.163)	
Current Account to GDP (-1)	-1.301*		-1.954***		-1.310	
	(-1.917)		(-2.635)		(-1.648)	
Reserves to GDP (-1)	-0.132		-0.078		-0.163	
	(-1.095)		(-0.644)		(-1.392)	
Public debt to GDP (-1)	-15.440***		-17.510***		-16.195***	
	(-2.790)		(-3.210)		(-2.952)	
Inflation (-1)	-9.207***		-10.291***		-9.077***	
	(-3.061)		(-3.266)		(-2.960)	
Political risk (-1)	-8.369***		-7.612**		-6.954**	
	(-2.936)		(-2.516)		(-2.402)	
IMF Net Loans (-1)	29.047		27.687		30.778	
	(1.391)		(1.390)		(1.476)	
Constant	595.586***	926.318***	597.325***	848.260***	631.377***	851.802***
	(7.247)	(4.661)	(5.959)	(3.948)	(6.905)	(4.184)
Observations	5,369	4,189	5,369	4,189	5,369	4,189
R-squared	0.350	0.447	0.332	0.440	0.367	0.456
Number of Panel	47	35	47	35	47	35
Country FE	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES

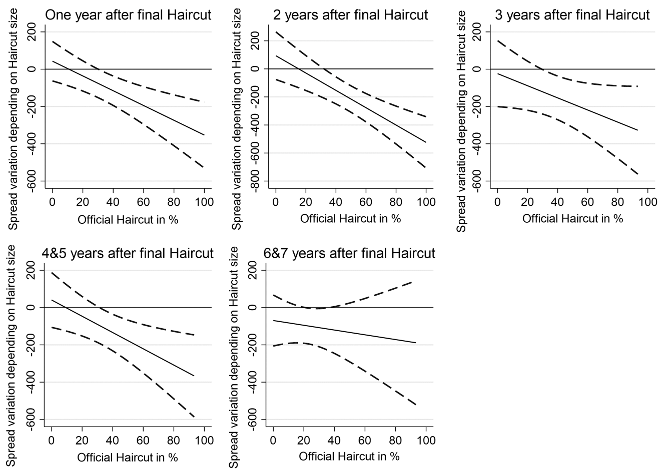
Notes: This table shows coefficients of an OLS fixed effects model at the country-period level. The dependent variable is the monthly average country yield spread over US Treasury bonds (EMBIG stripped spread) measured in basis points (bp). Country and year fixed effects are included. S.E at the country level, t-stats in parentheses. Significance levels: *0.10, **0.05, ***0.01.

Figure 10a: Expected effect on bond spread for different levels of private haircut



Notes: Each graph shows the marginal effect of private haircut on bond spreads, for different haircut sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 6, column 6. The spread increase after a restructuring is statistically significant for levels of haircut at which the lower confidence band is above the zero horizontal line.

Figure 10b: Expected effect on bond spread for different levels of official haircut



Notes: Each graph shows the marginal effect of official haircut on bond spreads, for different haircut sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 6, column 6. The spread decrease after a restructuring is statistically significant for levels of haircut at which the upper confidence band is below the zero horizontal line.

3.6. Discussion of the results

This section provides further discussion on the reasons behind the differences in sovereign risk in the aftermath of a final restructuring with private as opposed to official creditors. As we mentioned in the introduction, the higher cost of private defaults is most likely driven by a less creditor-friendly negotiation process, while Paris club official restructurings are supposed to guarantee a relatively smoother approach in the way in which deals are actually orchestrated. Hence, we focus on three reasons explaining this difference. The first reason is the greater overall visibility of private deals, as opposed to official ones. As previously mentioned, official defaults occur without much media coverage and hence are less likely to result in collateral media damage. On the other hand, defaulting on private debt is highly visible and more likely to result in a rating downgrade. For example, recent highly publicized cases of private restructurings (read Greece 2010 and Argentina v. NML Capital) indicate that private restructurings are considerably more influential for financial markets. A second reason may depend on the circumstance that official lenders, at least to some extent, shoulder the debt burden for private creditors, as suggested by new evidence from both Horn *et al.* (2020) and Schleg *et al.* (2019), which could explain why we find evidence of positive market sentiment in the aftermath of an official restructuring.[43] In order to disentangle this possible effect, we first distinguish in our sample of restructuring episodes between "pure" official restructurings (official restructurings occurring independently of others) and "twin" official restructurings involving both private and official creditors. In the case of our bond spreads sample, however, we consider a looser definition of "pure", and also include those official events that were anticipated by a preemptive private restructuring (Asonuma and Trebesch 2016).[44] This is for two reasons: on the one hand, we have too few cases of pure official restructurings due to the reduced size of the bond spread sample. On the other hand, we believe our original motivation stands for grouping them, as these two types of events have in common the fact of not being related to an actual private default.

Figures 11 and 12 (and accompanying Table B7 in the online Appendix B) show the ATE for the two different samples. Within Table B7, while panels A and B show ATEs of pure vs. twin official restructurings on changes in agency ratings, panels C and D show average

[43] For example, even the decline in bond spread after the DSSI (Lang *et al.* 2021) could be explained by the fact that private markets interpret the postponement of debt service repayments to official creditors as good news for their own debt service repayments (Essers and Cassimon 2021).

[44] Defaults may or may not precede restructurings (ex-post vs. preemptive restructurings). Asonuma and Trebesch (2016), Asonuma et al. (2016), Asonuma et al. (2021) and Asonuma and Joo (2022), using different outcome variables, find that preemptive defaults are generally less costly than post defaults. Following Asonuma and Trebesch (2016), in our sample, we identify a list of official restructurings that are preceded by a preemptive (private) restructuring; Chile (preemptive 1990), Dominican Republic (preemptive 2005), Grenada (preemptive 2005), Mexico (preemptive 1990), Morocco (preemptive 1990), Philippines (preemptive 1992), Senegal (preemptive 1985), Trinidad and Tobago (preemptive 1989), and Ukraine (preemptive 2000).

Table 7: ATE, Restructuring in countries with either only official deals ("pure") or both ("twin")

	Year						
Panel A: "Pure" official restructurings, rating	1	2	3	4	5	6	7
AIPW	0.34***	1.27***	1.60***	1.86***	1.16***	1.38***	1.44***
	(4.50)	(8.90)	(7.83)	(8.13)	(4.53)	(4.79)	(4.44)
Observations	8945	8945	8369	7793	7206	6622	6046
Panel B: "Twin" official restructurings, rating	1	2	3	4	5	6	7
AIPW	0.38***	0.98***	1.28***	1.24***	0.82***	0.53**	0.79***
	(8.23)	(11.07)	(9.65)	(7.65)	(4.38)	(2.51)	(3.33)
Observations	12441	12441	11613	10792	9965	9145	8341
Panel C: "Pure" official restructurings, bond spread	1	2	3	4	5	6	7
AIPW	-288.61***	-329.99***	129.16	142.12	-10.05	-395.11***	-647.64***
	(-5.84)	(-3.09)	(0.73)	(0.71)	(-0.06)	(-4.70)	(-8.87)
Observations	2757	2451	2162	1880	1648	1420	1203
Panel D: "Twin" official restructurings, bond spread	1	2	3	4	5	6	7
AIPW	-356.89***	-380.52***	-344.49***	-338.58***	-460.72***	-335.75***	-485.21***
	(-10.82)	(-6.02)	(-3.86)	(-4.10)	(-8.32)	(-6.82)	(-11.82)

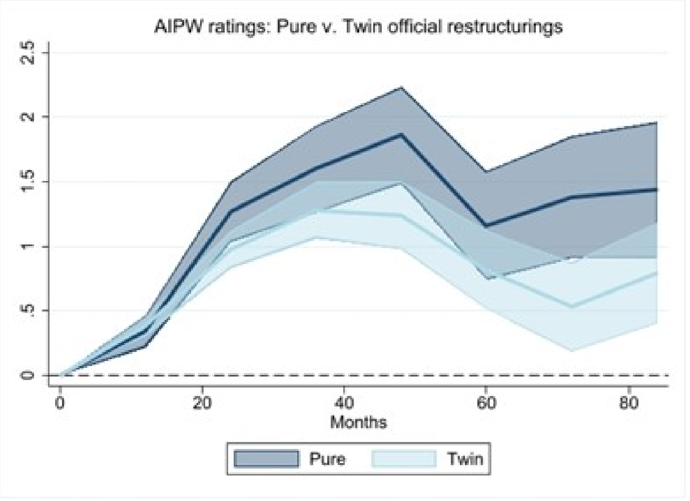
Notes: Panels A, C, and E show average treatment effects of a "pure" official restructuring (i.e., official restructurings not preceded by other events, or at most following a preemptive private restructuring) on changes in average agency ratings and bond spreads, respectively. Panels B, and D show average treatment effects of "twin" restructurings (i.e., countries with both official and private restructurings). T-statistics in parenthesis. The model uses predictors and controls for first and second stage listed in the Online Appendix B. Significance levels: *0.10, **0.05, ***0.01.

treatment effects of the extended sample of "pure" (i.e., pooling together pure official restructurings with those anticipated by a preemptive private deal) vs. twin restructurings on changes in bond spreads. Interestingly, we find different results in the case of rating and spread. As can be seen, the change in rating, with respect to the base year, is increasing under both types of classifications. However, such variation seems more pronounced in the case of pure restructurings, suggesting that agency ratings evaluate more positively the exit from an official agreement when not overlapping with a private private debt restructuring. On the other hand, when considering bond spreads, the decrease is greater when private restructurings are also taking place. Since bond spreads mainly reflect (forward looking) market sentiment of private creditors, some free-riding by private creditors -and/or an implicit subsidy from official bilateral creditors- may explain the more positive market reaction at times of financial turmoil.[45]

The last reason may depend on the different relationship between debtors and official or private creditors. More generally, contrary to official defaults, the relationship between debtors and private creditors may vary a lot across crises (and sometimes even during the same default episode). As illustrated by Trebesch and Zabel (2017), there are striking differences across debt crisis events. On the one hand, there are cases such as Russia during the 1990s, Ecuador 2008-09 or Argentina 2002-05, in which governments opted for a unilateral payment moratorium and sometimes even refused to negotiate with their foreign

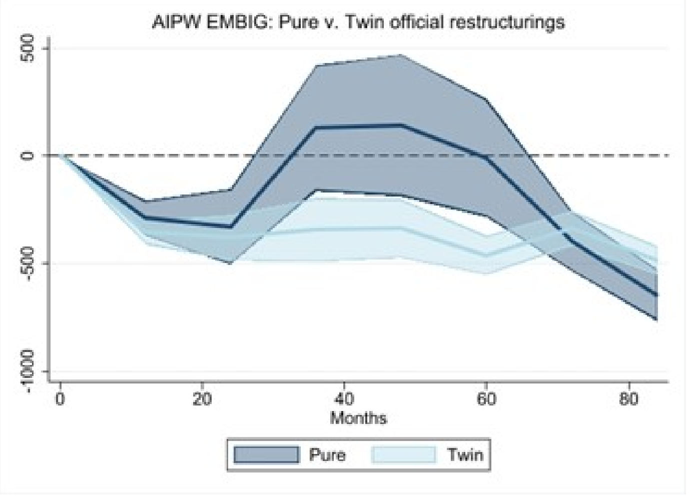
[45]We could not make the same comparison in the case of pure private restructuring as the estimated sample of only private defaulters was not large enough.

Figure 11: Year-by-year ATE of "pure" vs. "twin" official restructurings, ratings



Notes: Graphs show AIPW average treatment effect estimates for each h-step ahead forecast of change in agency ratings following the end of either a "pure" official restructuring (i.e., official restructurings not preceded by other events, or at most following a pre-emptive private restructuring) or a "twin" official restructuring (i.e., countries with both a private and official restructuring).

Figure 12: Year-by-year ATE of "pure" vs. "twin" official restructurings, bond spreads



Notes: : Graphs show AIPW average treatment effect estimates for each h-step ahead forecast of change in bond spreads following the end of either a "pure" official restructuring (i.e., official restructurings not preceded by other events, or at most following a pre-emptive private restructuring) or a "twin" official restructuring (i.e., countries with both a private and official restructuring).

banks and bondholders. On the other hand, there are debt crises that were solved in a consensual manner, with close creditor consultations and little (or no) missed payments (examples may include Ukraine in 1999-2000 and Uruguay in 2003).
What is more, recent evidence shows that disruptive private creditors litigation on external debt has been increasing over time.[46] More specifically, as recently shown by Schumaker *et al. (*2021), the existence of litigation costs has strengthened the hands of private external creditors and raised the cost of default for debtors. These authors find that legal disputes in the US and the UK disrupt government access to international capital markets. To the extent that litigation increases the default costs but involves only external debt held by private creditors, these findings may help us to understand the increase in borrowing costs in the aftermath of a default with private creditors.[47]
In order to provide some evidence of the importance of litigation costs for sovereign risk, in the next section, we extend our framework to empirically test how litigation costs may increase sovereign risk both during a debt crisis and in the aftermath of a default.

3.6.1. Litigation costs

Schumaker *et al. (*2021) provide new data on litigation costs, building, in particular, three measures of litigation. The first indicator is a dummy equal to one in those years in which a sovereign faces one or more pending creditor lawsuits. The second indicator is coded as one if one or more creditors attempt to seize assets of the respective sovereign. Finally, the third indicator is a variable built as a share of litigation to GDP, which is constructed by using the available information on case amounts and then summing the amounts at a country-year level. In this section, we use this last measure and consider either all the data on litigation (to GDP) occurring throughout the default period, or only the *final* one, that is the case amount associated with the debt crisis exit.
Since litigation costs are available at the country year level, we take as dependent variable the Institutional Investor's creditworthiness index (Institutional Investor Magazine), which was published twice a year since 1979 (up to 2016) in the March and September issues of the Institutional Investor Magazine.[48] We take annual observations (i.e., yearly averages of these bi-annual data) of this variable. This rating is based on information

[46]While defaulting governments were immune from legal action by foreign creditors for centuries, Schumaker *et al.* 2021 show that this is no longer the case. More generally, the interest in the legal aspects of sovereign debt contracts has been increasing over the more recent years (among others, see Bolton *et al*. 2020; Carletti *et al*. 2020; Fang *et al.* 2021).

[47]Schumaker *et al.* (2021) describe the evolution of the litigation environment. In particular, they distinguish among three different phases: (1) Erosion of sovereign immunity (1976-1991); (2) Entry of specialized hedge funds 1992-1999; (3) Asset seizures and *pari passu* (2000-2010).

[48]We used the Institutional Investor rating, in this case, because the data on ratings can be matched more consistently with the data on litigation, which are annual. In Table C3 and Figure C2, in the online Appendix C, however, we show that the results hold as well when using monthly data on bond spreads.

provided by economists and sovereign risk analysts at leading global banks and securities firms. The rating grades each country on a scale from 0 to 100 and is available for 178 countries over the period 1979-2016.[49] Unfortunately, we cannot directly control for litigation in our baseline specifications, as litigation can be observed both during the debt crisis and after the final restructuring. As in our baseline model, we exclude observations during crisis years in order to focus on the post-default period.
The sample of countries is the same as that used in the previous sections, while the data now go from 1990 to 2010, as litigation costs are available only up to 2010. More specifically, we estimate the following two equations:

$$IR_{i,t} = \alpha + \beta Z_{i,t-1} + \gamma C_{i,t} + \delta L_{i,t} + \zeta (C_{i,t} * L_{i,t}) + \eta_i + \tau_t + u_{i,t}\ , \tag{3.5}$$

and:

$$IR_{i,t} = \alpha + \beta Z_{i,t-1} + \gamma_j FC_{i,t-j} + \lambda_j FL_{i,t-j} + \eta_i + \tau_t + u_{i,t} \qquad j = 1, \ldots 3,\ 4\&5,\ 6\&7 \tag{3.6}$$

where $IR_{i,t}$ represents the annual Investor rating of a country i, at year t. $C_{i,t}$ is a dummy equal to one for every year of the default spell, while $L_{i,t}$ denotes the size of litigation to GDP. $FC_{i,t-j}$ is a dummy equal to one in the last year of the private debt crisis, while $FL_{i,t-j}$ denotes the amount of litigation to GDP associated with the end of the default spell. X is a vector containing the control variables (lagged one year). η_i, and τ_t denote country and year dummies, respectively. The list of controls is the same as the one described in the previous sections.
To investigate more carefully the importance of litigation for sovereign risk, contrary to previous specifications, we now consider both the duration of the debt crisis and up to seven periods after the end of default. Considering the scope of litigation within the crisis period is crucial because it helps quantify the determinants of a negative drop in ratings, while looking at the effect after the crisis retains the same interpretation as in our original analysis (that is, the reputational effects of a default event). Thus, we apply our baseline specification from the start of the debt crisis, and using duration data for private (Asonuma and Trebesch 2016) debt crises. As above, we then include up to seven-year lags of both the occurrence and the magnitude of *final* litigation.
The results are presented in Table 8. In columns 1-2, we control for both the duration of a private debt crisis, and the amount of litigation to GDP (expressed in percentage points) on

[49] As pointed out by Reinhart and Rogoff (2009), the Institutional Investor's index can be then seen as a survey-based measure of the perceived creditworthiness of a large number of countries, with two main differences with respect to the credit ratings provided by agencies. First, this index can be regarded as a continuous variable, while the credit ratings assigned by the rating agencies have the features of a discrete variable. Second, this index changes annually over time, while the ratings may remain constant for a long period of time.

private debt, with and without control variables, respectively. In columns 3-4, we include final private litigation to GDP (that is associated with the end of the debt crisis) up to seven years after the end of the default spell, with and without control variables, respectively.

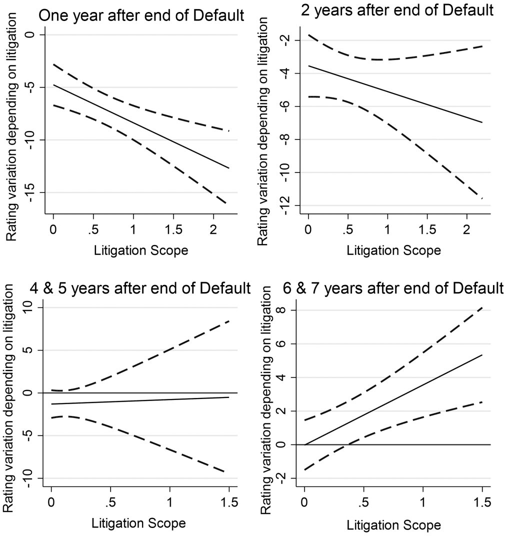
Table 8: Private default, Investor's index and litigation, OLS

	(1)	(2)	(3)	(4)
End of Private Default Dummy			-3.517***	-5.160***
			(-3.077)	(-3.939)
End of Private Default Dummy (-1)			-2.263*	-4.397***
			(-1.850)	(-3.420)
End of Private Default Dummy (-2)			-0.742	-2.873**
			(-0.611)	(-2.206)
End of Private Default Dummy (-3)			-0.246	-1.525
			(-0.206)	(-1.225)
End of Private Default Dummy (-4 & 5)			0.249	-0.421
			-0.233	(-0.395)
End of Private Default Dummy (-6 & 7)			0.783	1.01
			-0.748	(-1.016)
Litigation scope x End of Private Default			-3.244	-1.796
			(-1.473)	(-1.193)
Litigation scope x End of Private Default (-1)			-4.557***	-3.130**
			(-2.768)	(-2.366)
Litigation scope x End of Private Default (-2)			-1.67	-0.7
			(-0.866)	(-0.432)
Litigation scope x End of Private Default (-3)			-2.015	-1.574
			(-1.052)	(-1.042)
Litigation scope x End of Private Default (-4 & 5)			-0.487	1.702
			(-0.088)	(-0.456)
Litigation scope x End of Private Default (-6 & 7)			2.635	2.612
			-1.429	(-1.582)
Litigation scope	-0.905*	-0.432	-1.796	-1.182
	(-1.879)	(-1.566)	(-1.454)	(-1.284)
Private Default duration	-5.132***	-4.603***		
	(-2.892)	(-2.964)		
Private Default duration x Litigation scope	-3.164***	-2.490***		
	(-4.367)	(-3.938)		
Constant	31.015***	25.372***	28.980***	23.219***
	-24.68	(-8.26)	-27.328	(-7.433)
Observations	1,897	1,364	1,830	1,357
R-squared	0.598	0.657	0.585	0.656
Number of Countries	98	77	98	77
Controls	NO	YES	NO	YES
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

Notes: This table shows coefficients of an unbalanced panel data OLS regression with fixed effects at the country-year level. Standard errors are clustered at the country level. The dependent variable is the Institutional Investor's creditworthiness index (Institutional Investor Magazine). t statistics are in parentheses. Significance levels: *0.10, **0.05, ***0.01.

