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Impact of a COVID-19 emergency liquidity support package on bank performance and lending

A controlled interrupted time series (CITS) analysis

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About this report

The study evaluates the impact of the EBRD's COVID-19 emergency liquidity support package—known as the Solidarity Package on bank performance and lending across its countries of operation. This work was supported by the independent Evaluation Department of the European Bank for Reconstruction and Development under grant 2022.005842 - Regional: Second Stage of the Solidarity Package Evaluation – Counterfactual Analysis.

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Executive summary

The COVID-19 pandemic created unprecedented economic challenges, particularly for financial institutions tasked with maintaining credit flows amid market disruptions. Liquidity shortages threatened the stability of banks and their ability to support businesses, especially small and medium-sized enterprises (SMEs) that relied on continued financing to survive. One approach to mitigating these challenges was the implementation of emergency liquidity support programs to ensure credit availability and financial system resilience. This study examines one such intervention--the European Bank for Reconstruction and Development (EBRD)'s Solidarity Package (SP)—to assess its impact on bank lending and performance metrics.

This study evaluates the impact of the EBRD's COVID-19 emergency liquidity support package—known as the Solidarity Package (SP)—on bank performance and lending across its countries of operation. The SP was designed as a rapid-response financing initiative to provide immediate liquidity to financial institutions, ensuring that they could continue lending during periods of economic uncertainty. One of its key components, the Tier 1 Resilience Framework, focused on stabilizing banks by reinforcing their liquidity positions and enabling them to support struggling businesses and individuals. We employ a controlled interrupted time series (CITS) analysis to assess the effects of Tier 1 Resilience Framework support on key financial indicators, including lending volume, return on assets, return on equity, non-performing loan ratios, liquidity coverage ratios, and capital adequacy ratios.

Findings reveal that the SP had a significant positive impact on bank lending, with recipient banks increasing their lending by an average of €1.6 billion per year compared to non-recipient banks—an 8% annual rise over baseline levels. This suggests that the emergency liquidity support successfully facilitated credit flow, helping maintain financial stability during the pandemic. However, we found no statistically significant effects on other bank performance indicators, such as return on assets, return on equity, capital adequacy, or liquidity coverage ratios. While liquidity support enabled lending, it did not substantially alter broader financial health metrics, indicating that the primary benefit was in maintaining credit access rather than fundamentally reshaping financial risk or profitability structures.

We employ multiple robustness checks to validate the findings, including alternative model specifications, quarterly complete case and imputed data analyses, and matched controlled interrupted time series models. We use SDID and matching to try and address concerns related to differences in the treatment and control group. While CITS already accounts for differences in the groups at baseline, matching and SDID can further substantiate this. SDID is a relatively newer methodology and matches the groups on trends in the pre-period and measures the average of the outcome before compared to after (rather than over time like CITS). Despite these methodological differences, results from SDID are generally consistent with the CITS estimate, reinforcing the robustness of our findings. Despite these measures, the study still faces limitations related to data availability and sample size constraints. The availability of financial data varied significantly across banks, leading to gaps in key performance indicators that may affect the robustness of the results. Additionally, the relatively small sample size limits the generalizability of the findings and reduces the statistical power to detect more nuanced

effects beyond lending. These factors introduce potential biases and restrict the ability to draw broader conclusions about the long-term impact of liquidity support programs on financial stability.

Overall, these insights contribute to policy discussions on the role of financial institutions in crisis response, emphasizing the importance of liquidity support in ensuring continued access to credit. The findings align with prior research suggesting that well-targeted liquidity interventions can mitigate economic shocks and support financial stability. Future studies should explore the broader economic effects of similar programs, particularly in emerging markets, and assess the long-term implications of emergency liquidity support on financial resilience and economic recovery.

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1. Introduction

1.1 Background

On January 30, 2020, the World Health Organization declared COVID-19 a Public Health Emergency of International Concern and characterised the outbreak as a pandemic on 11 March 2020 (European Central Bank. 2017). The pandemic placed a significant strain on nations' economies as most nations took measures to restrict movement and economic activity. This triggered temporary credit difficulties in multiple of EBRD's countries of operations (COOs),[1] which in turn constrained the ability of countries to preserve their transition efforts made in the sphere of private sector development.[2] Adverse market conditions due to the COVID-19 pandemic created a liquidity gap in the early days of the pandemic between banks and their clients, many of which were small and medium-sized enterprises (SMEs) in need of support from banks to survive the crisis.[3]

EBRD reacted rapidly and was the first international financial institution to approve a tailored COVID-19 response package to support the private sector in its COOs. Already by March 13, 2020, in an attempt to proactively mitigate the negative economic impact of COVID-19 and preserve transition efforts made in the private sector, EBRD approved a Solidarity Package (SP) of €1B in emergency financing to its existing clients. On 23rd April 2020, the emergency liquidity financing under the Resilience Framework (Tier 1) was expanded to €4B. The SP was designed to provide rapid and targeted support to EBRD clients (financial institutions, SMEs, corporate and energy developers, sub-sovereign municipal, energy and infrastructure clients), with the main objective of preserving transition and strengthening resilience of banks, firms, and nations to the crisis.[4] Tier 1, the Resilience Framework (RF) of the SP, focused primarily on the 'Resilience' and 'Competitive' transition qualities (TQ), in large part by providing emergency liquidity to banks to ensure they had enough funding to remain solvent and could continue to provide loans to businesses and individuals.[5] Existing clients across EBRD's countries of operation were eligible to receive the emergency liquidity support provided that they could demonstrate that the COVID-19 crisis had impacted their financial status. Based on a review of EBRD project approval documentation, the

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[1] European Bank for Reconstruction and Development (EBRD) countries of operations (COOs) include all countries listed here: https://www.ebrd.com/where-we-are.html

[2] According to EBRD's terminology, 'transition efforts' are measured at the country level using an Assessment of Transition Qualities (ATQs). ATQs rely on various indicators, including aggregate versions of some of the measurable bank-level indicators we examine in this study. For more details on ATQs see EBRD Transition Reports: https://2021.tr-ebrd.com/structural-reform/. 'Transition efforts' are also measured at the level of an individual investment project of EBRD using transition impact measurement methodology attached to each project. Those refers to the development outcomes scores such as expected transition impact (ETI) and portfolio transition impact (PTI), which are rigorously assessed and monitored by EBRD Management.

[3] Ascertained from EBRD bank project approval documents.

[4] Retrieved from multiple EBRD Internal Documents including: EBRD Operations Committee Secretariat (2020). *Memorandum on Solidarity Package Guidelines*

[5] Retrieved from multiple EBRD Internal Documents including: EBRD Operations Committee Secretariat (2020). *Memorandum on Solidarity Package Guidelines*

emergency liquidity support was anticipated to mitigate adverse effects from COVID-19 on various outcomes including liquidity, lending, non-performing loans, asset quality, and capital.[6] These outcomes are described in more detail in the Material and Methods section.

As noted by many EBRD banks who received support under the Resilience Framework of the SP, this funding was essential to 'bridge a liquidity gap in financing' due to adverse market conditions created by the COVID-19 pandemic created a.[7] Many of these clients were SMEs, who would have most likely needed support from EBRD banks to survive the crisis. Based on the existing literature documenting similar shocks in the past, banks were expected to face liquidity constraints due to a sharp reduction in revenues (depositors withdrawing their savings), less demand, disruption of the supply chain, or difficulties to renew expiring credit/bond facilities due to lower appetite by local commercial banks and investors.[8] For example, Acharya and Steffen (2020) noticed a 'dash for cash' in the United States, which placed a strain on the creditors of those funds (banks). Decreased liquidity can lead to fewer and smaller loans to businesses that need them to continue functioning, lower return on assets (profitability), and depleted capital buffers.

