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Bank lending implications of climate stress tests

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Abstract

Do climate stress tests affect bank credit supply to brown firms? Using a difference-in-differences approach and detailed data on individual bank loans in the euro area, this paper provides novel evidence on the effects of the ECB's 2022 climate risk stress test. Despite no capital implications or public disclosures, participating banks significantly reduced credit to greenhouse gas-intensive industries relative to non-participants. Among affected firms, smaller borrowers were more negatively impacted. Notably, only the best-performing banks in the climate stress test significantly reduce their brown credit after participation. This is evidence that banks which are more advanced in climate risk management more proactively consider transition risks in their lending. In contrast, banks less advanced in managing climate risk do not to the same extent discriminate against polluting firms.

Keywords: Climate Risk, Climate Stress Test, Banking Supervision

JEL Codes: E51, G21, G28

Non-technical summary

In recent years, central banks and banking regulators have become increasingly concerned with the financial risks posed by climate change. While it is not in the mandate of central banks or supervisors to implement climate policies, they recognize that climate change and related policies can affect both the economy and financial stability. To address this, banking regulators have started encouraging banks to manage their exposure to climate-related risks carefully. One key tool are climate stress tests, which evaluate how well banks can handle climate risks. Unlike traditional stress tests, these climate stress tests do not result in penalties or public disclosure of individual results, raising the question of whether this approach is strong enough to prompt meaningful changes in banks' behavior.

This study investigates whether climate stress tests influence banks' lending practices, particularly toward carbon-intensive industry (i.e., "brown") firms. Using data from the European Central Bank's 2022 climate stress test, we examine if participating banks reduced lending to brown firms compared to those that did not participate. We find that banks involved in the stress test did indeed reduce their loans to brown firms. In particular, small borrowers are more adversely affected by participating banks' credit allocation policies following the stress test. However, inherent differences between participating and non-participating banks as well as other factors prevalent in our sample period (e.g., the broader adverse macroeconomic environment) have likely played a role in the lending dynamics we observe before and after the exercise.

To dig deeper, we focus on only the banks that participated in the stress test and examine how their performance in the test influenced lending behavior. Our findings suggest that only the highest-performing banks substantially reduced lending to brown firms, hinting that stronger climate risk management practices might give banks more ability or motivation to cut back on high-emission lending. Importantly, this result holds beyond the consideration of external economic conditions or inherent bank-level differences that could bias our results.

The findings suggest two main policy takeaways: First, only by enhancing their capacity to properly measure and stress test climate risks banks will be able to properly manage the risks. Climate stress tests and other supervisory activities aiming at fostering banks' climate risk management approaches are therefore instrumental in supporting the green transition. Second, the results show that additional measures may be needed, especially for banks that scored lower in climate risk assessments, to encourage industry-wide climate risk management. Both on the regulatory and supervisory side a range of measures are being implemented in order to induce the banking industry to more proactively measure and manage climate-related and environmental risks. Our results therefore underscore the importance of a strong and enforceable supervisory framework, including clearly defined expectations about climate stress testing and risk management more broadly, to ensure that banks more effectively integrate climate risks into their credit policies.

1 Introduction

Central banks and banking supervisors have paid increasing attention to climate change-related implications in recent years. While it is not in the mandate of central banks or supervisors to conduct climate policies, they need to be mindful of the risks to the real economy and the financial sector that climate change and policies aimed to mitigate it may entail.

Against this background, in recent years banking supervisors have provided extensive guidance and supervisory expectations to banks highlighting the importance that they properly and prudently manage their climate risk exposures. One example for such guidance are climate stress tests, in which regulators assess institutions' level of preparedness for properly managing climate risk without imposing disciplining action ex-post. Climate stress tests thus differ from traditional supervisory stress tests in two key aspects: Participating banks do not have to fear capital punishments contingent on their performance and individual bank results are not published.