As our variables of interest are concerned, *during the debt crisis*, we can observe that prolonged private debt crises are associated with a significant contraction of the Investor's rating of about 4.6 to 5.1 points in the Investor's rating per year, depending on the specification (all coefficients are significant at the one percent level). As the average duration of a private default is about 8 years in this sample, this result implies that the average GDP loss associated with private default is about 40 points in total. The coefficient of the interaction between the scope of litigation and duration is also negative and significant at the one percent level. The size of the coefficients goes from 2.5 to 3.2, implying a total contraction in the Investor's rating of about 8 points due to litigation costs.
After the end of the debt crisis, in column 4, we find that both the coefficients denoting the lags of the end of the default spell and those denoting the lags of the litigation size are always negative and generally significant up to five years after the final agreement. The best way to interpret these findings, however, is to look at Figure 13, which shows the expected variation in Investor rating conditional on the litigation size. The different panels correspond to how many years after the end of the default spell rating is being measured, and the dotted lines show 90 percent confidence bands. The effects are calculated from the specification of Table 8, column 4. The rating decrease due to litigation costs is statistically significant for levels of litigation at which the lower confidence band is below the zero horizontal line. We can observe a significant decrease in Investor rating for any size of litigation from one to three years after the end of default. After four to five years, the effect is not significant, while after six to seven years ratings start to improve.
In summary, we find that ratings decrease with litigation costs in the aftermath of a default. To the extent that litigation costs characterize deals with private creditors only, these results may explain the different outcomes in terms of sovereign risk of private versus official deals.

Figure 13: Expected effect on Investor rating for different levels of litigation scope



Notes: Each graph shows the marginal effect of litigation size on Investor rating for different litigation sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table 8, column 4. The rating decrease due to litigation costs is statistically significant for levels of litigation at which the lower confidence band is below the zero horizontal line.

3.7. Conclusions

This paper studies the relationship between sovereign debt default and a country's creditworthiness by considering the depth of a debt restructuring and distinguishing between commercial and official sovereign debt agreements.

We analyze 130 final restructurings, of 130 countries, over the period 1990-2018, and we consider agency ratings and bond spreads as indirect and direct measures of borrowing costs, respectively. Using both the adjusted inverse propensity-score weighted estimator and a standard panel data analysis, we find a lasting relationship between debt default and credit risk. More specifically, this paper provides evidence of the heterogeneous effect of final private and official restructurings on borrowing costs: (i) private events are more costly (in terms of higher sovereign risk) than official ones; (ii) the rating (spread) variation (increase) is larger for cases with deeper haircuts.

Our results point to the importance of the way in which debt restructurings are orchestrated, in line with the distinction between "hard" and "soft" defaults (Asonuma and Trebesch 2016, Trebesch and Zabel 2017). To the extent that Paris Club deals may represent an example of a "soft" default, this evidence suggests that they are associated with better outcomes in terms of borrowing costs.

To conclude, we find further evidence for the heterogeneity of the economic impact of debt restructurings, confirming that official and private defaults may have different costs and then induce selective defaults. Debtor countries, being aware that the consequences of default depend on who the defaulted creditors are, may then decide to prioritize their repayments accordingly. In particular, these results are consistent with Schlegl *et al.* (2019), who find that Paris club creditors bore higher losses (haircuts) than private creditors, over the years 1970-2015. The increase in borrowing costs we detect after private restructurings may then explain why private creditors are paid first and lose less than official ones. As a number of debt restructurings, including those with official creditors, become more likely over the next years, it will become crucial to consider who is going to bear the actual costs of sovereign debt renegotiation.

Appendix

Appendix A: Sample and descriptive statistics

Table A1a: Country sample, defaulters

Country	Private restructurings	Official restructurings
Albania	1991-1995	1993-2000
Angola		1989
Argentina	1982-1993, 2001-2005	1985-1992, 2014
Belize	2006-2013	
Benin		1989-2003
Bolivia	1980-1993	1986-2001
Bosnia Herzegovina	1992-1997	1998-2000
Brazil	1983-1994	1983-1992
Bulgaria	1990-1994	1991-1994
Burkina Faso		1991-2002
Cambodia		1995
Cameroon	1985-2003	1989-2006
Chile	1983-1990	1975-1987
Congo, Dem. Rep.	1975-1989	1976-1989, 2002-2010
Congo, Rep.	1983-1988, 2007	1986-2004, 2010
Costa Rica	1981-1990	1983-1993
Cote d'Ivoire	1983-1998, 2000-2012	1984-1994, 1998-2012
Croatia	1992-1996	1995
Cuba	1983-1985	1985-1986
Dominican Republic	1982-1994, 2004-2005	1985-1991, 2004-2005
Ecuador	1982-1995, 1999-2000, 2008-2009	1983-2003
Egypt, Arab Rep.		1987-1991
El Salvador		1990
Ethiopia	1990-1996	1992-2004

Continued on next page

Gabon	1986-1994	1987-1995, 2000-2004
Georgia		2001-2004
Ghana		1996-2004
Greece	2012	
Grenada	2004-2005	2006
Guatemala		1993
Honduras	1981-2001	1990-2005
Indonesia		1994-2005
Iraq	1986-2006	
Jamaica	1977-1990	1984-1993
Jordan	1989-1993	1989-2002
Kenya	1992-1998	1994-2004
Kyrgyz Republic		2002-2005
Macedonia	1983-1988, 1992-1997	1984-1988, 1995-2000
Mali		1988-2003
Mexico	1982-1990	1983-1989
Moldova	2001-2004	2006
Morocco	1983-1990	1983-1992
Mozambique	1983-1991, 2007	1984-2001
Nicaragua	1978-1995, 2007	1991-2004
Nigeria	1982-1991	1986-1991, 2000-2005
Pakistan	1998-1999	1981, 1999-2001
Panama	1984-1996	1985-1990
Paraguay	1986-1993	
Peru	1978-1997	1978-1996
Philippines	1983-1992	1984-1994
Poland	1981-1994	1981-1991
Romania	1981-1983, 1986	1982-1983
Russia	1991-2000	1993-1999
Rwanda		1998-2005
Senegal	1980-1985, 1990-1996	1981-2004
Serbia	1992-2004	2001
Seychelles	2008-2010	
Slovenia	1992-1996	
South Africa	1985-1993	
Sri Lanka		2005
Trinidad and Tobago	1988-1989	1989-1990

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Turkey	1976-1982	1978-1980
Uganda	1979-1993	1981-2000
Ukraine	1998-2000	2001
Uruguay	1983-1991, 2003	
Venezuela, RB	1983-1990	
Viet Nam	1982-1997	1993
Zambia	1983-1994	1983-2005

Notes: Countries in **bold** have only private restructurings, while countries in *italics* have only official restructurings.

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Table A2a: Country sample, non-defaulters

Andorra	Czech Rep.	Lesotho	Slovak Rep.
Armenia	Estonia	Libya	St. Vincent and the Gren.
Aruba	Faroe Islands	Liechtenstein	Suriname
Azerbaijan	Fiji	Lithuania	Taiwan
The Bahamas	French Polynesia	Macao	Tajikistan
Bahrain	Gibraltar	Malaysia	Thailand
Bangladesh	Hong Kong	Maldives	Tunisia
Barbados	Hungary	Malta	Turkmenistan
Belarus	India	Mauritius	Turks and Caicos Islands
Bermuda	Iran	Mongolia	United Arab Emirates
Botswana	Isle of Man	Montenegro	Uzbekistan
Cabo Verde	Israel	Namibia	
Cayman Islands	Kazakhstan	Oman	
China	South Korea	Papua New Guinea	
Colombia	Kuwait	Qatar	
Curacao	Latvia	Saudi Arabia	
Cyprus	Lebanon	Singapore	

Table A2a: List of agencies

Variable	**Observations**	**Countries**	**Years**	**Headquarter**	**Source**
Standard & Poor's (S&P)	24621	114	1977-2018	United States	Bloomberg
Moody's Investors Service	22950	117	1986-2018	United States	Bloomberg
Fitch Ratings	18596	99	1994-2018	United States/France	Bloomberg
Dominion Bond Rating Services (DBRS)	1609	20	2006-2018	Canada	Bloomberg
Dagong Global	6079	67	2010-2018	China	Bloomberg
Rating and Investment Information (R&I)	6189	28	1998-2018	Japan	Bloomberg
Japan Credit Rating Agency (JCR)	4041	21	1998-2018	Japan	Bloomberg
Capital Intelligence (CI)	4884	36	2002-2018	Cyprus/Kuwait	Bloomberg

Table A2b: Correlations between agency credit rating, 1990-2018

Agency	1	2	3	4	5	6	7	8
Standard & Poor's (S&P)	1							
Moody's Investors Service	0.979	1						
Fitch Ratings	0.991	0.987	1					
Dominion Bond Rating Services (DBRS)	0.977	0.992	0.988	1				
Dagong Global	0.869	0.913	0.907	0.919	1			
Rating and Investment Information (R&I)	0.934	0.955	0.957	0.954	0.973	1		
Japan Credit Rating Agency FN (JCR)	0.942	0.966	0.968	0.972	0.980	0.992	1	
Capital Intelligence (Cyprus)	0.974	0.991	0.988	0.989	0.942	0.979	0.986	1

Table A2c: Correlation between agency rating (mean) and EMBIG spread

	1	2
Agency rating (mean)	1	
EMBIG spread	-0.563	1

Table A3: Translation of sovereign ratings into numerical values

CI	Dagong	DBRS	Fitch	Moody's	JCR	R&I	S&P	Numerical scale
CYP (KWT)	CHN	CAN	USA (FRA)	USA	JPN	JPN	USA	
AAA	AAA	AAA	AAA	Aaa	AAA	AAA	AAA	21
AA+	AA+	AAH	AA+	Aa1	AA+	AA+	AA+	20
AA	AA	AA	AA	Aa2	AA	AA	AA	19
AA-	AA-	AAL	AA-	Aa3	AA-	AA-	AA-	18
A+	A+	AH	A+	A1	A+	A+	A+	17
A	A	A	A	A2	A	A	A	16
A-	A-	AL	A-	A3	A-	A-	A-	15
BBB+	BBB+	BBBH	BBB+	Baa1	BBB+	BBB+	BBB+	14
BBB	BBB	BBB	BBB	Baa2	BBB	BBB	BBB	13
BBB-	BBB-	BBBL	BBB-	Baa3	BBB-	BBB-	BBB-	12
BB+	BB+	BBH	BB+	Ba1	BB+	BB+	BB+	11
BB	BB	BB	BB	Ba2	BB	BB	BB	10
BB-	BB-	BBL	BB-	Ba3	BB-	BB-	BB-	9
B+	B+	BH	B+	B1	B+	B+	B+	8
B	B	B	B	B2	B	B	B	7
B-	B-	BL	B-	B3	B-	B-	B-	6
CCC+	CCC+	CCCH	CCC+	Caa1	CCC+	CCC+	CCC+	5
CCC	CCC	CCC	CCC	Caa2	CCC	CCC	CCC	4
CCC-	CCC-	CCCL	CCC-	Caa3	CCC-	CCC-	CCC-	3
CC	CC	CC	CC	Ca	CC	CC	CC	2
C	C	C	C	C	C			1
DDD		DDD		DDD			SD	1
DD		DD		DD				1
D	D	D	D	D	D	D	D	1
			RD	RD				1

Notes: 21 point scale. Fuchs and Gehring (2017).

Table A4: Descriptive statistics

Variable	N	Mean	SD	Min	Max
EMBIG	5382	368.25	296.96	0	3158.22
Credit rating	14605	11.38	3.67	1.2	19
Final Private Haircut Dummy	14605	0.01	0.02	0	1
Final Official Haircut Dummy	14605	0.01	0.03	0	1
Final Private Haircut	14605	0.46	0.31	0.11	0.95
Final Official Haircut	14605	0.62	0.35	0.05	1
Reserves to external debt	14605	19.33	15.94	0.89	119.74
General gov. gross debt to GDP	14605	43.82	29.04	1.56	183.07
Real GDP growth	14605	4.25	3.95	-15.14	34.46
Inflation	14605	6.19	6.83	-4.87	60.53
Primary len/borr	14605	0.25	5.05	-26.35	30.68
Current account	14605	-0.96	9.22	-54.14	45.46
Political risk	14605	66.77	8.91	34.75	86.58
IMF net loans	14605	0.01	0.62	-4.21	5.89

Appendix B: Augmented inverse propensity score weighted estimator

Table B1: Variable definitions and sources

Variable	Definition	Source
CONTROLS & PREDICTORS: USED IN BOTH STAGE 1 (LOGIT) AND STAGE 2 (LOCAL PROJECTION)		
Real GDP growth	Annual percentage change of GDP, constant prices	World Development Indicators, World Bank (2018)
Reserves to GDP	Total reserves (% of GDP)	World Development Indicators, World Bank (2018)
General gov. gross debt to GDP	General government gross debt to GDP	International Financial Statistics (2018)
PREDICTORS USED IN STAGE 1 (LOGIT) ONLY		
U.S effective federal funds rate	Federal Funds Effective Rate, Percent, Monthly, Not Seasonally Adjusted	FRED, Federal Reserve Bank of St. Louis
Share of past months in default	Share of past months in default, specific to years available for each country in sample	Built by the authors
High inflation dummy	Dummy = 1 for years when inflation > 50%	Built by authors, International Financial Statistics, IMF (2018)
Openness	Trade as % of GDP	World Development Indicators, World Bank (2018)
Primary balance	Government revenue/GDP – Government expenditure/GDP	International Financial Statistics, IMF (2018)
Bank credit to GDP	Domestic credit to private sector by banks, % of GDP	World Development Indicators, World Bank (2018)
Bank crises dummy	Dummy = 1 when country is under a banking crisis in a given year	Laeven and Valencia (2020)
CONTROLS USED IN STAGE 2 (LOCAL PROJECTION) ONLY		
Final Private Haircut	Private debt haircut, in percent	Built by the authors, based on Cruces and Trebesch (2013b)
Final Private Haircut Dummy	Dummy =1 in case of a private haircut	Built by the authors
Final Official Haircut	Official debt haircut, in percent	Built by the authors, based on Cheng et al. (2017)
Final Official Haircut Dummy	Dummy =1 in case of an official haircut	Built by the authors
Gen gov. primary lenn/borr	General government primary net lending/borrowing, % of GDP	International Financial Statistics, IMF (2018)
Inflation	Annual percentage change in average consumer prices	International Financial Statistics, IMF (2018)
Current Account	Current account balance, % of GDP	International Financial Statistics, IMF (2018)
IMF net loans	IMF net loans, ratio to GDP	World Development Indicators, World Bank (2018)
Political risk	ICRG political risk index	International Country Risk Guide, The PRS Group (2018)

Table B2: First stage, logit results

	Agency rating/EMBIG	Dyadic
General gov. gross debt to GDP (-1)	0.0105**	0.043***
	(1.97)	(6.55)
Per capita GDP (-1)	-0.031**	0.071***
	(-2.11)	(0.92)
Reserves to GDP (-1)	-0.0155	-0.096**
	(-1.32)	(-2.44)
High inflation dummy(-1)	-2.597*	-1.083
	(-1.75)	(-0.8)
Bank credit to GDP (-1)	-0.0155	-0.029**
	(-1.41)	(-2.3)
Primary balance (-1)	-0.006*	0.033
	(-1.97)	(0.74)
Openness (-1)	0.001	0.0155*
	(0.31)	(1.95)
Share of past months in default	3.376***	4.244***
	(5.84)	(7.44)
Federal funds rate (-1)	0.286***	0.194
	(4.77)	(1.53)
Bank crises (-1)	1.921**	3.859***
	(2.29)	(4.44)
Constant	-5.926***	-10.318***
	(-5.46)	(1.603)
Income group dummies	Yes	Yes
Pseudo R-squared	0.32	0.52
Adjusted pseudo R-squared	0.31	0.51
Observations	30,948	55,953

Notes: The model uses predictors listed in Table B1 in the first stage and income group dummies as fixed effect. Standard errors are clustered at the country level, t-statistics in parenthesis. Significance levels: *0.10, ** 0.05, *** 0.01.

Table B3: Comparing first stage models

	One-type model	Two-type model		Three-type model		
	Private or official restructuring years	Private restructuring years	Official restructuring years	Private restructuring years	Official restructuring years	Private or official restructuring years
General gov. gross debt to GDP (-1)	0.0105**	0.0116*	0.00909	0.0128*	0.0101	0.0124
	(1.97)	(2.14)	(1.78)	(2.11)	(1.78)	(1.85)
Real GDP growth (-1)	-0.031**	0.00440	-0.0338*	0.00347	-0.0350*	-0.0770***
	(-2.11)	(0.27)	(-2.16)	(0.21)	(-2.25)	(-3.61)
Reserves to GDP (-1)	-0.0155	-0.0351	-0.00703	-0.0345	-0.00654	-0.0619*
	(-1.32)	(-1.48)	(-0.54)	(-1.51)	(-0.50)	(-2.09)
High inflation dummy (-1)	-2.597*	-17.16***	-2.654	-17.87***	-2.591	-2.232
	(-1.75)	(-18.00)	(-1.69)	(-19.01)	(-1.57)	(-1.04)
Bank credit to GDP (-1)	-0.0155	-0.0119	-0.0206	-0.0121	-0.0225	0.00592
	(-1.41)	(-1.09)	(-1.40)	(-1.10)	(-1.59)	(0.61)
Primary balance (-1)	-0.006*	0.0197	-0.0111*	0.0239	-0.0107*	0.0286*
	(-1.97)	(0.79)	(-2.50)	(0.81)	(-2.35)	(2.00)
Openness (-1)	0.001	-0.00116	-0.000107	-0.000945	0.000136	0.00614
	(0.31)	(-0.29)	(-0.03)	(-0.23)	(0.04)	(1.54)
Share of past months in default	3.376***	2.149	3.280***	2.173	3.273***	5.167***
	(5.84)	(1.42)	(5.02)	(1.42)	(5.04)	(3.50)
Federal funds rate (-1)	0.286***	0.127	0.278***	0.129	0.272***	0.539***
	(4.77)	(1.81)	(4.23)	(1.80)	(4.18)	(6.63)
Bank crises (-1)	1.921**	3.076***	0.713	3.153***	1.035	2.184
	(2.29)	(3.77)	(0.77)	(3.75)	(1.00)	(1.37)
Constant	-5.926***	-6.664***	-6.278***	-6.807***	-6.300***	-9.588***
	(-5.46)	(-5.22)	(-4.83)	(-5.28)	(-4.94)	(-6.11)
Income group dummies	YES	YES	YES	YES	YES	YES
AIC	13,917	14,305	17,764			
Observations	30,948	30,474	30,948			

Notes: The table compares the AIC for 3 different discrete outcome models for the first stage. All models consider as the base outcome the non-restructuring years. "One-type model" is equivalent to a logit model with dummy equal to one for either private or official restructuring years. "Two-type model" is a multinomial logit where the outcomes are private restructuring years in one case and official in the other. The "three-type model" considers an additional outcome when restructuring years are for both private and officials. Income group fixed effects are included. Standard errors are clustered at the country level, t-statistics in parenthesis. Significance levels: *0.10, ** 0.05, *** 0.01.

