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Blended Binds: How DFI's Support Programs Stifle Bank Lending in Africa

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Abstract

Blended finance is central to development finance, catalyzing private capital alongside public resources to address global challenges in developing countries. Despite its growing importance, empirical evidence on its ability to leverage private lenders' credit supply remains limited. This paper addresses this gap by analyzing the lending behavior of supported banks in Africa. Using bank-level data and a comprehensive database of intermediated lending programs offered by major development finance institutions between 2010 and 2021, the study finds that supported banks reduce lending activity post-program, with a signicant 8% decline in loan growth. This phenomenon is attributed to the limited absorptive capacity of recipient banks, which leads them to prioritize new clients at the expense of existing borrowers. Additional analysis suggests that there is no spillover effect to ineligible banks. We also document that the lending activity of supported micro-finance institutions remains unchanged. … /…

Keywords: Blended Finance, Development Finance Institutions, Private Sector Support, Africa, Banks.
JEL Codes: D22, L25, N47, O12.

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1 Introduction

Access to finance remains a binding constraint for small and medium enterprises (SMEs) in developing countries, hampering growth and job creation. The predominant cause is an underdeveloped financial sector that lacks the resources and skills to manage credit risk for SME lending. A new approach to development finance involves blending public and private finance to address this market failure and expand access to credit for SMEs. Development finance institutions (DFIs) have emerged as key players in the global initiative to channel public funds from developed countries to private companies in developing countries. DFIs are public investors whose mission is to support private firms or projects that would not secure financing without their intervention, in line with the principle of additionality.

In this paper, we focus on intermediated lending by DFIs, which is a central tool for supporting SMEs in developing countries. Although limited in number, DFIs are important players in development finance. Their financial flows account for about 15% of both FDI and official development assistance in Africa (see Annex A). Intermediated lending accounts for a large share of DFI activity (between one third and one half to total amount disbursed). It consists of supporting financial institutions (banks, microfinance institutions) through credit lines or guarantees with the aim of stimulating the lending activity of the supported lenders. This approach is used to support small businesses that DFIs cannot finance directly because they are not equipped to make small loans and lack the soft information to identify, screen, and monitor these borrowers.

Rigorous empirical evidence on the impact of intermediated lending programs on the activity of supported banks is still lacking. This paper fills this gap by examining how the lending of supported banks is affected by the program. As such, it provides information of paramount importance for assessing the performance of DFIs in helping to ease access to credit in Africa.

To conduct the analysis, we first hand-collected data on the intermediated lending programs offered by the major DFIs in Africa. The database covers a total of 900 intermediated lending projects offered by 17 DFIs from 2010 to 2021. For each project, the database provides detailed information on the date, the recipient, the total amount, and the type of financial instrument used.

We then merge the project database with bank-level data obtained from Fitch Connect. We identify 156 African banks that received treatment between 2010 and 2021, and 796 African banks that never received treatment as of 2021. We examine whether banks that received assistance show an increase in lending activity. To address non-random assignment and to control for the staggered nature of the treatment, we use a matching approach combined with a stacked regression method.

We reject the hypothesis that supported banks increase their lending after signing an agreement with a DFI, which was expected if intermediated lending programs promote credit supply. In fact, we find the opposite, with a reduction in lending by supported banks. Our empirical result suggests an 8% reduction in lending over a five-year horizon. This result is robust to a number of sensitivity tests. We also document that the effect is stronger for the first support and for equity participation than for credit line programs (albeit always negative).

We therefore examine possible explanations for the unexpected negative effect. We reject the hypothesis that the negative effect is primarily due to the characteristics of the treatment itself or to the fact that DFIs target distressed banks. The most likely explanation, supported by our additional analysis, is the limited absorptive capacity of the treated banks. Supported banks appear to be forced to prioritize new clients at the expense of existing borrowers, which explains the observed reduction in lending due to a composition effect. The negative effect is stronger for small and inefficient banks and those with the highest relative support (as a percentage of total assets).

We further address two additional issues. First, we investigate the impact on ineligible banks. While DFIs directly influence treated banks, their activities may cause market distortions. Our results show that DFI investments have no discernible impact on the loan growth of ineligible banks. Second, we examine whether the negative effect found for banks is also present when DFIs support microfinance institutions (MFIs). We document that targeted MFIs do not reduce their lending, but they do not increase it either (as we could expected).

Our empirical results raise doubts about the effectiveness of intermediated credit programs in stimulating credit supply in Africa. While these initiatives may promote credit access for targeted segments as shown in previous works (Amamou et al., 2023; Aydin et al., 2024), our results suggest that there may be negative spillovers for other borrowers of the same bank. Future programs should be designed and monitored to ensure that they achieve their objectives without inducing side effects. In particular, program evaluations should not be limited to the segments targeted by the program, but should also consider a global overview of the bank's activities.

Related literature

Our paper contributes to a nascent but growing literature on development finance institutions (DFIs).[1]

[1] The term "DFIs" refers not only to legally independent DFIs, but also to development banks that have a private sector window, such as the BNDES in Brazil, JICA in Japan and many multilateral development banks (EIB, AfDB, etc).

Recent work has examined the economic impact of DFI investments on firms by comparing supported firms with their counterparts. An important finding is that the positive effect observed in Europe (Clò et al., 2022; Amamou et al., 2023; Aydin et al., 2024) is not always found elsewhere (Lazzarini et al., 2015; Ru, 2018; Kotchen and Negi, 2019; Barboza et al., 2023).

Another strand of the literature has examined the crowding-in effect of DFIs, which is at the heart of blended finance programs. Some papers have assessed the sensitivity of total investment to DFI investment at the national level (Barboza and Vasconcelos, 2019) and international level (Massa et al., 2016). However, the validity of this approach has been questioned by Carter et al. (2021). Other papers have focused on co-financing arrangements (often using data on syndicated loans). These papers have two main findings. First, the presence of development banks stimulates the financing of large and risky projects (Hainz and Kleimeier, 2012; Kotchen and Negi, 2019; Gurara et al., 2020) and financially constrained firms (Gong et al., 2023). Second, development banks facilitate the mobilization of other private lenders not only during the project (Degl'Innocenti et al., 2022) but also after project completion (Broccolini et al., 2021; Gatti et al., 2023). Nevertheless, mobilization effect is rather limited in low-income countries (Attridge and Engen, 2019) and the indirect effects do not seem to diffuse across sectors and countries (Mishra, 2023).

We contribute to this literature in four ways.

Firstly, we add to the existing literature by examining intermediated lending. Current evidence primarily centers around the direct investment of DFIs or co-financing arrangements, which are pertinent to large firms and projects but not necessarily applicable to SME financing. As far as we are aware, only three papers have delved into the realm of intermediated lending (Cassano et al., 2013; Amamou et al., 2023; Aydin et al., 2024). These studies often document a positive impact of intermediated lending programs on the ultimate beneficiaries.[2] However, these works fail to consider potential spillover effects on other borrowers associated with the treated banks. Our findings indicate that increasing lending to a specific segment may be detrimental to non-targeted borrowers. Our results also challenge the mobilization effect of DFIs observed in prior studies based on project finance (syndicated loans) when applied to intermediated lending.

Second, we focus our analysis specifically on the African continent. Despite the remarkable role of DFIs in Africa (see Appendix A), there is a lack of research. An exception

[2] Amamou et al. (2023) find that European firms experienced positive outcomes from EIB financing to banks during the 2008 GFC. Aydin et al. (2024) document that female-owned firms benefited from an ERDB program allocated to five banks in Turkey. However, the evidence from Cassano et al. (2013) is less clear because microenterprises, unlike other enterprises, do not benefit from the program offered by ERDB in Eastern Europe.

is the paper by Gajigo et al. (2022), which uses very similar data to ours to compare banks receiving DFI support in Africa, but does not examine the impact of these interventions. A specific analysis of Africa is needed because the existing evidence cannot be generalized as it appears to be context dependent.

Third, we collect data from many DFIs, unlike the literature, which relies on the proprietary databases of one DFI (e.g., EIB, ERDB) and limits its conclusions to that DFI. Indeed, collecting data from multiple providers is a time-consuming task, especially since many DFIs are not transparent. We contribute to this by collecting data from major DFIs operating in Africa. Our results are therefore not limited to one institution and provide a comprehensive picture.

Finally, to the best of our knowledge, we are the first to examine how DFIs may affect non-eligible companies. Existing research mainly focuses on treated banks or companies, while overlooking potential spillover effects on others. However, as documented in a specific context by Ru (2018)[3], we might expect that unassisted companies suffer from distorted competition.

The paper is structured as follows: The following section describes of the functioning of DFIs. Sections 3 and 4 present the data and the methodology. Section 5 discusses the the empirical results. Section 6 presents two extensions. The final section concludes the paper with some implications.

2 An overview of Development Finance Institutions

Development Finance Institutions (DFIs) are defined as "government-backed institutions that invest in private sector projects in low- and middle-income countries, alongside aid agencies and development banks, to promote job creation and sustainable economic growth and contribute to the Sustainable Development Goals" (European DFI). There are both multilateral and bilateral DFIs. Multilateral development banks often have either an independent structure dedicated to the private sector (e.g., the IFC for the World Bank Group) or a dedicated private sector window (e.g., the African Development Bank). In addition, many developed countries have established their own DFIs dedicated to emerging markets (e.g. US DFC for the US, DEG-KfW for Germany, BII for the UK, or Proparco for France). Table 1 provides a list of major DFIs operating in low- and middle-income countries.[4]

[3]Ru (2018) documents how unsupported (private) enterprises suffer from the loans offered by the China Development Bank to state-owned enterprises in China.

[4]While several developing countries have their own DFIs, it is noteworthy that the major DFIs operating in Africa come from outside the continent.

DFIs are emerging as key players in cross-border financial flows, particularly in low- and middle-income countries. In Appendix A, we conduct an analysis to quantify the importance of DFI financial flows relative to two major cross-border flows, namely official development assistance (ODA) and foreign direct investment (FDI), in Africa. DFI investments are at the crossroads of these two financial flows. According to our estimates (which should be considered as approximate figures), DFI flows account for about 15% of both FDI and ODA. This notable level of influence is particularly impressive given the relatively small number of actors involved (25) and the fact that, unlike other flows, DFIs focus exclusively on the private sector. Notably, our estimates are in close agreement with a similar exercise conducted by Massa et al. (2016) using a different approach.

Despite common characteristics, DFIs differ from other investors. Like other (private) investors, DFIs provide funds at market rates and their model is demand-driven. The minimum size of intervention is rarely less than several million dollars. However, this does not mean that DFIs are similar to private investors. First, their investments are expected to be additional. Additionality implies that DFIs finance projects that would not have received funding without their support (Carter et al., 2021). They prioritize projects that have significant potential to benefit communities, but are considered unattractive to private investors because of perceived high risks or returns below market benchmarks. Second, DFIs offer a range of complementary services, including technical assistance, and provide non-financial benefits such as policy support. These additional services are designed to enhance project success and mitigate potential challenges. Third, DFIs enforce higher environmental, social and governance (ESG) standards than private entities, reflecting their commitment to sustainable and responsible investment practices. Finally, DFIs play a catalytic role by leveraging private investors. Their investments are not sufficient to address the challenges facing the private sector in developing countries. As a result, they must leverage private resources.

DFIs invest in the private sector of developing countries through both direct and indirect channels. The direct channel implies that DFIs provide funds directly to companies (corporate loans) or to specific projects. The indirect channel is based on intermediated lending. DFIs support (local) financial intermediaries (banks, MFIs, funds, non-bank FIs) by providing them with resources (loans, equity) or risk-sharing mechanisms (guarantees). These additional resources are provided with the explicit objective of targeting specific enterprises or projects.

Direct financing and intermediated lending no longer have the same objectives. Direct financing is used to support large companies (e.g. multinationals) or large projects. In fact, DFIs often directly intervene with other (public or private) investors. Co-financing arrangements are structured to pool resources or spread risk to finance large projects (e.g. infrastructure) that are beyond the capacity of a single institution. In contrast, intermediated loans are formulated to divide a large amount (several million dollars) into many small loans to SMEs. This approach is necessary because a DFI cannot provide these loans directly, either due to high costs or lack of local knowledge to identify suitable borrowers. As shown in Table 1, intermediated lending represents an important part of DFI activity and is the main channel through which they are able to support local private sector development.

3 Data

3.1 DFI Investment Database

3.1.1 Construction of the database

Our research focuses specifically on DFI intermediated lending in Africa. We limit our analysis to Africa because of the significant challenges related to the lack of private investors and limited access to finance. Table 1 illustrates that the DFI portfolio globally includes a significant portion in the "Finance" and "Africa" categories.

To compile the list of DFIs, we first identify the major DFIs operating in Africa.[5] To do this, we supplemented the list provided by the OECD with members of EDFI (an association of European DFIs). We initially identified 25 DFIs, including 18 bilateral DFIs and 7 multilateral DFIs. We excluded two multilateral DFIs (Asian Development Bank and IDB Invest) because they do not operate in Africa. We also excluded two bilateral DFIs (SIMEST and BMI-SBI) because they finance domestic firms that invest abroad. Of the remaining 21 DFIs, four bilateral DFIs (FinDev, COFIDES, SOFID, CDP-SF) are not included in our analysis because we were unable to collect a list of their projects.

Next, we collected granular data on all projects located in Africa (including North Africa) and categorized as "finance" for the 17 DFIs included in our analysis. This process yielded a total of 1,740 projects. We excluded projects dedicated to funds, projects without information on date or recipient, and misclassified projects. After applying these filters, the database covered 1,261 projects over the entire period.[6] For the sake of comparability in the analysis, we limited the coverage period to 2010-2021, resulting in

[5]In our analysis, we adopt a restrictive definition of DFIs by relying exclusively on foreign institutions. Some countries have their own national DFIs (such as KfW in Germany). In Africa, however, domestic DFIs are rare, and when they exist, their activities are very limited.

[6]The main reason for the reduction in the number of projects was the exclusion of investment funds (78% of the excluded projects).

Table 1: List of DFIs

	Information on DFI				Portfolio		Inclusion in data	
Country	Acronym	Creation	Assets	Staff	Afr	Fin	Incl.	Reasons
Bilateral DFIs								
USA	US DFC	1969	12024	681	41	34	Yes	
NETHERLANDS	FMO	1970	11003	577	30	34	Yes	
UNITED KINGDOM	BII	1948	9390	426	50	29	Yes	
FRANCE	Proparco	1977	7838	438	41	33	Yes	
GERMANY	DEG	1962	7750	650	21	28	Yes	
NORWAY	Norfund	1997	2917	111	65	48	Yes	
AUSTRIA	OeEB	2008	1336	63	15	46	Yes	
BELGIUM	BIO	2001	1299	80	41	42	Yes	
FINLAND	FINNFUND	1980	882	85	48	20	Yes	
SWEDEN	Swedfund	1979	873	52	58	37	Yes	
DANEMARK	IFU	1967	730	97	35	32	Yes	
SWITZERLAND	SIFEM	2011	698	30	27	23	Yes	
ITALY	SIMEST	1991	675	171	11	0	No	Domestic
CANADA	FinDev	2018	214	38	40	67	No	No info
SPAIN	COFIDES	1988	188	87	15	24	No	No Info
PORTUGAL	SOFID	2007	23	11	75	27	No	No info
ITALY	CDP-DF	1991	na	25	33	64	No	No info
BELGIUM	BMI-SBI	1971	na	na	na	na	No	Domestic
Multilateral DFIs								
EUROPE	EIB	1958	766757*	3410*	na	na	Yes	
ASIA	ADB	1966	271741*	3687*	0	18	No	Coverage
WORLD	IFC	1956	105264	4200	21	38	Yes	
EUROPE	EBRD	1991	85318*	3000*	13	40	Yes	
AFRICA	AfDB	1964	50912*	2095*	96	16	Yes	
WORLD	ISDB	1975	35174*	932*	61	na	Yes	
AMERICA	IDB Invest	1986	9401	na	0	49	No	Coverage

The table displays the list of DFIs according to OECD. For each DFI, we report the country (if required), the name, the year of establishment, total assets, the number of employees, the portfolio composition (% of projects in Africa and to Finance). We signal by an asterisk after the asset and staff, DFIs that are not legally independent (i.e., a department within a development bank). For these institutions, we are unable to collect assets and staff dedicated on private sector activity and data cannot be compared with data from other DFIs. We precise in the last two columns if the DFI was included in data collection and if not the main reason for exclusion (No info = absence of information; Domestic = DFI only financed firms from the country of origin (e.g., SIMEST finances Italian firms operating abroad); and Coverage = No project in Africa). DFIs are classified by type (bilateral vs. multilateral) and per size (assets).
Data on total assets, number of employees and portfolio composition are obtained from diverse sources including institution website and EDFI. Data provided from diverse sources are not directly comparable and are given to provide an overview of relative size and portfolio distribution.