The banking sector plays an important role in economic resilience by encouraging the accumulation of savings and allocating these funds to the most productive investments, which supports innovation and economic growth (European Central Bank. 2017). In particular, banks play an essential role in the economy by supplying funding to businesses and households. The COVID-19 pandemic was expected to increase the need for this essential funding and impact a bank's ability to supply the credit needed, resulting in financial instability and various negative downstream effects on businesses, countries, and individuals' health and well-being (Acharya and Steffen 2020; Borio 2020). In response, many central banks and governments implemented policy interventions aimed at facilitating the flow of credit to firms, both directly and through banks (Minoiu, Zarutskie, and Zlate 2021).

Furthermore, as noted by the European Central Bank, 'during crises, the number of loans that cannot be paid back increases.' Compared to past crises, there were factors around the COVID-19 crisis that may make non-performing loan (NPL) resolution more likely than in the past (e.g., banks have higher capital buffers) while other factors could make it more challenging (e.g., government debt is higher, banks are less profitable) (Ratnovski 2020). In a 2020 report for the Vienna Initiative, EBRD anticipated that the COVID-19 pandemic would lead to an increase in NPLs in the banking sector in EBRD COOs, though only after COVID-19 forbearance measures were to expire (Cloutier and Wee, n.d.). High levels of NPLs are problematic because they impair bank balance sheets, depress credit growth, and delay economic recovery. While forbearance measures may dampen or delay increases in NPLs, this was nonetheless a concern cited by bankers in the SP project approval documents. For example, one bank's Solidarity Package documentation stated that 'in the context of COVID-19, we expect: an

[6] Ascertained from EBRD's Resilience Framework project approval documents.
[7] Ascertained from various background project approval documents.
[8] EBRD Operations Committee Secretariat (2020). *Memorandum on Solidarity Package Guidelines*

increase in non-performing loans (NPLs), mitigated by sufficient capital buffers above the regulatory requirement', as well as by initiatives introduced by the Central Bank (e.g., to postpone repayments for retail and corporate clients and restructure facilities).[9] They also cited concerns that liquidity and asset quality would decrease as sub-borrowers struggle to repay their loans from the banks.

Emergency liquidity assistance can serve as one essential response to alleviate these issues. For instance, during the euro-crisis, various liquidity assistance measures prevented loan growth from becoming even more negative (Gibson et al. 2020). The European Central Bank has also noted that banks' resilience during the pandemic (i.e., no significant deterioration in asset quality) has been largely due to stimulus and government support measures ("'The Banks Are Not Complacent'" 2022).

1.2 Literature

This paper contributes to the literature examining the impact of government support and policy initiatives on financial intermediation (Paravisini 2008; Aït-Sahalia et al. 2012; Demirgüç-Kunt, Pedraza, and Ruiz-Ortega 2021) and firms (Igan, Mirzaei, and Moore 2022). One such study examines the impact of policy announcements (e.g., liquidity support, borrower assistance and monetary easing) on bank stocks globally and finds an increase in returns of bank stocks, though exclusively in developed countries, with no impact detected in low-income countries (Demirgüç-Kunt, Pedraza, and Ruiz-Ortega 2021). Similarly, Ait-Shalia et al. (2012) find that financial sector policies in response to the 2008-2009 global financial crisis decreased interbank risk premia, while bailing out or letting banks fail led to an increase in risk premia. With regard to firms, Igan et al. (2022) find that fiscal policies (such as tax measures, loan guarantees, business grants or wage subsidies) have a positive effect on efficiency, profitability, liquidity, and survival of firms in the most pandemic-prone sectors.

Many other studies have examined governmental responses to financial crises as well, with mixed results on the effect of policy on macroeconomic performance (Claessens, Klingebiel, and Laeven 2005; Reinhart and Rogoff 2009; Taylor and Williams 2009). Bordo and Duca (2021) provide an overview of US-based policy tools (e.g., the U.S. Small Business Administration's Paycheck Protection Program, the large-scale grant-making program for small businesses, and the Federal Reserve's Corporate Credit Facilities, especially the Secondary Market Corporate Credit Facility) and note that they were generally successful in limiting the negative impact of COVID-19 on the economy, particularly on businesses and employment (Bordo and Duca 2021).

There is some evidence of the positive impact on credit, employment, and on-lending of loan guarantee schemes, which are typically targeting specific bank's clients (small businesses) (Barrot et al. 2019; De Marco 2020; Altavilla et al. 2021) and funding for lending schemes like the European Central Bank's Long-Term Refinancing Operations (LTRO), which are typically earmarked and closely monitored for on-lending (Jasova, Mendicino, and Supera 2018; Andrade et al. 2019; Gibson et al. 2020; Carpinelli and Crosignani 2021). Paravisini (2008) studies a similar on-lending program of the Inter-American Development Bank in Argentina and also finds that banks increase lending in

[9] Source: EBRD Internal Project Board Documents

response to additional external financing. Schiozer and Oliveira (2016) do not focus on the impact of a specific policy intervention but shed some light on the relationship between liquidity and lending in a market in crisis and find that while negative shocks to liquidity cause a significant decrease in bank lending, positive liquidity shocks do not necessarily promote new lending and have only a very small effect on the loan supply.

While these studies evaluate various policy instruments, very few examine the provision of emergency liquidity, and nearly none focus on support in lower and middle-income countries. This study primarily contributes to this scarce existing literature around effects of unrestricted liquidity support to banks on bank performance (Demirgüç-Kunt, Pedraza, and Ruiz-Ortega 2021) and lending (Minoiu, Zarutskie, and Zlate 2021). While Demirgüç-Kunt et al. (2021) examine various policy interventions on stock prices, Minoiu, et al. (2021) examine a specific policy, namely the Main Street Lending Program, and find that it increased banks' willingness to lend to large and small firms (that banks are more likely to renew maturing loans and originate new ones, and less likely to tighten standards). In this paper, we analyse the impact of a specific liquidity support package provided by EBRD to various banks around the world on their lending and performance, which are believed to contribute to strengthened resilience and the prevention of transition reversal in the countries in which those firms operate.

1.3 Research question

We assess how the SP Tier 1 Resilience Framework support impacted bank performance-related outcomes in the period following the intervention (specifically in the 27 months after signing, as most support was signed over in Q2 of 2020 and the most recent time period for which data were available as of the cut-off date for this evaluation was Q3 of 2022). In particular, we examine: 1) the difference between treatment and control in the level of the lending/performance outcome directly after the intervention period begins, and 2) the difference between treatment and control in the slope of the lending/performance outcome variable in the post-intervention compared to pre-intervention periods. This evaluation was focused on the banking industry, as the majority of the Tier 1 financing was given to banks - around 40% of the total SP's business income in new investments went to clients in the banking sector. This figure is the highest among Tier 1 financing (74%). While there were multiple rounds of SP funding (Tier 1, 2 and 3), we focus on Tier 1 since Tiers 2 and 3 were not directly related or perceived to be related to COVID-19 and not sufficiently different from business as usual.

This evaluation did not attempt to explore the heterogeneity of treatment effects at the country or economy level due to sample size constraints. The primary research question assessed through this evaluation is:

What was the impact of EBRD's Tier 1 liquidity support on bank performance-related outcomes during the COVID-19 pandemic?

EBRD's background documentation outlines that the Tier 1 Resilience Framework funding is theorised to contribute to the resilience of EBRD's countries of operation through various bank-level and economy-level indicators, including those we examine. Furthermore, the rationale provided in EBRD project approval documents indicates that

liquidity, lending, NPLs, and capital would be affected by COVID-19 and thus would be expected to be favourably affected by the SP support.

The rest of the paper is organised as follows. We provide an overview of the methods with a description of the data in the following Material and Methods section, present the results of our analysis in the Results section, and conclude with a discussion of the results and opportunities for future work in the Discussion and Conclusion.