Table B4: Second stage, average ratings, private and official restructuring

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
Final private haircut (dummy)	-0.670	-1.000	-1.962*	-2.254*	-1.348	-0.437	-0.765
	(-1.59)	(-1.10)	(-1.78)	(-1.67)	(-1.25)	(-0.31)	(-0.53)
Final official haircut (dummy)	0.335	0.843	1.122*	1.032	0.645	0.440	0.647
	(1.65)	(1.45)	(1.73)	(1.26)	(1.06)	(0.49)	(0.80)
Final private haircut	1.414***	2.463*	3.925***	4.115***	3.303***	2.848	3.500*
	(2.67)	(1.70)	(2.68)	(2.69)	(2.71)	(1.61)	(1.76)
Final official haircut	-0.002	-0.014	-0.022	-0.026	-0.022	-0.024	-0.031**
	(-0.36)	(-1.24)	(-1.55)	(-1.56)	(-1.37)	(-1.27)	(-2.38)
Reserves to GDP	0.010***	0.014**	0.015	0.015	0.015	0.013	0.004
	(2.72)	(2.12)	(1.59)	(1.36)	(1.40)	(1.16)	(0.37)
General gov. gross debt to GDP	0.003	0.008	0.014*	0.018**	0.021**	0.022*	0.019*
	(0.84)	(1.29)	(1.70)	(2.02)	(2.05)	(1.95)	(1.67)
Real GDP growth	0.024***	0.037***	0.041***	0.032**	0.030	0.025	0.008
	(2.81)	(2.73)	(2.79)	(2.03)	(1.66)	(1.33)	(0.46)
Inflation (annual % change)	0.003	0.008	0.011	0.010	0.007	-0.000	-0.002
	(0.48)	(0.86)	(0.83)	(0.58)	(0.38)	(-0.02)	(-0.11)
Gen gov. primary lenn/borr	0.017**	0.023*	0.016	0.003	0.000	0.017	0.023
	(2.34)	(1.78)	(0.84)	(0.13)	(0.02)	(0.56)	(0.84)
Current account balance to GDP	0.021***	0.037***	0.056***	0.062***	0.061***	0.051**	0.044**
	(4.71)	(4.08)	(3.56)	(3.17)	(2.89)	(2.41)	(2.19)
Political risk	-0.024**	-0.050***	-0.078***	-0.095***	-0.105***	-0.114***	-0.130**
	(-2.53)	(-2.86)	(-3.19)	(-3.15)	(-2.87)	(-2.70)	(-2.63)
IMF	0.027	0.026	0.034	-0.049	-0.063	0.015	0.100
	(0.64)	(0.40)	(0.43)	(-0.54)	(-0.58)	(0.13)	(0.86)
R-squared	0.18	0.24	0.30	0.36	0.42	0.51	0.60
Observations	14605	14605	13633	12656	11673	10697	9750
Time FE	YES	YES	YES	YES	YES	YES	YES
Country FE	YES	YES	YES	YES	YES	YES	YES

Notes: Table shows inverse propensity weighted regression results for each h-step ahead forecast on change in average agency ratings. The model uses controls listed in Table B1 and controls for country fixed effects and time-varying heterogeneity. Standard errors are clustered at the country level, t-statistics in parenthesis. Significance levels: *0.10, ** 0.05, *** 0.01.

Table B5: Second stage, bond spread, private and official restructuring

	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7
Final private haircut (dummy)	203.864	261.045	1394.773*	141.040	181.354	483.390	169.077
	(0.57)	(0.53)	(1.86)	(0.25)	(0.33)	(1.16)	(0.47)
Final official haircut (dummy)	-257.741	-338.290	-247.857	-232.389	-330.210	-274.153	-348.144
	(-1.62)	(-1.46)	(-1.45)	(-1.20)	(-1.52)	(-1.10)	(-1.09)
Final private haircut	58.770	-186.219	-1122.945	211.652	-116.724	-78.510	285.465
	(0.11)	(-0.26)	(-0.86)	(0.27)	(-0.15)	(-0.15)	(0.69)
Final official haircut	-1.834	55.770	-5.656**	-5.912**	-4.237*	0.089	6.836*
	(-0.61)	(1.27)	(-2.28)	(-2.49)	(-1.76)	(0.02)	(1.77)
Reserves to GDP	3.271*	6.615**	8.482**	6.421**	4.640**	3.576	4.661
	(1.83)	(2.11)	(2.17)	(2.18)	(2.14)	(1.65)	(1.68)
General gov. gross debt to GDP	-0.499	-2.647	-4.940	-7.376	-6.068	-4.758***	-1.233
	(-0.44)	(-1.08)	(-1.22)	(-1.40)	(-1.45)	(-3.50)	(-0.46)
Real GDP growth	-9.606	-12.788	-9.393	-2.757	6.972	12.421**	8.774*
	(-1.20)	(-0.74)	(-0.61)	(-0.26)	(1.28)	(2.20)	(1.86)
Inflation (annual % change)	-6.300**	-8.763	-13.291	-11.765*	-7.781*	-9.724***	-6.052*
	(-2.03)	(-1.68)	(-1.56)	(-1.79)	(-1.81)	(-3.48)	(-1.70)
Gen gov. primary lenn/borr	-3.762	-5.664	-8.886	-6.024	-4.399	-8.589	-8.407
	(-0.68)	(-0.70)	(-1.05)	(-0.72)	(-0.47)	(-1.19)	(-1.05)
Current account balance to GDP	-2.518	-1.944	-0.521	3.487	3.034	12.714**	12.674
	(-0.92)	(-0.39)	(-0.08)	(0.51)	(0.37)	(2.57)	(1.32)
Political risk	7.200	11.977	12.504	3.598	-7.328	-12.006	-7.494
	(1.42)	(1.15)	(0.98)	(0.32)	(-0.91)	(-1.53)	(-1.19)
IMF	15.224	25.200	56.404	60.695	11.162	-6.060	-45.195
	(0.67)	(1.04)	(1.19)	(1.04)	(0.32)	(-0.19)	(-1.37)
R-squared	0.21	0.33	0.43	0.39	0.35	0.42	0.60
Observations	5382	4833	4307	3785	3337	2923	2528
Time FE	YES	YES	YES	YES	YES	YES	YES
Pair FE	YES	YES	YES	YES	YES	YES	YES

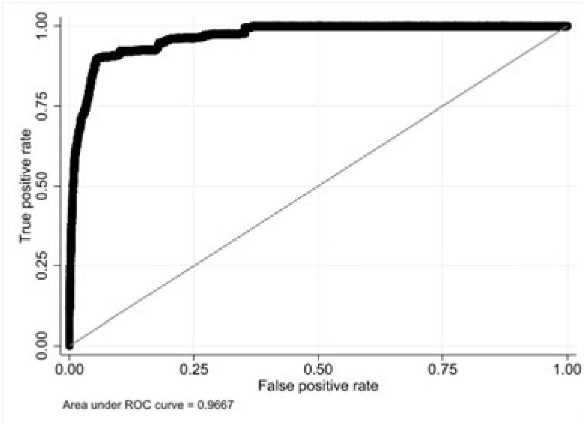
Notes: Table shows inverse propensity weighted regression results for each h-step ahead forecast on change in monthly secondary market yield spreads. The model uses controls listed in Table B1 and controls for country fixed effects and time-varying heterogeneity. Standard errors are clustered at the country level, t-statistics in parenthesis. Significance levels: *0.10, ** 0.05, *** 0.01.

Table B6: ATE on change in agency ratings, robustness checks

	Dyadic		North American Agencies		Moody's	
	Private	Official	Private	Official	Private	Official
Year 1	-0.62***	0.38***	-0.72***	0.33***	-1.41***	0.57***
	(-25.29)	(-15.44)	(-15.72)	(-7.29)	(-31.16)	(-12.65)
Year 2	-1.91***	0.89***	-1.04***	0.78***	-1.15***	0.78***
	(-38.59)	(-18.09)	(-11.36)	(-8.58)	(-11.97)	(-8.15)
Year 3	-3.34***	1.16***	-2.12***	0.99***	-2.47***	1.11***
	(-43.70)	(-15.24)	(-14.95)	(-6.95)	(-16.84)	(-7.55)
Year 4	-3.81***	1.22***	-2.44***	0.90***	-2.79***	1.21***
	(-39.88)	(-12.73)	(-13.69)	(-5.04)	(-15.26)	(-6.64)
Year 5	-3.54***	0.97***	-1.52***	0.43**	-2.10***	1.07***
	(-31.07)	(-8.53)	(-7.27)	(-2.07)	(-9.81)	(-5.02)
Year 6	-3.02***	1.02***	-0.4	0.14	-1.02***	0.43
	(-22.48)	(-7.58)	(-1.64)	(-0.58)	(-3.92)	(-1.64)
Year 7	-3.18***	1.16***	-0.66**	0.4	-0.98***	0.59*
	(-20.85)	(-7.59)	(-2.42)	(-1.46)	(-3.19)	(-1.93)

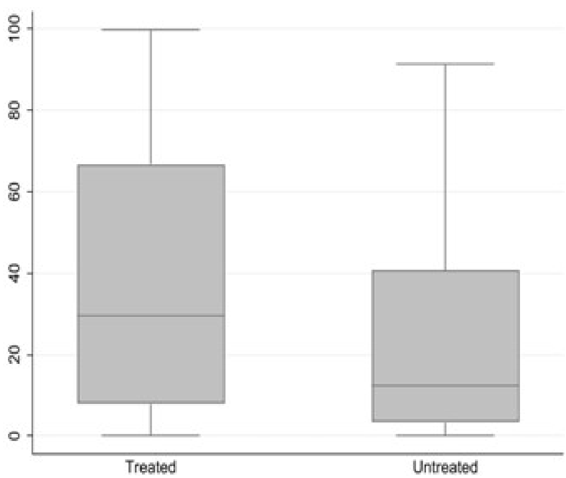
Notes: Table shows average treatment effect of private and official restructurings on change in agency ratings. The dependent variables are; the dyadic country-agency ratings (Columns 1 and 2); the monthly mean of the four North American agencies, i.e. Standard & Poor's, Moody's, Fitch, Dominion Bond Rating Services (Columns 3 and 4); and the monthly mean of Moody's (Columns 5 and 6). Standard errors (in parenthesis) are clustered at the agency-country level for Columns 1 and 2, and at the country level for columns 3-6. The model uses predictors and controls for first and second stage listed in the Online Appendix B and controls for agency-pair fixed effects in Columns 1 and 2, and country fixed effects in the other specifications, as well as time-varying heterogeneity in all. Significance levels: *0.10, ** 0.05, *** 0.01.

Figure B1: AIPW first stage ROC curve, dyadic dataset



Notes: The Receiver Operating Characteristic (ROC) curve plots the true positive rates against the false positive rates, such that the area under the curve (AUC statistic) indicates the predictive ability of the model. Under the null that the covariates have no predictive ability, the AUC is equal to 0.50, and perfect predictive ability corresponds to an AUC statistic of 1. Our first stage for estimating the probability of being in a debt crisis under our sample dyadic country-rating agency monthly data returns an AUC of 0.96.

Figure B2: Post-matching estimated propensity score, dyadic dataset



Notes: Boxplots of the estimated propensity scores, after the matching procedure, between countries that are under a debt crisis (treated countries) and countries that are not under a debt crisis (untreated countries), for our baseline dyadic country-agency dataset.

Appendix C: Restructuring size

Table C1: Variable Definitions and Sources

	Definition	Source
Dependent Variable		
Sovereign Rating	Sovereign rating on a 21-point scale, monthly (8 agencies, see Table A2b)	Bloomberg
EMBIG Spreads	Monthly average secondary market bond stripped yield spread, (EMBIG)	J.P. Morgan
Institutional Investor's Index	Perceived creditworthiness of a large number of countries, monthly	Institutional Investor Magazine
Variables of interest		
Final Private Haircut	Final Haircut of debt held by private creditors, in percent	Built by the authors, based on Cruces and Trebesch (2013b)
Final Private Haircut Dummy	Dummy = 1 in case of a final private haircut	Built by the authors
Final Private Face Value Reduction	Nominal final Haircut of debt held by private creditors, in percent	Built by the authors, based on Cruces and Trebesch (2013b)
Final Private Face Value Reduction Dummy	Dummy = 1 in case of a private final nominal haircut	Built by the authors
Final Official Haircut	Final Haircut of debt held by official creditors, in percent	Built by the authors, based on Cheng et al. (2017)
Final Official Haircut Dummy	Dummy = 1 in case of an official final haircut	Built by the authors
Final Official Face Value Reduction	Final Nominal Haircut of debt held by official creditors, in percent	Built by the authors, based on Cheng et al. (2017)

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Final Official Face Value Reduction Dummy	Dummy = 1 in case of an official final nominal haircut	Built by the authors
Private Default Duration	Dummy = 1 for each year of default to private creditors	Asonuma and Trebesch (2016)
End of Private Default Dummy	Dummy = 1 at the end of the default on debt held by private creditors	Asonuma and Trebesch (2016)
Litigation Scope	Ratio of the total case amounts to debtor countries' GDP	Shumaker et al (2021)
Control variables		
Current Account	Current account to GDP	World Development Indicators, World Bank (2018)
External Debt to GDP	Ratio of external debt to GDP	World Development Indicators, World Bank (2018)
Government Change	Dummy variable with a value of one	Database of Political Institutions, World Bank (2017)
GDP Growth	Per capita GDP (constant 2015 US$), Annual rate of change	World Development Indicators, World Bank (2018)
Inflation	Consumer price index (2010 = 100), Annual rate of change	International Financial Statistics, IMF (2018)
(Log) Population	Log of total population	World Development Indicators, World Bank (2018)
Net Len./Borr.	General government net lending/borrowing	International Financial Statistics, IMF (2018)
Per Capita GDP	Per capita GDP (constant 2005 US$)	World Development Indicators, World Bank (2018)

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Political Risk	ICRG Political risk Index	International Country Risk Guide, The PRS Group (2018)
IMF Net Loans	IMF net loans, ratio to GDP	World Development Indicators, World Bank (2018)
Reserves to GDP	Total reserves (% of GDP)	World Development Indicators, World Bank (2018)

Table C2: Private and Official Haircut and Agency Rating, Robustness Checks

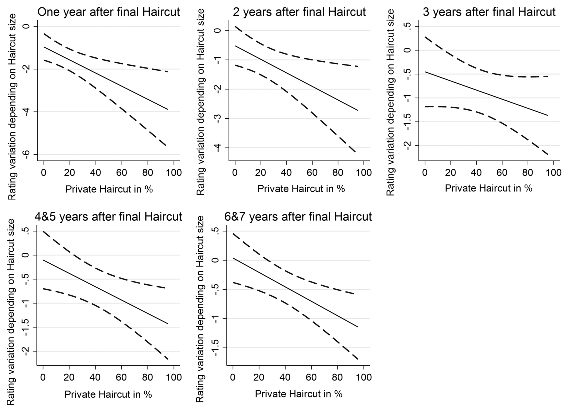
	(1)	(2)	(3)	(4)	(5)	(6)
Final Private Haircut (-1)	-0.031**	-0.022	-0.019	-0.017	-0.022	-0.031
	(-2.348)	(-1.223)	(-1.424)	(-0.952)	(-1.577)	(-1.176)
Final Private Haircut (-2)	-0.023**	-0.022	-0.019	-0.014	-0.020	-0.037
	(-2.018)	(-1.396)	(-1.397)	(-0.598)	(-1.402)	(-1.521)
Final Private Haircut (-3)	-0.010	-0.009	-0.009	-0.007	-0.007	-0.019
	(-1.204)	(-0.550)	(-0.842)	(-0.239)	(-0.661)	(-1.007)
Final Private Haircut (-4 & 5)	-0.014**	-0.023	-0.012	-0.020	-0.011	-0.015
	(-2.028)	(-1.610)	(-1.153)	(-0.765)	(-1.088)	(-0.890)
Final Private Haircut (-6 & 7)	-0.012**	-0.021*	-0.014	-0.021	-0.011	-0.020*
	(-2.452)	(-1.836)	(-1.592)	(-0.996)	(-1.373)	(-1.816)
Final Official Haircut (-1)	0.031***	0.052***	0.017	0.040	0.021**	0.034***
	(5.283)	(3.662)	(1.645)	(1.354)	(2.038)	(2.646)
Final Official Haircut (-2)	0.027***	0.046***	0.017	0.040	0.019	0.024
	(4.179)	(3.292)	(1.437)	(1.210)	(1.599)	(1.564)
Final Official Haircut (-3)	0.020***	0.035***	0.013	0.027	0.016	0.017
	(2.892)	(2.641)	(1.247)	(1.137)	(1.343)	(0.976)
Final Official Haircut (-4 & 5)	0.022***	0.034***	0.016*	0.031	0.018**	0.023*
	(3.990)	(3.287)	(1.848)	(1.612)	(2.027)	(1.802)
Final Official Haircut (-6 & 7)	0.015***	0.021***	0.016***	0.029***	0.015***	0.021***
	(4.027)	(3.303)	(3.346)	(2.639)	(2.972)	(3.614)
Final Priv. Haircut Dummy (-1)	-0.961**	-2.407***	-1.451**	-3.134***	-1.184*	-0.930
	(-2.552)	(-2.937)	(-2.368)	(-2.604)	(-1.858)	(-1.111)
Final Priv. Haircut Dummy (-2)	-0.519	-1.207	-0.606	-2.031	-0.394	-0.120
	(-1.291)	(-1.320)	(-0.837)	(-1.278)	(-0.508)	(-0.111)
Final Priv. Haircut Dummy (-3)	-0.453	-0.873	-0.544	-1.183	-0.547	-0.388
	(-1.021)	(-0.883)	(-0.912)	(-0.697)	(-0.852)	(-0.379)
Final Priv. Haircut Dummy (-4 & 5)	-0.100	0.061	-0.289	-0.205	-0.231	-0.273
	(-0.277)	(0.077)	(-0.548)	(-0.147)	(-0.424)	(-0.341)
Final Priv. Haircut Dummy (-6 & 7)	0.040	0.226	0.015	0.184	0.009	0.107
	(0.155)	(0.418)	(0.035)	(0.183)	(0.020)	(0.200)
Final Off. Haircut Dummy (-1)	-0.627	-1.127	0.015	-0.350	-0.186	-1.523*
	(-1.387)	(-1.064)	(0.018)	(-0.152)	(-0.220)	(-1.792)
Final Off. Haircut Dummy (-2)	-0.867*	-1.711	-0.471	-1.588	-0.554	-1.381
	(-1.698)	(-1.618)	(-0.514)	(-0.605)	(-0.595)	(-1.177)
Final Off. Haircut Dummy (-3)	-0.807*	-1.630*	-0.632	-1.442	-0.762	-1.101
	(-1.764)	(-1.851)	(-0.905)	(-0.863)	(-0.906)	(-0.994)
Final Off. Haircut Dummy (-4 & 5)	-0.994***	-1.621***	-0.788	-1.502	-0.844	-1.172
	(-3.175)	(-2.663)	(-1.472)	(-1.285)	(-1.485)	(-1.427)
Final Off. Haircut Dummy (-6 & 7)	-0.637***	-0.949***	-0.756***	-1.439**	-0.702***	-0.670**
	(-4.401)	(-3.407)	(-2.978)	(-2.568)	(-3.279)	(-2.095)
GDP Real Growth (-1)	0.037***	0.034*	0.146**	0.019	0.031	0.030
	(2.651)	(1.706)	(2.283)	(0.612)	(1.382)	(0.996)
Primary Balance to GDP (-1)	-0.005	0.017	0.004	0.012	0.014	0.011
	(-0.315)	(0.653)	(0.171)	(0.279)	(0.583)	(0.323)
Current Account to GDP (-1)	-0.016*	-0.062***	-0.018	-0.032	-0.017	-0.027
	(-1.748)	(-3.467)	(-1.331)	(-1.220)	(-1.293)	(-1.646)
Reserves to GDP (-1)	-0.004	0.001	0.003	-0.011	0.006	0.011
	(-0.537)	(0.251)	(0.298)	(-0.698)	(0.549)	(0.827)
Public Debt to GDP (-1)	-0.050***	-0.081***	-0.040***	-0.087***	-0.040***	-0.041**
	(-5.637)	(-5.839)	(-3.459)	(-3.992)	(-3.309)	(-2.484)
Inflation (-1)	0.392	-1.814	1.278	-1.106	1.273	2.042
	(0.229)	(-0.581)	(0.476)	(-0.176)	(0.500)	(0.649)
Political Risk (-1)	0.163***	0.237***	0.144***	0.222***	0.153***	0.171***
	(8.225)	(7.709)	(5.191)	(4.369)	(5.331)	(4.872)
IMF Net Loans (-1)	-0.061	-0.102	-0.131	-0.239	-0.130	-0.113
	(-0.938)	(-1.003)	(-1.168)	(-1.275)	(-1.204)	(-0.683)
Change in Government			-0.216			
			(-1.641)			
Population			0.000			
			(0.674)			
Growth			-0.123**			
			(-2.126)			
Constant	6.113***		5.026**		5.145**	3.949
	(3.516)		(2.064)		(2.112)	(1.213)
Observations	41,373	43,614	13,211	13,262	13,262	10,861
R-squared	0.405		0.427		0.414	0.384