This lack of ex-post regulatory action raises the question whether such passive central bank climate policy actions are sufficient to induce bank-level responses which alleviate their climate risks. Given the novel nature of climate stress test, the existing literature lacks empirical work convincingly identifying their effects. The current literature therefore cannot fully answer (1) whether climate stress tests induce bank-level changes in credit, (2) whether these changes have firm-level implications, and (3) if so, why that is the case. Finding answers to these questions is important since central banks will need to clearly communicate how any proposed policies are justified within their various mandates. This is especially relevant considering recent threats by the European Central Bank (ECB) to fine banks over climate risk shortcomings.[1]

Do climate stress tests affect bank credit supply to brown (i.e., greenhouse gas intensive sector) firms? Using detailed information on individual bank loans in the euro area,

[1] Some euro zone banks may be fined after missing ECB climate goal, Reuters, 5 June 2024.

we use a difference-in-differences design to study whether, following a climate stress test, participating banks adversely change their credit supply to brown firms, relative to non-participating banks. The ECB's 2022 climate risk stress test, to which a selection of banks was exogenously exposed, provides a suitable setting. Our results show that participating banks reduce their lending to brown firms. In further disentangling credit supply and demand, we find that small brown borrowers are more adversely affected than their large counterparts. However, fixed-effects regressions and robustness tests show that unobservable time-varying bank characteristics and selection effects within treated banks drive the overall results. Indeed, participating institutions are significantly different from non-participating institutions across several dimensions. Moreover, they endogenously lend less than non-participating banks in the months following the climate stress test.

To overcome this limitation and more fully study the effectiveness of the ECB's climate stress test, we restrict our sample to participating banks only and analyze the effect of bank-level performance in the climate stress test. We find that only the best-performing banks significantly reduce their brown credit after participation, pointing to a selection effect: Best-performing banks are likely in a better spot to reduce their brown credit, or more inclined to do so. Scoring banks without any ex-post supervisory action does therefore not suffice as a supervisory pressure instrument.

The rest of the paper is structured as follows. Section 2 provides a brief review of the related literature. Section 3 explains the institutional details of the ECB's 2022 climate risk stress test. Section 4 describes the empirical strategy and data. Section 5 presents the paper's results, followed by robustness tests presented in section 6. Section 7 presents bank performance analyses and section 8 concludes.

2 Related literature

The literature on bank credit and climate transition risks has generally documented that banks price climate policy risk exposure in their lending behavior. Ivanov et al. (2022) find that high-emission firms face shorter loan maturities, lower access to permanent forms of bank financing, higher interest rates, and higher participation of shadow banks in their lending syndicates. Delis et al. (2023) and Chava (2014) also show that banks charge higher loan rates to price-in environmental risk faced by highly-exposed firms. Kacperczyk and Peydró (2022) find that firms with higher carbon footprints previously borrowing from banks which committed to decarbonization subsequently receive less credit. Reghezza et al. (2022) study the Paris Agreement in 2015 as aggregate climate regulatory event and find that European banks reallocated credit away from polluting firms.

As to physical risks, Ouazad and Kahn (2022) show that in the aftermath of natural disasters, lenders are more likely to approve mortgages that can be securitized, thereby transferring climate risk. Nguyen et al. (2022) find that lenders charge higher interest rates for mortgages on properties exposed to a greater risk of sea level risk. Conversely, Murfin and Spiegel (2020) find no evidence of significant valuation effects between otherwise similar homes but for which the time to inundation will differ depending on the pace of the sea level rise. Overall, the literature on bank credit and climate risk suggests that bank-level responses to such risks are already present without direct supervisory intervention.