2. Material and Methods

We employed controlled interrupted time series (CITS) to assess the impact of EBRD's Tier 1, as it controls for bank-level time invariant characteristics as well as time-varying characteristics common to treatment and comparison banks.

We restrict our final sample based on the availability of each outcome variable, keeping only those banks with at least one instance of the outcome after the SP was disbursed, and three instances prior. Our control group was then restricted to banks in the countries that exist in the final sample of Tier 1 SP banks for each outcome. We also dropped banks that did not receive the SP in the same defined intervention period as the majority of the other banks.

The analysis included two separate comparison groups. The first group was EBRD banks that had contracts signed from 2017 onwards (recently active) and that did not receive Tier 1 SP funding. The second group was non-EBRD banks that are operating in the same countries as Tier 1 banks as well as EBRD banks that did not receive the Tier 1 SP funding. Additional details on the decisions made in constructing the analytic sample and comparison banks as well as other methodological considerations can be found in Appendix A.

2.1 Data

We employ data from two sources, EBRD's internal client and project data and the S&P Capital IQ Pro.[10] A description of the outcome variables and covariates from these datasets (as well as the theorised direction of impact on each outcome) is provided in Table 1.

S&P Capital IQ Pro hosts an extensive amount of data on the financials of companies and financial institutions globally. It provides an external, consistent, objective source of data for both the treatment and control groups and across countries, with published methodologies for how each indicator was calculated.

We obtained quarterly data from Q1 2017- Q3 2022 on the outcomes listed in Table 1 and covariates listed in Table A.1 for all banks in all EBRD COOs. Once the outcome data were downloaded and processed, they were merged to the EBRD bank data and assessed for missingness. Based on our review of EBRD's existing theory of change and indicator documentation, as well as through conversations with bankers and a broad review of literature, we identified the variables described in Table 1 as important outcomes likely to be impacted by the SP.

[10] S&P CapitalIQ Pro data retrieved from https://www.spglobal.com/marketintelligence/en/solutions/sp-capital-iq-pro

Table 1: Outcomes assessed in this impact evaluation and theorised pathways to impact

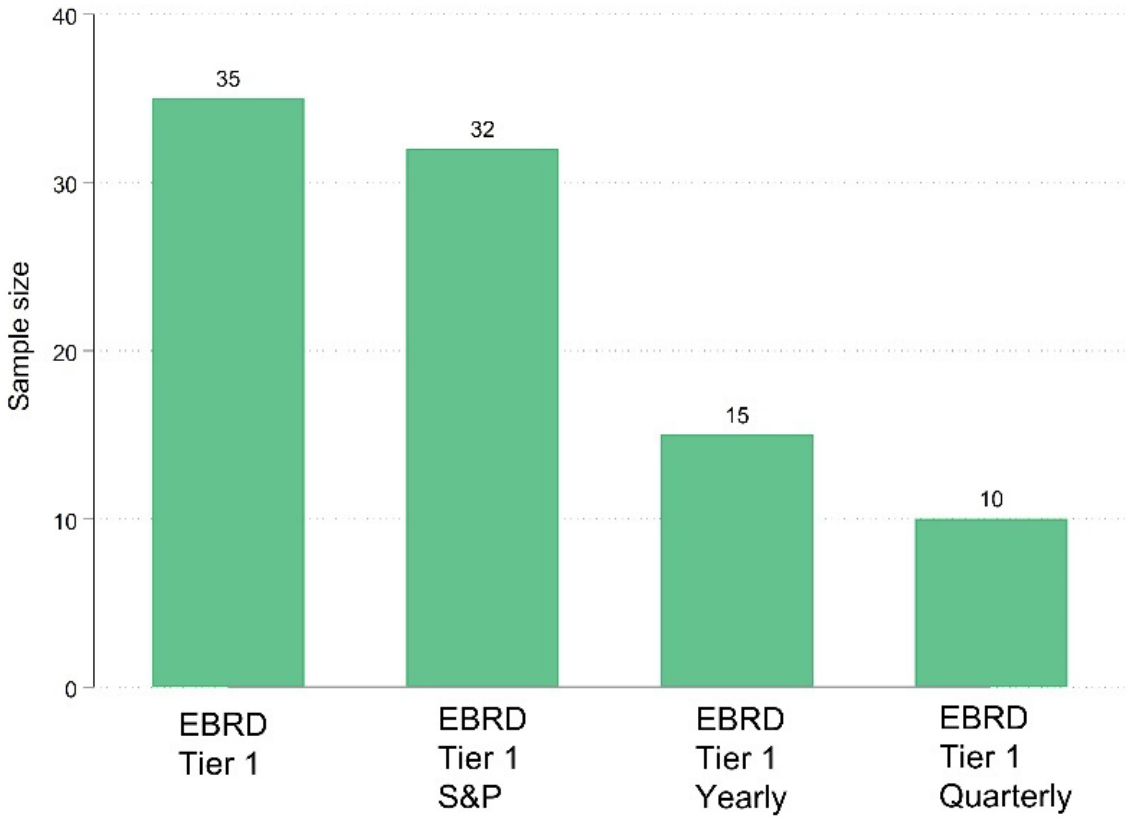
Outcome	Indicator name	Description
Lending	Gross Loans to Customers	Loans and finance leases held for investment or held for sale, net of unearned discount and gross of loss reserves. Does not include accrued interest on loans.
Liquidity coverage ratio (LCR)	Liquid assets/total deposits & borrowings	High quality liquid assets as a percent of net cash outflows over a thirty-day period as defined by local regulatory requirements.
Return on average assets (ROAA)	SP Return on average assets	Net income as a percent of average assets.
Return on average equity (ROAE)	SP Return on average equity	Net income as a percent of average equity.
Non-performing loans (NPL) ratio	Non-performing loans/Total loans	Nonperforming loans, net of guaranteed loans, as a percent of loans before reserves.
Capital adequacy ratio (CAR)	Total capital ratio	Capital as a percent of total risk-weighted assets of a bank.

EBRD's internal client and project data used for this study contains detailed information on 3,442 firms and 602 financial institutions that were EBRD clients at any point from 1999 to the present (according to the operation signing date). Of these financial institutions, 162 received at least one round of SP financing and 35 of these clients received emergency liquidity funding under Tier 1 Resilience Framework.

EBRD's data includes details related to the country of origin, amount, and timing of past EBRD financing, as well as receipt of the SP. This data is primarily used to indicate which banks from the S&P database are EBRD clients that received the SP as well as when they received the support.

Capital IQ Pro included 3,233 banks in these countries of operation, with varying degrees of missingness. Of these banks, 296 exist in EBRD program data as past and current EBRD clients. Figure 1 provides an illustrative example of the final sample for one outcome variable of interest, lending. While 35 banks received Tier 1 funding, only 32 of those merge to the S&P data, and only 15 of those are not missing any data when we aggregate up to yearly values (after dropping the banks missing more than 3pre/1post and the 3 banks with the intervention not provided in 2020).

Figure 1: Data cascade for lending, starting with EBRD Financial Institutions that received Tier 1 SP support.



For example, only 14 of the 32 Tier 1 banks were not missing any outcome data, while 8 of the 32 were already missing data on the lending outcome for all 23 quarters, with the rest missing at least 12 quarters of data, primarily in the post-intervention period.

Due to the amount of missing data, we performed the primary analysis by year rather than by quarter, though we also ran sensitivity checks on quarterly complete case and quarterly imputed data, as described in Appendix B. Our assumption in employing the yearly data is that we would still see an increase in the average of each outcome throughout a given year, even if the outcome is typically employed as a short-term indicator (e.g., LCR).