Table C3: Private Default Duration, Litigation Scope, and Agency Rating

	(1)	(2)	(3)	(4)
End of Private Default Dummy (-1)			218.677*	300.124***
			(-1.703)	(-2.933)
End of Private Default Dummy (-2)			115.417	184.106**
			(-1.312)	(-2.212)
End of Private Default Dummy (-3)			71.681	72.886
			(-0.745)	(-1.001)
End of Private Default Dummy (-4 & 5)			15.821	49.74
			(-0.208)	(-0.737)
End of Private Default Dummy (-6 & 7)			-71.313	-36.209
			(-1.140)	(-0.737)
Final Litigation scope (-1)			-252.302	-198.082
			(-1.552)	(-1.478)
Final Litigation scope (-2)			-240.426**	-102.646
			(-2.299)	(-0.785)
Final Litigation scope (-3)			-154.418	13.274
			(-1.306)	(-0.122)
Final Litigation scope (-4 & 5)			431.497***	406.617***
			(-4.002)	(-3.388)
Final Litigation scope (-6 & 7)			401.049***	276.924**
			(-3.084)	(-2.047)
Litigation scope	1,744.05***	1,564.70***		
	(-4.143)	(-3.568)		
Private Default duration	-155.75*	-212.22**		
	(-1.954)	(-2.602)		
Private Default duration x Litigation scope	3,612.54***	3,703.16***		
	(-8.793)	(-7.934)		
Constant	317.02	714.558*	362.215***	502.128
	(-1.205)	(-1.873)	(-3.204)	(-1.469)
Observations	5,528	5,137	4,931	4,708
R-squared	0.562	0.622	0.345	0.42
Number of Countries	43	40	42	40
Controls	NO	YES	NO	YES
Country FE	YES	YES	YES	YES
Year FE	YES	YES	YES	YES

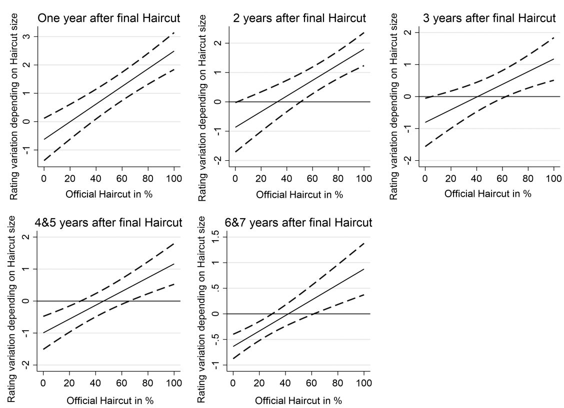
Notes: This table shows coefficients of an unbalanced panel data OLS regression with fixed effects at the country-year level. Standard errors. are clustered at the country level. The dependent variable is bond spread. t statistics are in parentheses. Significance levels: *0.10, ** 0.05, *** 0.01.

Figure C1a: Expected effect on dyadic agency rating for different levels of private haircut



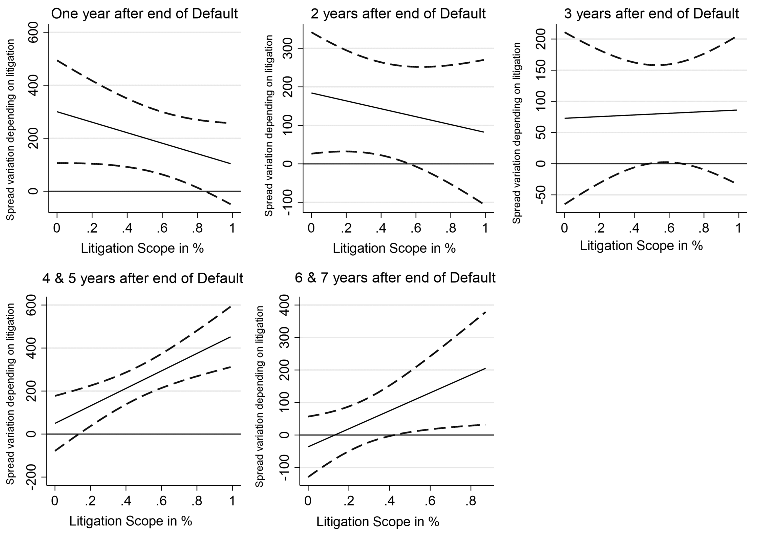
Notes: Each graph shows the marginal effect of private haircut on (mean of) agency rating, for different haircut size and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table C3, column 1. The rating contraction after a restructuring is statistically significant for levels of nominal haircut at which the upper confidence band is below the zero horizontal line

Figure C1b: Expected effect on dyadic agency rating for different levels of official haircut



Notes: Each graph shows the marginal effect of official haircut on (mean of) agency rating, for different haircut size and at different lag lengths. The dashed lines show 90 percent confidence bands. The effects are calculated using the coefficients from Table C3, column 1. The rating increase after a restructuring is statistically significant for levels of haircut at which the lower confidence band is above the zero horizontal line.

Figure C2: Expected effect on bond spread for different levels of litigation scope



Notes: Each graph shows the marginal effect of litigation size on bond spread for different litigation sizes and at different lag lengths. The dashed lines show 90 percent confidence bands. The spread increase due to litigation costs is statistically significant for levels of litigation at which the lower confidence band is above the zero horizontal line.

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Chapter 4

Political determinants of subnational European aid

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Abstract

This paper introduces a new goecoded dataset of ODA bilateral project aid from 18 main European donors, and uses it to evaluate the role of sub-national political determinants of European bilateral aid projects. Using dyadic donor-region data, I examine whether more European aid is allocated to the birth regions of political leaders, controlling for indicators of need and various fixed effects and focusing on the 1992-2020 period. We find that political leaders' birth regions face, on average, an higher probability to receive an ODA projects from European donors in the years when they hold power compared to what the same region receives at other times, even though the effect is quantitatively small. In terms of commitments, however, the allocated amount is lower. We finally find some important heterogeneity in the channel through which aid is implemented. Specifically, I find that aid implemented through recipient public entities goes more to the birth region of the leader, when they are in power.

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4.1. Introduction

With the current war in Ukraine, the capability of European countries as providers of aid has been in the spotlight.[1] While these commitments may be exceptional in nature, they are a reflection of a much longer trend in European concessional lending. The European Union (EU) and its member states provide more than half of global development aid (OECD-DAC 2019). Generally the EU's focus has been on boosting the effectiveness of development assistance by increasing partner country ownership of strategies, and combining traditional financing with private-sector and domestic resources (e.g., OECD 2005).[2] In an era where the global economy is running into headwinds and nationalist tendencies are on the rise, official lending in the forms of grants and loans from both multilateral development agencies and bilateral lenders will be called to fill the gap in global capital flows.[3] Importantly, these alternative flows are determined by factors other than mere financial returns, and the question on their exact impact on local economic activity is relevant.

In this paper, I evaluate the role of sub-national political determinants of European bilateral aid projects, focusing on the period 1973-2020. To the best of my knowledge, this paper is the the first to track the allocation of 18 major European ODA donors bilateral aid projects across the developing world, by providing sub-national information on the their distribution.[4] To evaluate the importance of political factors at the subnational level, I use the Political Leaders Affiliation Database (PLAD) for 177 countries between 1989-2020 (Dreher et al. 2019), containing information on the birthplaces of the effective leaders of countries around the globe. Using dyadic donor-region data, I examine whether more European aid is allocated to the birth regions of political leaders. I run Ordinary Least Squares (OLS) model and control for a number of subnational variables and various fixed effects. As in Dreher et al. (2019), I rely on variation across regions and over time in tandem with binary variables for the years just prior to and after the political leader's term in office. While political leaders' birth regions face, on average, an higher probability to receive an ODA project from European donors in the years when they hold power compared to what the same region receives at other times, the allocated amount is, on average, significantly

[1]The EU institutions and its member countries have committed as of October 31st, 2023, 133.5 billion Euros of humanitarian, military, and financial aid, more than all other global donors combined (Trebesch et al. (2023))

[2]For example, in the specific case of Africa, given their proximity, the EU has outlined a road map to serve as the basis for negotiations on a specific new partnership between these two continents, the joint EU-African Union (AU) strategy.

[3]As recently documented by Horn et al. (2020), official lending is much larger than commonly known, often surpassing total private cross-border capital flows, especially in times of global turmoil when private flows generally shrink.

[4]In this study, I focus on European Official Development Assistance, which is provided by governments and multilateral institutions to developing countries' governments with the aim to promote developmental objectives.

lower. When allocating financial flows donors decide to give less to leaders' birth regions, arguably anticipating that these resources would be dissipated. I do not find increases in European aid in the years before leaders assume power or after they leave office, which suggests that these effects are causal.

In order to pinpoint the channels through which this effect occurs, I leverage different sources of heterogeneity within the project level data for European project aid combined with relevant proxies of political pressure on the recipient country (see Dietrich 2013). As the empirical evidence on aid implementation shows, bilateral aid transfers are at great risk of aid capture through agency problems and bureaucratic inefficiencies in poorly governed countries (Svensson 2000; Brautigam and Knack. 2004; Reinikka and Svensson 2004; Djankov et al. 2008). On the other hand, in countries with better governance, more effective institutions limit exploitative elite behavior. In the data bilateral project aid is delivered through one of six channels: donor-country public institutions, recipient-country public institutions, NGO's, multilateral organizations, universities and research institutions, or private sector actors. I aim to test if different aid actors are more or less exposed to political distortions, given the differing degrees of local capture these channels are prone to.

I find that when aid is channeled through donor public entities (both local and national) aid projects are less subject to political capture by recipient countries. On the other hand, I find the opposite when projects are carried out through recipient public entities. Interestingly, aid delivered through either NGOs or multilateral organizations seem also more prone to political capture. I find instead that aid either channeled through research institutions or private actors are not sensitive to this political distortion.

Finally, I provide evidence on the behavior of the main seven individual donors: France, Germany, Italy, the Netherlands, the so called Nordic countries (Denmark, Finland, Norway and Sweden), Spain and the United Kingdom. How effects vary across different European donors is a related question which is very important, given donors' heterogeneity. I find a statistically significant effect only in the case of Nordic donors, who allocate less aid to the leaders' birth regions when they are in power, while for the other donors the effect is not significant.

This paper builds upon and contributes to the empirical literature on aid allocation. There is some empirical evidence linking a country's geopolitical proximity to DAC donors with a variety of types of preferential treatment (e.g., Alesina and Dollar 2020; Faye and Niehaus (2012), Kuziemko and Werker 2006; Kilby 2009). More specifically, the paper closely relates to an emerging strand of literature on sub-national aid determinants, such as Anaxagorou et al. (2020); Dreher et al. (2019) and Dreher et al. (2021), focusing on aid projects from China and the World Bank. The closest contribution is that of Dreher et al. (2019) in which they investigate whether, when, and why African political leaders use foreign aid to favor their birth regions.

In summary, this paper contributes to the current literature on aid by compiling a geocoded and disaggregated dataset on European projects and by providing new evidence on the political determinants of European aid projects. The dyadic nature of the data contributes to enrich the source of heterogeneity in the donor-recipient ties.
The rest of the paper is organized in the following manner. Section 2 presents the related literature. Section 3, describes the data and how it is combined from different sources; in the same section, I also discuss descriptive evidence on aid. Section 4 illustrates the empirical model and the identification strategy. Section 5 provides evidence on aggregate, bilateral region-level aid flows, while Section 6 presents the individual donors results. Section 7 shows the robustness analysis and the final section concludes.

4.2. Related Literature

This paper relates to at least two broad streams of the literature. First and foremost, the literature on aid allocation. This literature tries to disentangle the various motives of donors when giving aid, usually referring to commercial, geo-strategic, developmental, and "good policy"-related motives.[5] The analysis has initially been made at the country level (see, e.g., Alesina and Dollar 2000: Alesina and Weder 2002; Dreher et al. 2009; Dreher et al. 2011; Faye and Niehaus 2012; Kilby 2009; Kuziemko and Werker 2006; Bueno de Mesquita and Smith 2009; Dietrich 2013; Chauvet and Wagner 2018b) and has considered aid from both OECD-DAC donors and the so-called "new" donors (or non-DAC donors).[6] More recent evidence was provided at the subnational level (Hodler and Raschky 2014; Dreher et al. 2019; Anaxagorou et al. 2020; Dreher et al. 2021a; Dreher et al. 2021b).[7] Hodler and Raschky (2014) study political favoritism in a large sample of subnational administrative regions from all over the world, finding that the birth regions of political leaders have higher levels of nighttime light than other regions when those leaders are in power, which suggests that governments and foreign donors are systematically directing additional resources to those areas. Dreher et al. (2019) show that political leaders' birth

[5] Much of the literature on aid allocation has evaluated whether commercial and political donor interests have shaped the allocation of aid, but recipient country "need" and "merit" have also featured prominently (Dollar and Levin 2006, Claessens et al. 2009, Fleck and Kilby 2010, Höffler and Outram 2011).

[6] Dreher et al. (2011) find that "new" and "traditional" donors behave similarly. Dreher and Fuchs (2015), focusing on China, find that its aid is not influenced by the governance characteristics of recipient countries (such as their natural resources), but favord poor and populous countries, and countries that vote with in line with China in the UNGA (Dreher et al., 2018).

[7] Hodler and Raschky (2014) study political favoritism in a large sample of subnational administrative regions from all over the world, finding that the birth regions of political leaders have higher levels of nighttime light than other regions when those leaders are in power, which suggests that governments and foreign donors are systematically directing additional resources to those areas. Anaxagorou et al. (2020) document that African leaders divert Chinese aid towards regions with a high concentration of political supporters. However, no evidence of such preferential treatment is found for World Bank aid.

regions receive substantially larger financial flows from China in the years when they hold power compared to what the same region receives at other times. They observe no such pattern of favoritism in the spatial distribution of World Bank development projects. Anaxagorou et al. (2020) document that African leaders divert Chinese aid towards regions with a high concentration of political supporters. However, no evidence of such preferential treatment is found for World Bank aid.[8]

A conclusion of this literature is that recipient governance has limited importance for shaping bilateral aid commitments, while bilateral foreign aid is primarily seen as an instrument to advance donor goals.[9] The closest contribution is the paper by Dreher et al. (2019), using data from 47 African countries over the period 2000–2012, show that the home regions of political leaders receive substantially more Chinese aid than other subnational regions, and argue that this effect can be explained by electoral motives.[10]

Most directly related to the main question in this paper are studies that address the choice of the optimal aid channel (see, for example, Hefeker 2006; Cordella and dell'Ariccia 2007; Outtara and Strobl 2008; Bermeo 2009; Dietrich 2013; Dietrich 2016; Dreher et al. 2017; Marchesi and Masi 2021).Bermeo (2009) shows that donor governments employ sector allocation decisions strategically, in response to the quality of governance in the recipient country. Dietrich (2013) analyzes whether OECD donor governments condition decisions to channel funds through nonstate development actors when state institutions present a problem for effective aid delivery. She finds that, in poorly governed recipient countries, donors bypass recipient governments and deliver more aid through non-state actors, ceteris paribus. In recipient countries with higher governance quality, donors engage the government and give more aid through the government-to-government channel. Arguably, these governments may have better knowledge than the donor or international implementing agents about what type of outside intervention is needed and how to make the aid implementation of development projects most cost-effective (Dreher et al. 2017).[11]

[8]In turn, when aid allocation is driven by political influence aid is likely to be less effective (e.g., Dreher et al. 2013, Dreher et al. 2018a, Kilby 2015). Quite a few papers have also explored sub-national aid effectiveness (e.g., Bluhm et al. 2020; Cruzatti et al. 2020; Isaksson and Kotsadam 2018; Gehring et al. 2022; Dreher and Lohman 2015; Chauvet and Ehrhart 2018a; Dreher et al. 2021 and Marchesi et al. 2022).

[9]According to Bueno de Mesquita and Smith (2009), it would be mere coincidence if bilateral aid would substantially contribute to recipient development.