Another relevant strand of literature studies bank-level and real effects of traditional stress tests. Testing different hypotheses surrounding the effect of bank stress tests on credit supply, Acharya et al. (2018) find support of the Risk Management Hypothesis: Stress-tested banks reduce credit supply particularly to relatively risky borrowers to decrease their credit risk. The authors identify various channels at work which, however, all rely on ex-post supervisory requirements for banks to hold more capital relative to their credit risk exposure. Relatedly, Cortés et al. (2020) show that banks most affected by stress tests reallocate credit away from riskier markets and toward safer ones. They also raise interest rates on small loans. Quantities fall most in high-risk markets where stress-tested banks own no branches, and prices rise mainly where they do. As to real effects, Gropp et al. (2019) find that banks with higher capital punishments reduce lending to corporate and retail customers, resulting in lower asset, investment, and sales growth for firms obtaining a larger share of their bank credit from the such banks. Focusing on public disclosure effects, Flannery et al. (2017) find that stress test disclosures are associated with significantly higher absolute abnormal returns and higher abnormal trading volume, suggesting that they generate significant, new information about stress-tested banks. Schuermann (2014) further finds that the disclosure of results is a critical component of stress tests as it allows to reestablish trust. The literature on traditional stress tests therefore largely establishes that regulatory action in the form of capital punishments or result disclosures are key to induce bank-level responses.

Another set of papers has considered the impact of banking supervision. Relying on new metrics for supervisory scrutiny during the 2016 EU-wide stress test, Kok et al. (2023) find that the disciplining effect on credit risk is stronger for banks subject to more intrusive supervisory scrutiny during the exercise. Hirtle et al. (2020) show that banks that receive more supervisory attention hold less risky loan portfolios, are less volatile, and are less sensitive to industry downturns, but do not have lower growth or profitability. Similarly, Corell and Papoutsi (2024) show that euro area banks pass on the cost of complying with a large-exposure framework to borrowers above the exposure threshold via interest rate premia and reduction in credit. Abbassi et al. (2023) analyze the response of banks' to the asset quality review conducted by the ECB and study the associated real effects. They find that after its announcement, reviewed banks reduce riskier security holdings and credit supply, with negative spillovers on asset prices and firm-level credit availability. This literature therefore suggests that supervisory attention beyond direct regulatory interventions can already induce bank- and firm-level effects.

A recent set of papers has studied climate stress tests in particular. Most of these papers have been normative in evaluating the design of climate stress tests (Acharya et al., 2023; Battiston et al., 2017; Jung et al., 2023). Oehmke and Opp (2022) study "green" capital requirements as a potential regulatory consequence of climate stress tests. They find that higher capital requirements for dirty loans can reduce clean lending, while decreases in capital requirements for clean loans can increase dirty lending. This is because changes in capital requirements affect credit allocation via the marginal loan, which can be clean or dirty.

Given the novelty of climate stress testing exercises, the literature lacks a clear empirical identification of their effects. Fuchs et al. (2023) are the first to empirically study the bank-level and real effects of climate stress tests. The authors find that stress-tested banks in the French bank climate pilot exercise increase loan volumes but charge higher interest rates for high-emitting borrowers. Affected firms increase their climate risk management efforts but do not exhibit changes in their emissions. We contribute to the authors' results by studying the ECB exercise as a relatively more exogenous identification setting and by using more comprehensive credit data. Notably, Fuchs et al. (2023) focuses on the French climate stress test exercise, which involved voluntary participation. Additionally, their analysis is limited to syndicated loans, which may influence the types of banks included in the study.

This paper's first contribution lies in its attempt to reconcile the first two strands of literature highlighted above. We therefore want to shine light on the following questions: Do climate risk specific stress testing and banking supervision affect banks' credit risk management in the context of climate risk? And to which degree are supervisory follow-ups or regulatory punishment necessary to meaningfully impact banks' management of climate risks in their credit business?

As to the literature on climate stress tests, one contribution is to provide more empirical evidence alongside Fuchs et al. (2023). Going beyond, the paper's main contribution lies in its unique position to more convincingly identify (1) bank-level responses to climate stress tests, (2) their firm-level implications, and (3) the underlying mechanisms driving banks' climate risk management in their credit business.