2.2 Empirical Models

2.2.1 Controlled interrupted time series (CITS)

Since multiple time points are available for each bank's outcome values in both the pre- and post-intervention periods, we used controlled interrupted time series (CITS) to assess the impact of SP for Tier 1 banks. CITS employs the continuation of pre-treatment trends as the counterfactual for the observed post-treatment trends and incorporates a control group comparison into the before-after design that accounts for other events that may occur over the same period as the intervention which affect our outcome of interest (Bernal, Cummins, and Gasparrini 2018). This design allows us to control for other time-variant confounders (e.g., seasonality and market fluctuations) by comparing to a control group undergoing similar changes, as well as comparing multiple periods before and after the intervention.

Below is the regression equation used for a controlled interrupted time series.

$$Y_{ti} = \beta_0 + \beta_1 T_{ti} + \beta_2 X_{ti} + \beta_3 T_{ti} X_{ti} + \beta_4 G_i + \beta_5 G_i T_{ti} + \beta_6 G_i X_{ti} + \beta_7 G_i X_{ti} T_{ti} + \epsilon_{ti} \quad (1)$$

Eq. (1) is a segmented regression equation for slope change with a control series, adapted from Linden and Adams (2011) by Bernal, Cummins, and Gasparrini (2018).

Y_t is the outcome variable at time t, T is a variable representing the time since the start of the study and X is a dummy variable indicating the pre- or post-intervention period. There are two time variables in a CITS design: the time T since the start of the study, and t, the time interval of the data for a given unit. β_0 represents the intercept at T=0 for the intervention group, β_1 is the change in outcome associated per time unit increase (representing the underlying pre-intervention trend), β_2 is the level change following the intervention in the intervention group and β_3 indicates the slope change following the intervention (using the interaction between time and intervention: TX_t). G represents the intervention group ($G = 1$) or control group ($G = 0$). β_4 represents the difference in intercept at T=0 between the intervention and control group, and β_5 represents the slope difference between the intervention and control group in the pre-intervention period. β_6 represents the difference between the change in level of the outcome in the control and intervention group associated with the intervention directly after the intervention period begins. β_7 represents the difference between the change in slope of the outcome in the control and intervention group associated with the intervention in the post-intervention compared to pre-intervention periods. Therefore, both β_6 and β_7 are the parameters of interest for the measures of effect.

For the CITS design to be valid, we need a clearly defined intervention time period. We used signing dates from EBRD's intervention dataset to establish when each treatment bank received the intervention. Signing dates indicate when treatment banks may have changed their behaviour due to receiving the intervention, even if they ultimately do not receive or use the funding. By using signing dates, we are also not including any banks that may have been eligible for the program but were not approved by EBRD's Board.

For the models that are presented in this analysis, we aggregated banks' outcome indicators to the yearly level by averaging the quarterly values and ran all analyses at this level (as described in the Material and Methods section). We ran sensitivity analyses with data at the quarterly level (rather than yearly), once by imputing missing values and once while dropping banks with missing values for any quarter (see Appendix B Table B.3 and B.2, respectively). As an additional check of the model specification, we also ran the yearly analysis using synthetic difference-in-differences (SDID) and matched CITS models (see Appendix B).

Each model used bank-year panel-level specifications and included controls for country, baseline assets and time-varying loans received by banks. Robust standard errors were specified. For the matched controlled interrupted time series, the analyses controlled for country and time-varying loans received by banks. A full description of the covariates included in each model can be found in Table A.1.

3. Results

3.1 Descriptive Statistics

At baseline, EBRD Tier 1 banks are generally larger and perform better than non-Tier 1 banks. Table 2 depicts some key indicators and covariates at baseline. Descriptions of the outcomes are provided in Table 1, while descriptions of the covariates can be found in Appendix A, Table A.1. On average at baseline, Tier 1 banks have higher gross loans, ROA, ROE, total assets, and total loans received. Tier 1 banks have a lower average NPL ratio than non-EBRD banks (fewer non-performing loans to total loans), which indicates that any outstanding loans present a lower risk to the bank. Tier 1 also has the lowest CAR. Below a certain threshold, a higher CAR is generally expected to be safer as it indicates that the bank is more well protected against shocks. However, after exceeding the minimum requirement to be deemed 'secure' (above 10.5% in most countries), there is a trade-off between increasing CAR and a more efficient use of assets – in the form of lending or ROA or ROE, for instance (Nickolas 2023). Tier 1 had lower levels of liquidity in the pre-period as well, but similar to CAR, there is a trade-off involved in increasing LCR when the bank is already liquid enough to cover its debts. In general, this aligns with our expectations that Tier 1 banks will perform better at least partly due to criteria required for banks to receive SP funding (e.g., having strong business fundamentals and high probability that after recovery of the market they will be able to repay their loan and their liquidity will be adequate for the next 1-1.5 years). In general, EBRD banks are likely to be healthier than other market players that might not be meeting EBRD's minimum risk criteria required to engage with EBRD. However, the CITS design and model specification enable us to control for these baseline differences.

Table 2: Descriptive statistics by treatment and comparison groups at baseline (in 2019).

Outcome Indicators	EBRD Tier 1 Banks	Non-Tier 1 Banks
Gross Loans (000s EUR)	14,626,001.00	3,431,931.74
ROAA (%)	1.63	1.75
ROAE (%)	13.88	10.10
LCR (%)	35.88	44.41
NPL Ratio (%)	4.59	6.63
CAR (%)	17.84	24.93
Covariates		
Loans Received (000s EUR)	2,232,572.75	346,393.78
Bank Size (000s EUR)	25,292,208.00	5,456,002.00

*Note: the observations included for each outcome and covariate fluctuate slightly based on availability of data in 2019 for each outcome. The covariate summary statistics are for the lending sample available in 2019, and therefore may vary slightly in the analysis for each outcome.

3.2 CITS

Table 3 illustrates the results of the yearly CITS specification controlling for baseline assets, country, and time-varying loans received. Nearly all of the sensitivity checks, including the quarterly imputed, quarterly complete case, and SDID (in Appendix B),

depict similar results. There was no statistically significant impact on return on assets, return on equity, NPL ratio, liquidity coverage ratio, and the capital adequacy ratio over time in the treatment group relative to the control group. Of all analysed outcomes, the SP had a statistically significant impact only on lending. Compared to the comparison group, the treatment group increased their average lending by 1.6 billion euros (SE: 627,777,300) each year after having received the emergency liquidity support, which is equivalent to an 8% yearly increase over baseline lending for the Tier 1 banks. This is a relatively large and meaningful increase in lending that is attributable to the SP support.

Table 3: Tier 1 vs. non-Tier 1 banks: Controlled interrupted time series for each outcome variable controlling for baseline assets, country, and time-varying loans received to banks.