[10]To provide support for this interpretation, the authors interact their birth region variable with country-level measures of electoral pressures, such as the timing of national elections and the degree of electoral competitiveness. In Africa, for example, the logic of political survival is generally governed by clientelism, whereby politicians provide particularistic rewards to their core constituents (or "clients") in exchange for votes (among others see Wantchekon, 2003).

[11]Dreher et al. (2017) investigate the degree of leeway donors of foreign aid should grant to recipient governments when their preferences over how to implement the aid are different, and both the donor and recipient possess some private information about the most effective policies. They shows that donors should stay in control of how their aid is spent when their own private information is more important than the private information of the recipient. Marchesi and Masi (2021) focusing on the importance of informational asymmetry between

In sum, previous studies looking at patterns of subnational politically-motivated aid allocation has so far either focused on a single bilateral donor (China) or a specific multilateral donor (the World Bank), this paper contributes by showing evidence of political distortions on the allocation of subnational European aid projects across countries. Moreover, exploiting information on the channel of foreign aid delivery, the paper provides new information on the mechanism of distortions in aid allocation.

4.3. Data

This paper introduces a new, geocoded, dyadic panel dataset of European ODA projects constructed from raw project data in the OECD DAC Creditor Reporting System (CRS). I start by describing the geocoding process, then I proceed illustrating the full dyadic dataset including information on the aid implementation channels. Finally I illustrate the political variable of interest and the remaining control variables.

4.3.1. Geocoding

Utilizing textual information associated to projects, I identify geographic entities within the recipient country and subsequently geocode them. The next question describes in detail the construction of the ODA project aid dataset. Then, I provide a description of the main treatment variable, the leaders birthplace region, as well as a number of subnational level controls used.

The OECD CRS provides project-level data on OECD donors beginning in 1973, up to when the data collection finishes in 2020. The raw data contains both financial information on commitments, disbursements, and received amounts (in USD) as well as information on project characteristics such as implementing agencies, scope, and descriptions. The first contribution of this work is to exploit text data on project titles and descriptions in order to identify and then geocode projects at the first-order administrative (ADM1) level, allowing then for a study of the subnational determinants of aid allocation. Contrary to other data on project aid, such as World Bank or Chinese aid projects, data on geocoded European aid projects are available only in a limited number. The importance of this (geocoded) lender-side microdata in the literature on aid allocation and effectiveness is evident by the breadth of recent work which explores the mechanisms through which foreign aid has sub-national level effects.

The lack of precise data on OECD ODA prevents researches from addressing these questions, despite the historically important role of OECD (and European in particular) ODA. Because the focus is on the relationship between European DAC donors and their respective recipient countries, I select the main 18 European bilateral donors and construct

levels of government, find that a country's lack of transparency does influence the probability that a project is implemented locally rather than nationally.

a dyadic dataset of geocoded ODA project aid from 1973 to 2019. The remainder of this section gives an overview of the construction of the geocoded European ODA dataset. Appendix C provides a detailed step by step procedure.

Project titles and descriptions provide text data from which geographical entities can be identified. The procedure for the extraction and identification of geographical entities in use can be outlined as follows. First, I collect raw data from the OECD CRS on 18 European donors for a total of around 1,170,000 unique projects from 1973 to 2020. I then exploit the text descriptions of projects to extract candidate geographical entities which can be matched to known cities, regions, or administrative entities within the receiver country. CRS aid data provides titles, short, and long descriptions of aid projects which are all used as sources of information. For each project I run a Named Entity Recognition (NER) algorithm through a (pre-trained) RoBERTa base transformer model for entity identification. This particular class of algorithms use deep learning models to identify specific categories within a text, including geographic entities. The model finds at least one geopolitical entity for 433,000 projects, or roughly 37% of all reported CRS projects.[12] From these projects, identify 533,191 unique project-location pairs, as some projects may be destined for more than one location. The extracted entities then undergo a series of data cleaning and cross-checking.[13]

In summary, tests on a "golden", hand-coded data sample indicate an in-sample accuracy of 77 percent for the NER model. Missed elements, or false negatives, include the majority of the total model errors. These include instances where the string length was too short, when the strings were only in a non-English language, and instances where the model missed the entity for no discernible reason. A lesser issue is that of false positives, or when the model identifies an entity when it is not really there. Some of these cases occur because of semantic reasons, when the model identifies political entities as geographic ones or incorrectly labels generic terms as geographic entities. These false positives can be easily thrown out by cross-checking with the geocoder output and with the given recipient and donor country names. I detail instead in Appendix C how through a simple algorithmic procedure relying on a term frequency - inverse dictionary frequency and KNN fuzzy matching I account for false negatives, when the NER model fails to extract entities from strings that contain them.

As a final step, I georeference the list of extracted and cleaned entities associated to each project, resulting in a donor country - recipient region dyadic panel. This final sample consists of an unbalanced panel of 2,486 ADM1 regions covering aid projects allocated between 1973 and 2020 in around 160 countries. Figure 1 provides a first glimpse at this data; the plots show by macro region the log scaled total commitment amounts of

[12] Note that this does not mean that the model misses 63 % of geopolitical entities in the data. The vast majority of projects do not contain entities that can be geocoded. Appendix C discusses this in detail.

[13] In the online Appendix C, I explain in greater detail the evaluation of the NER model accuracy on the dataset, data cleaning, and how I deal with false positives and false negatives.

geocoded European aid at the ADM1 level. While some regions are more favored, overall European aid flows to most regions in developing countries, where missing regions indicate the lack of aid flows in the sample period. Within Europe, aid is mostly concentrated in the Balkans and Turkey, with some aid going to Eastern European countries like Belarus, Moldova, and Ukraine. However, globally, European aid unsurprisingly flows to emerging regions, such as Africa, Latin America, and some parts of Asia, with evidence of greater concentration of aid resources in Latin America to certain regions while in Africa aid tends to be allocated across the continent.[14]

4.3.2. A geocoded dataset of European ODA

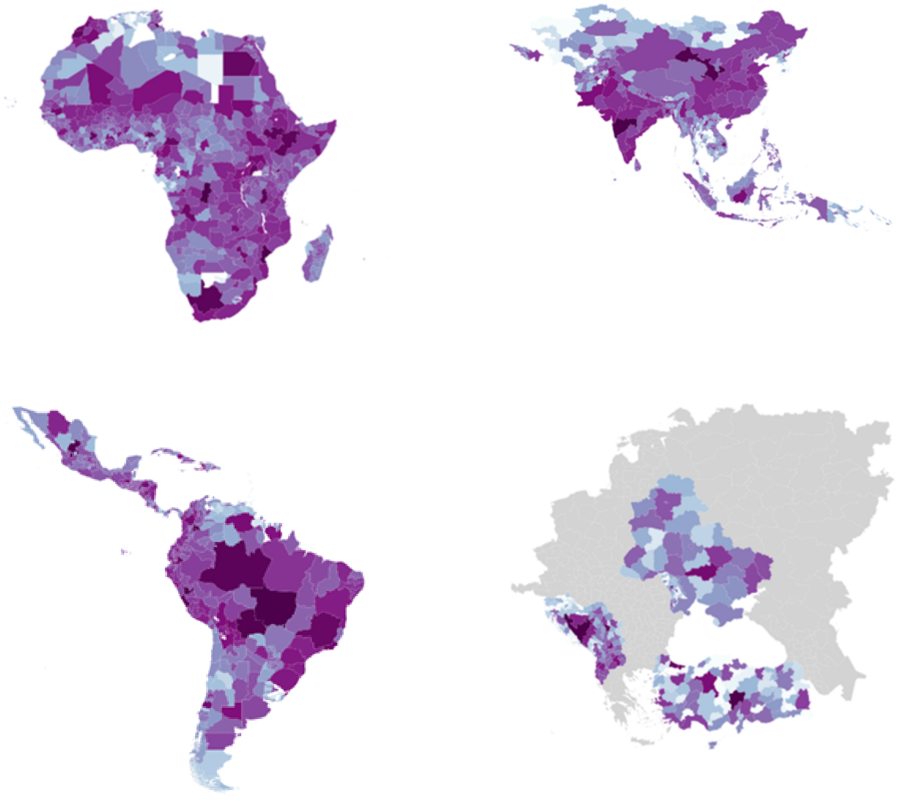
This section presents some descriptive statistics on the aid data described in Section 4.3.1. OECD-DAC aid is collected for different types of aid flows, including grants, loans, or equity investments. The overwhelming majority of projects however are ODA grants. Table B3, in Appendix B, provides some basic summary statistics for the sample of projects. ODA grants comprise close to 95 percent of all aid projects. The average commitment value for these grants is about 400,000 U.S dollars, but with large deviations, as the largest of projects reaches almost 500 million in committed U.S dollars.

The dyadic structure of the raw aid data also provides important insights. Figure 2 shows the size of total project flows between donor countries and recipient macro-regions. Among all geocoded projects, Spain and Germany are the most active donors, with Italy and other traditional donors such as the U.K, Norway, Belgium, and France following close behind. In particular, European donors commit on average more to the different parts of Africa and the Middle East, followed by South America. Figure A3 in the Appendix also shows that when considering the number of projects, they flow overwhelmingly to Sub-Saharan Africa.

The Appendix provides a series of additional descriptive statistics. Figure A4 focuses in on a subset of the top recipient countries. Unsurprisingly, countries in Southern Asia like India and Pakistan receiver the greatest number of projects as well as the overall largest commitments. The plot also tells us something about the nature of these projects. For example, I find cases such as Bolivia, where the difference between the number of projects and the total commitment amounts is significantly larger than its counterparts. In other words, Bolivia and in general other countries from Latin America like Peru and Colombia seem to attract many small projects. From a donor perspective, some other trends are obvious. First, Figure A5 shows that the traditional donors such as Germany and France are the most active both in total projects and committed amounts, while for some donors such as Spain, there is a preference for many projects but a smaller committed amount. The cross-tabulation in Table A3 helps rationalize the findings from Figures A4 and A5: in the

[14]Table A3 in Appendix A provides a decomposed donor-by-recipient macro region summary of aid flows.

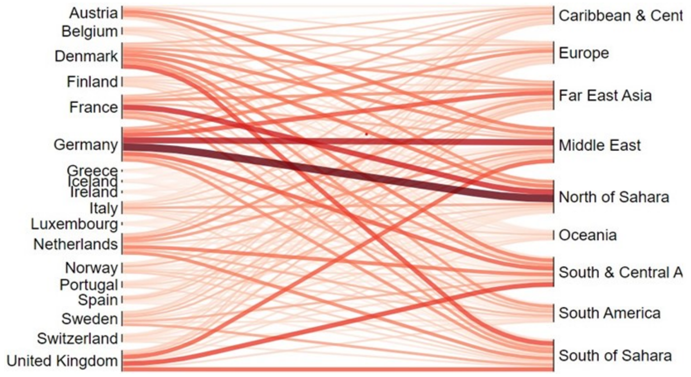
Figure 1: Log total aid commitments



Notes: Figures show the spatial distribution at the ADM1 level of the log of total aid commitments for the recipient countries for bilateral aid projects from the 18 European donors considered over the full CRS sample period from 1973-2020. Color scale goes from lightest (fewer commitments) to darkest (more commitments). White blocks indicate a value of 0 committed aid over the sample period, while grey blocks are regions not in the sample of recipients.

case of Spain for example, there are strong donor-macro region specific links, explaining where the many, small, projects committed are going.

Figure 2: Project flows, committed amount



Notes: Plot of average aid commitments over the full sample from each donor country to recipient regions.

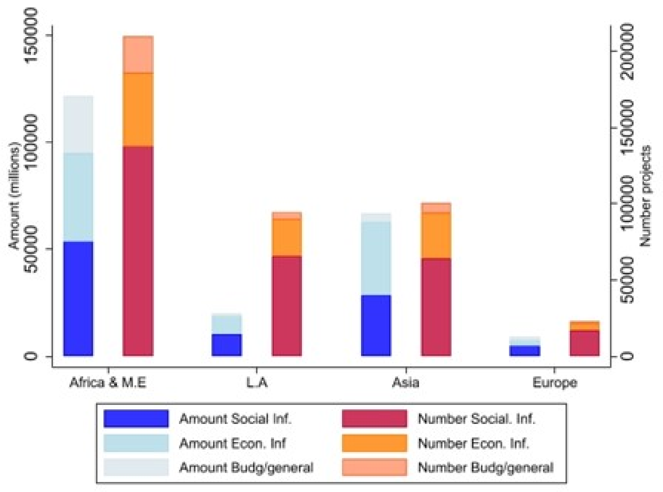
Not all aid is created equally though. OECD-DAC data provides project-specific descriptions of the purpose each aid project, which can be aggregated into the sector categories described before. The CRS data provides 41 granular aid-sector categories which fall into macro-categories of trade and tourism, energy, banking and business, industry, transport and infrastructure, environmental protection, agriculture, emergency, social infrastructure, or multisector/unspecified. In turn, for the purpose of the analysis, I focus on 3 broad categories of aid; Social Infrastructure, which includes projects in the realms of health, education, and civil society, Economic Infrastructure which covers in productive sectors or infrastructure relevant for such production, and finally general budget aid which also includes food and emergency aid.[15]

Figure 3 puts together different elements of the data. The figure shows the total number and amount of projects by the three broad aid categories, for each of the 4 macro regions. It highlights how social infrastructure aid, across regions, is the most widely distributed in the sample. A particular curiosity alluded to before is the pattern in aid allocation in Latin America, where donors seem to have a strong preference for many projects with (relatively) small committed amounts. The vast majority of projects are of the social infrastructure category, which captures things such as education, health, and basic civil-society initiatives. After, aid to production, which covers productive sectors, and economic infrastructure meaning the infrastructure relevant for such production, comprises a significant portion of total aid.

General aid, meaning budget support, emergency aid, debt support, are of course an important inflow of resources, but are practically less relevant for the analysis as the figure

[15]Table A1 in the Appendix shows the specific CRS aid sector codes and the aggregations.

Figure 3: Aid projects by geographic regions and sectors



Notes: Figure shows the total amount of aid commitments and the number of unique projects by aid sector (social, economic, or budget/general) and by recipient macro region.

suggests. Figure C4 in the Appendix explains why. The plot shows both the total commitment amounts, in million of U.S dollars, and the total project numbers, for the aid data both for the raw CRS data (i.e., before geocoding) as well as in the final sample. The amount and number of projects for budget aid in the final geocoded dataset shrinks dramatically with respect to the raw data and disproportionately more so than other aid categories. This is a reflection of the fact that this aid type is by nature mostly not subnational or localizable, so it drops from the analysis when assigning locations to projects. Finally, there are a series of minor categories of multisector aid, unspecified, or miscellaneous which are quantitatively less relevant to the final analysis. Additionally, Figure A1 in the Appendix A also shows how these aid categories have evolved over the years, with social infrastructure aid always being the most relevant.[16]

Aid implementation channels

The novel mechanism this paper proposes for how regional favoritism may be expressed as a political distortion is through the role of aid agencies tasked with the implementation of the project. Based on the reported categories in the raw CRS data, I group these "aid channels" in six broad categories: donor-country public entities, recipient-country public entities, NGOs, multilateral organizations, universities and research institutes, and private

[16] Figure A1 also shows a particularity of the raw data, which is that in 2005 OECD CRS data was significantly under reported, hence the large dip.

sector actors. Each category implies specific mechanisms in which project aid is implemented, and I argue each is differentially exposed to political distortions by the recipient government.

It is important to distinguish between government and non-state actors in aid implementation. In most cases, at least in the sample, aid is allocated through a public entity. Specifically, I observe aid channelled either through *donor public entities* or *recipient public entities,* both at the national and local level. Following Dietrich (2013) I define aid delivered through non-state actors as aid which does not directly engage government authorities at all. Bypassing recipient governments allows donors to work around the difficulties of enforcing aid contracts in situations where the probability of aid capture is high.

OECD donors bypass recipient governments and channel a greater proportion of their aid through non-state development actors for a number of reasons. These actors are relatively more shielded from misallocation than would be the case in government-to-government transfer. This argument is based on the understanding that *issue-focus* and competition generate incentives for bypass actors to, at a minimum, contain corrupt practices, thus reducing the amount of aid threatened by capture. For example, under the concept of issue focus, non-state development actors generate the majority of their funding through poverty reduction projects, thus making their organizational survival more dependent on their performance in this issue area. Given the multitude of non state development actors, donors can potentially punish bad implementation performance by switching to another organization. In some aid-receiving countries, however, most notably in failed states, there is actually no real choice as donors might not face a true choice between the two channels because recipient governments may be functionally incompetent, potentially making bypass the only viable aid delivery channel.

Local NGOs are important development partners for donors in this aspect. Their issue-focus and local knowledge about what types of projects are needed make them attractive to donors who seek to deliver services effectively. Not all local NGOs are equally virtuous and capable, however. In poorly governed countries, the quality of service delivery of local NGOs may be compromised by a lack of expertise and organization as well as corruption (see for example Barr, Fafchamps and Owens (2005). Often aid is channeled through *international NGOs* such as Oxfam, Doctors Without Borders or Care International, which allow donors to pursue their development objectives abroad. International NGOs have an issue-focus and have better knowledge of local capacities than donor staff in headquarter offices. This analysis does not make this further distinction however between domestic or international NGO.

In regions of the world where NGO partners are not represented on the ground, or where aid projects may require economies of development, donors can turn to *multilateral organizations*. Like international NGOs, many multilaterals are specialized and involved with the local sector. What differentiates them from smaller NGOs is the size of their operations and their capacity to mount emergency response interventions quickly as well as to sustain more long-term service delivery programs.

Another important type of non-state development channel are *private actors.* Donor governments often outsource development assistance to the private sector by awarding contracts to private contracting firms. They often complement the implementation of development activities by NGOs and IOs by offering technical expertise and capacity that other implementing agents may lack.

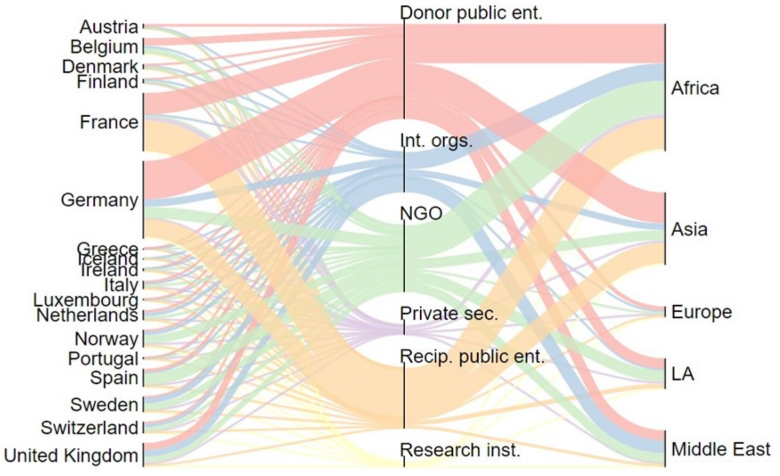
Finally, aid may be dispensed and implemented through *research-driven institutions*, such as universities or think tanks. Often these aid channels are related to very specific projects with a high degree of agency specific competencies that would make the project otherwise unfeasible if it were to be implemented by other parties. These projects are therefore a small share of bilateral aid. Appendix A provides a series of descriptives for the main aid implementation channels I consider in the analysis.