3 The ECB's 2022 climate risk stress test

Climate stress tests are a novel tool to assess banks' resilience to climate risks, with several regulators having completed such exercises in the recent past (e.g., Bank of England in 2021, the French Prudential Supervision and Resolution Authority (ACPR) in 2020, and the Federal Reserve in 2023). While these climate stress testing exercises share common features (e.g., credit risk projections under different climate shock scenarios), the ECB's 2022 climate risk stress test is unique along two key dimensions. First, it is the only exercise in which the names of participating banks are not public knowledge. Second, it is one of the few exercises that was not voluntary, instead targeting 104 significant institutions under ECB supervision at the time. Further institutional details are explained below.

3.1 Timeline and scope

Figure 1 describes the timeline of the exercise. While the actual exercise was implemented in Q1 and Q2 of 2022, the ECB first announced it at the start of Q4 2021. Importantly, this is the official date at which participating banks were first notified about their involvement in the exercise.[2] There was no EU-wide stress test by the European Banking Authority (EBA) and no overlap with other central banks' climate stress tests in 2022.[3]

[2] See Letter to banks. Note that the general announcement that the ECB would run a climate stress test in 2022 was made earlier, at the end of 2020 / beginning of 2021. Until the official notification of individual banks, the Single Supervisory Mechanism (SSM) conducted an industry consultation on the draft methodology of the test. Given the innovative nature of the exercise and the uncertainty about the final methodology and approach, we assume that ex-ante adjustments and anticipation effects before the announcement are not material.

[3] The ECB/SSM however in parallel ran a desktop-based solvency exercise, the 2022 SSM Vulnerability Analysis, in response to the Russian invasion of Ukraine and the energy crisis that followed. It did not involve the banks directly and also did not have any direct capital implications, so should arguably not be influencing the banks' lending decisions.

How does the 2022 climate risk stress test fit into the rest of the ECB's supervisory climate risk exercises? The exercise is just one of several supervisory activities to address such risks in the euro area. Figures A1 and A2 place the exercise and its timeline in the ECB's general climate risk supervisory framework. Both figures highlight the thematic review on climate-related and environmental risks as a possible threat to identification: Considering its content and timeline overlaps, we could potentially pick up bank-level reactions to the thematic review, instead of the climate risk stress test. While this cannot be fully ruled out, the relatively qualitative nature of the former alleviates this concern: The thematic review focused on banks' broader climate risk management approaches, including governance issues. The climate stress test entailed extensive data collections and quantitative analyses, including stress projections, providing banks with more tangible outcomes in terms of their ability to measure climate risk in the first place and the identification of their vulnerabilities to such risks.

3.2 Structure and modules

The climate risk stress test exercise consisted of three distinct modules: Module 1 comprised an overarching questionnaire to assess how banks are building their climate stress test capabilities for use as a risk management tool.[4] Module 2 was a data collection and peer benchmark analysis with the objective to assess whether banks were able to provide good quality climate data and allow for comparing banks across a common set of climate risk metrics. The first metric captured how much banks rely on income from carbon-intensive industries, based on the EU Technical Expert Group on Sustainable Finance taxonomy of NACE-classified industries.[5] This metric referred to the period from 1 January 2021 to 31 December 2021. The second metric captured the volume of greenhouse gas emissions banks finance, based on scope 1 to scope 3 emission intensity metrics and as at 31 December 2020. Finally, Module 3 mimics a bottom-up stress test targeting transition and physical risks, asking banks to provide projections of financial

4 See Climate risk stress test - SSM stress test 2022, Annex A.1 for a detailed overview of the questions.
5 See figure A3 for the full list of industries.

losses (mainly credit risk and to a lesser extent market risk) under a variety of short- and long-term climate scenarios. Figure A4 provides an overview of its structure.