	(1) CITS Loans (000sEUR)	(2) CITS ROA (%)	(3) CITS ROE (%)	(4) CITS NPL Ratio (%)	(5) CITS CAR (%)	(6) CITS LCR (%)
Change since start	100,925.21	0.06	-0.46	0.97*	-0.03	1.03
	(62,920.87)	(0.13)	(1.05)	(0.42)	(0.29)	(0.65)
Base level diff (TvC)	762,263.06	0.38	2.98	-0.03	-5.31*	-5.10
	(2,285,303.87)	(0.29)	(1.96)	(0.73)	(2.24)	(3.77)
Base slope (TvC)	-1,094,584.33	-0.11	-0.36	-0.23	0.70	-1.16
	(936,649.38)	(0.15)	(1.16)	(0.53)	(0.43)	(1.82)
Avg change in level (2020)	93,849.53	-0.67*	-2.76	-1.12*	-0.07	-0.23
	(125,974.19)	(0.30)	(2.77)	(0.49)	(0.49)	(0.99)
Post-pre avg change in trend	-15,519.77	0.71***	7.33***	-1.54*	-0.39	0.43
	(121,538.16)	(0.15)	(1.35)	(0.62)	(0.41)	(0.72)
Post-pre change in level (TvC)	1,037,215.04	0.06	-2.94	0.70	0.81	2.87
	(591,606.80)	(0.34)	(2.99)	(0.58)	(0.68)	(2.51)
Post-pre change in trend (TvC)	1,624,850.18**	0.23	3.17	0.07	-0.33	0.60
	(627,777.26)	(0.26)	(2.46)	(0.76)	(0.64)	(2.25)
Constant	-1,141,083.53***	1.42***	7.12***	3.59**	13.27*	39.84***
	(129,615.18)	(0.33)	(1.94)	(1.18)	(6.32)	(5.16)
Baseline Tier 1 Mean	20,032,843.48	1.73	15.16	3.79	16.92	30.28
Tier 1 Obs	15	17	17	12	18	13
Control Obs	84	102	102	51	111	83

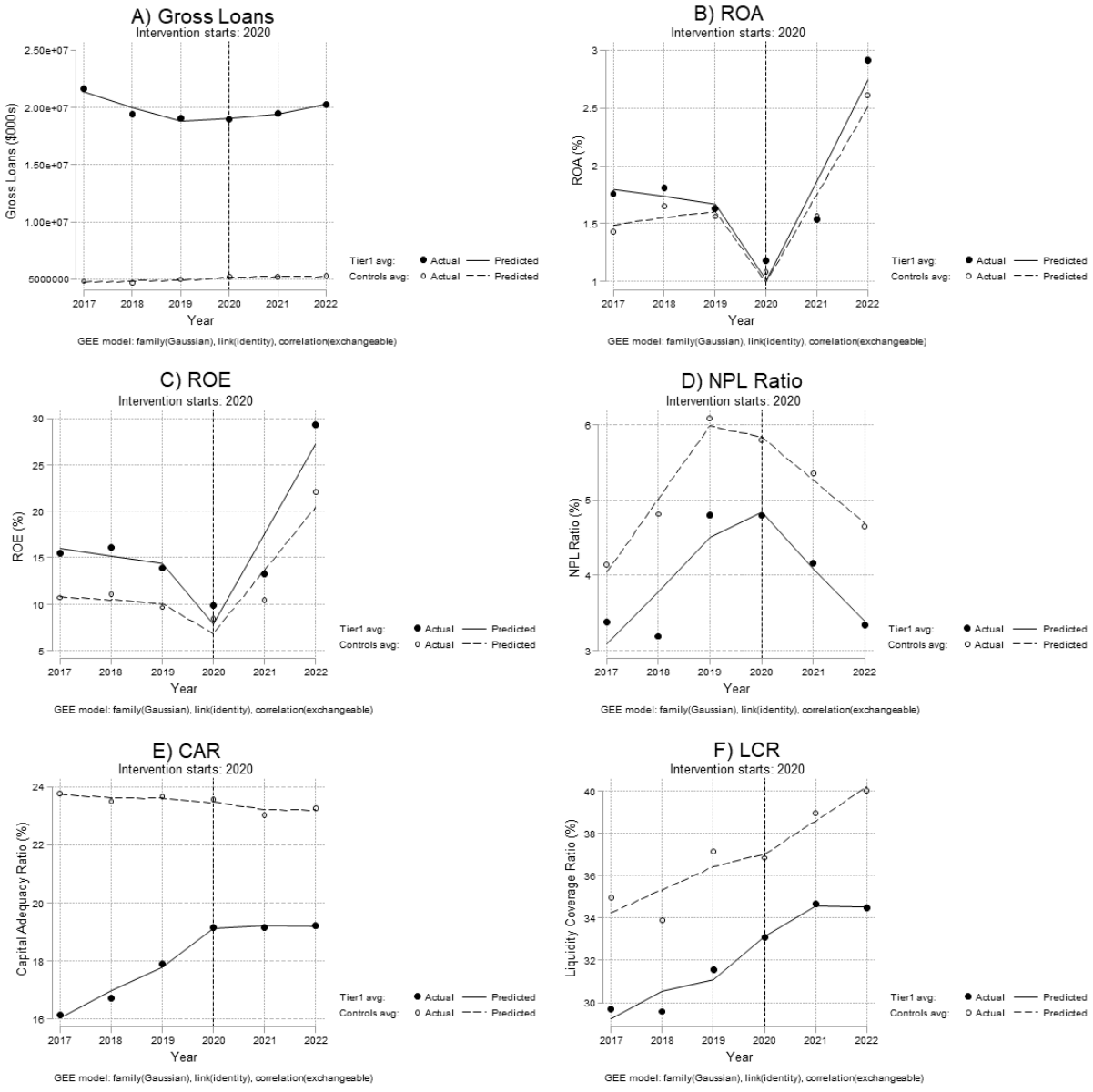
Standard errors in parentheses
** p<0.05, ** p<0.01, *** p<0.001*

3.2.1 CITS graphs

These graphs illustrate the CITS design with country, base assets, and net loans received as covariates. They also clarify the intuition behind the CITS compared to pre/post or other analyses. For example, if we were to compare the ROA results before vs. after the intervention for the treated banks, there would appear to be a large increase in ROA. However, when we include a control group, this effect is muted, and we do not detect a significant impact. CITS also controls for the change in trends before and after, thereby depicting a change over time, not just the average of the points pre/post, as is

typically the case for a difference-in-difference (DID) analysis. This is especially evident in the gross loans graph. On average, the lending before the SP looks higher than after; however, while gross loans were declining before, they are increasing after, while the control group's trends did not change notably. Therefore, we see a positive effect using the CITS design, which accounts for changes in both levels and trends.

Figure 2: CITS event study plots. Dotted lines indicate the trends for the control group.



3.3 Robustness Checks

Since we explore the effect of the SP on multiple different outcomes, and are thereby testing multiple hypotheses, we may be concerned that we are incorrectly concluding that we see an effect in one outcome and none of the others tested simply because the probability of rejecting the null increasing as the number of hypothesis tests performed increases (Coppock 2015). To some extent, we are less concerned about this issue since the composition of our sample is slightly different for each model and thus the

models are run independently (and the effect is a local average treatment effect for that particular group); however, we hypothesise that some of the outcomes are interrelated, and that the SP could have an impact regardless of the sample composition. In order to check whether our results hold under a multiple hypothesis test, we use a conservative Bonferroni adjustment (our alpha of 0.05 divided by number of tests we ran). Since we test the impact of the SP on six different outcomes, using this adjustment, we would get a new p-value target of 0.0083 (0.05/6). The p-value on the lending outcome of interest was 0.00965 (significant at the 1% level) so this effect would still be close to significant at a 5% level, and certainly at a 10% level (0.1/6 = 0.0166). In some of the sensitivity checks the impact on lending was slightly less significant (e.g., at a 5% level). However, this Bonferroni correction can be overly conservative as it does not account for the strong correlation between the different outcomes (Mckenzie 2012).

As mentioned previously, in addition to the yearly controlled interrupted time series analysis, several other specifications were employed as robustness checks, including different cuts of the data (quarterly complete and imputed data) and different or adjusted models (synthetic difference-in-difference (SDID) and matched interrupted time series).

As outlined in Appendix B Table B.2 and Table B.3, the CITS using quarterly complete case and quarterly imputed data depict very similar results as the yearly analysis. Regardless of the way we adjust for missing observations (aggregating up to yearly, dropping any banks missing any quarter, or imputing banks that are missing a quarter with the average for the year), we find positive and significant effects on lending, indicating the results are robust to different ways of adjusting for missing data.