900 projects (Table 2). Details on each step of the database construction can be found in Appendix B.

3.1.2 Limitations of the database and quality checks

The database has two limitations.

The first concerns the completeness of the database. We are fairly confident in our

Table 2: Data collection

DFI	Initial	Final	Collection	Cleaning	Analysis
AfDB	1972	2018	289	191	78
BII	2002	2020	116	100	84
BIO	2010	2021	82	45	45
DEG	2017	2021	34	20	20
DFC	2009	2022	155	100	97
EBRD	2012	2022	71	71	59
EIB	1998	2022	139	121	63
FinnFund	2017	2021	13	8	8
FMO	2010	2022	97	74	73
IFC	1995	2022	447	342	192
IFU	1997	2020	28	3	2
ISLDB	2010	2021	26	32	20
Norfund	2006	2021	52	20	31
OeEB	2010	2022	13	3	2
Proparco	2013	2022	149	122	117
SIFEM	2012	2021	6	1	1
Swedfund	2008	2021	23	8	8
TOTAL			1,740	1,261	900

The first column displays the acronym of the DFI. The following two columns reports the data for the initial year and the final year of projects considered. The fourth column presents the number of projects hand-collected without retreatment and filters, the following columns the number of projects after data cleaning. The last column is the number of projects per DFI used in the analysis (period restricted to 2010 to 2021).

ability to collect data from major DFIs operating in Africa. Excluded DFIs are small actors on the continent (Table 1). However, since we rely on information provided by the DFIs themselves, we cannot know whether they report all their projects. To assess the quality of our database, we conducted a comparison with the OECD's Credit Reporting Standard (CRS) database. CRS provides project-level information. The results of this evaluation, presented in the last subsection of Appendix B, document that our database not only includes more projects, but also provides information on each project that is rarely available in the CRS (such as the name of the recipient).

Second, public data do not provide detailed information on contract terms, which has several implications for our analysis. First, we cannot know the duration of the project. Only a limited number of projects provide the duration, and we were unable to identify a clear pattern (based on other characteristics such as financial instruments, DFI, or amount). As a result, we consider each project has permanent in the analysis (which is limited to five years after implementation). Second, the date and amount are those provided at the time of approval. A project may take time to be implemented and we do not expect a contemporaneous effect. In addition, the amount reflects the maximum amount at the time of signature and may not accurately reflect the actual amount disbursed, particularly in the case of guarantees. Therefore, we only use this variable in the extended analysis and not in the baseline analysis.

3.2 Bank data

3.2.1 Description of data and the sample

We merge data on DFI projects with bank data extracted from Fitch Connect. There are two main reasons for focusing on banks. First, banks represent the majority of financial intermediaries targeted by DFIs. Second, we have access to comprehensive financial variables for a significant sample of African banks over time. In contrast, comparable data for other financial intermediaries are either unavailable or cover only a limited number of institutions or years (such as MixMarket for MFIs). FitchConnect is an international database that provides financial information for a large number of banks worldwide.[7] We focus on active commercial banks - those with positive assets - operating in Africa between 2005 and 2021, leaving us with 1,009 commercial banks. The sample of banks excludes holdings.

A bank is classified as receiving treatment if it has received support from a DFI, regardless of the nature of the DFI and the financial instrument. We only consider treatment directly assigned to commercial bank. In other words, a bank is not treated if the holding received a support but the bank did not. For our analysis, we assume that the treatment is permanent as we are unable to extract information on time coverage of the program. If a bank has received support from a DFI more than once, we consider the year of the first treatment.

Based on this definition, we classify all banks into one of the following groups: (i) 57 banks that received treatment before 2010, (ii) 156 banks that received treatment between 2010 and 2021, and (iii) 796 banks that have never received treatment as of 2021. For the remainder of our analysis, we exclude banks that were treated before 2010. This choice results in a sample of 952 banks (156 treated banks and 796 never treated banks). Table C1 shows the number of treated banks ranges from 5 to 25 banks per cohort.

[7]FitchConnect stands out as an international database that provides financial statements for a large number of banks worldwide and offers a longer time dimension compared to BankFocus, another international database often used in the literature. It is important to note that inclusion in FitchConnect, as well as BankFocus, is not a random process and several small and local banks are not represented in these global databases.

3.3 Descriptive statistics

3.3.1 DFI project database

We first display descriptive statistics of DFI investments. The analysis is organized into three levels of examination: project, country, and recipient.

Table 3: Summary statistics on investment database

	Obs	Mean	Std. Dev.	Min	Max
amount_usd	880	43.597	94.271	0	982
Type==Bank	900	0.634	0.482	0	1
Type==Holding	900	0.069	0.253	0	1
Type==MFI	900	0.182	0.386	0	1
Type==Other FIs	900	0.114	0.319	0	1
Instrument==Equity	845	0.101	0.301	0	1
Instrument==Grant	845	0.017	0.128	0	1
Instrument==Guarantee	845	0.200	0.400	0	1
Instrument==Loan	845	0.579	0.494	0	1
Instrument==Technical assistance	845	0.020	0.140	0	1
Instrument==intermediated investment	845	0.084	0.278	0	1
DFI==AfDB	900	0.087	0.282	0	1
DFI==BII	900	0.093	0.291	0	1
DFI==BIO	900	0.050	0.218	0	1
DFI==DEG	900	0.022	0.147	0	1
DFI==DFC	900	0.108	0.310	0	1
DFI==EBRD	900	0.066	0.248	0	1
DFI==EIB	900	0.070	0.255	0	1
DFI==FINNFUND	900	0.009	0.094	0	1
DFI==FMO	900	0.081	0.273	0	1
DFI==IFC	900	0.213	0.410	0	1
DFI==IFU	900	0.002	0.047	0	1
DFI==IslDB	900	0.022	0.147	0	1
DFI==NORFUND	900	0.034	0.182	0	1
DFI==OeEB	900	0.002	0.047	0	1
DFI==Proparco	900	0.130	0.336	0	1
DFI==SIFEM	900	0.001	0.033	0	1
DFI==SWEDFUND	900	0.009	0.094	0	1

The table displays the descriptive statistics at the project level (for the period 2010-2021).

The database of DFI investments consists of a total of 900 projects between 2010 and 2021, described in Table 3. The average project size is about $44 million, but the median is only $12.5 million. Approximately two-thirds of the projects are allocated to banks, followed by MFIs (18%), other FIs (11%), and holding companies (7%). There are significant differences in the amounts associated with each type of financial intermediary, both between and within them (Figure C1 in the Appendix). Microfinance institution (MFI) projects tend to be smaller than others. Loans are the most common financial instrument (57%). The number of guarantees is also noteworthy (20%), as only a limited number of DFIs offer this product. Equity represents one tenth of the projects. Finally, we document that IFC accounts for 21% of all projects, followed by Proparco, US-DFC, BII, AfDB, and FMO. Figure C2 illustrates that multilateral DFIs (AfDB, EBRD, EIB, IFC, and IslDB) provide larger support than bilateral DFIs.

The majority of projects are concentrated in a few countries, specifically Nigeria (15%), Kenya (10%), South Africa, and Egypt (7%), as shown in Figure C3 (panel a).[8] Countries in East Africa and the Gulf of Guinea are significant recipients of projects. In contrast, 16 countries received fewer than three investments, and six countries received no projects. There is a strong correlation between the distribution of amounts and projects (panels a and b of Figure C3). The largest economies on the continent, such as South Africa, Nigeria, Morocco, and Egypt, receive the largest projects (panel c of the same figure). A larger share of projects is distributed to MFIs in West, Central, and East Africa (panel d), which explains the limited size of projects in these countries.

At the recipient level, we identify 420 beneficiaries. More than half of them are banks (231), followed by other FIs (80) and MFIs (78). There is a large disparity in the amount received. Fifty percent of the supported FIs received 3.5% of the total cumulative investments (blue line in Figure C4). Conversely, the top 10% received two-thirds of the total (black-dashed line), and the top 1% received almost one-fifth of the total amount disbursed by DFIs. We then look in detail at the top 10 recipients in Table C2. The main recipients are located in large economies (Nigeria, Egypt and South Africa) and banks dominate. Table C2 reveals another fact: many recipients are members of a financial group. We identify 49 financial groups with at least two institutions financed by DFIs and 16 financial groups that received more than ten investments (Table C3). These 16 groups account for a third of all projects (315 out of 900) and a quarter of the total amounts received.

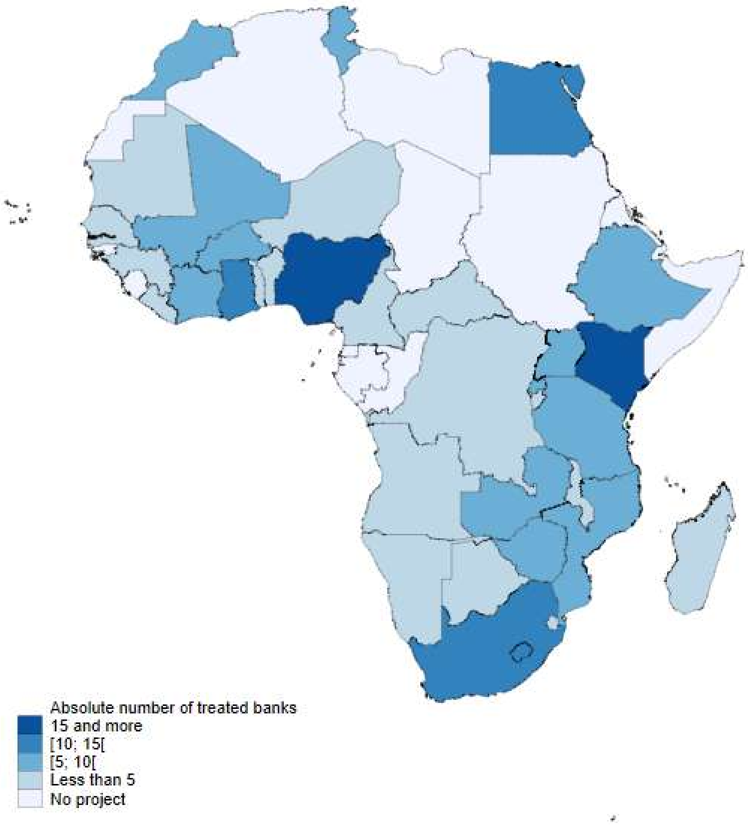
3.3.2 Treated banks vs. untreated banks

We then turn to the treated banks after merging the DFI project and bank databases. Panel a) of Figure 1 shows that the geographical distribution of treated banks is consistent with the distribution of all projects discussed above, with some exceptions (such as Morocco and Ethiopia). Evidence using the relative number of banks treated provides a different picture (panel b of Figure 1).

[8] For the geographic analysis, we exclude holdings, leaving us with 838 projects.

Figure 1: Distribution of treated and untreated banks

Panel a) Absolute number of treated banks



Panel b) Relative number of treated banks

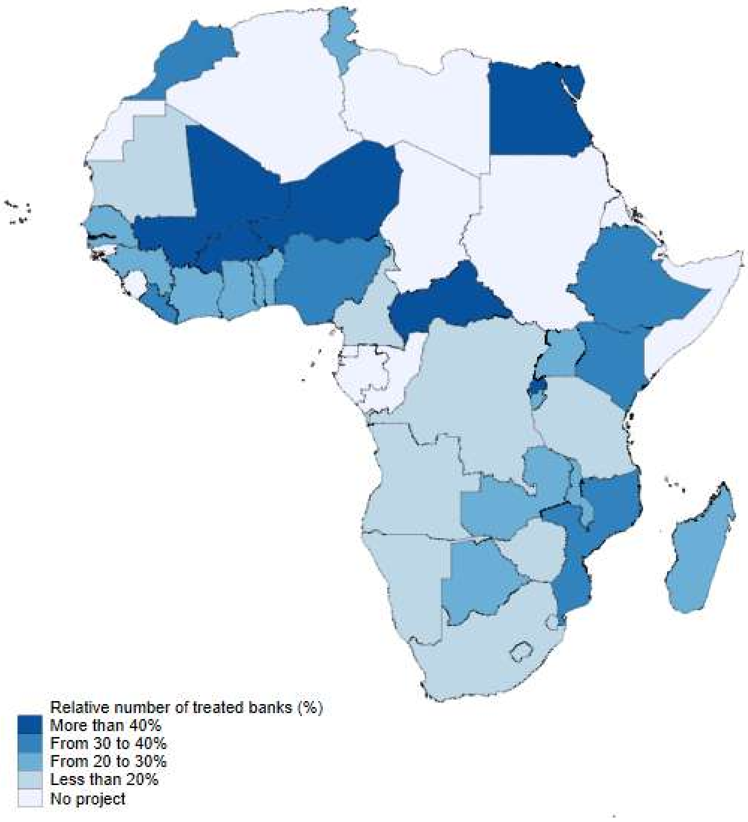


Table 4: Mean differences between treated and untreated banks, unmatched sample

	Treated			Untreated			Difference					
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean		SMD		Distribution	
Loan Growth	3.946	48.88	741	28.61	1058.4	4,852	-24.66	*	-0.033		0.131	***
Return on Assets	1.501	3.528	722	1.425	5.917	4,981	0.076		0.016		0.103	***
Return on Equity	12.91	23.98	719	8.165	131.0	4,921	4.747	**	0.050		0.150	***
Net interest margin	0.019	0.055	777	0.043	0.689	5,136	-0.024	**	-0.049		0.144	***
NPLs	0.001	0.005	556	0.001	0.003	3,030	0.000		-0.058		0.131	***
Provisions	1.626	3.155	644	2.391	10.69	4,125	-0.765	***	-0.097		0.097	***
Size (absolute)	3 879	12 838	790	2 358	11 590	5,484	1 521	***	0.124	***	0.177	***
Size (relative)	7.144	10.24	790	7.290	13.93	5,484	-0.146		-0.012		0.157	***
Deposit to assets	76.15	14.84	761	69.44	21.73	5,132	6.713	***	0.361	***	0.156	***
Loan-deposit ratio	69.98	29.92	759	295.2	5446.9	5,011	-225.2	***	-0.058	*	0.160	***
Equity to assets	14.94	13.51	790	18.44	18.03	5,483	-3.499	***	-0.220	***	0.174	***
Group dummy	0.423	0.494	790	0.383	0.486	5,484	0.040	**	0.081		0.040	
GDP PER CAPITA	2 408	2 031	790	2 969	2 764	5,320	-560.6	***	-0.231	***	0.122	***
PRIVATE CREDIT / GDP	33.38	29.21	700	33.64	34.22	4,907	-0.254		-0.008		0.117	***

The table reports mean, standard deviation and the number of observations for all outcome and matching variables for both treated banks and untreated banks. The last columns report difference in mean between the two group of banks (using Welsh method), the standardized mean difference and associated t-statistics using formula proposed by Imbens (2015) and the D-statistic of equality in distribution from a Kolmogorov-Smirnov test. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

In some markets where many banks were treated (e.g., South Africa and Côte d'Ivoire), the treated banks represent only a small fraction of the total number of banks, while in other countries (such as the Sahel) we find the opposite. A major explanation lies in the number of banks operating in each country. In the least financially developed - and often concentrated - markets, DFIs target large players, while the distribution of their support is more dispersed in developed banking systems.

Finally, we examine the main differences between treated and untreated banks. We compare lending activity as measured by loan growth. We also consider measures of performance (ROA, ROE, NIM) and portfolio quality (NPLs and loan loss provisions), as well as bank characteristics reflecting their size, funding structure, and activity. Table 4 examines whether treated and control banks differ on these characteristics. We compare banks before they receive the treatment to capture the intrinsic differences between treated and control banks.[9] Since the mean difference is not always meaningful, we also report the standardized mean difference (SMD) and the Kolmogorov-Smirnov test for equality of distributions.

Table 4 shows that treated banks had lower growth rates than untreated banks, but were more efficient (with higher ROA and ROE and lower NIM) and less risky (with lower provisions) than untreated banks. In addition, treated banks are larger in absolute terms, but do not necessarily have a higher market share than untreated banks. Treated banks are more effective at attracting deposits and have lower levels of capitalization. Consistent with previous evidence, treated banks are more likely to be part of an international banking group. Finally, treated banks operate in countries with lower income levels, but there is no statistically significant difference in terms of financial development. These descriptive statistics suggest that treated and untreated banks differ in their intrinsic characteristics, and cannot be directly compared.