Given the passive stock-taking nature of Module 1 and the uncertainty surrounding projections made in Module 3, we expect any observable bank-level reaction to originate in its completion of Module 2. While we refrain from hypothesizing any mechanisms ahead of establishing a significant reaction in the first place, the qualitative nature of Module 1 and the long-term scope of Module 3 speak against immediate adjustments in credit supply, both upon announcement of and participation in the exercise. Module 2, on the other hand, led to material information production on the side of banks, next to signalling clear metrics that the ECB, as banking supervisor, cares about. Bank-level reactions to Module 2 are also likely to dominate any effects from the thematic review as a passive stock-taking exercise.[6]

3.3 Aggregate results

The key findings of the respective modules can be summarized as follows.[7] Module 1 showed that banks had made considerable progress in their climate stress-testing capabilities. However, the exercise also revealed many shortcomings, data gaps and inconsistencies across institutions: Importantly, just 20% considered climate risk as a variable in their loan decisions; although as we will argue in this paper the climate stress test itself seem to have acted as a catalyst for selected banks to more proactively account for these risks in their lending decision ex-post. Module 2 revealed that participating significant institutions generate non-negligible income from activities related to GHG-intensive industries: The share of interest income related to the 22 most GHG-emitting industries amounted to more than 60% of total non-financial corporate interest income on average. Moreover, banks lacked actual data regarding GHG emissions, with ca. 70% of reported emission intensities relying on proxies. Finally, Module 3 showed that par-

[6] Also consider that the thematic review covered 107 significant institutions and 79 less significant institutions. There are therefore overlaps both in the treatment and control banks used in the empirical strategy, alleviating concerns that the thematic review affects the two groups differentially. See section 4.1.1 for details.

[7] See 2022 climate risk stress test for details on aggregate findings.

ticipating banks are, to varying degrees, exposed to the materialisation of acute physical risks in Europe (e.g., drought and heat events, flood risk). The risks banks are facing in this regard are closely linked to the geographical location of their credit businesses and could in some cases lead to non-negligible losses. Banks further did not have robust long-term transition plans and showed little differentiation between possible long-term scenarios.

Aggregate results therefore showed that there are still many challenges banks are facing with regard to climate risk stress testing. Accordingly, the exercise was deemed a useful learning exercise for banks and supervisors, with a hope to act as a catalyst to strengthen banks' efforts to develop climate risk stress-testing frameworks.[8] Next, we explain how we empirically determine whether such adjustments are reflected in banks' credit business.

4 Empirical analysis

We employ a set of standard triple difference-in-difference specifications to assess the question empirically, whereby we progressively saturate the specification with fixed-effects across multiple dimensions. This allows to (1) establish that any observed effect is robust across different specifications, and (2) determine which unobservable time-invariant or time-varying characteristics are possibly driving the baseline results. The empirical strategy, possible threats to identification, and the data and sample used are discussed in the following subsections.

[8] See ECB report on good practices for climate stress testing as guidance for the industry in overcoming some of these challenges, published by ECB Banking supervision.

4.1 Methodology

4.1.1 Baseline regression

The baseline analysis is done at the bank-firm-month-level and considers the amount (in log of EUR mn) of loans to borrower j from bank b at time t:

$$\begin{aligned}\ln(LoanAmount)_{j,b,t} = {} & \beta_0 + \beta_1 Post_t + \beta_2 Treat_b + \beta_3 Brown_j + \beta_4 (Post_t \times Brown_j) \\ & + \beta_5 (Treat_b \times Brown_j) + \beta_6 (Post_t \times Treat_b) \\ & + \beta_7 (Post_t \times Treat_b \times Brown_j) + \gamma X_{j,b,t} + FE + \epsilon_{j,b,t}\end{aligned} \tag{1}$$

where $Post_t \in (PostAnn_t, PostCST_t)$, capturing possible differential responses to the announcement of (i.e., after October 2021) vs. the participation in (i.e., after July 2022) the climate stress test. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. Recall that the exercise targeted 104 out of 113 significant institutions under ECB supervision at the time. We use the 200 largest less significant institutions as at 31 December 2021 as control banks.[9] $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise.[10]

We progressively saturate this specification with different sets of fixed-effects, namely on a quarter, bank, firm, bank-quarter, firm-quarter, and bank-firm dimension. Standard errors are clustered at the bank level. We further control for time-varying bank-firm characteristics via $X_{j,b,t}$. In particular, we interact the brown indicator with time-varying bank characteristics along which our treatment and control groups significantly differ ex-ante.[11] This is to account for observable characteristics which could affect how the treatment and control banks extend credit to brown firms, beyond their participation

[9] We refrain from using the exempted 9 significant institutions as control banks as they were excluded for specific reasons (e.g., undergoing organizational changes). This renders them less suitable to use as a comparison, as they were affected by other events occurring at the same time. The downsides of using less significant institutions as control banks are their clear differences in nature, e.g. their size or regulatory status. See section 4.2 for a more detailed discussion.