These results are relatively robust to different model specifications. When we run the yearly lending analysis using a recently developed SDID specification, we find similarly large and significant impacts of the SP on lending for the banks in the treatment sample (LATE). Similar to CITS, the SDID does not require a parallel assumption to be met. While the outcome of interest is measuring the average of the outcome before compared to after (rather than over time like CITS) it also adjusts for differences in the trends prior to the treatment. See Appendix B for more details on this robustness check.

The magnitude and direction of our lending results also remain the same in the yearly matched CITS models for the full sample control (Table B.1), though the results are no longer significant at a 5% level (with a p-value of 0.09). It is possible we can no longer confidently detect an effect in this specification due to a decrease in the sample size when matching. This specification was constrained due to the small number of potentially relevant covariates available to match on and the small size of our sample, which limited our ability to match on both country and base assets and instead only enabled matching on one covariate at a time. Future analysis should be performed on a larger scale, ideally with more banks with complete data and more covariates (e.g., for matching) to improve the confidence of these results.

4. Discussion

Overall, we found that the CITS analyses identified significant impacts on lending for the included treatment banks compared to the comparison group, with insignificant impacts on all other performance-related outcome variables.

We find a significant impact of the SP on bank lending for the banks in the analytic sample, indicating that funds from the SP were used by banks to increase their lending by 1.6B EUR each year compared to non-Tier 1 EBRD banks and compared to non-EBRD banks relative to before the SP, all other things remaining equal. This should be interpreted as a local average treatment effect (LATE), given the small size of the analytic sample of banks (i.e., those that received the Tier 1 intervention and had sufficient outcome data), which may not be generalizable to EBRD's full roster of bank clients or the banking sector more broadly.

Nevertheless, this is a sizeable increase in lending of 8% year over year compared to the average baseline lending for Tier 1 banks in the sample. This is consistent with the intent and expectation of this support and aligns with what past studies examining the effect of similar liquidity support measures have found. For example, Minoiu, Zarutskie, and Zlate (2021) determined that certain liquidity measures in response to the COVID-19 pandemic (e.g., the Main Street Lending Program) successfully facilitated the flow of credit to firms, while Gibson et al. (2020) showed emergency liquidity assistance prevented growth in loans from decreasing even more during crises, and Paravisini (2008) found increased external financing of financially constrained banks in Argentina (while not in a time of crisis) also increased their lending. Our findings contribute to this literature by providing another example of emergency liquidity support that appears to have increased lending in recipient banks. As further theorised in EBRD's theory of change documentation, this increased lending may also ultimately, albeit in a small way, contribute to increased economic resilience.

At the same time, we suggest caution in the interpretation of these results, for several reasons. First, the small sample size due to gaps in data availability in S&P Capital IQ limited the statistical power of this analysis. The results also do not withstand all of our sensitivity checks. While the magnitude and direction remain the same for lending in the quarterly imputed and complete case models, as well as the synthetic difference-in-differences (SDID) and matched models, the matched results are insignificant and the SDID results are more sensitive to sample composition. The matched results may lose their significance due to a decrease in our sample size. The small size of our sample limited our ability to match on both country and base assets and instead only enabled matching on one covariate at a time. See A.5 for more details on the matched design and B.2 for details on the SDID design.

Furthermore, while we detected significant effects of the SP on lending, it appears that there is no impact of the SP on the other performance-related measures. The insignificant results in the other outcomes indicates we cannot conclude that there was an impact of SP on those measures. This could be due to a variety of factors, including the limitations mentioned above related to data availability. It is also possible that there is no effect of the SP on those measures, perhaps because they are too distal from the SP. Some of the changes in these outcomes may actually flow through increased lending and therefore may only appear later or/and too weakly for us to be able to detect. For example, NPLs and ROA directly rely on gross loans. There are also a variety of trade-offs between some of these measures (e.g., a decrease in CAR could occur with an increase in lending if the alternative to lending is setting aside more funds).

This work contributes to the field in several ways. First, this paper presents empirical evidence in line with the literature that certain liquidity support can lead to increased lending in recipient banks and provides an example of this effect in banks operating not only in high-income countries but also in emerging economies. Furthermore, as one of the first impact evaluations within this sector that leverages controlled interrupted time series, this analysis demonstrates that this is a viable method and could be used in future banking sector evaluations, particularly with larger samples.

Based on our review of the literature in the financial sector, very few papers employ an interrupted time series design, and none include a control group alongside a time series design (e.g., Boozer et al. 2016; Crookes et al. 2020). In comparison, far more papers within this sector employed methods such as DID and time series analysis independently. CITS is similar to both of these methods but improves on a traditional time series model by including a variable that measures the slope prior to the intervention, as well as a control group, both of which help establish a counterfactual to isolate the effect of the intervention.

As outlined by Somers et al. (2013) and Habib et al. (2021), CITS also provides certain advantages over DID. CITS controls for differences in baseline means as well as trends in the treatment and comparison group, which accounts for confounding factors occurring before or at the same time as the intervention in both treatment and control groups (Bernal et al., 2018). The main effect being measured by CITS is thus the change *over time* in treatment versus the comparison group. DID on the other hand, measures only the difference in *means* before versus after for treatment compared to the control group. CITS has more stringent data requirements, as at least three periods prior to the intervention must be available to accurately estimate the baseline trends, but avoids issues inherent to DID, such as the parallel trends assumption. Since CITS adjusts for difference in trends prior to the intervention, parallel trends between treatment and control group are not required. While we could have performed both a CITS and DID with the data available, we found the parallel trends assumption was violated and due to this constraint, focused our analysis only on CITS.

5. Conclusion

Overall, this analysis found that there were some significant impacts on lending from the emergency liquidity support that EBRD provided to their clients. Though this is a local average treatment effect and cannot be generalised to the rest of the Tier 1 treatment sample, this does indicate that the SP likely played a role in the increase in lending experienced. These findings contribute to the scarce existing literature on the influence of liquidity assistance (including on banks in emerging economies) and find similar significant, positive effects on lending. The strength of this work is that this is one of the first impact evaluations within this sector and this analysis has demonstrated that controlled interrupted time series is a viable method for these types of evaluations. This also demonstrates that additional work that assesses the impact of liquidity support on bank performance outcomes in low and middle-income countries is needed.

Appendix A: Data and Methods

A.1 Outcome data

We had access to two potential external datasets: S&P CapitalIQ and Orbis. We attempted to combine the datasets to improve our sample size but noticed that Orbis was missing data for nearly every 4th quarter. Furthermore, the Orbis and S&P outcomes were not sufficiently similar for certain outcomes and banks for us to be confident in the interchangeability for all outcomes. Considering this uncertainty and the fact that our sample size would not have been increased much (if at all), we only employed the S&P data. Descriptions of the data and outcome variables employed can be found in Table 1 of the text.

A.2 Treatment analytic sample

Our data is available quarterly from 2017-2022, which is aggregated up to yearly by taking the average of the quarters per year per bank. In order to ensure we have enough data for our analyses, we first drop any banks missing more than one quarter after the intervention period and three periods (quarters) prior to treatment. We then focused on banks that received the SP in the same quarter/year as most other Tier 1 banks. For the yearly analysis this intervention period was 2020, and for the quarterly analysis this was Q2 of 2020. We excluded any banks that did not receive the SP in that period. Finally, for the yearly analysis we drop any banks that are missing the outcome variable of interest in any year.

A.3 Covariates

We included the covariates outlined in Table A.1 in the analysis. We calculate the total assets at baseline (2019, prior to the pandemic), as is consistent with the literature (e.g., Igan et al., 2022). While differences in baseline characteristics are implicitly accounted for in the CITS model, including base assets as a proxy for the size of the bank provides an extra level of assurance that we control for the significant differences we see in the size of banks between the treatment and control groups. Covariates are typically controlled for at baseline, but we include receipt of loans over time to account for how funding may have changed over time, and particularly around the time of the intervention (due to COVID-19).