4 Empirical strategy

The objective of this paper is to assess the impact of intermediated lending on loan activity of treated banks. Our baseline model is a difference-in-difference (DID) with staggered

[9] Treated and untreated banks are likely to differ because (i) the treatment affects their characteristics (*treatment effect*) and/or (ii) the two groups are intrinsically different (*sample selection effect*). Our interest lies here in intrinsically (*ex-ante*) differences. To do this, we proceed as follows: For each year, we identify treated and untreated banks. The first group includes banks that were treated in that year, while the control group includes all banks that were not treated in that year, including banks that were never treated and banks that will be treated in the future. Banks that were treated before the current year are excluded. We then look at the characteristics of the banks in the previous year. We follow the same procedure for the 11 cohorts and then aggregate the data to compare the differences between treated and untreated banks before treatment. Table C4 shows results using pooled data.

treatment. However, we cannot adopt a simple Two-Way Fixed Effects (TWFE) due to both a sample selection issue (Table 4) and limitations of TWFE to estimate a staggered DID with heterogeneous effects (De Chaisemartin and d'Haultfoeuille, 2023). As a result, we propose a new approach that combines a matching procedure with a stacked regression approach, allowing us to address both issues. We work in four steps, which are described below. In our presentation, an event is defined as the moment when a bank is treated (so we have a maximum of 156 events).

First, for each event, we identify a sample of control banks that are similar to the treated bank using a matching procedure. To avoid the problem of a forbidden control group unit (De Chaisemartin and d'Haultfoeuille, 2023), we restrict the sample of potential controls to never-treated and not-yet-treated banks.[10] The matching process is done at the event level. We use Mahalanobis distance matching (we discuss this choice among different matching procedures in detail in Appendix D). We use eight matching variables. Six bank-level variables capture the characteristics of the bank, including its absolute size (log of total assets), its relative size (market share), its funding structure (ratio of deposits to total liabilities), its capitalization ratio (ratio of equity to assets), its transformation activity (ratio of loans to deposits), and a dummy indicating whether the bank belongs to an international banking group. We also include two country-level variables, namely GDP per capita and the ratio of private credit to GDP. All matching variables are measured in the year prior to the event ($t-1$). Matched control banks are identified as the five banks with the lowest Mahalanobis distance to the treated bank, and we allow for substitution. This means that a control bank can serve as a control more than once. In addition, as the control may affect banks in the same country, we allow control banks to operate in a different market. This approach also allows us to have a large sample of possible control units. Further technical details and diagnostics of the matching are provided in Appendix D.

Second, we construct a unique dataset for each event that compiles observations for the treated bank and its five control banks. Time is normalized with respect to the treatment time, where t_0 represents the treatment year for the treated, and t_n (or t_{-n}) denotes n year(s) after (or before) the treatment year. We restrict ourselves to a window of five years before and after treatment (mainly because we lack information on treatment duration).

Third, we append all the event databases into a single, comprehensive stacked database. A bank may appear multiple times in the stacked database for two reasons. First, a late-treated bank may serve as a control for an early-treated bank. Second, a never-treated

[10] For the latter group, we restrict the sample to not-yet-treated banks in $t+3$ or more to avoid having a control bank treated shortly thereafter.

bank may serve as a control for multiple treated banks. We create a unique identifier for each event database that is common to the treated bank and the five control banks.

Finally, we use the stacked database to estimate the effect of the program on the treated banks. In line with previous studies using a similar approach (Gormley and Matsa, 2011; Cengiz et al., 2019; Deshpande and Li, 2019), we employ the following DID model:

$$Y_{it} = \gamma Post_t + \beta Post_t \times Treated_i + \alpha_i + \mu_{ct} + \varepsilon_{it} \quad (1)$$

In the model, the unit of observation is the bank-event pair (indexed i[11]) and time (t, normalized to the treatment and ranging from $t-5$ to $t+5$).

The dependent variable (Y_{it}) is measured as the loan growth of bank i between periods $t-1$ and t. Loan growth is calculated using total gross loans. Unfortunately, FitchConnect does not provide details on the breakdown of gross loans by borrower (corporate and household) or maturity. In addition, gross loans reflect the total value of loans and we do not have access to the number of loans.

The interest variable is the interaction between $Treated_i$ and $Post_t$. $Treated_i$ is an indicator that equals one if bank i is the treated bank and zero if it is a control bank. $Post_t$ is a dummy variable that equals one for the post-treatment period (from t_0 to t_{+5}). The $Treated_i$ dummy at the level is not included at it is absorbed by bank fixed effects (α_i).

The coefficient of interest (β) measures whether the loan growth of the treated banks differs from that of the control banks after the implementation of the program. We expect a positive coefficient indicating that treated banks increase their lending relative to control banks after receiving the treatment. The coefficient associated with $Post_t$ measures the post-treatment loan growth for control groups and is expected to be zero, as there is no reason for control banks to be affected by the treatment.

To ensure causal identification, we follow the stacked regression literature (Gormley and Matsa, 2011; Cengiz et al., 2019; Deshpande and Li, 2019) by saturating the model with bank-by-event fixed effects (α_i). Bank-by-event fixed effects imply that we examine the loan growth of the same bank over time within a 5-year window before and after the treatment date. This approach is more conservative than adding bank and event fixed effects separately. By including bank-by-event fixed effects (α_i), we treat a bank that appears in two different events as two different banks. We also include country year fixed effects (μ_{ct}). As stated above, we do not restrict control banks to be in the same country

[11]To simplify the notation, we assume that the index i includes the pair bank i and event e. In other words, the same bank appearing twice or more (in different events) is considered as an unique bank at each appearance (the index i could be written ie). We generally refer to "bank" while we are talking about "bank-event".

as treated banks.[12] Consequently, treated and control banks do not always operate in the same country. Lending may be affected by a demand shock. We include country-year fixed effects to fully account for changes in the supply of bank loans. Adding a set of country-year fixed effects also allows us to control for any unobserved factors that occur at this level. The standard errors are clustered at the event level.

In addition to assessing the effect of treatment on DFI-supported banks, we also examine the moderating effect of some event or bank variables. To do this, we use a triple interaction model as follows:

$$\begin{aligned} Y_{it} = & \gamma_1 Post_t + \gamma_2 Post_t \times Z_{i/e} \\ & + \beta_1 (Post_t \times Treated_i) + \beta_2 (Post_t \times Treated_i \times Z_{i/e}) \\ & + \alpha_i + \mu_{ct} + \varepsilon_{it} \end{aligned} \tag{2}$$

where $Z_{i/e}$ is the moderator measured at the bank level (indexed i[13] or event-level (indexed e). Moderators are measured in the year before the treatment (time-invariant). The coefficient β_1 associated with the interaction of $Post_t \times Treated_i$ quantifies the effect of the treatment in the absence of any effect of the moderator ($Z_{i/e} = 0$). The coefficient β_2 indicates whether the moderator shapes the observed relationship.[14]

5 Empirical results

5.1 Baseline results

We examine the impact of the treatment on the treated banks by comparing their lending activity before and after the treatment with a control group. Table 5 presents the empirical results. For the sake of transparency, we provide different specifications with different sets of fixed effects. Column (1) presents results without fixed effects, followed by the inclusion of event fixed effects (column 2), bank fixed effects (column 3), both event and bank fixed effects (column 4), and bank-event fixed effects (column 5), our

[12] As explained above, there are two main reasons to consider banks operating in other countries as possible control units. First, treatment of a major bank in a market may affects its competitors and the SUTVA (Stable-Unit Treatment Value Assumption) is not respected. Even if at the end of the paper, we document that untreated banks do not seem affected by the treatment, this issue may remain and we prefer to adopt a conservative approach. Second, the pool of control units is rather limited in small markets that will limit our ability to find relevant controls.

[13] As before, we use the index i only for banks, while we refer to bank-event (ie) to account for cases where the same bank may appear in different events. Furthermore, for treatment characteristics, we apply them to both treated and control units.

[14] The good practice consists on incorporating the two double interactions in addition to the triple interaction. However, we cannot include interaction $Treated_i \times Z_{i/e}$ because its effect is captured by bank-event fixed effects (α_i).

preferred specification. The variable of interest is the coefficient associated with the interaction between the $Treated$ dummy and the $Post$ dummy (β in Equation 1), which we expect to be positive if banks increase their total lending after receiving assistance.

Table 5: Baseline results

	Loan Gr. (1)	Loan Gr. (2)	Loan Gr. (3)	Loan Gr. (4)	Loan Gr. (5)
Post=1	1.030	0.274	-0.234	-0.446	-0.447
	(0.794)	(0.850)	(0.557)	(0.773)	(0.797)
Treated=1 × Post=1	-1.218	-1.540	-3.107**	-3.041**	-2.952**
	(1.460)	(1.379)	(1.245)	(1.267)	(1.315)
Fixed effects	None	Event	Bank	Bank and Event	Bank-Event
Observations	6117.0	6117.0	6117.0	6117.0	6117.0

The table reports the results of the coefficient associated to the interaction between $Post$ and $Treated$ dummies in equation 1. Dependent variables is the loan growth. Column (1) displays results from a model without fixed effects, column (2) includes only group fixed effects, column (3) bank fixed effects, column (4) include group and bank fixed effects separately and (5) bank-by-group fixed effects. All models include country-year dummies. Standard errors are clustered at the bank-group level. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

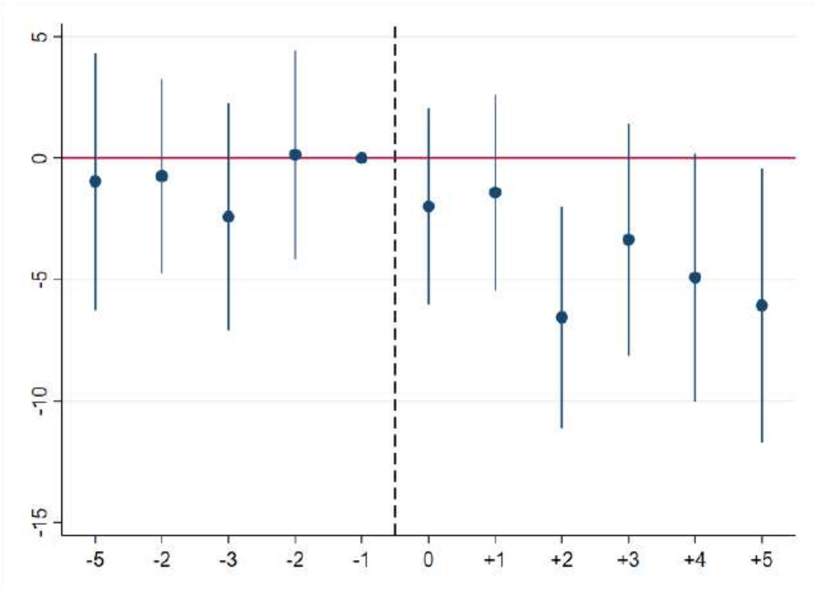
Our results are not as expected as assisted banks reduce their lending instead of increasing it. The coefficients associated with the interaction between $Post$ and $Treated$ are consistently negative and statistically significant when controlling for unobserved bank characteristics in columns (3) to (5). The economic impact of treatment is also noteworthy. In our preferred specification (column 5), the effect of the treatment is a reduction in loan growth of 8% over the 5-year horizon.[15] Finally, as expected, we do not observe a control group effect. The coefficient on $Post$ is zero, indicating that the loan growth of the control banks is unchanged before and after the treatment. This result points out that control units are not directly affected by the treatment (in line with SUTVA).

We extend our analysis by using an event study approach[16] to examine how lending

[15] The coefficient associated with the interaction indicates that loan growth was reduced by 3 percentage points over a five-year horizon. To gauge the importance of the effect, it is useful to compare it with pre-treatment loan growth. For pooled banks (treated and untreated), annual loan growth from $t-5$ to $t-1$ averaged 6.34%. Over a 5-year horizon, this figure implies loan growth of 36%. In other words, DFI-supported banks see a reduction in their lending activity by almost 8% over a 5-year horizon (from 36 to 33%). The figure is quite similar if we restrict the analysis to treated banks. The annual growth of treated banks is 7.25% from $t-5$ to $t-1$. The 5-year compound growth is thus 42% and the treatment effect is 7%.

[16] The event study model is as follows:
$Y_{it} = \gamma_t + \sum_{t=-5}^{t=5} \delta_t (Treated_i \times \gamma_t) + \alpha_{ie} + X_{it} + \varepsilon_{it}$
In the event study model, we replace $Post_t$ with a set of period-by-group fixed effects (γ_t). As is

Figure 2: Baseline results from an event study



The figure displays the coefficients associated with the interaction between time dummies and $Treated$ dummies. Dependent variables is loan growth. Models include time, bank-group and country-year dummies. Standard errors are clustered at the bank-group level. Figures represent the point estimate (circle) and the confidence interval at the 5% level.

activity evolves around the treatment period. Results are displayed in Figure 2. The event study allows us to first test the parallel trend assumption by examining the coefficients associated with the pretreatment interactions. Consistent with the parallel trend assumption, the coefficients associated with the interaction between the treated dummy and the pretreatment years are not statistically significant from zero (either individually or jointly). In addition to the lack of statistical effect, there is also a lack of a clear pre-treatment trend, which is rather reassuring for the validity of this assumption. Second, the event study allows us to examine the dynamics of the treatment over time by examining the coefficients associated with interactions between *treated* and post-treatment years. The results show that the treatment effect is delayed over time. Immediately after receiving assistance (from t_0 to t_{+1}), there is no significant difference between treated and control banks. However, two years after the treatment, DFI-assisted banks reduce their lending relative to similar control banks. The time lapse is in line with expectations since we use the date of signature. There is no reason to expect an effect immediately after the signing of an agreement between a DFI and a bank, as it takes time to disburse

common practice in event studies, we exclude the year immediately preceding the treatment (t_{-1}) to prevent collinearity. Consequently, all results are relative to this year. The coefficients of interest are δ_t, which represent the differences between treated and control banks in the outcome Y_{it} before and after treatment. We expect no differences before treatment (from t_{-5} to t_{-2}) to validate the parallel trend assumption. Treatment will affect banks if outcomes diverge after a bank receives treatment (from t_0 to t_{+5}).

funds after approval.

We conclude baseline analysis by examining whether negative effect is heterogeneous. We first consider the possible heterogeneous effect of financial instrument. There is no inherent reason to assume that one financial instrument would be more harmful than others, even if all instruments are not equivalent and are not designed to address the same market failure (Eslava and Freixas, 2021). We rely on model presented in Eq. 2 to study the effect of three instruments: loan, equity participation or guarantee. Results are displayed in Table E1 in Appendix. We note that the negative effect is stronger for equity participation and reduces for loan. However, the triple interaction is not statistically significant. Second, we examine whether the effect observed in the last column of the same table applies only to the first support or to the first and subsequent supports. We create a dummy equal to one for the second and subsequent support. The results presented in the Appendix document that the effect is mainly present for the first support. For the subsequent support, we continue to see a negative, albeit insignificant, effect. However, the lack of statistical significance may be due to the limited number of situations where a bank receives more than one support within five years.

5.2 Robustness checks

We conduct a series of robustness checks to corroborate our primary econometric results. The results of all sensitivity tests are presented in Appendix E (from Tables E2 to E4).

First, we examine whether the results are affected by the matching procedures used in the baseline analysis. We vary the number of matches from 1 to 9 closest banks in columns (1) to (4) of Table E2. Second, we include the lagged dependent variable in the matching variable in column (5). We then exclude certain controls based on characteristics that could introduce bias into the results. Distance matching identifies five controls for each treated bank, regardless of the distance between the treated and the control. Column (6) excludes control banks for which the distance to the treated firms is large (i.e., exceed the top decile of the distribution). We also exclude future treated banks as potential controls in column (7), as these banks may have specific characteristics. Finally, for each treated bank, we exclude controls that belong to the same banking group in column (8).

We then re-evaluate the baseline model using an alternative approach to calculating the dependent variable. To mitigate the regression-to-the-mean effect, loan growth is defined as the change in loans from year t to $t-1$ divided by the simple average of loans in the same period (instead of using the initial value). Result is presented in column (1) of Table E3. We then include control variables in the following columns. In the second column of the same table, we add variables used for matching. In the following columns, we add a proxy for bank financial performance by including return on assets (column 3), return on equity (column 4), and net interest margin (column 5). Finally, we include two measures of portfolio quality in the remainder of the table by considering the ratio of NPLs to gross loans (column 6) and the ratio of provisions to loans (7).

We finally examine whether empirical results are similar when we use the raw data (i.e., before applying the matching procedures). To do so, we consider the initial data and employ a standard TWFE model where i and t correspond to bank i and year t. In columns (1) and (2) of Table E4, we first run the model without and with control variables, denoted as $\mathbf{\Omega}_{it}$, consistent with those used in the matching. Since the TWFE method is no longer appropriate for staggered DID, we also implement the approach developed by Callaway and Sant'Anna (2021) in columns (3) and (4).[17] Robustness checks confirm our primary results. The coefficients consistently reach statistical significance. Differences in marginal effects can be attributed to variations in the mean of the dependent variable across specifications.