Figure 4 provides a visual decomposition of how total aid flows for the sample of 18 European donors from 1973 to 2020 has been distributed to different recipient macro regions through the main aid channels. This distinction is based on the reported aid channel names and codes in the raw CRS data. By size, public institutions on both the donor and recipient side account for the largest share of aid flows over the years, followed closely by NGOs. On the aid donor side, I find in the raw data that traditionally active and relevant donors such as Germany have an apparent preference for home-country based aid agencies for dispensing aid. Similarly, France also dispenses a large chunk of its aid through home-country agencies, but seems to slightly favor recipient country public entities overall[17]. Other donors exhibit very strong preferences for non-state actors such as NGOs and multilateral organizations. Spain and the Nordic countries like Sweden and Norway dispense almost all of their aid through NGOs. In general, multilateral organizations are rarely favored by one particular donor country as a channel for providing bilateral project aid, and likewise private sector and university/research institutes are a residual category. Table A2 shows the total amount in million of USD for these different aid channels to each macro region, while Table A4 provides examples of reported names of the different aid channel agencies.

On the recipient side, the composition of aid channels seems to be rather balanced. Africa receives an equal mix of aid from donor public entities, recipient public entities, NGOs and multilateral organizations. The Middle East stands out as a bit of an outlier, receiving almost no aid through local (recipient country) public entities. Similarly, Latin America receives very little aid through local public entities (with respect to donor country ones) and interestingly almost none from multilateral organizations.

[17]The magnitude of French aid dispensed through recipient country public entities is also a reflection of colonial heritage, where many countries host local offices of French development agencies creating institutional links for aid projects. Similarly, many higher level bureaucrats in ex-colonies are French-educated, and therefore draw on specific skills to gain ownership of projects.

Figure 4: Total flows by aid channel type to macro regions



Notes: Plot of total aid flows over the sample by the different aid channel types and recipient macro regions.

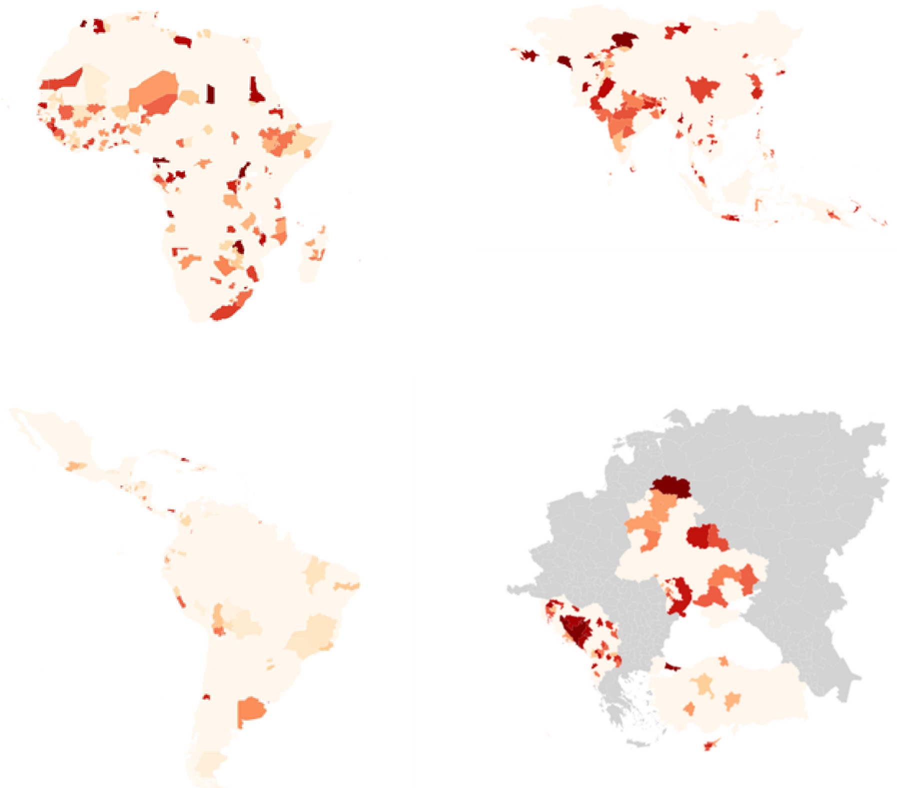
4.3.3. Leader birthplace region

In order to capture politically driven favoritism at the subnational level, I take a binary variable $INPOWER_{c,r,t}$ which is equal to 1 if the current political leader of country c at time t was born in a given region r, and is zero otherwise. The geocoded data of political leaders and their birthplace region follows the definition of Dreher et al. (2019). This variable represents the main variation in the identification strategy, where changes in leadership within a country results in a regions obtaining or losing the status of leader birthplace region.

The data provides information on the birthplace for 177 country leaders between 1989-2020. I merge this data to ADM1 - year level with the aid data. Figure 5 shows the average intensity of treatment as the share of years that a leader born in an ADM1 region was in power, for separate macro regions. The plots show that with respect to aid allocation, leader birthplace is more geographically concentrated. For example, in Latin America, a few regions account for the majority of treatment years, as leaders are both typically born in the same regions. In Africa instead the concentration is due both to the fact that in some countries leaders may, like in Latin America, be coming from the same regions, but also due to the fact that leaders tend to stay in power for a prolonged period of time.

In order to better disentangle this within-region and between-region variation in treatment, I provide some summary statistics in Appendix B, Table B4 on the average number of years a region in each of the macro regions corresponds to a treated region and how often it switches in and out of treatment, for example if a multiple consecutive leaders hail from different or the same region. On average, in the sample, the average share of years that a

Figure 5: Share of years leader from ADM1 region was in power



Notes: Figures show share of years that the respective country's' leader hailing from a given ADM1 region was in power. Darker colors indicate that the ADM1 region was assigned as treated for a longer period of time. Grey blocks are regions not in the sample of recipients.

region remains treated is about 5% across macro regions, meaning about 2.5 years. There is some slight variation, with leaders in Africa and the Middle East typically staying in power longer. With respect to the between-region variation I can exploit, largest number of region-treatment-status switches occurs again in Africa and the Middle East, with 1,647 changes between regions over the entire period. The lowest value is in Europe, with only 535, indicating for example that leaders may both stay in power longer but are also coming from the same birth region when there is a change in power.

Finally I include a set of additional subnational determinants of aid which are literature standard, merging these controls at the ADM1-year level in the dataset.[18] In particular, I follow closely the specification of Dreher et al. (2019). First, I include measures of regional economic activity through the use of nighttime lights. In order to cover as many years as possible in the panel, I combine DMSP satellite data and DMSP converted VIIRS satellite data on nighttime lights from AidData. This data provides a useful proxy for regional GDP estimates in parts of the world where data coverage is spotty. This satellite data covers almost all countries in the sample and begins in 1992. Our measure then corresponds to the log of the average nighttime light intensity in region r at time t.

I control for two measures of size of the recipient region, geographical size as well as population. The geographic size is given as the area of the recipient region. This is computed as the log of square kilometers as given by the shapefile boundaries, which coincide with the AidData provided boundaries and are in turn based on the GeoBoundaries boundary files.[19] Population is also measured in log, and is the total count of the population per recipient administrative region r at time t, based on the WorldPop Population Count 1km mosaic data. I also control for a series of geo-economic determinants, given that bilateral aid projects might be tied to the presence of key economic infrastructure. From the World Port Index, I include the total number of ports present in region r at time t, as well as the (log) number of mines based on the Mineral Resources Data System from the U.S Geological Survey. Finally, I include a dummy that captures if the region is also host to the country's capital. The next section presents the empirical analysis.

4.4. Empirical strategy

In this section I outline the empirical strategies of choice. I investigate whether the birthplace region of a country's leader receives disproportionately more aid when said leader is in power. I measure aid allocation $Aid_{d,c,r,t}$ in two ways: both with a dummy when a region in a year receives a project commitment as well as the (log) of total commitments. I

[18] All subnational variables are aggregated to the ADM1 level according to the boundary files provided by GeoBoundaries.

[19] See geoboundaries.org for the boundary files.

focus on the period 1992-2020 as the number of allocated projects is homogeneous during this period.[20]

To capture the role of the leaders birthplace in aid allocation, I estimate two regressions on the dyadic sample. The first contains the set of subnational determinants $X_{c,r,t}$ described in the previous section, while the second contains instead donor country - recipient region pair fixed effects $\square_{d,r}$ to account for any specific, time-invariant, links between donor countries and a given aid receiving region. Both specifications include donor-recipient country-year fixed effects $\tau_{d,c,t}$. All standard errors are clustered at the donor country-recipient region level.

$$Aid_{d,c,r,t} = \alpha + \beta INPOWER_{c,r,t} + \gamma X_{c,r,t} + \tau_{c,t} + \varepsilon_{d,c,r,t} \tag{4.1}$$

$$Aid_{d,c,r,t} = \alpha + \beta INPOWER_{c,r,t} + \square_{d,r} + \tau_{c,t} + \varepsilon_{d,c,r,t} \tag{4.2}$$

Our different specifications serve specific purposes. First, the model with region-level controls is a useful tool to compare results across the literature on aid allocation, as I consider many of the same subnational level determinants. This gives a first insight into what are some of the drivers of European bilateral project aid allocation. Evaluating whether European aid is on average motivated by the presence of key economic infrastructure such as ports and mines, or whether European aid flows more to richer (more nighttime lights) regions or to the capital is a useful first contribution to the growing literature on regional determinants of aid. While these controls are time-variant, in practice there can be very little variability, as the number of newly constructed ports or mines may take decades to materialize. Finally, omitting region fixed effects allows us to exploit between-region variation which could be relevant to measure the importance of a leaders birthplace region in the allocation of aid when there is little within-region variation, i.e. leader changes are infrequent.

A shortcoming of the above approach is that a statistically significant effect of these regions on aid might be spurious and could simply reflect the fact that certain regions receive more aid than others because of variables that I do not control for and that are unrelated to leaders. Equation (2) precludes such spurious results by exploiting region-specific variation over time exclusively, accounting for any number of unobservable political, cultural, or economic links between a donor country and an aid recipient region. Controlling for both country-year- and region-fixed effects absorbs a large share of the variation in the variable of interest, so this approach represents the more conservative specification. Therefore in this second model, I exploit the within-region variation to estimate the effects of a leader,

[20] Results are robust when considering the full period starting in 1973.

connected by birth to a specific region *r*, coming into power[21]. The next section describes the results.

4.5. Results

4.5.1. Baseline results

Table 1 presents the results testing for the presence of political distortions under the form of regional favoritism, as in Dreher et al. (2019). These results form the baseline for what will then be the main results when considering aid implementing agency heterogeneity. The table shows the effects on both a dummy when the region has a committed project, as well as the committed amount under the two specifications described in the previous section. Observing the regional-level controls, I find that aid amount is negatively associated to nighttime light intensity, indicating that larger projects flow more to less-economically developed regions. The linear probability of having a project seems to only marginally depend on the regional population, and likewise the effect on commitment amount of population is almost nil. I find that the size of projects is negatively associated to the number of mines. Ports seem to increase the probability of receiving a project, but at the same time there is a negative association between project size and the number of ports. Finally, there is a strong capital-city effect, as the probability of allocation to the capital city region with is strongly associated to the capital dummy, but once again the relationship turns negative when considering project amount. On the variable of interest *In power*, I find significant albeit quantitatively small effects across the different specifications. In the preferred specification with donor country-recipient region pair fixed effects as well as donor-recipient-year fixed effects, the leader's birthplace has a marginally higher probability of receiving a project with respect to its counterparts but receives on average around 1% fewer commitments. These results provide insight onto European donors' strategies: on the one hand, they allow recipient country leaders to benefit from the allocation of many, but small, projects, while instead being more cautious in the implementation of larger projects. Finally, I also check the results when considering the log of disbursements, finding results which are consistent with commitments.

4.5.2. Aid implementing agencies and political proximity

This section tests the primary hypothesis of this paper, that the actors responsible for aid implementation are one channel through which political distortions operate. The aid literature points to agency problems as significant sources of aid capture, particularly in

[21] Region-fixed effects imply that the estimates can only be based on countries with at least one change in the political leaders' birth region during the sample period. Table B4 gives an idea of what this means for each macro region.

Table 1: Leader birthplace and aid allocation

	Project dummy		Log(Commitments)		Log(Disbursements)	
	FE	Controls	FE	Controls	FE	Controls
In power	0.010***	0.021***	-0.011**	-0.025***	-0.014***	-0.022***
	(0.002)	(0.003)	(-2.30)	(0.007)	(0.005)	(0.007)
Log(nighttime lights)		0.016***		-0.014***		-0.014***
		(0.001)		(0.001)		(0.001)
Log(population)		0.001***		-0.001***		-0.001***
		(0.000)		(0.000)		(0.001)
Log(mines)		-0.001		-0.005**		-0.005***
		(0.001)		(0.002)		(0.002)
Number ports		0.004***		-0.003***		-0.002***
		(0.000)		(0.001)		(0.001)
Is capital		0.107***		-0.113***		-0.116***
		(0.005)		(0.009)		(0.011)
Area		0.034***		-0.019***		-0.022***
		(0.001)		(0.002)		(0.002)
Road density		0.005***		-0.005***		-0.005***
		(0.000)		(0.000)		(0.001)
Observations	1,297,692	839,862	1,297,692	839,862	1,297,690	839,862
R-squared	0.579	0.394	0.281	0.170	0.302	0.183
Donor-Recipient Region pair FE	YES	NO	YES	NO	YES	NO
Donor x Recipient x Year FE	YES	YES	YES	YES	NO	YES

Note: Table shows the effects on aid allocation, considering a dummy for project presence, log(commitments), or log(disbursements) of a region being the birthplace of the country's present leader. Standard errors clustered at the donor-recipient region level in parenthesis, $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

countries with poor governance (Svensson 2000; Brautigam and Knack. 2004; Reinikka and Svensson 2004; Djankov et al. 2008). In particular, Dietrich (2013) finds that OECD donors channel aid through non-state actors when recipient country institutions are weak, while in recipient countries with higher governance quality donors prefer official government channels. Following this logic, I separate aid as allocated through six different reported categories in the CRS data: donor public entities, recipient public entities, NGOs, multilateral organizations, research institutes, and private sector actors. As explained in Section 4.3, aid delivered through these channels will be differentially subject to political capture based on their characteristics. To test this, I estimate the baseline model with fixed effects using these six different aid types as the dependent variable.

Table 2 presents the results. With respect to aid which is channeled through donor public entities (both local and national), I find a negative and significant effect, at the one percent level, of the leader being in power. Projects financed by European countries and kept "in house", are then less subject to political capture by recipient countries. On the other hand, when the project is under the responsibility of the recipient public entities (both local and national), I find a positive and significant (albeit very small) coefficient, indicating the presence of local capture of aid. A perhaps more surprising result is the observed relationship between *In power* and aid delivered through either NGOs or multilateral. I find a positive effect which corresponds to roughly one percent more NGO or multilateral-delivered aid going to the leader's birthplace region when they are in power. In principle, these institutions should behave as independent actors with strong issue-focus and therefore driven by the project goal (Dietrich 2013; Dietrich 2016). However, the results suggest that these aid actors may indeed be susceptible to aid capture.[22] On the other hand, I find that aid either channeled through research institutions or private actors are not subject to political capture. In particular, in the case of research institutions, I find a negative and significant, at the ten percent level, coefficient, while there is no significant effect for private actors. Research institutions and private actors typically carry out specialized projects further away from the recipient public sector, hence they are less exposed to political capture.

Table 2: Leader birthplace and aid implementation channels

	Donor public entities	Recipient public entities	NGOs	Multilaterals	Research Institutions	Private actors
In power	-0.019***	0.004*	0.01**	0.013***	-0.005*	-0.002
	(0.004)	(0.003)	(0.004)	(0.002)	(0.003)	(0.002)
Observations	1,297,692	1,297,692	1,297,692	1,297,692	1,297,692	1,297,692
R-squared	0.247	0.204	0.332	0.298	0.251	0.147
Donor-Recipient Region pair FE	YES	YES	YES	YES	YES	YES
Donor x Recipient x Year FE	YES	YES	YES	YES	YES	YES

Note: Table shows the effects on aid allocation (measured as log committed amounts) for aid disbursed through different channels. Standard errors clustered at the donor-recipient region level in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

While the results in Table 2 reflect the proximity of an aid implementing agency to political capture risk, the degree of proximity may vary over time. For example, donor public entities, which on average are not subject to political capture, may be influenced by varying degrees of political proximity with the recipient government. To test this, I proxy for political alignment between the donor and recipient through a measure of distance between ideal voting points (Bailey et al., 2017) using data on the United Nations General Assembly voting (Voeten et al., 2009).[23] I interact this measure with *In power* and estimate how its effect varies with the measure of political alignment. More specifically I test this specification using the six different aid categories as the dependent variable. While Table 3 presents the regression results, Figure 6 plots the marginal effect of a change in voting distance on aid commitments for different types of aid channels.

[22] It should be noted that both categories contain numerous sub-distinctions which could be driving this result.
[23] The index is normalized to have a mean of 0 and standard deviation of 1 and represents for higher values of the index less political alignment between two countries, and vice-versa.

Table 3: Leader birthplace, implementation channels, and political alignment

	Donor public	Recipient public	NGO	Multilaterals	Research	Private
In power	0.022**	0.015**	0.034***	0.020***	-0.046***	-0.010**
	(0.010)	(0.006)	(0.009)	(0.007)	(0.008)	(0.004)
Voting distance	-0.023***	-0.000	-0.011***	0.001	-0.070***	-0.007***
	(0.005)	(0.003)	(0.004)	(0.003)	(0.004)	(0.002)
In power x voting distance	-0.027***	-0.007**	-0.015***	-0.004	0.026***	0.005**
	(0.006)	(0.004)	(0.005)	(0.004)	(0.005)	(0.002)
Constant	0.029***	0.002	0.021***	0.006	0.071***	0.002
	(0.007)	(0.004)	(0.007)	(0.004)	(0.006)	(0.003)
Observations	1,209,564	1,209,564	1,209,564	1,209,564	1,209,564	1,209,557
R-squared	0.193	0.151	0.276	0.247	0.184	0.105
Donor-Recipient Region pair FE	YES	YES	YES	YES	YES	YES
Donor x Recipient x Year FE	YES	YES	YES	YES	YES	YES

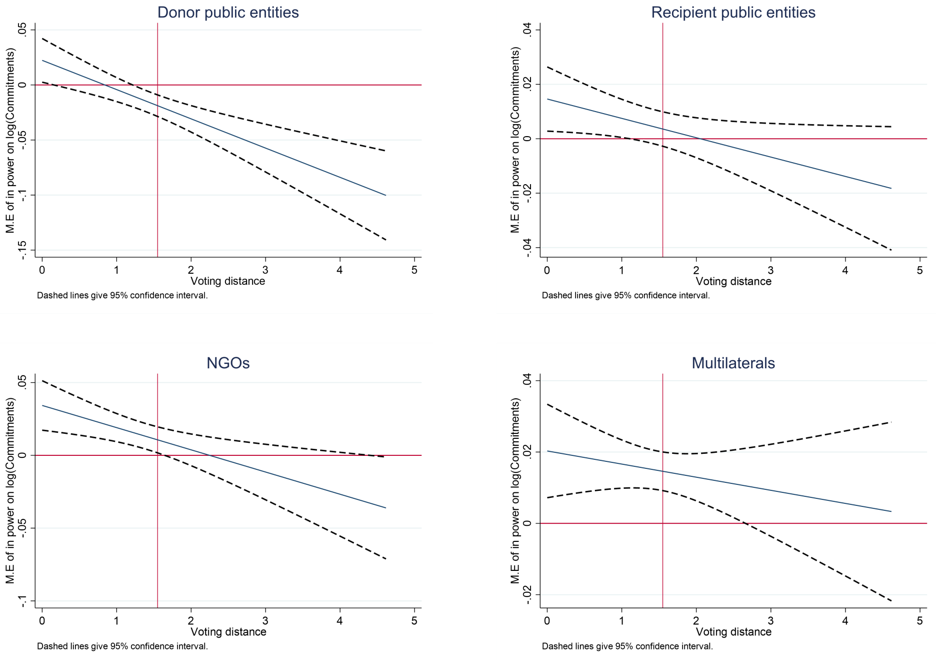
Note: Table shows the effects on aid allocation (measured as log committed amounts) for aid disbursed through different channels when interacted with UNGA ideal voting point distance. Standard errors clustered at the donor-recipient region level in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 6 first shows the expected variation in aid allocated through donor public entities, of the leader being in power conditional on UNGA voting distance. As can be seen the effect is generally negative, consistently with previous results, unless the political distance is minimal when it becomes not significant. Arguably when donor and recipient are politically close the precautionary behavior of the donor disappears. Interestingly, for aid channeled through recipient country public entities, I find that when donor and recipient are politically allied and aid is channeled through the recipient public entities, the birth region of the leader in power receives more aid. On the other hand, this positive effect disappears as the political distance increases. Finally, aid channeled through both NGOs and multilateral institutions tends to be more influenced by political capture when the voting distance between recipient and donor is smaller. In the next section I test for differences in the behavior of different donors.