We also considered including specific country forbearance measures and firm ownership, as also appeared in the literature.[11,12] However, country forbearance measures from the University of Oxford's COVID-19 Government Response Tracker required additional extensive cleaning so the receipt of loans from the S&P Capital IQ was used to control for lending received during COVID instead. In addition, the forbearance measures would have remained constant within each country and would be accounted for in the country fixed effects models that we ran. Ownership was not

[11] Brown, M. and R. De Haas. Foreign banks in emerging Europe. Economic Policy, 2012. p. 57-98.
[12] De Haas, R., Ferreira, D., and Taci, A. What determines the composition of banks' loan portfolios? Evidence from transition countries. Journal of Banking and Finance, 2010. 34: p. 388-398.

included since all the Tier 1 banks in our sample (and nearly all banks) had the same ownership status (stock corporation).

Table A1: Covariates included in the models.

Variable	Data Source	Database code	Definition	Transformation
Country	Capital IQ S&P Pro (and EBRD data)	SP_country_name	Country of operation	Integer (country_int)
Total assets	Capital IQ S&P Pro	275808	All assets owned by the company as of the date indicated, as carried on the balance sheet and defined under the indicated accounting principles	Total assets in 2019
Receipt of loans	Capital IQ S&P Pro	273673	Net loans and advances made to banks after deducting any allowance for impairment	None (time-varying from 2017-2022)

A.4 Comparison group selection

For the comparison group, we kept only non-EBRD and non-Tier 1 banks that were in the same countries as our final sample of Tier 1 banks for each outcome. We also exclude banks that are missing any year of outcome data to ensure consistency in our final sample of estimates available over time.

A.5 Model specifications

Unit of analysis

Since there were multiple banks within the treatment groups, we used a panel-level CITS model (xtitsa package in Stata) to account for between-bank differences. Compared to other CITS models where the treatment group is aggregated to one value at each timepoint before running the regression, this method allows us to retain the bank-level granular data at each timepoint. By using panel specifications, we are able to incorporate different autocorrelation structures and account for both between-group and within-group variation.

Due to the issues with missingness in our outcome variables (described above), we decided to perform yearly analysis, with quarterly complete case and imputed as a sensitivity check indicating no large differences. In the quarterly complete case, the data is at the quarterly level but we drop any banks missing any quarter of data. For the quarterly imputed specification, we first impute any missing quarters with the average value of the outcome that year, and then drop any banks missing all four quarters of at least one year. In the quarterly analysis we also had to drop any banks with the SP signed outside of Q2 of 2020, which resulted in the yearly analysis enabling us to maintain the largest sampling of banks. The benefit of still including the quarterly analysis as a sensitivity check is that CITS relies on multiple time points pre and post the intervention, and there are less points pre/post in the yearly analysis.

Matching procedure

We used coarsened exact matching (CEM package in Stata) to identify matched units to use in the matched CITS analyses. Unlike other matching methods (such as Mahalanobis or propensity score), researchers have to first decide on the matching bandwidth before running the algorithm and then check for balance between the groups after the algorithm has finished. While exact matching allows researchers to match treated units to control on exact covariate values, the datasets used often do not have sufficient observations to successfully match each treatment unit to its exact comparison unit, especially for continuous variables. CEM mitigates these issues through how the matching algorithm is implemented. CEM coarsens each covariate into strata that are then used to identify exact matches and then the original (uncoarsened) values of the covariate remain in the dataset. The algorithm provides bounds on the maximum imbalance based on the coarsening decisions that the user makes ex ante. This ensures that the user can adjust both the number of strata and the amount of imbalance within the same algorithm rather than having to manually check ex post. There is a trade-off within this decision as increasing the number of strata (narrower bins for that covariate value) reduces the bounds for maximum balance on that variable but could decrease sample size (if sufficient matched units are not found within for each narrower bin). Since baseline asset size is a continuous covariate value, we decided to use CEM to identify matches.

Appendix B: Supplementary Results

B.1 Matched CITS

In order to account for how characteristics such as bank size may have affected the comparability of our comparison groups to our treatment group, we conducted matched controlled interrupted time series. Tier 1 EBRD banks tended to be larger than the comparison banks so we created matched comparison group using baseline values for total assets. Coarsened exact matching was used to identify matched pairs with multiple controls per treatment bank (Blackwell, et al., 2010).

Table B1: Controlled interrupted time series matched on baseline assets for each outcome variable, controlling for country and time-varying loans received to banks.

	(1) CITS Loans (000sEUR)	(2) CITS ROA (%)	(3) CITS ROE (%)	(4) CITS NPL Ratio (%)	(5) CITS CAR (%)	(6) CITS LCR (%)
Change since start	103,490.02	0.06	0.01	0.60***	0.14	0.30
	(196,531.48)	(0.11)	(0.86)	(0.16)	(0.23)	(0.53)
Base level diff (TvC)	4,991,250.53	0.26	3.64	0.17	-5.68*	-3.32
	(5,094,872.61)	(0.24)	(2.02)	(0.65)	(2.24)	(4.17)
Base slope (TvC)	-914,168.87	-0.10	-0.68	0.07	0.55	-0.32
	(747,187.98)	(0.13)	(0.99)	(0.33)	(0.43)	(2.13)
Avg change in level (2020)	792,389.82	-0.75*	-4.25	-0.71*	-0.05	1.93
	(739,964.53)	(0.30)	(2.52)	(0.28)	(0.36)	(1.13)
Post-pre avg change in trend	-706,374.32	0.64***	6.73***	-1.13***	-0.50	1.28
	(569,158.88)	(0.12)	(1.22)	(0.25)	(0.36)	(0.72)
Post-pre change in level (TvC)	-384,412.82	0.09	-1.88	0.37	0.72	0.04
	(955,995.96)	(0.34)	(2.75)	(0.39)	(0.62)	(2.30)
Post-pre change in trend (TvC)	1,230,466.26	0.16	2.45	-0.24	-0.58	0.02
	(723,312.49)	(0.25)	(2.46)	(0.50)	(0.64)	(2.43)
Constant	713,106.08	1.55***	7.32***	3.02***	17.72***	39.47***
	(597,719.03)	(0.30)	(1.87)	(0.88)	(0.94)	(4.63)
Baseline Tier 1 Mean	20,032,843.48	1.73	15.16	3.79	16.92	30.28
Tier 1 Obs	15	17	17	12	18	13
Control Obs	84	102	102	51	111	83

Standard errors in parentheses
** p<0.05, ** p<0.01, *** p<0.001*

Note: The model assessing the change in loans controls for baseline values of loans received to banks

B.2 Quarterly complete case analyses

We also ran analyses using quarterly data. In Table B.2, we conducted the analyses using a complete case sample where any banks missing outcome or covariate analyses were dropped.

CITS

Table B2: Provides the CITS quarterly complete case results for each outcome.