[10] Other metrics could be used to classify firms as "brown", e.g. based on GHG emission intensity ex-ante. We restrict the analysis to the industry-defined classification used by the ECB.

[11] See section 5.1 for details.

in the exercise.[12]

4.1.2 Supply vs. demand

Do adjustments to credit upon participating in the climate stress test stem from changes in bank credit supply or firm credit demand? The baseline regression does not allow to simultaneously estimate and disentangle the credit supply and demand channels. To achieve this, we employ the empirical strategy introduced by Khwaja and Mian (2008), which exploits the granularity of data at hand by using the sample of firms with multiple banking relationships: Using firm fixed-effects, in first-differenced data, we are able to compare how the *same* firm's loan growth from one bank changes *relative* to other more affected (i.e., treated) banks. To the extent this within-firm comparison fully absorbs firm-specific changes in credit demand, the estimated difference in loan growth can be plausibly attributed to credit supply adjustments induced by being a treated bank.

Intensive margin

We run two separate regressions to first consider the intensive margin (i.e., changes in credit supply for existing loans). The first specification uses a standard OLS regression:

$$\begin{aligned}\Delta \ln(LoanAmount)_{j,b} = \beta_0 + \beta_1 Treat_b + \beta_2(Treat_b \times Brown_j) \\ + \beta_3(Treat_b \times Brown_j \times Large_j) + \gamma_1 X_b + \gamma_2 Z_j + \epsilon_{j,b}\end{aligned} \tag{2}$$

Note that we now include an additional firm-level dummy variable to capture differential effects based on firm size: $Large_j$ is equal to 1 for firms with above-median values for log total assets as at 31 December 2021, 0 otherwise.[13] We further control for bank- and firm-level observable characteristics via X_b and Z_j, respectively. Note that the OLS estimates of $\hat{\beta}_1, \hat{\beta}_2, \hat{\beta}_3$ will be biased if the treatment is correlated with unobservable characterstics affecting credit demand contained in $\epsilon_{j,b}$. This is likely the case: Treated

[12] See section 4.2 for examples and an extensive discussion of related identification concerns.

[13] The dummy was not included in the baseline analysis as a four-dimensional interaction term for clarity and expositional reasons.

banks are likely to experience differential demand from brown and large firms given their significance, regardless of their involvement in the exercise. We adopt a new method for identifying bank credit supply via the following, second specification:

$$\begin{aligned}\Delta \ln(LoanAmount)_{j,b} = \beta_1 Treat_b + \beta_2(Treat_b \times Brown_j) + \beta_3(Treat_b \times Brown_j \times Large_j)\\ + \alpha_j + \gamma X_b + \epsilon_{j,b} \qquad (3)\end{aligned}$$

Since the comparison is now across banks for the *same* firm, firm-specific demand shocks are absorbed by the firm fixed-effect α_j. However, this specification still does not control for a possible correlation between treatment status and unobservable characterstics affecting credit supply: Treated and control banks might extend credit differentially to brown and large firms given their differences in nature.[14] Nonetheless, running this analysis is a useful exercise to disentangle credit supply from demand, allowing to at least control for differential effects stemming from firm-level credit demand.