5.3 Why does lending activity decrease for treated banks?

The baseline results are puzzling because we document a negative treatment effect when we expect a positive effect. This subsection explores several explanations for this unexpected result. In this subsection, we use the triple interaction described in Eq. 2 (unless explicitly stated that we use a different approach).

5.3.1 Political motivation

We first focus on the political economy explanation. DFIs are not isolated to political considerations (Dreher et al., 2019; Frigerio and Vandone, 2020). Banks may be targeted for political reasons, and there is no reason to expect a positive effect of the treatment (but also no reason to see a negative effect). We present two tests that challenge this view.

First, we assumed that bilateral DFIs can more easily allocate funds for political or donor interests than multilateral DFIs. Since bilateral DFIs are owned by only one state, a government can easily shape their lending for political reasons. In contrast, multilateral DFIs have many different countries on their boards. Therefore, allocating resources according to political interests is more complex, although not impossible. We therefore examine in column (1) of Table 6 whether the negative effect is stronger for banks supported by bilateral DFIs. Contrary to the political economy hypothesis, we document that the type of DFI (multilateral or bilateral) does not mitigate the effect.

[17] The Callaway and Sant'Anna (2021) approach also allows us to estimate an event study model. Although not shown here, the results are consistent with those shown in Figure 2.

Second, we test this hypothesis by considering that it may be easier to allocate funds for political purposes in countries with higher levels of corruption. To test this hypothesis, we classify the recipient country according to its level of corruption (based on the score for control of corruption in the World Governance Indicators). We create a dummy equal to one if the score is below zero, indicating that the country is more corrupt than the world average. We interact this dummy with the treatment interaction. The result, shown in column (2) of Table 6, shows that the level of corruption in the recipient country does not play a role in explaining the negative impact of the treatment. In fact, the triple interaction is positive (though not statistically significant), indicating that the effect is attenuated in more corrupt countries.

Table 6: Political economy and contract terms hypotheses

	Political economy		Contract terms			
Dep. var. →	Loan gr.	Loan gr.	Loan gr.	Loan gr.	Loan gr.	Int. Rate
Moderator (Z) →	Multi DFIs	Corruption	After-2015	Group	treated-gp	None
	(1)	(2)	(3)	(4)	(5)	(6)
Post × Treated	-2.845*	-5.477	-3.267*	-2.653	-2.408*	-0.00006
	(1.659)	(4.214)	(1.983)	(1.664)	(1.287)	(0.00005)
Post × Treated × Z	-0.159	2.825	0.652	-0.584	-1.429	
	(2.457)	(2.825)	(2.484)	(2.576)	(2.631)	
Fixed effects	Bank-Event	Bank-Event	Bank-Event	Bank-Event	Bank-Event	Bank-Event
Country-year	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,117	6,117	6,117	6,117	6,117	6,193

Dependent variables is the loan growth in columns (1) to (3). We report results of a triple interaction model, described in Eq. 2. Two moderators are considered to consider the political economy hypothesis: a dummy variable equal to 1 if the DFI providing the support is a multilateral DFI (AfDB, EIB, ERDB, IFC or IslDB) in column (1) and a dummy equals to one if the recipients is located in corrupted countries (WG index is below 0) in the year previous the treatment in column (2). The contract term hypothesis is considered by three triple interaction model with the following moderators: a dummy equal to 1 if the treatment occured after 2015 in column (3), if the bank treated belongs to a banking group in column (4), if the bank treated belongs to a banking group that have already being treated in column (5). In the last column, we rerun the baseline model (Eq. 1) but the dependent variable is the implicit interest reate computed as the ratio of interest income to gross loans. We only display coefficients associated with interaction between *Post* and *Treated* (β_1 in Eq. 1) and with the triple interaction (β_2) in Eq. 1). Models is estimated on stacked dataset. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

5.3.2 Contract terms

A second strand of the explanation is that DFIs attach restrictive conditions to the support, which may alter the lending of the treated banks. We consider three possible conditions: exclusion lists, strict lending procedures, and a shift towards long-term loans.

First, DFIs often impose exclusion lists, which may cause the treated banks to stop lending to certain clients (borrowers that control the banks may subsequently pick up).

Exclusion criteria often relate to environmental considerations (e.g., no lending to coal projects). There has been an increase in the number of projects with exclusion criteria, especially after the 2015 UNFCCC Paris Agreement on climate change. Therefore, we examine how the impact of treatment differs for projects signed before and after the 2015 Paris Agreement. The results presented in column (3) of Table 6 do not support this explanation. The effect is even more pronounced for projects implemented before the 2015 Paris Agreement.

Second, DFIs often require treated banks to update their lending procedures, including due diligence and know-your-customer (KYC) requirements. As a result, some borrowers previously financed by treated banks may no longer be eligible for financing. To test this hypothesis, we make the following assumption. Banks belonging to a banking group are likely to implement stricter KYC requirements and due diligence, especially if one of the banks in the group has already been financed by a DFI. According to this explanation, we should observe a soften negative effect for banks belonging to a banking group, especially if a subsidiary of the group has been treated in the past, as they already adjusted their procedures. We apply this test in columns (4) to (5) of Table 6. The empirical results are not consistent with this explanation. The effect is stronger for banks belonging to a group, but the difference is not statistically significant.

Third, DFIs often provide long-term funds to banks with the aim of encouraging them to make long-term loans. If the treated banks were indeed shifting towards longer-term loans, we might observe a slowdown in annual growth. Unfortunately, our available data do not disaggregate loans by maturity, which prevents us from directly testing this hypothesis. However, we can indirectly assess this hypothesis by assuming that long-term loans typically carry higher interest rates than short-term loans. Therefore, if the treated banks were to shift to longer-term loans, we would expect to see an increase in interest rates. To measure interest rates, we examine interest income relative to gross loans. We rerun the baseline model (eq. 1) using interest rates as the dependent variable. The results in the last column of Table 6 show a negative and insignificant effect, which is not consistent with the hypothesis of a shift from short-term to long-term loans.

5.3.3 Distress bank targeting

We now shift our focus to the fact that DFIs may prioritize their funds to distressed banks. Consequently, it may not be surprising to observe a decline in lending activity for these banks, even if DFI support helps to mitigate the contraction. However, three pieces of evidence challenge this view.

First, descriptive statistics comparing treated and untreated banks, presented in Table

Table 7: Distress bank support hypothesis

Panel A: Triple interaction model					
	Performance			Portfolio quality	
Dep var. (Y) →	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.
Moderator (Z) →	ROA	ROE	NIM	NPLs	Provision
	(1)	(2)	(3)	(4)	(5)
Post × Treated	-2.511	-3.913**	-2.741*	-3.916**	-0.789
	(1.823)	(1.875)	(1.533)	(1.727)	(1.596)
Post × Treated × Z	-0.114	0.083	-16.209	13.915**	-1.291*
	(0.681)	(0.080)	(24.480)	(6.477)	(0.692)
Observations	5,836	5,836	5,946	4,584	5,457
Fixed effects	Bank-Event	Bank-Event	Bank-Event	Bank-Event	Bank-Event
Country-year dummies	Yes	Yes	Yes	Yes	Yes

Panel B: Alternative dependent variables					
	Performance			Portfolio quality	
Dep var. (Y) →	ROA	ROE	NIM	NPLs	Prov.
	(1)	(2)	(3)	(4)	(5)
Post × Treated	0.019	1.153	-0.004	-0.000	-0.011
	(0.157)	(1.193)	(0.003)	(0.000)	(0.182)
Observations	6,023	6,013	6,188	4,553	5,623
Fixed effects	Bank-Event	Bank-Event	Bank-Event	Bank-Event	Bank-Event
Country-year dummies	Yes	Yes	Yes	Yes	Yes

The table reports the results of the coefficient associated to the interaction between *Post* and *Treated* dummies in Eq. 1. We consider the five dependent variables in each columns, with three dependent variables for performance (ROA, ROE and NIM) and two measures of porfolio quality (NPLs and Provisions). For each model, we only report the coefficient associated to the interaction between *Post* and *Treated* as well as the number of observations. Panel A considers a triple interaction models, where moderator change in different columns. Panel B considers a model where dependent variable is ROA (column 1), ROE (2), NIM (3), NPls (4) and Provisions (5). *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

4, show that DFI-supported banks have better performance (ROA, ROE) and portfolio quality than their counterparts (before treatment).

Second, we examine the differential effect of the treatment according to the initial level of performance and portfolio quality using the triple interaction model (Eq. 2). In this specification, the moderator (Z_i) is the bank's performance (measured by ROA, ROE, and NIM) or portfolio quality (proxied by NPLs and provisions) in the year prior to the treatment. Panel A of Table 7 shows the empirical results.[18] The empirical evidence is not consistent with the hypothesis of support for distressed banks. The level of performance (columns 1 to 3) has no moderating effect on the negative effect of the DFI program. Meanwhile, the effect of portfolio quality is ambiguous, as the two regressions in columns (4) and (5) show contradictory results. According to the econometric result in column (4),

[18] We also run a model with a dummy variable that takes a value of one if the bank's moderator is above the mean.

the negative effect is stronger for banks with better portfolio quality (i.e. lower NPLs). In the next column, we find the opposite effect. In short, the empirical results are not clearly consistent with the distressed bank targeting hypothesis.

Third, we examine whether the treatment has a direct impact on performance and portfolio quality. According to this view, distressed treated banks would use additional funds to clean up their balance sheets. To do so, we re-run the baseline model (Eq. 1) but only change the dependent variable using the three performance measures (ROA, ROE, and NIM) and two proxies for portfolio quality (NPLs and provisions). Panel C of Table 7 documents the lack of a clear effect of the treatment on performance and portfolio quality.

5.3.4 Absorptive capacity

A final explanation tested in this paper is that treated banks are constrained by their limited absorptive capacity, mainly due to limited human resources. Supported banks may need to reallocate some resources to new lending targets, resulting in less lending to their original borrowers. This reallocation, coupled with a composition effect (new loans are smaller), could lead to a decline in overall loan growth. To test this assumption, we examine whether the estimated impact is shaped by the bank's absorptive capacity. We consider three different measures of absorptive capacity: treatment intensity, bank size and bank efficiency.

Table 8: Effect of DFI support on loan growth, by intensity of the treatment

Dep var. → Moderator (Z) →	Loan Gr. Int >1 (1)	Loan Gr. Int >2 (2)	Loan Gr. Int >3 (3)	Loan Gr. Int >4 (4)	Loan Gr. Int >5 (5)	Loan Gr. Int >6 (6)	Loan Gr. Int >7 (7)	Loan Gr. Int >8 (8)
Post × Treated	-3.463	-2.170	-1.211	-1.084	-1.561	-1.859	-2.005	-2.547*
	(2.501)	(1.592)	(1.274)	(1.164)	(1.218)	(1.204)	(1.267)	(1.340)
Post × Treated × Z	0.781	-1.472	-5.017*	-6.852*	-6.243	-6.817	-9.379	-5.517
	(2.772)	(2.444)	(3.007)	(3.573)	(4.066)	(4.817)	(6.157)	(6.394)
Observations	6,117	6,117	6,117	6,117	6,117	6,117	6,117	6,117

Dependent variables is the loan growth. We classify each event according to the importance of treatment for the treated bank in percent of its total assets. We consider eight thresholds from 1% to 8% of bank total assets in columns (1) to (8). All models include country-year dummies. We only display coefficients associated with interaction between *Post* and *Treated* (β_1 in Eq. 1) and with the triple interaction (β_2) in Eq. 2). Models is estimated on stacked dataset. Standard errors are clustered at the bank-group level. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

First, we examine whether the impact of the treatment is shaped by its intensity for assisted banks. To measure th intensity, we use the ratio of DFI support to total bank assets. To assess the impact of treatment intensity, we introduce a binary variable indicating whether treatment intensity exceeds a cutoff ranging from 1% to 8% of the bank's total assets.[19] Table 8 presents the results, showing that the negative impact of treatment increases with increasing intensity. Although the triple interaction is not always statistically significant, the coefficients associated with the triple interaction consistently show a strong negative trend as the intensity increases. When the marginal effects of the treatment are calculated for both groups of banks (below and above the threshold), a clear and strong negative impact emerges when the treatment is intense (see table E5 in the Appendix).

We then examine whether the treatment has different effects on some banks according to their size. Small banks face more constraints due to their limited absorptive capacity related to their (human and technical) resources. We use different proxies to measure bank size, distinguishing between absolute (total assets) and relative (market share) size. For both measures, we use a continuous variable as well as dummy to indicate when bank size exceeds the median. The results, shown in colmuns (1) to (4) of Table 9, indicate that the negative impact of the treatment is more pronounced for small banks and tends to dissipate for larger banks. These results are consistent across different measures of bank size. We see that the negative effect of DFI intervention is largely reduced (column 4) or even vanishes (column 2) for banks those size exceeds the median of the sample. It should be noted that we get similar results if we control for treatment intensity.

Finally, we consider bank inefficiency as a potential barrier to absorption. Indeed, the least efficient banks may find it difficult to finance new borrowers without restricting their loans to other borrowers. These banks are more likely to make a choice and thus limit their overall lending. We measure inefficiency using two common measures in the literature. On the one hand, we consider the cost-income ratio.[20]. The second measure is the ratio of overhead costs to total assets. As before, we consider both continuous and dummy variables for the moderators. Columns (5-8) of Table 9 suggest that the least efficient banks (i.e., those with higher levels of cost-income ratio and overhead costs) suffer more after receiving support from a DFI. In detail, we document that the triple interaction is always negative, although only statistically significant for the overhead variable as a moderator. Nevertheless, in Appendix (Table E6) we measure the effect of the treatment for different banks according to their level of inefficiency. We compute the effect of the treatment for very efficient banks (bottom decile), efficient banks (bottom

[19]Descriptive statistics indicate that treatment represents 6% of total assets on average, but the median is only 2%.

[20]To calculate costs, we exclude interest expenses to maintain a measure of operating costs, in line with the common operating ratio used in banking.

Table 9: Effect of DFI support on loan growth, by bank size and efficiency

Dep var. →	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.	Loan Gr.
	Size				Inefficiency			
Moderator (Z) →	Total assets (in USD)		Market share		Cost to income		Overhead cost	
	Cont.	Dummy	Cont.	Dummy	Cont.	Dummy	Cont.	Dummy
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post × Treated	-20.196***	-7.678**	-4.946**	-7.544**	0.658	-2.046	0.866	-0.864
	(6.060)	(3.002)	(2.012)	(3.063)	(2.734)	(1.777)	(1.999)	(1.739)
Post × Treated × Z	2.480***	6.697**	0.345*	5.515*	-4.238	-1.927	-53.211**	-4.176*
	(0.754)	(3.274)	(0.181)	(3.321)	(2.882)	(2.569)	(24.068)	(2.441)
Obs.	6,117	6,117	6,117	6,117	5,642	5,642	5,642	5,642

Dependent variables is the loan growth. We consider two moderators: bank size in columns (1) to (4) and bank inefficiency in columns (5) to (8). We consider two measures of bank size: the absolute size, measured by total bank assets (columns 1-2) and the relative size, measured by national market share (columns 3-4). We also consider two measures of inefficiency, namely cost-income ratio (columns 5-6) and the ratio of overhead costs to total assets (columns 7-8). For each moderator, we consider both the continuous measure (Cont.) and a dummy if the value for the bank is above the median (Dummy). All models include country-year dummies and the intensity of the treatment. The table only reports coefficients associated to the double interaction between *Post* and *Treated* (β_1) and the triple interaction between *Post*, *Treated* and Z (β_2). Standard errors are clustered at the bank-group level. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

quartile), moderately efficient banks (median), inefficient banks (top quartile), and very inefficient banks (top decile). We document that even though the effect of the treatment is negative for all banks, it is moderate and statistically insignificant for efficient banks. However, inefficient banks suffer a lot from the treatment.

6 Extensions

We conclude the analysis with two extensions. First, we examine the impact of DFI investments on untreated banks. Second, we investigate whether our results are similar when intermediated lending is allocated to MFIs.

6.1 Impact on untreated banks

We examine the possible spillover effects of DFI investments on untreated banks. This exercise has two main goals. First, since only a limited number of well-established banks are eligible for DFI funding, other market participants may suffer from market distortions if treated banks are favored. Second, this analysis is a way to test the absence of spillovers required for the application of DID (test the SUTVA).