	(1) CITS Loans (000sEUR)	(2) CITS ROA (%)	(3) CITS ROE (%)	(4) CITS NPL Ratio (%)	(5) CITS CAR (%)	(6) CITS LCR (%)
Change since start	1,306.43	0.03	-0.06	0.31**	-0.03	0.36*
	(18,876.00)	(0.03)	(0.21)	(0.11)	(0.10)	(0.18)
Base level diff (TvC)	3,117,451.35	0.59*	6.13*	1.64	-5.44	-7.69
	(2,107,371.07)	(0.28)	(2.15)	(0.93)	(3.09)	(4.88)
Base slope (TvC)	-417,607.68	-0.04	-0.36	-0.11	0.29*	0.35
	(220,119.02)	(0.03)	(0.26)	(0.15)	(0.15)	(0.57)
Avg change in level (2020)	242,189.53	-1.17***	-5.99*	-0.82*	0.77	-1.21
	(210,423.04)	(0.33)	(2.38)	(0.40)	(0.74)	(0.98)
Post-pre avg change in trend	-27,236.07	0.22***	2.17***	-0.53**	-0.06	0.24
	(58,834.11)	(0.04)	(0.37)	(0.20)	(0.19)	(0.23)
Post-pre change in level (TvC)	-16,202.95	0.14	-3.43	0.70	1.51	-1.21
	(408,912.11)	(0.40)	(2.64)	(0.46)	(0.99)	(1.77)
Post-pre change in trend (TvC)	381,077.83*	0.12	1.73*	0.11	-0.31	-0.43
	(166,819.87)	(0.09)	(0.72)	(0.24)	(0.24)	(0.70)
Constant	84,450.86	1.36***	7.37***	0.08	0.00***	0.00***
	(0.30)	(0.30)	(1.70)	(0.67)	(0.00)	(0.00)
Baseline Tier 1 Mean	19,268,032.53	1.96	17.99	4.01	16.53	28.95
Tier 1 Obs	10	11	11	8	9	7
Control Obs	53	73	75	36	56	54

Standard errors in parentheses
** p<0.05, ** p<0.01, *** p<0.001*

B.3 Quarterly imputed analyses

To address the missing data issue, we imputed missing outcome values within the quarterly data. We imputed values by taking the average of the quarterly values within each year.

Table B3: Presents the quarterly imputed values for the CITS models for each outcome.

	(1) CITS Loans (000sEUR)	(2) CITS ROA (%)	(3) CITS ROE (%)	(4) CITS NPL Ratio (%)	(5) CITS CAR (%)	(6) CITS LCR (%)
Change since start	23,410.39	0.00	-0.18	0.25**	0.03	0.28
	(14,548.62)	(0.03)	(0.20)	(0.09)	(0.06)	(0.15)
Base level diff (TvC)	2,261,714.93	0.53	4.75*	0.23	-4.74*	-9.82*
	(2,325,107.75)	(0.29)	(2.32)	(0.91)	(2.33)	(4.51)
Base slope (TvC)	-373,034.29	-0.03	-0.26	-0.06	0.18	0.21
	(230,047.17)	(0.03)	(0.24)	(0.13)	(0.12)	(0.53)
Avg change in level (2020)	133,193.44	-0.90***	-4.97**	-0.75*	0.08	-0.56
	(137,901.17)	(0.26)	(1.86)	(0.29)	(0.46)	(0.81)

	(1) CITS Loans (000sEUR)	(2) CITS ROA (%)	(3) CITS ROE (%)	(4) CITS NPL Ratio (%)	(5) CITS CAR (%)	(6) CITS LCR (%)
Post-pre avg change in trend	-13,979.74	0.22***	2.11***	-0.43**	-0.16	0.16
	(38,931.94)	(0.04)	(0.31)	(0.16)	(0.12)	(0.19)
Post-pre change in level (TvC)	-649,498.55	-0.04	-3.55	0.51	1.17	0.12
	(887,323.54)	(0.34)	(2.27)	(0.38)	(0.83)	(2.41)
Post-pre change in trend (TvC)	661,138.87*	0.10	1.45*	0.05	-0.16	-0.54
	(337,897.04)	(0.08)	(0.72)	(0.21)	(0.18)	(0.71)
Constant	-1,428,718.73***	1.42***	7.55***	3.08**	12.99*	39.27***
	(146,584.67)	(0.33)	(1.95)	(1.17)	(6.57)	(4.99)
Baseline Tier 1 Mean	19,529,350.86	1.91	16.84	3.79	17.37	31.39
Tier 1 Obs	11	12	12	9	14	9
Control Obs	84	102	102	51	111	83

Standard errors in parentheses
** p<0.05, ** p<0.01, *** p<0.001*

B.4 Synthetic difference-in-difference for Gross Loans (Yearly)

Synthetic difference-in-differences (SDID), developed by Arkhangelsky et al. (2021), builds on traditional difference-in-differences (DID) and synthetic control (SC) methods to estimate the causal impact of an intervention (Arkhangelsky et al. 2021). SDID avoids common pitfalls and offers greater flexibility than both DID and SC methods. Unlike DID, SDID is valid regardless of parallel trends in the data. In comparison to SC, SDID both optimally weights time periods, and allows for the existence of level differences between the treatment and control groups (Clarke et al. 2023).

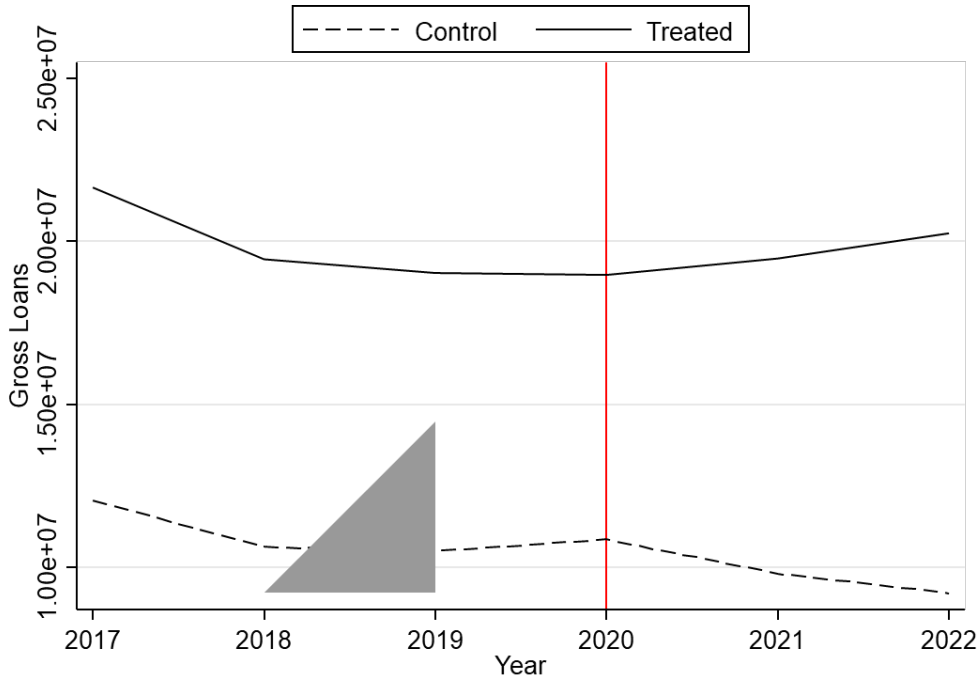
We run this analysis to further support the validity of our results and the CITS model specification, as the SDID (with a new Stata package released during the writing of this paper) also adjusts for pre-trends and allows for diverging trends in the pre-period. The SDID is added as a robustness check, as this model is more recent and the paper by Clarke et al. (2023) explaining the specification and the associated Stata package, was only released in 2023, after the primary analysis for this paper was already completed. However, the results of applying this model for the yearly analysis on lending are very similar to the results in the primary CITS specification, indicating a large positive and significant effect of the SP on lending in the group of banks included in the sample of treated banks. We specified a placebo inference procedure (rather than bootstrap or jackknife) since the number of treated units is small, there is only one period of treatment, and the number control units is strictly larger than the number of treated units.

Table B4: Presents the results of a synthetic difference-in-difference model estimating the impact of the SP on lending.

	(1) SDID Loans (000sEUR)
Average Treatment Effect of the Treated	1,070,000***
	(251,000)

Standard errors in parentheses
** p<0.05, ** p<0.01, *** p<0.001*

Figure B1: illustrates the trends in the treated and control units, as well as which years get weighted more heavily in the SDID model.



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