4.5.3. Donor and recipient country heterogeneous effects

Given the significant heterogeneity in the recipient country sample, the mechanism through which subnational political distortions influence the allocation of aid could vary. Table 4 presents the baseline model estimated on different subsamples of recipient countries based on macro regions. As shown in Table 4, in the case of both Africa and Latin America the coefficient of the variable of interest remains negative but is no longer significant. This result suggests that the correction which takes place on average in the full sample disappears when the political capture is tested for political leader in Africa and in Latin America. This result provides some initial evidence on the importance of heterogeneity in the role of political capture in aid allocation, however, it provides no clear

Figure 6: Marginal effects of UNGA voting distance



mechanism for explaining this difference. In order to document a testable channel through which the local political factor may differentially influence the allocation of aid, the next section explore the heterogeneity in the way in which aid is implemented.

Table 4: Leader birthplace and aid allocation, by recipient macro region

	Africa & Middle East	Latin America	Asia	Europe
In power	-0.007	-0.002	-0.019**	-0.025**
	(0.008)	(-0.03)	(-2.14)	(-2.45)
Observations	520,598	267,618	377,212	133,689
R-squared	0.284	0.289	0.257	0.283
Donor-Recipient Region pair FE	YES	YES	YES	YES
Donor x Recipient x Year FE	YES	YES	YES	YES

Note: Table shows the effects on aid allocation measured as the log of commitments of a region being the birthplace of the country's present leader. Four main macro regions are considered. Standard errors clustered at the donor-recipient region level, T-statistics in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

While Table 1 presents the aggregate baseline results accounting for donor-recipient region pair fixed effects, I exploit further the dyadic nature of the dataset by carrying out a heterogeneity analysis in Table 5, running the fixed effects model on a donor-by-donor subsample. I consider the top 6 European donors in the sample, plus the group of Nordic countries. In this case, the pair fixed effects in each subsample are simply recipient-region fixed effects given that it is always one donor, except for the Nordic countries, where I retain donor country - recipient region pair fixed effects. I find essentially two sets of European donors. For France, Italy, and the U.K., the pattern is similar to the baseline results, where the birth region receives more projects but with committed amounts which are no larger with respect to other regions. In the case of Germany, Spain, and the Netherlands instead I find no discernible effects for either projects or committed amounts, indicating that aid projects from these donors are less prone to political distortions. Finally, for Nordic countries the observed effect is the opposite, as the birth region of the leader actually receives fewer committed amounts of aid. This last result is in line with previous findings in the literature on aid allocation by Nordic countries, which points to them being more "virtuous" donors.

Table 5: Leader birthplace and aid allocation, donor by donor

	France	Italy	UK	Germany	Spain	Netherlands	Nordic
In power	-0.004	-0.031	0.002	0.007	-0.043	-0.020	-0.022**
	(0.020)	(0.031)	(0.020)	(0.020)	(0.030)	(0.010)	(0.008)
Observations	72,094	72,094	72,094	72,094	72,094	72,094	288,375
R-squared	0.221	0.279	0.262	0.257	0.321	0.207	0.241
Donor - Recipient Region pair FE	NO	NO	NO	NO	NO	NO	YES
Recipient Region FE	YES	YES	YES	YES	YES	YES	NO
Donor x Recipient x Year FE	NO	NO	NO	NO	NO	NO	YES
Recipient x Year FE	YES	YES	YES	YES	YES	YES	NO

Note: Table shows the effects on aid allocation measured as the log of commitments of a region being the birthplace of the country's present leader. Seven main donors are considered, Nordic donors include: Denmark, Finland, Norway and Sweden. Standard errors clustered at the recipient region level in parenthesis, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.6. Robustness

In this section, I carry out a series of robustness tests to better understand what alternative explanations may be driving the results. It could be that the allocation of aid to a leader's birthplace contributes to their election into office. In another scenario, if aid flowed more to a leader's birthplace in the years after they left office, it could indicate that other region-specific, time-variant trends are determining European aid allocation. Table D1, in Appendix D, controls both for the pre and post period with a dummy equal to 1 in the leaders' birthplace for the two years before and after they come into power. Results show that there are no anticipation effects from a leader being in power on the committed amount, as I find no statistically significant effect on the coefficients capturing the two years before and after his term.

An important issue to address is the sensitivity of results to the composition of the sample. I first run a subsample analysis by aid sector. Specifically, I take the three broad aid sector categories; social infrastructure, economic infrastructure, and budget/general aid.[24] As shown in Table D2 the results are consistent with the baseline, in the case of social infrastructure and budget/general aid. On the other hand, the coefficient for the effect of political capture on economic infrastructure aid is not statistically significant, suggesting that, on average, the correction in aid allocation does not take place when economic motives are at play.

Finally, because of the way projects are described there may be an overrepresentation of the capital city in the recipient country, I replicate the baseline specification dropping all projects in the data which were georeferenced to the capital city.[25] I find results which are consistent with the baseline model.

4.7. Conclusions

This paper introduces a new goecoded dataset of ODA bilateral project aid from 18 main European donors and uses it to evaluate the presence of political distortions in their allocation. I obtain a dyadic donor country-recipient region-year panel of 2,486 ADM1 regions covering aid projects allocated between 1973 and 2020 in around 160 countries. Using data on the birthplace of effective leaders in a country (Dreher et al. 2019), I examine if there are political distortions in the subnational allocation of European aid.

In the main fixed effects model, I rely on variation over time to estimate the effect of a leader being in power on the allocation of aid projects to their birth region. Together with binary variables for the years prior to and after the political leader's term in office, this approach allows us to estimate causal effects. I find that political leaders' birth regions face, on average, an higher probability to receive more ODA projects from European donors in the years when they hold power compared to what the same region receives at other times. On the other hand, when allocating financial flows donors decide to give less to leaders' birth regions, arguably anticipating that these resources would be dissipated.

I then explore the role of aid implementing agencies as a channel through which these political distortions may occur. I separate aid as allocated through six different actors (i.e., donor public entities, recipient public entities, NGOs, multilateral organizations, research institutes, and private sector actors) and estimate the baseline FE model. I find that when aid is channeled through donor public entities (both local and national) projects are less

[24] See Table A1, in Appendix A, for details on aid sector categories.

[25] For example, if the project is described as "UNHCR delegation in Nairobi", I do not want to identify this project as subnational, even if the geocoder would reference it to a subnational entity.

subject to political capture. The opposite occur for aid channeled through recipient public entities. Interestingly, aid delivered through either NGOs or multilateral organizations seem also more prone to political capture. Finally, I find that aid either channeled through research institutions or private actors are not sensitive to this political distortion.
Focusing on individual donors, I find a statistically significant effect only in the case of Nordic donors, who allocate less aid to the leaders' birth regions when they are in power. For the other donors (i.e, France, Germany, Italy, Netherlands, Spain and UK) the effect is not significant.
While this paper tests just one political distortion, and focuses on aid implementation channels as one mechanism, it is important to note that other similar dynamics may be at play. Future research might then want to expand the evidence on the allocative distortions of aid projects and their implementation. For example, historical ties at the donor-region level will likely matter significantly, particularly in conjunction with the present forces of regional favoritism. Second, any bias towards the home region of an incumbent political leader should be more acute in the run-up to an election. In particular, a relevant test would be to see whether the effect is more pronounced during executive elections. Finally, the negative effects on aid allocation in leaders birthplace may be driven by competing political forces, namely the birth region or political allies, ministers, or family members. These questions are for now left for future research.

Appendix

Appendix A: European aid data

Table A1: OECD aid sectors

Broad sector	Sector names and codes
Social infrastructure	Education (111), Basic education (112), Secondary education (113), Post-secondary education (114), Health (121), Basic health (122), Population policies/programmes (130), Water supply & sanitation (140), Government & civil society (151), Conflict peace & security (152), Other social infrastructure (160)
Economic infrastructure	Agriculture (311), Forestry (312), Fishing (313), Banking and Financial services (240), Business and other services (250), Industry (321), Trade policies (331), Tourism (332), Transport and storage (210), Communications (220), Construction (323), Energy Policy (231), renewable energy generation (232), non-renewable energy generation (233), Hybrid energy (234), Nuclear energy (235), Energy distribution (236), Mineral resources and mining (322)
Budget/general support	General budget support (510), Development food assistance (520), Other commodity assistance (530), Actions related to debt (600), Emergency response (720), Reconstruction relief (730), Disaster prevention (740)
Other	General environment protection (410), Multisector (430), Unspecified (998)

Table A2: European project aid, by recipient macro regions and aid channel

	Recipient macro region				
	Africa	Asia	Europe	Latin America	Middle East
Aid channel type					
Donor public entities	26684.95	21007.25	2793.916	7063.088	6990.928
International and multilateral orgs.	11891.62	4517.049	1106.46	1058.325	8692.232
NGO	22789.64	6920.471	1356.832	7785.445	6454.58
Private sector	2731.278	1736.86	385.2488	1115.194	420.6715
Recipient public entities	20723.02	13776.93	1408.235	3121.063	2089.415
Research institutions	1546.986	1260.646	100.5631	489.7966	175.8284

Notes: OECD total aid (in millions of USD) dispensed in the full sample between 1973 and 2020 for the 18 European donor countries across macro regions and by aid channel type.

Table A3: European Project aid by donor and recipient macro regions

	Recipient macro region			
	Africa & M.E	Asia	L.A	Europe
Donor country				
Austria	1.09118	0.5906	0.1853	0.7738
Belgium	2.12382	0.6823	0.6772	0.3958
Denmark	2.20157	1.5053	1.3714	1.3369
Finland	0.83729	0.6292	0.2502	0.3965
France	5.07442	4.1261	3.5552	1.4275
Germany	3.42193	5.4473	1.5019	4.33
Greece	0.21838	0.2146	0.0314	0.9067
Iceland	0.66513	0.1244	0.126	0.072
Ireland	1.0493	0.4662	0.1998	0.2909
Italy	1.04825	1.2102	0.4	0.6063
Luxembourg	0.58792	0.6355	0.228	0.5957
Netherlands	3.1403	2.728	1.5055	2.5874
Norway	2.16905	1.1708	1.2877	0.9286
Portugal	2.47444	1.1603	0.0763	1.874
Spain	1.36894	1.1461	1.1974	1.3854
Sweden	2.74259	1.7005	1.0408	1.8137
Switzerland	1.42967	1.7045	0.8832	2.4457
United Kingdom	3.7959	6.5013	0.929	0.7297

Notes: OECD average aid (in millions of USD) at the ADM1 level decomposed by donor country and recipient macro region.

Table A4: Examples of reported aid channel names

Aid channel type	Sample names
Donor public entities	Public sector, KfW, Donor Government, Public sector institutions, Embassy of Finland, BTC - Belgian Technical Cooperation, Central Government, C.E.I. - 8XMILLE, Federal Ministry for Economic Cooperation and Development, Enabel - the Belgian development agency, MAE, Finnfund, Corps Suisse d'Aide, Federal State of Bavaria, Flemish provinces, AECID - Spanish Agency Of International Cooperation For Development, Danida, Public corporations, Vía Directa, Foreign Office, Direct Line, Euskal Fondoa, The Swedish Institute, OeKB - Oesterreichische Kontrollbank, VLIR - Vlaamse Interuniversitaire Raad - Flemish Interuniversity Council, Federal State of North Rhine – Westphalia, Fredskorpset, Swiss Humanitarian Aid Unit, Ministry for Foreign Affairs in Helsinki, Behörde in Österreich, Other public entities in donor country

Table A4: Examples of reported aid channel names

Aid channel type	Sample names
Multilateral organizations	UN Development Programme, United Nations Children's Fund, World Food Programme, International Bank for Reconstruction and Development, United Nations Office of the United Nations High Commissioner for Refugees, UNICEF, United Nations Relief and Works Agency for Palestine Refugees in the Near East, FAO-Food and Agricultural Organisation, United Nations Office of Co-ordination of Humanitarian Affairs, UN Women, World Bank Group (WB), United Nations Entity for Gender Equality and the Empowerment of Women, Organization for Security and Co-operation in Europe (OSCE), International Organisation for Migration, UNESCO/United Nations Educational, Scientific and Cultural Organization, UNODC - United Nations Office on Drugs and Crime, ILO - International Labour Organisation, European Union Institution (EU), United Nations Department of Political Affairs, IOM - International Organisation for Migration, European Commission (EC), UNFPA, IBRD/International Bank for Reconstruction and Development/The World Bank, Asian Development Bank, UNIDO - UN Industrial Development Organisation, European Bank for Reconstruction and Development (CEI Fund at the EBRD)

Table A4: Examples of reported aid channel names

Aid channel type	Sample names
NGOs	Donor country-based NGO, National NGOs, Misean Cara, CEI - 8XMILLE, Unione delle Chiese Metodiste e Valdesi - 8XMILLE, Horizon 3000, Kirkens Nødhjelp, ACPP - Assembly for Peace Cooperation, United hands, Red Cross Spain, Vicente Ferrer Foundation, Finn Church Aid, Siemenpuu, Regnskogfondet, Fida International, UNICEF Foundation- Spanish Committee, Entreculturas Foundation - Faith and Joy, Farmamundi, Medicus Mundi association, Diakonia, ABILIS foundation, FORUT - Solidaritetsaksjon for utvikling, PROCLADE Foundation, Save the Children, UNRWA - Spanish Committee, Fastenopfer, Caritas Spain, Flyktninghjelpen, Via Don Bosco, Intered Foundation, Mundubat Foundation, Redd Barna-Norge, Siemenpuu, Alboan Foundation, Suomen Lähetysseura ry, Assembly for Peace Cooperation (ACPP), World Vision, CARE Norge, Concern Worldwide, MPDL - Movimiento por la Paz, el Desarme y la Libertad, Caritas Suisse, Swissaid
Private sector	BIO - Belgian Investment Company for Developing Countries, FAMSI- Andalusian Fund of Municipalities for International Solidarity, Vitens International, IUCN, The AECF/Africa Enterprise Challenge Fund, Fons Mallorquí de Solidaritat i Cooperació, St. Catherine's Medical Group, Niras Finland Oy, Micro Finance Institutions (deposit and non-deposit), Scatec Solar AS, NIRAS SWEDEN AB, Global Alliance for Improved Nutrition, Ernst & Young, The Aecf/Africa Enterprise Challenge Fund, Esperienza s.r.l., Fundación Centro de las Nuevas Tecnologías del Agua (CENTA), KPMG, Niras Sweden AB, KEWASNET/Kenya Water and Sanitation Civil Society Network, IUCN - International Union for the Conservation of Nature, Pitiusas Fund of Cooperation, Technological center of the Sea (CETMAR), Gum Arabic Training Programs & Workshops, Norled AS, Grupo Arlo SAS, Piacenti Spa, Busoga Forestry Company Ltd

Table A4: Examples of reported aid channel names

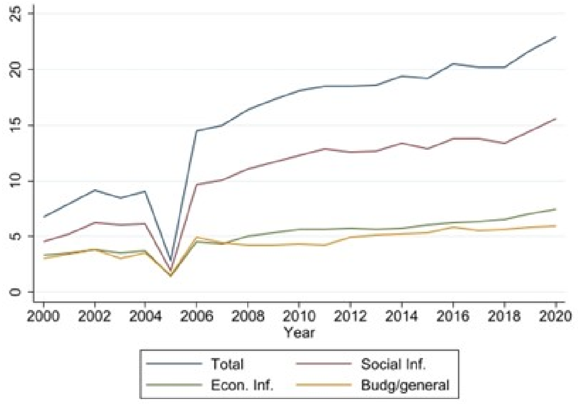
Aid channel type	**Sample names**
Recipient public entities	Central Government, Local Government, Third Country Government (Delegated co-operation), Various recipient country ministries (Finance, Development, Health, Agriculture, Foreign Affairs, Economic Planning, Environment), DFID - Department for International Development, PEA - Palestine Energy Authority, High Judicial and Prosecutorial Council of Bosnia and Herzegovina, Community of Lenca Municipalities of the Lempire center -Colosuca, Hebron Rehabilitation Committee, Missenyi District Local Government, Medellín city council, Dhaka Water Supply and Sewerage Authority, La Antigua Guatemala City Council, Ghana Highway Authority, Kampala Capital City Authority, Gobierno Provincial De Cabo Delgado, Ministerio de Cooperación Saharaui, City of Belgrade, Municipality Nisporeni

Table A4: Examples of reported aid channel names

Aid channel type	Sample names
Research institutions	University/college or other teaching institution, research institute or think-tank, Granada University, Valencia Politechnical University, Politechnic University of Madrid, VLIR - Vlaamse Interuniversitaire Raad - Flemish Interuniversity Council, Politechnic University of Catalonia, University of Valencia, Direct Line, Carlos III Madrid University, University of the Basque Country/ Euskalerriko Unibersitatea (UPV/EHU), Alicante University, Uppsala universitet, Karolinska Institutet, Sevilla University, Stockholms universitet, University of Zaragoza, Autonomous University of Madrid, Lunds universitet, Cordoba University, University of Salamanca, University of Las Palmas de Gran Canaria, University of Valladolid, University of Girona, Balearic Islands University Sveriges lantbruksuniversitet, International University of Andalucia, CIUF - Conseil Interuniversitaire de la Communauté franįaise de Belgique, Göteborgs universitet, ITM Institute for Tropical Medicine Antwerpen, Huelva University, ARES - Académie de Recherche et d'Enseignement supérieur, Makerere University, Finnish Meteorological Institute, Institut Tropical et de Santé Publique Suisse, SIU - Senter for internasjonalisering av h°yere utdanning, TMC - Troms° Mineskadesenter/Tromsoe Mine Victim Resource Center, Addis Ababa University, Institut for Menneskerettigheder Havforskningsinstituttet

Notes: Examples of different reported aid channel names by aid channel type in the sample of 18 European donors from 1973-2020. Names are ordered by frequency in data.