To examine the possible side effect on untreated banks, we focus on ineligible banks. Ineligible banks are defined as banks that are too different from treated banks to be considered by DFIs (e.g., due to very small size). To identify ineligible banks, we use the outcome of the matching (see Appendix D). Ineligible banks are banks that were never matched to treated banks. Thus, we identify 588 ineligible banks.

We estimate the following model on the subset of ineligible banks:

$$Y_{ict} = \beta DFI_{ct} + \mathbf{\Omega}_{ict} + \alpha_i + \gamma_t + \varepsilon_{ict} \tag{3}$$

The dependent variable (Y_{icy}) is the loan growth of bank i in country c in year t. The variable of interest (DFI_{ct}) measures the importance of DFI investment in country c in year t. We consider the market share of all treated banks supported by DFIs in the country over the year (DFI_{ct}).[21] We add to the model a list of control variables ($\mathbf{\Omega}_{ict}$) that includes the bank-level variables used for matching. We also include bank (α_i) and year (γ_t) fixed effects to control for time-invariant unobserved bank-level characteristics and common shocks, respectively.

The empirical results, displayed in Table 10, suggest that ineligible banks are not affected by the intensity of DFI support in their country. TColumns (1) and (2) present the model using an annual measure of DFI support, while columns (3) and (4) use a cumulative measure. All specifications control for country and year fixed effects, while control variables are only included in columns (2) and (4). The variable DFI has a negative coefficient in all specifications, but it is never shown to be statistically significant. Even if we disregard statistical significance, the impact of DFI support at the national level on the loan growth of ineligible banks is rather limited.

The results are robust to several sensitivity tests (available upon request) that consider an alternative measure of DFI support [22] and testing for a non-linear effect of the DFI variable. We also consider a model that includes all untreated banks (ineligible banks and

[21] We compute two measures of DFI_{cy}. First, we consider the annual value of DFI investments in the country. This measure implicitly assumes that the treatment period is annual. We also consider a cumulative measure of DFIs by summing the value of current market share with past cumulative market shares. In this specification, as before, we assume that the treatment is permanent. Suppose that in the same year DFIs finance two banks (A and B) with respective market shares of 25% and 10% in country c in year t, the value of DFI_{ct} is 0.35. Now suppose that the DFIs also support a third bank in the next year, which accounts for 5% of total assets. The annual DFI variable takes the value of 0.35 in year t and 0.05 in year $t+1$, while the cumulative version of the DFI variable takes the values of 0.35 and 0.4 in years t and $t+1$, respectively.

[22] We compute a measure of DFI support that takes into account not only the market share of treated banks but also the importance of DFI support. Technically, we simply multiply the market share of treated banks by the ratio of support to total assets. As before, assume that DFIs invest in banks A and B, which have 30% and 10% of the national market share, respectively. We now consider another piece of information. We know that the investment in Bank A represents 5% of its assets and the investment in Bank B represents 10% of its assets. The DFI variable is now calculated as $0.10 \times 0.30 + 0.10 \times 0.10$. The DFI variable shifts from 0.35 to 0.04 (both variables are not comparable).

Table 10: Effect of national level of DFI support on loan growth of non-eligible banks

	loan_growth (1)	loan_growth (2)	loan_growth (3)	loan_growth (4)
DFI	-38.403	-39.989	49.924	-11.336
	(110.829)	(115.782)	(143.098)	(151.420)
size		-0.000		-0.000
		(0.000)		(0.000)
market_share		-0.374***		-0.374***
		(0.089)		(0.093)
deposit_ass		-0.251**		-0.251**
		(0.118)		(0.118)
solvency		0.387**		0.388**
		(0.191)		(0.191)
transformation		-0.000		-0.000
		(0.000)		(0.000)
group_dummy		7.309		7.306
		(6.114)		(6.164)
Observations	3668.000	3417.000	3668.000	3417.000

Dependent variables is the loan growth. The variable of interest DFI measures the market share of banks supported by DFIs. The analysis is restricted to non-eligible banks (i.e., banks that are never matched with treated banks). Columns (1) and (2) consider a non-cumulative measure of DFI and columns (3) and (4) a cumulative measure (assuming that treated banks are always treated). All models include country and year dummies. Standard errors are clustered at the bank-group level. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

untreated but matched banks). Indeed, focusing on ineligible banks may be problematic. The impact of DFI support on untreated banks may be larger for eligible but untreated banks, which compete directly with treated banks, than for banks that are very different from treated banks. The empirical results (available on request) are not affected by this change.

6.2 Impact on microfinance institutions

Finally, we turn to the possible differential impact of intermediated lending on microfinance institutions (MFIs). The descriptive statistics presented in Section 3 document that MFIs are the second type of financial intermediaries financed by DFIs in Africa. An interesting question is whether the negative effect observed for banks is similar for MFIs. On the one hand, MFIs also suffer from the problem of absorptive capacity. On the other hand, DFIs may be more effective in supporting MFIs that suffer from very limited access to long-term funding.

Table 11: Effect of treatment on Microfinance Institutions

Panel A: Loan growth					
Method	TWFE	TWFE	TWFE	CS	CS
	(1)	(2)	(3)	(4)	(5)
Post × Treated	-0.402***	0.029	0.083	0.026	0.056
	(-4.31)	(0.27)	(0.70)	(0.19)	(0.18)
CV	No	No	Yes	No	No
MFIs FE	Yes	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No	No
Obs.	2,214	2,214	1,281	2,054	1,081
# MFIs	470	470	344	359	212
# treated MFIs	32	32	28	26	19

Panel B: Borrower growth					
Method	TWFE	TWFE	TWFE	CS	CS
	(1)	(2)	(3)	(4)	(5)
Post × Treated	-0.345***	-0.044	-0.098	-0.144	0.137
	(-2.62)	(-0.32)	(-0.32)	(-0.65)	(0.25)
CV	No	No	Yes	No	No
MFIs FE	Yes	Yes	Yes	Yes	Yes
Country-year FE	No	Yes	Yes	No	No
Obs.	1,951	1,951	1,089	1,800	859
# MFIs	419	419	299	326	186
# treated MFIs	31	31	26	25	18

Dependent variables is the loan growth in Panel A and active borrower growth in Panel B. *Post* id a dummy equals to 1 for year after treated received the treatment (0 otherwise) and *Treated* is a dummy equals to one for treated MFIs. Columns (1) to (3) employ a simple TWFE model and columns (4) and (5) display results using the Callaway and Sant'Anna (2021)'s method. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

To examine the impact on MFIs, we merge the DFI investment database with Mix-Market, which collects financial data on MFIs worldwide. However, the empirical analysis is constrained by the limited coverage of MixMarket. In particular, we can only consider a limited number of MFIs, and basic financial information is often missing. This limitation forces us to consider a simple TWFE as follows:

$$Y_{it} = \beta Post_t \times Treated_i + \mathbf{\Omega}_{it} + \alpha_i + \mu_{ct} + \varepsilon_{icy} \tag{4}$$

where i, t and c refer to MFI, year and country, respectively. We consider two dependent variables. In line with the previous analysis, we consider loan growth. We also use information on active borrowers available in MixMarket. The model is an extension of a standard DID. The variable of interest is the interaction between a dummy for treated ($Treated_i$) and a dummy that takes the value 1 after the treatment ($Post_{it}$) and 0 otherwise. We control for time-invariant unobserved individual characteristics by adding MFI fixed effects (α_i). Country year fixed effects (μ_{ct}) allow us to control for shocks that occur at the country year level.[23] Finally, we control for time-varying MFI control variables. We use the same set of control variables as in equation 3 (with the exception of the dummy for group and macro control variables, which are absorbed by the country-year dummies). Finally, in addition to the simple TWFE, we also use the estimator of Callaway and Sant'Anna (2021).

The results presented in Table 11 show that DFIs do not stimulate MFI lending (Panel A) or the growth of active borrowers (Panel B), regardless of the specification used. Column (1) shows a negative treatment effect on loan growth (Panel A) and borrower growth (Panel B), consistent with our previous findings for banks. However, this result is no longer robust when we include country-year fixed effects (columns 2-3). The coefficient becomes positive, though close to zero, in Panel A and remains negative but statistically insignificant in Panel B. The approach using the method of Callaway and Sant'Anna (2021), developed to deal with the problem of the forbidden control group, yields very similar results. The treatment effect is never statistically significant.

7 Conclusion

Blended finance is the new mantra in development finance to bridge the financing gap to achieve the Sustainable Development Goals and finance the transition by leveraging private sector investment for development. Development finance institutions (DFIs) play a key role in mobilizing private flows for development, as their mandate is to finance private projects in developing countries. This paper focuses on the main instrument used by DFIs to support small and medium sized enterprises: intermediated lending. Under these programs, DFIs provide funds or risk-sharing instruments to banks and microfinance institutions. Local financial intermediaries then blend these (public) funds with their commercial funds and on-lend them to the type of borrowers specified in the clauses of the programs (e.g., young entrepreneurs).

Despite their prominent and growing role in development finance, there is a lack of evidence on the effectiveness of DFI intermediated lending. This paper fills that gap. Specifically, it examines whether DFI-supported banks increase their lending after receiving the program in Africa. The paper combines bank-level data with a hand-collected database of 900 projects offered by 17 DFIs in Africa between 2010 and 2021. The final sample consists of 952 banks, with 156 banks treated between 2010 and 2021 and 796

[23]Time dummies or $Post_{it}$ are included in these fixed effects.

untreated banks.

The empirical results show that treated banks do indeed reduce their lending relative to control banks after treatment. The effect is also economically significant, as the loan growth of supported banks was reduced by 8%. This result is surprising. We consider a number of possible explanations. The most convincing is that the reduction in lending is due to the limited absorptive capacity of the recipient banks. According to this explanation, borrowers must prioritize new clients, sacrificing some of their previous borrowers due to their limited ability to absorb loans. We, however, document that there are no spillover effects across banks.

This implication of the reallocation effect within targeted banks is rather mixed. One might expect that reallocation can have positive effects if banks shift from well-established firms, which have access to funds from other investors, to financially constrained firms. In contrast, substitution may have negative effects if it occurs within the group of financially constrained firms (e.g., sacrificing small agricultural firms in favor of women entrepreneurs). A limitation of our analysis is that we cannot test which view dominates due to the insufficient granularity of the data. Access to better data (e.g., a full list of beneficiaries crossed with a credit registry) will greatly help researchers to better understand who benefits and who bears the costs of these programs. Furthermore, our analysis is limited to Africa. The impact of these programs is certainly contextual.

Future programs should be aware of the risk of possible spillovers within a bank, given that resources are fungible but human resources are limited. DFIs should monitor the activities of the bank as a whole, rather than focusing only on targeted borrowers.

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Online Appendix

Appendix A. The weight of DFI flows in Africa

The objective of this Appendix is to quantify the volume of development finance institution (DFI) investment in Africa and to compare it with other major cross-border financial flows in Africa, in particular official development assistance (ODA) and foreign direct investment (FDI). DFI investment represents a hybrid of these two flows. It is important to note that this exercise is complex, and the figures presented are approximations that provide a broad perspective on the importance of DFIs in Africa. First, we explain the methodology used to compare DFI investments with ODA and FDI in Africa, detailing the data mobilized and the filters applied. We then discuss the main results.

A1. Methodology

The aim is to compare DFI investment, ODA and FDI in Africa (including North Africa). A major challenge in this comparison is the measurement of DFI investment flows. Currently, there are no existing estimates for these flows, and information in a standardized format is not readily available.

To address this challenge, we use the Credit Reporting System (CRS) data provided by the OECD[1]. CRS data have been compiled to facilitate the automatic exchange of information. The CRS database reports flows from DAC donors to developing countries. CRS data are provided at the project level and identify the originating agency responsible for the flow. However, CRS data are not without their shortcomings. First, the quality of project registration varies widely among donors and agencies. Second, there is a potential for double counting, particularly because the data are designed to identify bilateral flows. Conversely, some projects may be missing or there may be a lack of information on the donor agency.

CRS data serve as a means to identify projects financed by development finance institutions (DFIs). After extracting CRS project-level data for the period 2006-2021, we first limit our analysis to the DFIs listed in Table 1. It is worth noting that three bilateral DFIs are missing from the CRS: FMO, IFU, and BMI-SBI. This omission poses a particular challenge, especially for FMO, which is the largest European DFI (see Table 1).

We then identify the relevant financial flows for each DFI. All flows from legally independent DFIs (bilateral DFIs and IFC) are treated as DFI flows. We need to apply

[1] CRS data are available here

filtering rules for other multilateral DFIs that are part of multilateral banks (African Development Bank, European Development Bank, etc.). We apply the following rules. First, we exclude official development assistance (ODA) grants identified by the FlowCode variable in CRS. Second, we use information on the type of aid (AidType variable) to exclude budget support (AidType=A), scholarships and student costs in donor countries (AidType=E), debt relief (AidType=F), administrative costs (AidType=G), and other in-donor expenditures (AidType=H). Finally, we use the ChannelCode variable to exclude funds directed to public institutions (ChannelCode <= 13000).

Finally, we compare total DFI commitments in current US dollars with ODA and FDI. Data on ODA and FDI come from the World Development Indicators. Specifically, ODA is measured by net official development assistance received in current US$ (code DT.ODA.ODAT.CD in the WDI), and FDI is measured by net FDI inflows in current US$ (code BM.KLT.DINV.CD.WD in the WDI).

A2. Results

We begin by providing an overview of the information coverage of DFI investments across institutions, noting two key considerations.

First, our data exploration tends to be conservative due to the lack of financial information for certain DFIs and years. In particular, data for FMO, IFU, and BMI are not available for the entire period. In addition, we lack data for the UK DFIs (BII, formerly CDC) after 2015. This is particularly damaging as FMO and BII are major players in Africa.

Second, caution is needed in interpreting the evolution over time, given the rather limited coverage in the early years. The observed positive trend may reflect both an actual increase in DFI activity and improvements in registration procedures. It is important to note that large multilateral DFIs such as the IFC or the EIB did not report financial flows before 2011, contributing to the limited coverage in the early years.

Table A2 shows the values of DFI commitments, ODA, and FDI in current millions of US dollars, along with the ratios of DFI to ODA and DFI to FDI. Both the absolute (Panel A) and the relative (Panel B) importance of DFI investment flows show an increasing trend. However, as noted above, this trend is ambiguous as it combines a potential real increase in flows with improved reporting practices. Another notable observation is the year-to-year instability of flows, not only for DFI investment but also for FDI.

To assess the significance of DFI flows in Africa, we focus our analysis on the last five years, during which registration has improved significantly. Moreover, the use of funds helps to mitigate short-term fluctuations. Since 2017, DFI investments account for 17%

Table A1: Coverage of CRS data

Bilateral DFIs			Multilateral DFIs		
DFIs	Begin	End	Multilateral DFIs	Begin	End
US DFC	2006	2021	EIB	2011	2021
FMO	NA	NA	ADB	2006	2021
BII	2006	2014	IDB	2010	2021
PROPARCO	2006	2021	IFC	2012	2021
DEG-KfW	2006	2021	ERBD	2009	2021
NORFUND	2006	2021	AfDB	2009	2021
OeEB	2008	2021	IsDB	2006	2021
BIO	2013	2020			
FINNFUND	2006	2021			
SWEDFUND	2015	2021			
IFU	NA	NA			
SIFEM	2018	2021			
SIMEST	2016	2021			
FINDEV	2018	2021			
COFIDES	2018	2019			
SOFID	2010	2019			
CDP-DF	2020	2021			
BMI-SBI	NA	NA			

Table A2: Comparison of DFIs flows with ODA and FDI

	Amount in million USD			Ratio	
Year	DFI	FDI	ODA	DFI/FDI	DFI/ODA
2005	283	29587	33369	1.0	0.8
2006	216	36194	41660	0.6	0.5
2007	978	51643	36350	1.9	2.7
2008	1013	60340	40272	1.7	2.5
2009	1551	51066	42390	3.0	3.7
2010	1219	45891	42798	2.7	2.8
2011	2885	47161	46486	6.1	6.2
2012	3944	55778	46446	7.1	8.5
2013	5054	51983	52031	9.7	9.7
2014	7038	55097	47867	12.8	14.7
2015	3688	55172	45094	6.7	8.2
2016	5292	43278	44559	12.2	11.9
2017	6188	40066	47818	15.4	12.9
2018	4941	43393	48848	11.4	10.1
2019	10356	40419	50678	25.6	20.4
2020	7970	32775	64864	24.3	12.3
2021	7701	79107	65012	9.7	11.8
Average (2017-2021)				17.3	13.5

of foreign direct investment and 14% of official development assistance. As highlighted earlier, these figures are likely to be lower estimates due to the lack of information on two prominent DFIs operating in Africa (FMO and BII). A back-of-the-envelope estimate suggests that including investment flows from both DFIs would raise the ratios to 21% and 16%, respectively.[2]

The figures presented in the Appendix indicate that DFI investments account for about 15% to 20% of both official development assistance (ODA) and foreign direct investment (FDI) in Africa. Notably, this observation is consistent with the findings of a previous study conducted by Massa et al. (2016), which, although using a different approach, reported very similar figures. While these percentages should be considered as approximations, they underscore that DFIs play a significant role as foreign investors in Africa and cannot be considered as minor contributors to the region's economic landscape.

[2]This calculation is based on two assumptions: first, that the evolution of flows from BII follows the same trend as observed in the period 2011-2014, and second, that FMO has flows similar to BII.

Appendix B. Construction of the DFI database

Appendix B describes the procedures for obtaining the final database of DFI investments from the initial survey to the final database.

B1. Data collection

The scope of the database focuses on the operations of DFIs in Africa and is oriented towards the financial sector. First, we need to identify relevant DFIs. To this end, we have cross-checked the lists provided by the OECD (here) and European DFIs (here). Two DFIs have changed their names: US DFC (formerly OPIC) and BII (formerly CDC). The final list includes 17 bilateral DFIs and 7 multilateral DFIs. Table 1 in the main text provides basic information on the DFIs.

From the list of 25 DFIs, 17 DFIs are included in the final data. Four bilateral DFIs (COFIDES, FinDev, CDP-DF or SOFID) are excluded because we are unable to extract granular information on their projects. Two other bilateral DFIs (BMI-SBI and SIMEST) are excluded because they exclusively finance firms from their country of origin investing abroad. Finally, we exclude two regional DFIs because they do not operate in Africa (Asian Development Bank and Inter-American Development Bank). The final list of DFIs therefore includes 12 national DFIs (BII, BIO, DEG-KFW, DFC, FINNFUND, FMO, IFU, NORFUND, OeEB, Proparco, SIFEM, SWEDFUND) and five multilateral DFIs (AfDB, EIB, ERDB, IFC, ISDB). Table 1 documents that our analysis covers large DFIs and excluded DFIs are the smallest ones.

For each selected DFI, we collected information on all individual projects that met the two selection criteria: "sector = finance" AND "continent = Africa" (including North Africa). Data were web-scraped for 6 DFIs (AfDB, BIO, FMO, Norfund, Proparco, and SIFEM), hand-collected for 10 DFIs, and collected from the Common Reporting Standard (CRS) for one DFI (Islamic Development Bank).[3] We extracted data in the second half of 2022. For each project we collect the following information

- the exact name of the recipient;
- the exact year;
- the type of instruments (loans, guarantees, equity, technical assistance, etc.);
- the total amount of the assistance.

[3] CRS was also used to add projects for Norfund.

B2. Data cleaning

Applying our inclusion criteria to the list of 17 DFIs, we collected 1,740 projects. We then cleaned the data to obtain a usable database. We first withdrew certain projects and then applied some filters.

B2.1. Re-treatment

Rework is required because some assets are assigned to multiple clients. Two situations may arise: (i) a project is allocated to several Financial Intermediaries (FIs) operating in the same country; (ii) a project is allocated to different branches (in different countries) of the same FIs. In the first case, we retained only projects that clearly specified the list of eligible FIs (e.g., "the loan is allocated to Bank A and Bank B"), but excluded projects with an unclear description (e.g., "all local banks are eligible"). For the latter situation, we distinguished between projects dedicated to the holding company or the entire group and those dedicated to specific subsidiaries belonging to the same group (e.g., "the loan is dedicated to Bank A operating in Kenya, Uganda and Tanzania"). We have created a special classification for investments allocated to holding companies.

After identifying these specific cases, we retreated the database to add one investment per entity. For example, if a project is allocated to 3 banks in country A, we considered there to be 3 different projects (we coded them by indicating that the three banks benefited from the same financing).

In order to decompose the total amount, we used the distribution information, if available. However, for the majority of projects we did not have this information. In the absence of a distribution rule, we divided the total amount by the number of recipients.

We also had to reprocess data for BII (formerly CDC) due to the peculiarities of the UK DFI. BII often channels funds through an investment fund, but provides the name of the final beneficiary. We therefore identified, where possible, the name of the ultimate client(s).

B2.2. Filter rules

We then applied filtering rules to obtain a usable database. First, we dropped investments allocated to a fund because we are unable to track the final beneficiaries and locate the recipients (many funds are registered in non-African countries and it is a rather complex task to identify their final beneficiaries). We therefore exclude 377 projects dedicated to funds.

Second, we excluded projects where basic information (such as the name of the beneficiary or the year) was not available.

Finally, we checked whether the financed company was a financial company. We excluded projects that did not meet this criterion.

B3. Data harmonization

The final task is to harmonize the data. All financial values are converted into current USD. It should be noted that we were unable to convert the project amount into USD for older projects provided by AfDB (AfDB uses its own currency and the exchange rate with USD is not available before 2003).

We have harmonized the type of financing using the following categories: Equity, Loan, Guarantee, Grant, Technical Assistance.[4]

We then harmonized the name of each recipient, as DFIs may use different names for the same client. We also tracked whether an institution's name changed over time (e.g., Microcred became Baobab). For each individual institution, we assigned a unique identifier that allowed us to track the same institution despite a name change. We also extracted additional information about each recipient. We classified each recipient into one of four categories of financial institutions: Bank, MFI, Other FIs (such as insurance, leasing companies, FinTech, etc.), and Holding. We then identified the nationality of the recipient and whether it belonged to a group (and if so, the name of the group and its nationality).

Finally, we identify syndicated investments, defined as those financed by several DFIs. A financial institution may be financed by several DFIs for the same project (syndicated loans, joint guarantee, each DFI buying a share of the capital, etc.). We were not able to manually extract whether an investment was single or syndicated. We therefore developed a procedure to identify potential syndicated investments using three criteria: client name, year and product type. An investment is considered syndicated if two (or more) DFIs invest in the same client, in the same year, using the same type of product. Using this procedure, we identify 1,063 non-syndicated projects and 86 (potential) syndicated projects (accounting for 198 investments in the database).[5]

The final database contains 1,261 projects. In the remainder of the analysis, we use only investments made from 2010 to 2021, leaving us with 900 projects (see Table 2 in the manuscript). In fact, the time coverage differs across DFIs (Table 2) due to differences in available public information. We are often able to collect data for the last ten years. In terms of the first year available, a concern is DEG-KfW and Finnfund, for which we

[4]We also created a final category "intermediated investment" for BII, which uses a specific channel.

[5]Among the syndicated projects, 67 are provided by two DFIs, 14 by 3 DFIs, 3 by 4 DFIs, and 2 by 5 DFIs. Syndicated projects account for 20% of the cumulative amount and are mainly loans (79% of all syndicated projects are syndicated loans) and banks (82% of all syndicated projects).

could not go further than 2017. Proparco, another large player, does not publish projects before 2013. For other DFIs, however, we are able to collect data up to 2010. In terms of final data, we are often able to collect data up to 2021. A notable exception is the African Development Bank (AfDB), for which the most recent year available is 2018.

B4. Comparison with CRS data

An alternative of the current database is the Common Reporting Standard (CRS) database, described in Appendix A. Like our database, CRS provide project-level information and we are able to identify the donor agency (see Appendix A). We therefore compare DFIs investment database with CRS data. To do so, we downloaded CRS data and restricted the analysis to the same perimeter than in our database: flows to financial sector in Africa from 2010 to 2021.

Table B1 shows the main results of the comparison. The first panel reports information from our database and panel B focuses on the CRS data. We are able to identify almost the same number of flows (900 vs. 886), but they are not identical. The CRS data provide flows from 14 of the 17 DFIs included in our analysis, but provide flows from FinDev that are not included in our database.

The number of flows per DFI sometimes differs between our data and the CRS data. For example, we identified 59 projects financed by the EBRD, while the CRS data shows 146 projects. By examining the CRS data in detail, we are able to understand the main explanations. First, CRS data include flows to all financial intermediaries, including investment funds. In our data, we excluded investment funds from the analysis. Second, the way the data are reported can vary widely. For example, BIO made a loan to ETI, which is the holding company of the Ecobank group. In our database, we consider only one observation and code it as "holding". In the CRS, the same investment is split into more than 30 projects. The CRS data report one row for ETI, but also one row for each subsidiary of Ecobank. This choice is dictated by the objective of the CRS data (to track flows from one country to another). Finally, we identified a non-negligible number of double counts in the CRS data. Conversely, the DFI project database may have identified more flows than the CRS data, as in the case of Proparco and FMO. One possible explanation is that data reported in the CRS are not always attributed to DFIs, but may pass through other institutions (e.g., development banks or ministries).

Next, we examined whether final beneficiaries were reported in the CRS data. To do this, we read (short and long) descriptions of all projects in the CRS data. A project is identified if the final recipient is explicitly mentioned.[6] Using these criteria, we are

[6] For investments transiting through an investment fund, we identified the final recipient only if we know the name of the fund AND the amount received by the final recipient.

able to extract information on final beneficiaries for only 140 projects (out of 886 initially identified).

Finally, we assess the validity of our database by focusing on 140 projects with complete information in the CRS data. We verify that our database includes these investments. The last two columns of Table B1 present the main results. The data considered in this paper includes more than 80% of the projects (117 out of 140) with full information provided in the CRS.

Table B1: Comparison with CRS data

DFI	Panel A: Our database			Panel B: CRS data			Comparison	
(Acro)	First	Last	Nb.	First	Last	Nb.	CRS	Our data
AfDB	2010	2018	78	2010	2021	69	29	20
BII	2010	2020	84	2010	2018	75	0	-
BIO*	2010	2021	45	2018	2020	84	9	7
DEG	2017	2021	20	2010	2021	52	0	-
DFC	2011	2021	97	2018	2021	74	0	-
EBRD	2012	2021	59	2012	2021	146	11	9
EIB	2010	2021	63	2013	2021	49	16	10
FINNFUND	2017	2021	8	2012	2020	10	0	-
IFC	2010	2021	192	2012	2021	233	7	4
IslDB	2010	2021	20	2010	2021	25	20	20
NORFUND	2017	2021	31	2010	2021	35	29	29
Proparco	2011	2021	117	2018	2021	30	17	16
SIFEM	2019	2019	1	2019	2019	1	1	1
SWEDFUND	2012	2020	8	2018	2020	2	1	1
FMO	2012	2021	73					
IFU	2018	2020	2					
OeEB	2014	2021	2					
FINDEV				2020	2020	1	0	
TOTAL			900			886	140	117

Panel A shows the first and last year as well as the number of projects for each DFI reported in our database (from 2010 to 2021), Panel B shows the same variables for CRS data. The last two columns compare data from our database with CRS data. The penultimate column reports the number of projects in the CRS data with complete information on final recipients. The last column shows the number of projects among those reported in the previous column that are also available in our database.

Appendix C. Descriptive statistics

Table C1: Distribution of cohorts

Cohort	# banks	# obs.
Never	796	7,291
Treated before 2010	57	869
Treated from 2010 to 2021	156	2216
Treated in 2010	*14*	*202*
Treated in 2012	*11*	145
Treated in 2013	*14*	207
Treated in 2014	*25*	350
Treated in 2015	*21*	328
Treated in 2016	*18*	239
Treated in 2017	*9*	120
Treated in 2018	*13*	197
Treated in 2019	*7*	91
Treated in 2020	*5*	71
Treated in 2021	6	102
Treated in 2011	*13*	164

The table reports the number of banks per cohort.

Table C2: Top 10 of recipients

Panel A) financial intermediaries with the large number of projects						
Rank	Name	Country	Type	Inv.	Sum total	Average
1	ACCESS BANK	Nigeria	Bank	20	639	32
2	ETI (ECOBANK)	Togo	Holding	14	668	48
3*	NATIONAL BANK OF EGYPT	Egypt	Bank	12	3157	263
- *	EQUITY BANK	Kenya	Bank	12	888	74
-	ENDA TAMWELL	Tunisie	MFI	12	125	10
-	ADVANS COTE IVOIRE	Côte d'Ivoire	MFI	12	14	1
7	KCB BANK	Kenya	Bank	11	617	56
-	FIRST CITY MONUMENT BANK	Nigeria	Bank	11	316	29
9*	FIRST RAND BANK	South Africa	Bank	9	728	81
10*	BANQUE CENTRALE POPULAIRE	Morocco	Bank	8	702	88
-	QNB EGYPT	Egypt	Bank	8	589	74

Panel B) financial intermediaries with the largest cumulative amount of investment received						
Rank	Name	Country	Type	Inv.	Sum total	Average
1*	NATIONAL BANK OF EGYPT	Egypt	Bank	12	3157	263
2	BANK MISR	Egypt	Bank	6	2293	382
3*	EQUITY BANK	Kenya	Bank	12	888	74
4	ZENITH BANK	Nigeria	Bank	7	804	115
5	COMMERCIAL INTERNATIONAL BANK	Egypt	Bank	6	800	133
6	UNION BANK OF NIGERIA	Nigeria	Bank	4	755	189
7	ABSA HOLDING	South Africa	Holding	3	746	249
8*	FIRST RAND BANK	South Africa	Bank	9	728	81
9	STANDARD BANK OF SA	South Africa	Bank	4	710	178
10*	BANQUE CENTRALE POPULAIRE	Morocco	Bank	8	702	88

The table reports the top 10 of recipients according to the number of projects received (panel A) or the cumulative amount (panel B). A star signals recipients that are in the top 10 for both measures.

Table C3: Cumulative investment by financial group

Group name	Type	Nationality	# projects	# projects FIs	# country	Amount (cum)
ECOBANK	Bank	Togo	43	14	14	949
SOCIETE GENERALE	Bank	France	31	14	13	513
ADVANS	MFI	Luxembourg	26	8	6	81
ACCESS BANK GROUP	Bank	Nigeria	25	4	4	703
FIRSTRAND GROUP	Bank	South Africa	24	8	6	1541
BOA Group	Bank	Morocco	24	13	12	1278
BANQUE ATLANTIQUE (BCP)	Bank	Morocco	18	8	5	969
TUNISIE LEASING FACTORING	Other FI	Tunisia	18	8	6	144
STANDARD BANK GROUP	Bank	South Africa	15	8	6	1402
EQUITY GROUP	Bank	Kenya	14	2	1	1151
KCB GROUP	Bank	Kenya	14	3	3	634
IM	Bank	Kenya	14	6	5	315
BAOBAB	MFI	France	13	8	7	41
ORAGROUP	Bank	Togo	12	6	5	122
COFINA	MFI	Côte d'Ivoire	12	4	3	66
ACCESS MICROFINANCE	MFI	Germany	12	5	5	23

The table reports the total investments per financial group made by DFIs. The table only presents groups with more than 10 investments received.

Table C4: Mean differences between treated and untreated banks, unmatched sample

	Treated			Untreated			Difference					
	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean		SMD		Distribution	
Outcome variables												
Loan Growth	3.270	31.96	2095	42.23	1424.3	6359	-38.96	**	-0.039		0.078	***
Return on Assets	1.564	3.656	2052	1.543	7.345	6510	0.021		0.004		0.082	***
NPLs	0.001	0.004	1537	0.002	0.008	3736	0.000	***	-0.066		0.086	***
Provisions	1.767	3.330	1860	2.521	13.02	5335	-0.754	***	-0.077		0.081	***
Matching variables												
Size (absolute)	3 841	12 259	2216	2 342	11 143	7291	1 499	***	0.128	***	0.176	***
Size (relative)	7.056	10.14	2216	7.832	14.93	7291	-0.776	***	-0.061		0.139	***
Deposit to assets	74.85	16.00	2127	68.85	22.19	6814	5.994	***	0.310	***	0.128	***
Loan-deposit ratio	77.24	72.44	2120	288.2	5155.3	6638	-211.0	***	-0.058	***	0.174	***
Equity to assets	14.96	12.51	2216	18.25	18.94	7290	-3.285	***	-0.204	**	0.130	***
Group dummy	0.424	0.498	2216	0.379	0.485	7291	0.045	***	0.152		0.075	***
GDP PER CAPITA	2 484	2 287	2001	2 950	2 767	6322	-466.8	***	-0.184	***	0.096	***
PRIVATE CREDIT / GDP	33.48	29.82	1706	33.30	34.61	5576	0.178		0.006		0.102	***

The table reports mean, standard deviation and the number of observations for all outcome and matching variables for both treated banks and untreated banks. The last columns report difference in mean between the two group of banks (using Welsh method), the standardized mean difference and associated t-statistics using formula proposed by Imbens (2015) and the D-statistic of equality in distribution from a Kolmogorov-Smirnov test. *, ** and *** signal statistical significativity at 10, 5, and 1% respectively.