Figure A1: Average number of ADM1-level projects per year



Notes: Yearly plot of the average number of geocoded projects at the ADM1 level by broad aid sector. Drop in 2005 reflects CRS reporting gap.

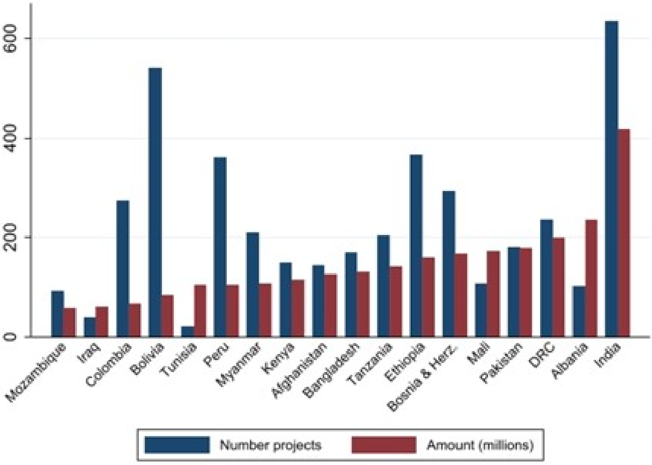
Figure A2: Country specific example geocoded aid

Notes: 2019 plot of European bilateral aid projects to the Democratic Republic of Congo. Regions are the ADM1 administrative boundaries, red dots are individual European projects.

Figure A3: Projects to macro regions

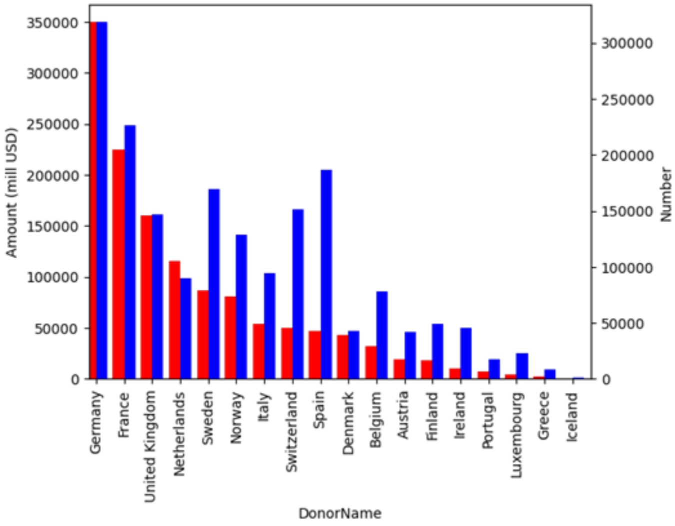
Notes: Plot of number of unique projects over the full sample from each donor country to recipient regions.

Figure A4: Number and size of projects to recipient countries, top 30th percentile



Notes: Plot shows the committed amounts and number of unique projects for the top 30th percentile of recipient countries based on committed amounts.

Figure A5: Project amounts and numbers by donors



Notes: Plot of committed amounts and number of unique projects over the full sample for each donor country.

Appendix B: Summary statistics and variables

Table B1: Definition and Sources of Variables

Variable	**Description**	**Source**
Log(commitments)	Log of total aid commitments in millions of USD	OECD CRS
Number of projects	Number of total aid projects, where a single project is identified as the combination of donor country, recipient country, CRS identifier, and extracted unique location associated with the project	OECD CRS; author's calculations
Log(nighttime lights)	Log of the reported average DMSP-OLS satellite values per region-year for years before 2014. For years after 2014, log of the reported average of DMSP-converted VIIRS satellite values per region-year	AidData
Log(population)	Log of the total population per region-year	AidData
Number of ports	Number of ports per region	World Port Index
Log(number of mines)	Log of the number of mines per region-year	US Geological Survey Mineral Resources Data System
Capital	Dummy variable equal to 1 if the region hosts the country's capital	Author's calculations

Table B2: Summary Statistics

Variable	N	Mean	SD	Min	Max
Subnational Determinants					
Log(nighttime lights)	69,608	-0.295	2.649	-12.0036	2.57
Log(population)	51,070	13.105	5.776	-4.605	37.24
Log(number of mines)	75,320	-1.334	3.287	-4.605	1
Number of ports	75,320	0.102	0.573	0	19
Capital region	75,320	0.057	0.233	0	1
OECD Geocoded Aid					
Log(total commitments)	75,320	-0.105	1.524	-13.815	6.936
Total number of projects	75,320	6.684	27.817	0	1,065
Log(social infrastructure)	75,320	-0.241	1.321	-13.815	6.591
Number social infrastructure	75,320	3.791	16.428	0	1,061
Log(economic infrastructure)	75,320	-0.157	1.113	-13.815	6.591
Number economic infrastructure	75,320	1.401	5.681	0	198
Log(budget/general/emergency)	75,320	-0.063	0.696	-9.396	6.586
Number budget/general/emergency	75,320	0.498	3.739	0	268

*Notes:*These summary statistics refer to the ADM1-year level sample of the geocoded aid data.

Table B3: Summary Statistics OECD Flows

Flow Type	Share total flows	Avg. commitments (in millions)	S.d	Min	Max
ODA Grants	0.94	0.431	3.178	0	490.294
ODA Loans	0.05	3.903	20.281	0	697.861
OOF	0.002	2.369	8.173	0	106.219

Notes: Summary statistics of project commitments, in millions of USD, for European donors over the geocoded sample. Remaining categories of aid flows include Equity Investments and Private Development Finance, which together constitute less than 0.01% of aid flows.

Table B4: Treatment Summary Statistics

Macro Region	Avg. Share of Years Treated	Number of Treatment Switches
Africa & Middle East	0.065	1647
Asia	0.051	1105
Latin America	0.054	727
Europe	0.057	535

Notes: Table shows by macro region both the average share of years an ADM1 region is considered treated, i.e., leader born in region is in power, as well as the total number of times regions switch treatment status.

Appendix C: Geo-entity extraction and data cleaning

The geocoding of CRS projects requires first the extraction of appropriate geographical information from CRS projects. Text data associated to individual projects is entered in a free-form manner, so that the quality of descriptive information varies. This section outlines first the nature of the raw CRS data, then the steps taken in cleaning and evaluating this raw data, before explaining the geographical entity extraction model and supplementary text matching algorithms used. Finally, we present a series of model accuracy evaluation metrics and robustness checks and cleaning of the final geocoded data.

Raw data description

The OECD collects and publishes ODA data in 3 phases: i) aggregate level preliminary ODA data for the prior calendar year and forward spending plans for the next 3 years, ii) final detailed data including all project level data (CRS) for the prior calendar year, iii) update and revisions in June and September. For our work we rely on the Creditor Reporting System (CRS) data which provides detailed information on individual aid activities from which the aggregate data is derived. We bulk download the data under text file formats, as this is the only way to obtain commitments data prior to 1995 or disbursements prior to 2002. The CRS database provides information about projects along several dimensions: donor and recipient countries and regions, income group of receivers, donor agencies, channel of delivery (government, NGO, institutes), flow type (grants, loans, other official flows), sectors and sub-sectors of aid, commitment and disbursement amounts, expected start and completion dates, year, and project titles and descriptions. The data we are interested in for the purpose of identifying geographic locations comes primarily from the project title and description. Figure C1 shows a sample of our raw data with the text data. The following sections provide an overview of our data compiling procedure.

Figure C1: Raw CRS data

crsid	donorcountry	receivercountry	projecttitle	longdescription	year
2010010962	Germany	Afghanistan	Stabilisation of German led region	Stabilisation program for Kunduz, Takhar, Badakhshan and Northern Baghlan. AKF/KfW	2010
1999001509z	Sweden	Afghanistan	UNICEF/AFG/99/EPI		1999
2009001595	United Kingdom	Afghanistan	Helmand Alternative Livelihoods Programme (HALP): Management Consultant	Helmandi farmers within the Food Zone increase wheat production and are supported by government institutions more able to implement rural livelihoods and counter-narcotics programmes.	2010
2011001477	Denmark	Afghanistan	Region of Origin Phase IIB 2012-2013	NSP is a national programme and an implementing partner for the ROI programme.	2011
2011000493	Netherlands	Afghanistan	BBC Kunduz	Civic Education	2014
2003015457z	Netherlands	Afghanistan			2005
2004004006z	France	Afghanistan	APPUI AU MINISTERE AFGHAN DE L'AGRICULTURE		2004
2009000802	Netherlands	Afghanistan	KAB Dihzak Irrigation - SADA	KAB-URU UTR project irrigatie en stabiliteit	2009
2009060903	France	Afghanistan	Actions dans le domaine de la gouvernance		2009
2008010266	Germany	Afghanistan	Skateboarding hall Kabul	Build a skateboarding facility in Kabul to engage youth throughout Afghanistan, building technical skills, confidence and life opportunities	2008
2009001452	Sweden	Afghanistan	Afghanistan elec obs EU09	Val i Afganistan augusti 2009. 1 LTO rekryterad.	2010

Initial data cleaning

A first step consists in cleaning the data from duplicate entries. The provided CRS identifier fails to identify unique projects, and the presence of many projects lacking an ID requires pre-analysis cleaning. Furthermore, a share of duplicate observations arises from the presence of large multi-sectoral projects, encompassing more than one type of aid. First, we drop all projects which are not bilateral, meaning that the recipient is either a macro region or unspecified. Then, we identify and drop all duplicate entries in the remaining sample. From our raw data of 18 selected European donors, we are left with 1,169,133 unique projects. The second part of the data pre-processing consists in dealing with non-English text. Recent advances in language models have improved the accuracy even on non-English texts. However, we use a Python library for translating text which is identified as non-English as additional information for entity extraction model to work on.

Running the model

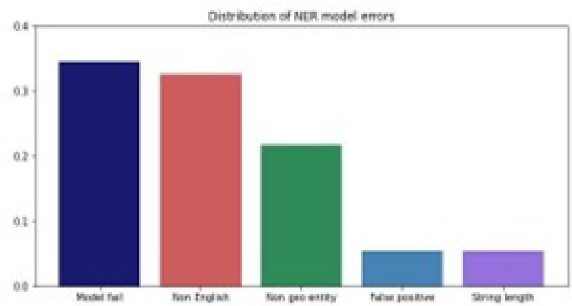
To extract geographic entities from our data, we rely on the Spacy library for natural language processing tools. We use the (pre-trained) Spacy core English transformer pipeline and leverage the Named Entity Recognition (NER) model. These models are typically used to identify within text pieces of information such as names, actions, or geopolitical entities. The advantage of this specific pipeline is in its speed, flexibility, and method of processing text data. Transformer models process all inputs bidirectionally, unlike traditional recurrent neural networks which process sequentially. This allows first for greater parallelization in computations and hence speed, and improved accuracy because the model learns to interpret sentences, or pieces of string, from multiple directions. Furthermore, this described parallelization has allowed for these models to be trained on massive datasets, thereby resulting in more accurate models. Specifically, we use the RoBERTa-base model trained on the entire English language Wikipedia and the online book corpus, a large online collection of digitalized books. When running the model on our data, the different components of the pipeline, such as the NER, all interact with the transformer component simultaneously, and different components not required can be switched off, allowing for gains in speed in our processing of the data. We run apply the NER feature of this pipeline to our three sources of text information for each project: the project title, the short description, and the long description. We obtain as an output then for each of these input strings a list of extracted entities by the model. Figure C2 shows a stylized example of what the model would identify in our text data.

Figure C2: Geo-entity extraction sample

Evaluating geo-entity extraction model accuracy

In the end the model finds a geographic entity in at least one of the strings provided for 436,447 unique projects, or about 37%. We note that this does not mean that 63% of information in projects is missed, as many projects simply do not have geographic information contained in the text or do not have text at all. We then evaluate our model with a use of a golden dataset. This is a random sample of 200 unique projects which are then hand-coded with the correct outcome the model should predict. This can either be the name of a geographical entity if it exists in the string, or nothing if not entity exists. We then run the same model on this dataset and confront the model outcomes to the true outcomes. We find that the model correctly identifies the outcome for 72% of projects. Figure C3 shows the decomposition of the remaining 28% of model errors on this golden dataset.

Figure C3: Model error decomposition



We can classify errors along 5 broad categories and by 2 error types. False positives represent the instances where the model classifies some part of the string a geopolitical entity when it is not. False negatives are the cases where the model misses an entity. First, for around 35% of the cases the model fails, either picking up false positives or reporting false negatives, for non-discernible reasons. This can be due to tricky syntax in the sentence for example. Around 25% of errors instead are due to the model reporting entities which are not geopolitical and hence not geo-referenceable. The most common example is proper names like People's Bank. We report as a separate category of "False Positives" those cases where for example the "Swedish" was tagged, but it provides no relevant information to the location of the project. There are also a small number of false positives deriving from erroneous input of the raw data. These make up less then 10% of cases. Also, less than 10% of cases are those false negatives where the model fails to pick up an entity due to the string length being too short. Finally, errors derive from the non-English language text. We will mention in the next sections possible solutions to this.

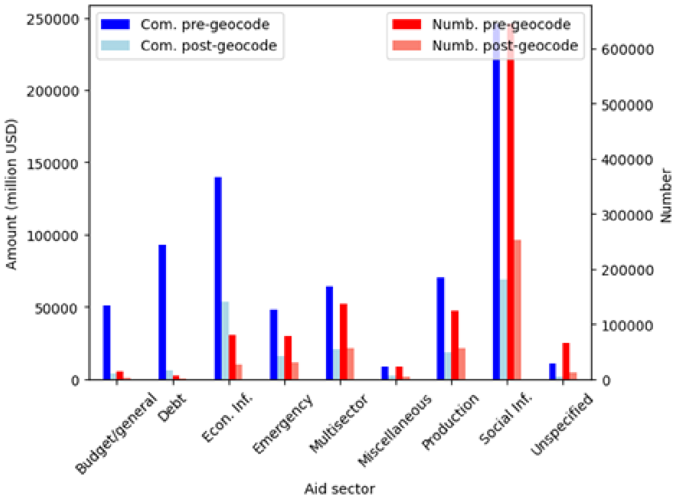
Geocoding and final data cleaning

The extracted entities are geocoded with the GoogleV3 geocoding API. Through the functionalities provided by the geocoder and a series of personalized algorithmic approaches, we perform a series of data cleaning and cross-checking procedures. First, to cross check the output from the geo-entity extraction model, we rely on the country recipient names in the raw data and a hierarchical application of a fuzzy matching algorithm. Specifically, we apply a term frequency - inverse dictionary frequency algorithm combined with a K-nearest neighbour (KNN) approach. In the first part, we split texts into chunks and filter out "noisy" words based on the frequency of words in the full dataset. The KNN algorithm then matches the candidate words with a hierarchical dataset of country administrative region names. This dataset consists in a set of organized text files, where for each country we have lists of ADM1 names and cities within these regions. The procedure is essentially a record-linkage approach, which returns a closeness score for each matched candidate word. We then only keep the match ranked as most precise. The use of this additional information for each project title and description is as a robustness. The advantage is that the algorithm always extracts at least one match for each string. The fact that we only match within a list of receiver country-specific regions and cities mitigates the issue of random matches. Furthermore, the availability of a precision score associated to each match, unlike with the NER output, allows us to quantitatively evaluate each match. We can use this additional information in the following way to deal with false positives and false negatives in the NER output.

Identifying false positives is straightforward. First, we run a simple string matching between the NER output and the KNN output in the instances when the KNN output precision metric corresponds to certainty (close to 100% matching). If in turn the NER output and the KNN are sufficiently close, we are more confident in the NER output. Similarly, we can run our record-linkage algorithm directly between the NER output and the country-specific list of geographic entities. Finally, it should also be noted that false positives are also thrown out in the geocoding procedure when the geocoder library is not able to identify the input as a geographic location. In fact, the main advantage of a quality geocoder such as GoogleV3 is its ability to screen the input text for misleading extracted entity names and identify them without returning coordinates. Examples include the tagging of names corresponding to donor or recipient countries ("India", "German"), to global geographic locations ("Indian Ocean"), or erroneously tagged text ("skateboard"). The geocoder, operating with a bias for recipient country locations of the aid project, would not return a location for these.

Dealing with false negatives is trickier. As we showed in Figure C3, the majority of missed cases stem from the presence of non-English language text. We can credibly fill in some of these gaps for the cases where the KNN output has precision close to 100%, and the NER or CRS provided geography data is missing. However, relying on the KNN output without a cross-reference when the precision metric is not very high results in too many errors. Given that the original text is supplemented with translated text, when possible, we believe that missed entities due to this error type are minimal. To finish, Figure C4 shows the distribution of the share of total projects for the raw European ODA data, before the geo-entity extraction and geocoding, and on the final dataset with only the geocoded and collapsed data. As can be seen, the distribution is largely the same, providing evidence that the procedure outline in this section did not introduce excessive biases in the data through sample selection.

Figure C4: Model error decomposition



Appendix D: Robustness

Table D1: Leader Birthplace and Aid Allocation, Lead and Lag Effects

	Log(Commitments)
In power	-0.012**
	(0.005)
Pre-power (2 years)	0.005
	(0.006)
Post-power (2 years)	-0.078
	(0.064)
Observations	1,297,692
R-squared	0.276
Donor - Recipient Region pair FE	YES
Donor x Recipient x Year FE	YES

Notes: Table tests for the presence of anticipation or post-treatment effects. Pre-power is a dummy equal to 1 for the leader's birthplace region in the 2 years leading up to the effective control taken by the leader. Post-power is a dummy equal to 1 for the leader's birthplace region in the 2 years after he is removed from power. Standard errors clustered at the donor-recipient region level. Standard errors in parenthesis, *** p<0.01, ** p<0.05, * p<0.1

Table D2: Aid Categories

Aid Categories	Social Infrastructure	Economic Infrastructure	Budget/Emergency/General
In power	-0.010** (0.004)	-0.004 (0.003)	-0.005*** (0.002)
Constant	-0.071*** (0.000)	-0.029*** (0.000)	-0.009*** (0.000)
Observations	1,297,690	1,297,691	1,297,691
R-squared	0.276	0.245	0.222
Donor - Recipient Region pair FE	YES	YES	YES
Donor x Recipient x Year FE	YES	YES	YES

Notes: Estimates for effects of in power on log(commitments) for aid in different sectors. Standard errors clustered at the donor-recipient region level in parenthesis, *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Baseline without centroids

	Project dummy	**Log(Commitments)**	**Log(Disbursements)**
In power	0.009*** (0.002)	-0.012*** (0.004)	-0.014*** (0.005)
Constant	0.071*** (0.000)	-0.072*** (0.000)	-0.091*** (0.000)
Observations	1,297,692	1,297,690	1,297,692
R-squared	0.561	0.275	0.307
Donor - Recipient Region pair FE	YES	YES	YES
Donor x Recipient x Year FE	YES	YES	YES

Notes: Table re-estimates the baseline model with fixed effects after dropping aid projects which were geocoded to the country's capital. Standard errors clustered at the donor-recipient region level in parenthesis, *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

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