Figure C1: Distribution of total amount, by type

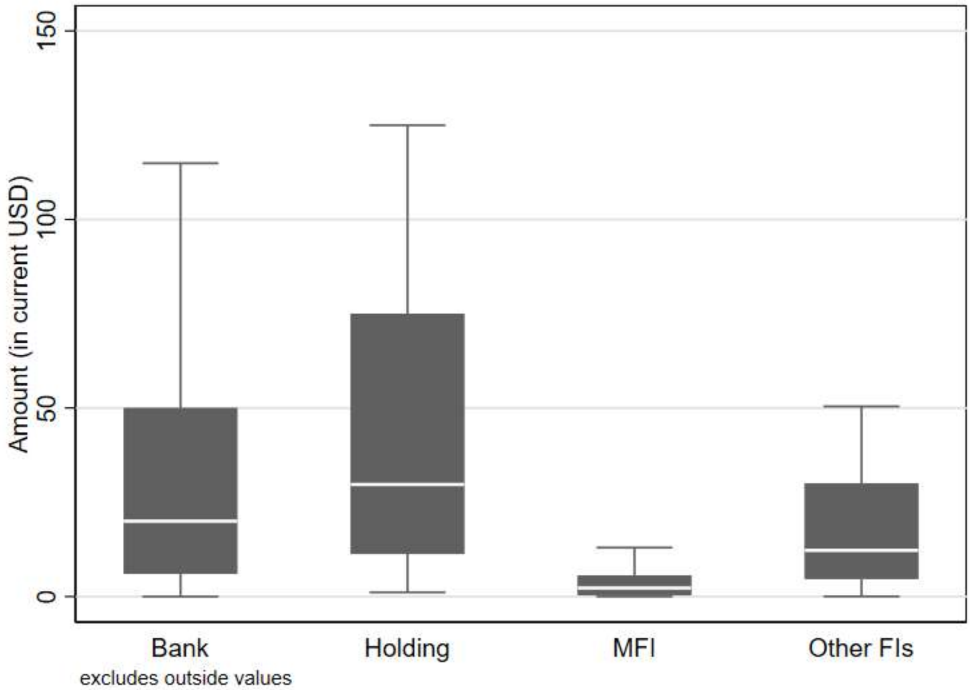
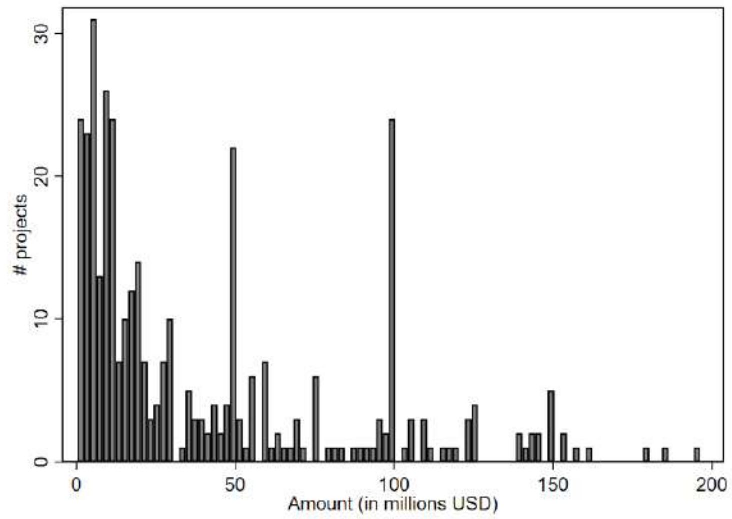


Figure C2: Distribution of projects for multilateral and bilateral DFIs

Panel a) Multilateral DFIs



Panel b) Bilateral DFIs

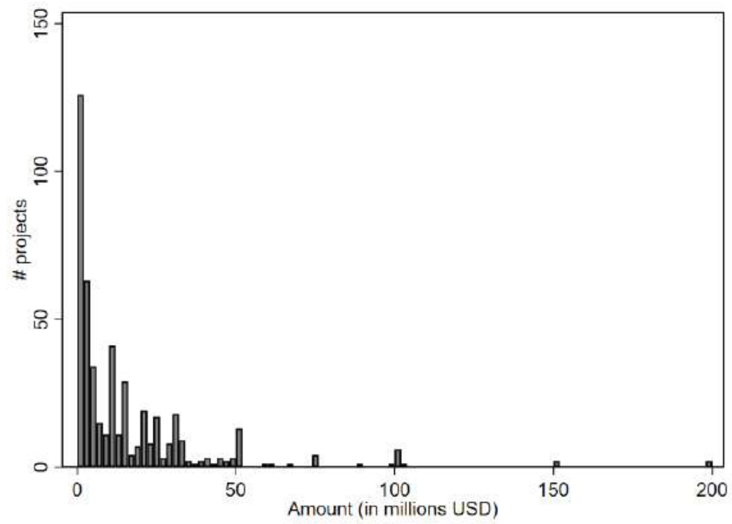
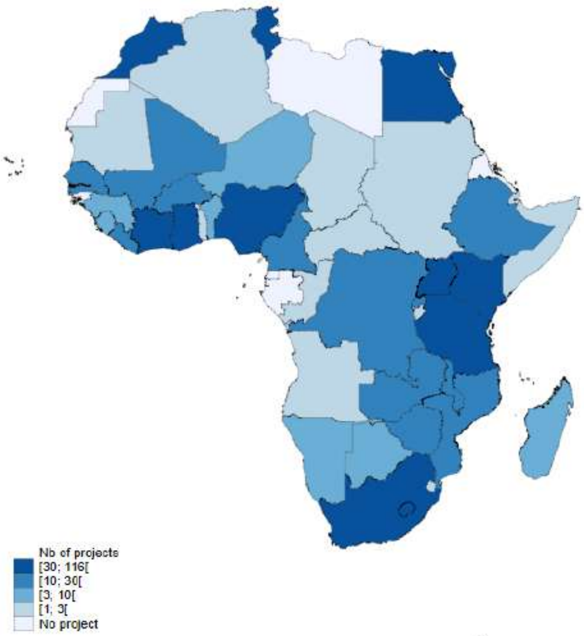
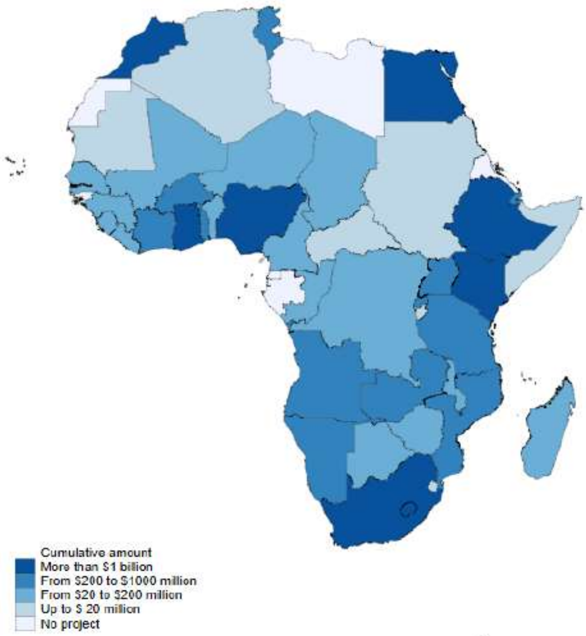


Figure C3: Geographical allocation of projects

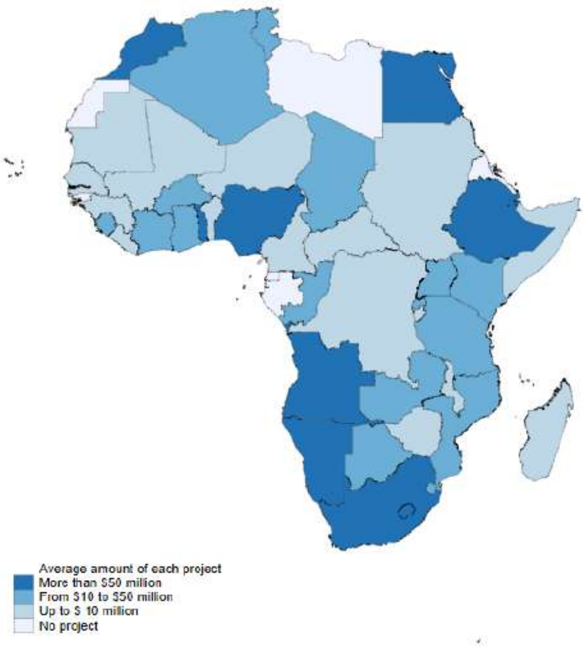
Panel (a): Number of projects



Panel (b): Cumulative amount



Panel (c): Average amount



Panel (d): Share of projects allocated to MFIs

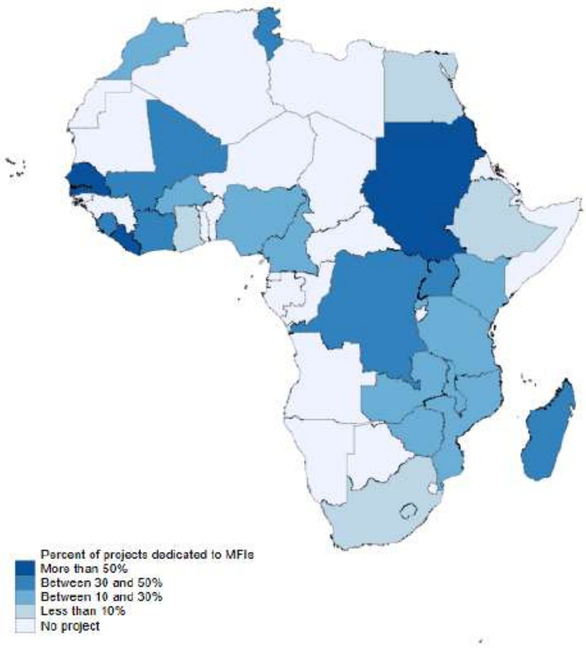
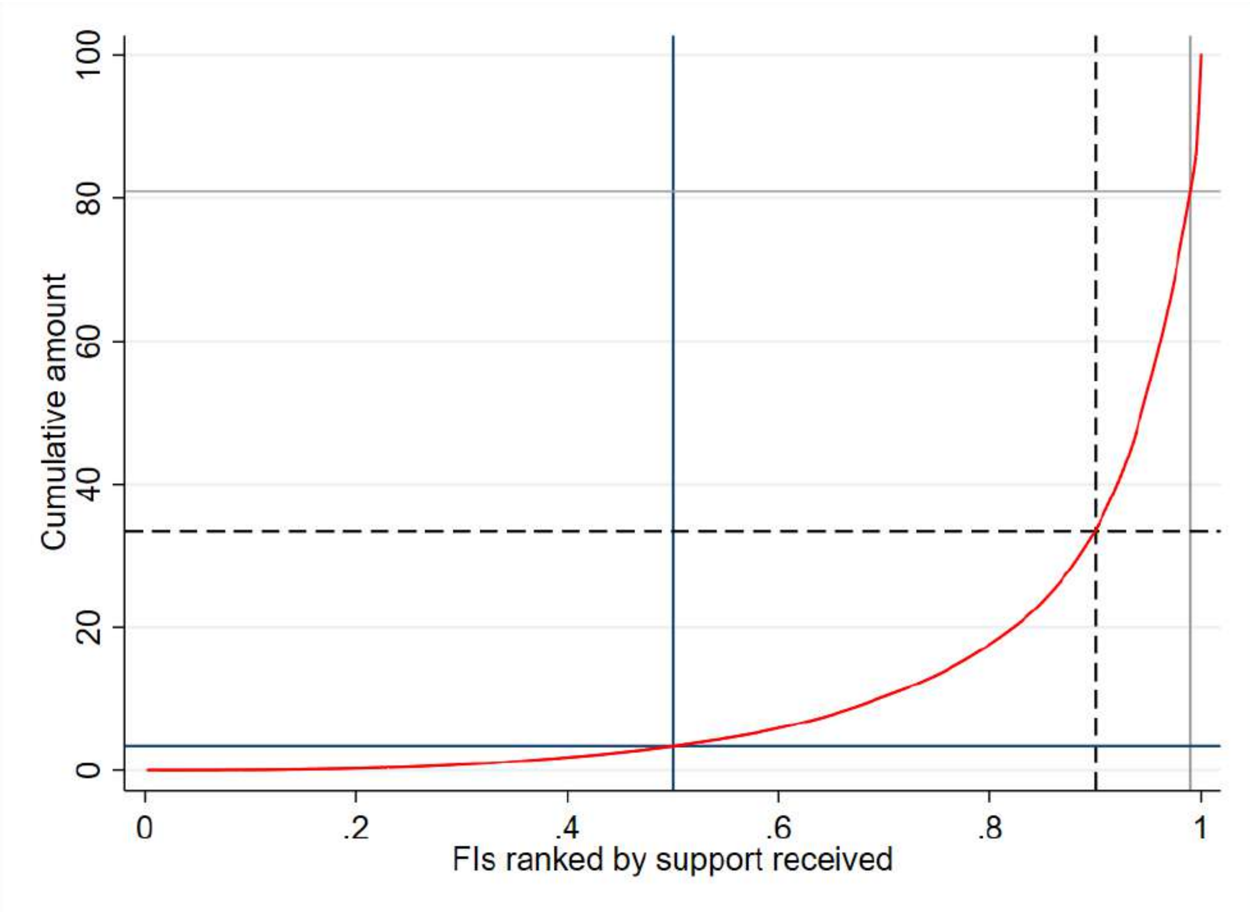


Figure C4: Lorenz curve of the distribution of total amount by financial intermediaires



Appendix D. Description of the matching procedure

The Appendix describes the specific procedures used to match control and treated banks. The matching procedures are described in detail before the main results of the matching procedures are discussed.

D1. Matching procedures

D1.1. Definition of the pre-treatment period

In a context with different treatment periods, it is important to clearly define the pre-treatment period. In our paper, we follow the approach of defining the pre-treatment period as the period just before the implementation of the treatment. This approach ensures that the matching is based on relevant pre-treatment characteristics. As a result, for a bank treated in year t, the pre-treatment period is defined by the previous year $(t-1)$. An alternative would be to match banks based on their characteristics in the initial period (t_0). However, this approach needs to be treated with caution, as the initial characteristics may not be relevant for late treated banks, and some banks would be excluded due to lack of information in the initial period.

D1.2. Definition of the pool of control units

In the present study, the identification of the pool of potential controls in the context of a staggered difference-in-differences (DID) model in economics involves considering all banks that have not yet received treatment by period t. To avoid the forbidden control group problem (De Chaisematin and d'Haultfoeille, 2023), we exclude already treated banks from the pool of possible control units.

We impose a second restriction to identify the pool of possible matched units. We exclude banks treated between t and $t+3$. The inclusion of future treated as control groups is subject to a trade-off. On the one hand, the late treated are more likely to be similar to the early treated, especially when the treatment time is exogenous (Deshpande and Li, 2019). On the other hand, since we want to track differences between treated and control units over time (from t to $t+5$), we need matched banks that are not treated from t to $t+5$. By dropping from the pool of possible matched units banks treated in the three years after the initial treatment, we are sure to observe untreated banks at least until $t+3$ and also allow late treated banks to serve as a control.

Table D1 shows for each cohort the total number of banks, the number of treated banks and the number of possible matched banks. We can see that the pool of possible untreated banks largely exceeds the group of treated banks even after applying the filtering rules (the minimum ratio is 21 to 1 for the 2014 cohort and the maximum 74 to 1 for the 2020 cohort). As expected, the number of possible controls decreases over time (as some possible controls are now treated).

D1.3. Matching techniques

There are three main matching methods in the literature: perfect matching (including coarsened exact matching), distance matching and propensity score matching. This paper uses a distance matching approach. The commonly used propensity score matching (PSM) is not used for two reasons. First, PSM is rather inefficient when the number of treated is rare or very frequent. The empirical models used in PSM (logit or probit) are not designed to handle large proportions of zero or one. In our analysis, less than 3% of the banks in each cohort are treated, which limits the usefulness of PSM. Second, PSM is not well designed for staggered treatment. It is unclear whether the propensity model should be computed for each cohort or for the pooled sample. Therefore, perfect matching and distance matching are most appropriate in our context. These approaches allow the selection of the closest unit(s) based on matching variables by considering the treatments one at a time. Perfect matching is particularly well designed for studies that combine both a handful of well-identified matching variables and a large pool of control units (an excellent example is given by Britto et al., 2022). We do not meet these criteria. As a result, we rely on a distance matching model that allows us to include more matching variables and have a limited pool of control units. From a technical point of view, we use the Mahanobilis distance, which is not sensitive to the scale of the matching variables (unlike the Euclidean distance).

A final decision about the matching technique concerns the matching algorithm, i.e., deciding who are the matched controls. There is a large literature, mainly focused on propensity score matching, discussing this point (Caliendo and Kopeinig, 2008). The main discussions are about (i) the number of matches per treated unit (only one match unit or multiple matches); (ii) the fact to allow replacement or not; and, (iii) the possibility to exclude matches if the distance or score is above a threshold (caliper). In this application, we made the following choice. We consider (i) the five closest matching units for each treated bank, (ii) we allow replacement (i.e., a control unit can be matched with several treated units), and (iii) we do not consider a caliper (maximum distance above which we exclude the matched unit).

First, we consider five closest matched units because we have a limited number of treated units. The risk of using only the closest match is that the results are driven by some control units. In addition, matched banks may disappear from the analysis in the future for various reasons (treated in the future, lack of information). By allowing five matches, we expect to maintain sufficient observations for control banks from t to $t+5$.

Second, we allow substitution, which implies that a control unit can be matched with several treated units. The choice is mainly driven by the fact that a control unit can be matched with a treated unit from different cohorts. For example, a control bank A can be matched with a treated bank B in 2012 and another bank C in 2016 because both B and C have similar characteristics (bank C can also be a control bank for bank B from 2012 to 2015). If we do not allow substitution, we face the question of how to treat Bank A in our analysis. Should we exclude this bank for future treatment? If so, the sample of possible control units for the late treated will be greatly reduced. Therefore, there is a risk of not finding relevant matched control units. Therefore, we prefer to allow replacement to avoid this problem. Our empirical model described in the manuscript allows us to control for the fact that the same bank is considered multiple times by adding bank group fixed effects.

Third, we do not consider a caliper in the baseline model. Unlike PSM, there is no reference to identify a caliper for distance. One approach is to use the distribution of distance and exclude the extreme 5 or 10% in terms of distance. We use such an approach in robustness checks.

D1.4. Matching variables

The choice of matching variables is crucial for the validity of the matching procedures. As the number of variables increases, so does the degree of similarity between treated and matched control units. However, there is a risk of excluding some possible untreated cases due to missing variables. Therefore, we restrict our choice to a limited number of matching variables. Another question is whether to include lagged dependent variable(s) in the set of matching variables. On the one hand, including lagged outcomes may help to reduce differences between treated and untreated. On the other hand, if the treatment is endogenous, there is a risk that using the outcome variable in the set of covariates will reduce the matching.[7]

In the absence of previous studies on DFI support at the firm level, the selection is driven by our own knowledge of DFIs and discussions with some DFI staff. For the matching procedure, we consider both bank- and country-level variables. At the bank level, we control for bank size by using both absolute size (in constant USD) and relative size by relying on the bank's national market share. It is well known that DFIs tend to focus on large and well-established banks in each country. We also consider the bank's

[7] In our analysis, suppose that a DFI provides a credit line to a bank that is experiencing temporary difficulties (lower growth). By using lagged growth, we will compare the treated bank facing a temporary difficulty with a possible control bank facing a chronic situation of low growth. Mechanically, we will document a positive effect of the treatment after the treated bank returns to normal.

Table D1: Sample of all banks, treated banks and possible matched banks, per cohort

Cohort	Full sample	Eligible to matching		Matched	
		Treated	Control	Treated	Control
2010	618	14	520	10	50
2011	658	13	531	7	35
2012	690	11	545	9	45
2013	687	14	523	11	55
2014	701	25	532	23	115
2015	690	21	509	17	85
2016	673	18	486	12	60
2017	667	9	479	6	30
2018	624	13	433	11	55
2019	606	7	418	6	30
2020	552	5	372	4	20
2021	349	6	222	5	25
SUM	7,515	156	5,570	121	605

business model by examining its funding structure and intermediation activity. Funding structure is proxied by the ratio of deposits to total funding and the ratio of equity to assets. These variables allow us to control for the needs of banks. DFIs offer the opportunity to provide long-term loans, as opposed to deposits. Bank activity is measured by the transformation ratio, defined as the ratio of loans to deposits. We expect treated banks to be more active than their counterparts. We also control for banks belonging to an international banking group. The descriptive statistics show that DFIs support banks that belong to an international group. At the country level, we include a measure of economic development (GDP per capita) and financial development (using the ratio of private credit to GDP). Both variables are proxies for the need for external finance dedicated to the private sector (Dreher et al., 2019).

D2. Results of the matching and validation

D2.1. Sample

Table D1 shows, per cohort, the number of banks, the sample of banks eligible for matching (see above for details), and the final outcome of the matching procedures. We see that out of 156 treated banks, we are able to match 121 banks with control banks.

We then consider the full sample to examine how many control banks are matched with treated banks (the same banks can be matched with several treated banks, as we allow for replacement). The initial sample considers 952 banks, including 156 treated banks and 796 control banks. Table D2 shows the number of banks (treated and untreated)

Table D2: Number of banks according to their matching status

Status	Treated	Untreated
TOTAL	156	796
No match	34	588
Match (total)	122	208
- Only one match	*101*	*83*
- Multiple matches	*21*	*125*

that have been matched. We are able to match 122 treated banks and 34 unmatched banks. Note that this number differs from the previous table (121) because one treated bank was matched only as a control bank. We also note that another 21 treated banks are matched more than once. This is due to the fact that some late treated units are control units for early treated banks. A more detailed analysis (available upon request) shows that 11 treated banks served as a control bank for another (late) treated bank once, 5 twice, and 6 three or more times.

Of the 796 untreated banks, only a quarter (208 out of 796) are matched with a treated bank. Of these, 40% are matched with a treated bank only once (83 out of 208), and the rest of the matched untreated banks serve as controls for more than one treated bank. Specifically, 45 banks are matched with two treated banks, 29 banks with three, 18 with four, 8 with five, and the rest (25) with more than five treated banks.

D2.2. Validation

Finally, we examine the outcome of matching by comparing differences between treated and control units before and after matching in table D3. As expected, the matching procedures reduce differences between treated and control units for matching variables. Interestingly, we also see a reduction in divergence for other variables, including loan growth, performance (ROA, ROE, NIM), and portfolio quality (NPLs, provisions to loans). For all variables, the means are not statistically significant after matching, except for the loan-to-deposit ratio (while there are differences for 11 variables before matching). Meanwhile, while there are all variables for which the distribution between treated and control is the same, we see a sharp reduction in the divergence with only four variables with a difference. In addition, the degree of statistical difference has been greatly reduced.

In short, the matching procedure has reduced differences between treated and untreated banks not only for the variables used for matching, but also for the majority of outcomes (even if these outcomes are not included in the matching procedure). Thus, we are fairly confident that the matching process allows us to combine banks that are fairly similar.

References (not cited in the manuscript)

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Table D3: Comparison of variable means and distribution before and after matching

	Before matching								After matching							
	Mean						Dist.		Mean						Dist.	
	Untreated	Treated	Welsh t-test		SMD		(Kol.-Smir.)		Untreated	Treated	Welsh t-test		SMD		(Kol.-Smir.)	
Outcome variables																
Loan Growth	28.61	3.946	-24.66	*	-0.033		0.131	***	0.226	1.153	0.927		0.132		0.151	**
Return on Assets	1.425	1.501	0.076		0.016		0.103	***	1.753	1.436	-0.317		-0.095		0.085	
Return on Equity	8.165	12.91	4.747	**	0.050		0.150	***	13.66	11.85	-1.806		-0.091		0.094	
Net interest margin	0.043	0.019	-0.024	**	-0.049		0.144	***	0.032	0.026	0.010		0.151		0.0132	
NPLs	0.001	0.001	-0.0003		-0.058		0.131	***	0.0011	0.0008	-0.0002		-0.183		0.233	***
Provisions	2.391	1.626	-0.765	***	-0.097		0.097	***	1.810	1.643	-0.167		-0.067		0.156	**
Matching variables																
Size (absolute)	2 358	3 879	1521	***	0.124	***	0.177	***	2 793	2 901	108		0.013		0.167	***
Size (relative)	7.290	7.144	-0.146		-0.012		0.157	***	6.911	6.770	-0.141		-0.013		0.068	
Deposit to assets	69.44	76.15	6.713	***	0.361	***	0.156	***	74.10	75.06	0.964		0.057		0.065	
Loan-deposit ratio	295.2	69.98	-225	***	-0.058		0.160	***	88.1	78.87	-9.233	*	-0.123		0.101	
Equity to assets	18.44	14.94	-3.499	***	-0.220	***	0.174	***	15.53	14.47	-1.059		-0.103		0.119	
Group dummy	0.383	0.423	0.040	**	0.081		0.040		0.441	0.471	0.030		0.060		0.030	
GDP PER CAPITA	2 969	2 408	-561	***	-0.231	***	0.122	***	2 437	2 318	-119		-0.061		0.101	
PRIVATE CREDIT	33.64	33.38	-0.254		-0.008		0.117	***	29.39	32.82	3.432		0.125		0.068	
Obs.	5,784	790							690	121						

The table reports the means of the untreated and treated banks before (left) and after (right) matching. The last columns report the mean difference between the two groups of banks (using the Welsh method), the standardized mean difference, and the D-statistic of equality of distribution from a Kolmogorov-Smirnov test. *, **, and *** indicate statistical significance at 10, 5, and 1%, respectively (for SMD, we compute the corresponding t-statistic using the formula proposed by Imbens (2015)).

Appendix E. Additional empirical analysis

Table E1: The impact of treatment based on financial instruments

Dep. Var. →	Loan Gr	Loan Gr	Loan Gr	Rank
Moderator (Z) →	Loan	Equity	Guarantee	(1st and more)
	(1)	(2)	(3)	(4)
Post × Treated	-3.929*	-2.805**	-2.895**	-2.788*
	(2.115)	(1.367)	(1.464)	(-1.86)
Post × Treated × Z	1.646	-2.648	-0.228	-0.511
	(2.574)	(3.299)	(2.733)	(1.943)
Observations	6,117	6,117	6,117	6,117
Fixed effects	Bank-Event	Bank-Event	Bank-Event	Bank-Event
Country-year dummies	Yes	Yes	Yes	Yes

Dependent variables is the loan growth. We report results of a triple interaction model, described in Eq. 2. We consider three moderators: a dummy equal to 1 if the treatment is a loan in column (1), if the treatment is an equity in column (2), the treatment is a guarantee in column (3). In column (4), the simple interaction reports the results for the first support. The triple interaction measures the effect for an additional support (over the five year period). We only display coefficients associated with interaction between $Post$ and $Treated$ (β_1 in Eq. 1) and with the triple interaction (β_2) in Eq. 1). Models is estimated on stacked dataset. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

Table E2: Robustness checks 1: Alternative matching

	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Nb Matches	5	1	3	7	9	5	5	5	5
Difference w/ baseline	None	None	None	None	None	w/ Y(t-1)	w/out outl.	w/out late	w/out bk group
Post=1 × Treated=1	-2.952**	-4.731**	-3.533**	-2.778**	-2.786**	-3.453**	-2.948**	-2.933**	-2.987**
	(1.315)	(2.167)	(1.592)	(1.274)	(1.228)	(1.745)	(1.329)	(1.319)	(1.320)
	6117	2,074	4,133	10,274	10,274	5,785	5,919	6,085	6,003

The table reports the results of the coefficient associated to the interaction between *Post* and *Treated* dummies in equation 1. Column (0) reports the baseline results extracted from Table 5 (column 5). Dependent variables is the loan growth. Columns (1) to (8) provide sensitivity tests based on a different matching procedures. In columns (1), (2), (3), and (4), we consider the 1, 3, 7, and 9 closest matches instead of the 5 closest matches (baseline). Column (5) considers a matching procedures with lagged dependent variables in the set of matching variables. Column (6) is the baseline matching procedures but we exclude matched banks if distance is in the top percentile. Column (7) excludes future treated as possible controls. Columns (8) excludes controls that are banks belonging to the same group of the treated. All models include country-year dummies and bank-group fixed effects. Standard errors are clustered at the bank-group level..

Table E3: Robustness checks 2: Alternative dependent and control variables

	(0)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var. →	Y	Alt Y	Y	Y	Y	Y	Y	Y
Control. Var. →	None	None	Match	ROA	ROE	NIM	NPLs	Prov.
Post = 1 × Treated = 1	-2.952**	-2.960**	-2.852**	-3.229**	-3.229**	-3.378**	-3.250**	-3.430**
	(1.315)	(1.316)	(1.320)	(1.360)	(1.360)	(1.326)	(1.504)	(1.448)
Obs.	6,117	6,117	6,117	5,990	5,990	5,939	4,417	5,608

The table reports the results of the coefficient associated to the interaction between *Post* and *Treated* dummies in equation 1. Column (0) reports the baseline results extracted from Table 5 (column 5). Dependent variables is the loan growth. Column (1) is the baseline model but dependent variable is an alternative measure of loan growth. Columns (2-7) display results when additional control variables are included: all variables used for the matching (column 2), return on assets (3), return on equity (4), net interest margin (5), NPLs to loans (6) and risk provisions to loans (7). All models include country-year dummies and bank-group fixed effects. Standard errors are clustered at the bank-group level..

Table E4: Robustness checks 3: Estimation on raw data

	(1)	(2)	(3)	(4)
Method	TWFE	TWFE	CS	CS
Post=1 × Treated=1	-4.399**	-5.492**	-4.527*	-6.054*
	(-2.04)	(-2.16)	(-1.61)	(-1.92)
Obs.	9,019	6,675	9,019	6,675
Controls	No	Yes	No	Yes
Aver(Y)	13.594	9.752	12.890	9.507

The table reports the results based on a non-transformed database. Dependent variable is the loan growth. In two first columns, we display results using the Two-Way Fixed Effects (TWFE) estimator. Columns (3) and (4) used the method developed by Callaway and Sant'Anna (2021). Control variables are excluded in columns (1) and (3) and include in columns (2) and (4). The list of control variables include the asbolute size (in current dollars), the relative size (market share), the ratio of deposits to total assets, the solvency ratio (equity to total assets), the tranformation ratio (loans to deposits), a dummy if the bank belongs to a banking group, the log of GDP per capita, and the ratio of private credit to GDP. Standard errors are clustered at the bank-level. *, **, and *** signal statistical significance at 10, 5, and 1%, respectively.

Table E5: Impact of intensity of the treatment for banks below and above the threshold

Threshold (Z) →	1	2	3	4	5	6	7	8
Below	-3.463	-2.170	-1.211	-1.084	-1.561	-1.859	-2.005	-2.547*
(Z=0)	(2.501)	(1.592)	(1.274)	(1.164)	(1.218)	(1.204)	(1.267)	(1.340)
Above	-2.681*	-3.641*	-6.228**	-7.935**	-7.804**	-8.675*	-11.384*	-8.064
(Z=1)	(1.457)	(1.960)	(2.767)	(3.430)	(3.902)	(4.724)	(6.040)	(6.246)

The table reports the results of the coefficient associated to the treatment (interaction between *Post* and *Treated*) for different levels of treatment intensity ranging from 1% to 8% of total assets.The first row shows the effect for banks with assistance below the threshold, and the second row shows the effect for banks with assistance above the threshold. *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

Table E6: Impact of treatment for efficient and inefficient banks

Panel A: Cost-to-income					
	Very efficient	Efficient	Moderate	Inefficient	Very inefficient
Percentile →	10	25	50	75	90
Post x Treated	-1.750	-2.445*	-3.164**	-4.006**	-5.072**
	(1.549)	(1.390)	(1.385)	(1.585)	(2.050)

Panel B: Overhead cost					
	Very efficient	Efficient	Moderate	Inefficient	Very inefficient
Percentile →	10	25	50	75	90
Post x Treated	-0.581	-1.535	-2.847**	-4.737***	-6.549***
	(1.578)	(1.404)	(1.365)	(1.716)	(2.313)

The table reports the results of the coefficient associated to the treatment (interaction between $Post$ and $Treated$). We consider the impact of the treatment for banks having different level of (in)efficiency measured by both cost-income ratio (Panel A) and overhead costs to total assets (Panel B). Very efficient banks are those having a level of inefficiency equals to the bottom decile (percentile = 10), efficient for those at the bottom quartile (percentile = 25), moderate at the mediam (percentile = 50), inefficient for those at the top quartile (percentile = 75) and very inefficient for those at the top decile (percentile = 90). *, ** and *** signal statistical significance at 10, 5, and 1% respectively.

“ Sur quoi la fondera-t-il l’économie du monde qu’il veut gouverner ? Sera-ce sur le caprice de chaque particulier ? Quelle confusion ! Sera-ce sur la justice ? Il l’ignore. ”

Pascal

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