Extensive margin

Does the climate stress test affect the extensive margin of banks by inducing them either to stop lending to firms altogether or reduce the intake of new firms? We test the extensive margin of credit supply using the following OLS and firm fixed-effects specifications:

$$\begin{aligned}Y_{j,b} = \beta_0 + \beta_1 Treat_b + \beta_2(Treat_b \times Brown_j) + \beta_3(Treat_b \times Brown_j \times Large_j)\\ + \gamma_1 X_b + \gamma_2 Z_j + \epsilon_{j,b} \qquad (4)\end{aligned}$$

and

$$\begin{aligned}Y_{j,b} = \beta_1 Treat_b + \beta_2(Treat_b \times Brown_j) + \beta_3(Treat_b \times Brown_j \times Large_j)\\ + \alpha_j + \gamma X_b + \epsilon_{j,b} \qquad (5)\end{aligned}$$

[14] We partially address these concerns with robustness tests in section 6. See section 4.2.1 for a detailed discussion.

where $Y_{j,b} \in (EXIT_{j,b}, ENTRY_{j,b})$. For each loan, we create the outcome variable $EXIT_{j,b}$, which is 1 if the loan is not renewed at some point during the post-period, 0 otherwise. $ENTRY_{j,b}$ is equal to 1 if the loan was made for the first time in the post-period, 0 otherwise. As before, we use the firm fixed-effects approach to control for changes in loan demand at the firm level, and test whether the same firm borrowing from different banks is more likely to exit or enter a loan arrangement with a treated bank.

Note that we run the supply vs. demand analysis only for the participation period. To capture participation effects, $\Delta \ln(LoanAmount)_{j,b}$ considers the change in the log loan amounts prior to and after the participation quarters (i.e., before January 2022 and after July 2022). The extensive margin outcome variables $EXIT_{j,b}$ and $ENTRY_{j,b}$ are constructed using the same logic.

While firm fixed-effects address identification concerns stemming from differential credit demand, we refrain from taking the Khwaja and Mian (2008) approach off-the-shelf without acknowledging that our setting is inherently different. First, the bank-level response to the climate stress test exercise is not entirely exogenous. This is reflected in the ongoing identification concern of differential credit supply of treated and control banks. Second, we cannot cleanly and convincingly control for firm's bank-specific loan demand and its potential correlation with the treatment. While we could partly account for bank-specific loan demand via firm times loan-type fixed-effects, we can only qualitatively make a case for why we do not expect it to correlate with banks' involvement in the climate stress test. We turn to this and other related concerns in subsection 4.2.

4.1.3 Firm-level effects

Next, we provide estimates of the impact of the bank-level treatment on firm-level outcomes such as a firm's total borrowing and other real outcomes. The former examines whether firms can negate the effects of adverse credit supply adjustment due to the treatment from existing banks in aggregate by borrowing from other banks. The latter examines if affected firms experience adverse changes in real variables. We run the following first-differenced equation:

$$\Delta Y_j = \beta_0^F + \beta_1^F \bar{Treat}_j + \gamma X_j + \epsilon_j \tag{6}$$

where ΔY_j is the change in the firm-level attribute of interest. We specifically consider firms' total borrowing from all banks, their default probability, profit margins, total assets, and scope 1 emission intensities. We now also use a firm-level formulation of the treatment indicator $\bar{Treat}_j$, which captures how many of a firm's loans are on average extended by treated banks ex-ante (i.e., we take the average of the bank-level treatment indicator per firm). We use the same logic in constructing firms' exposure to observable bank controls, which are included in X_j, next to the standard firm-level controls. Unlike before, we can no longer put in firm fixed-effects to account for firm-specific factors' possible correlation with the treatment, since the specification is aggregated to the firm level.

4.2 Identification concerns

There are three possible threats to identification: (1) Selection effects via ex-ante differences between treatment and control banks and firms, (2) other events occurring at the same time as the exercise and their possibly differential effect on treatment and control banks and firms, and (3) possible correlation between bank-specific loan demand and a bank's treatment status. We now explain these identification concerns and our attempts to address them.

4.2.1 Selection effects

Results could be driven by ex-ante differences between treatment and control banks. While time-invariant and time-varying observable and unobservable differences can be controlled for via the inclusion of bank and firm control variables and various fixed-effects, the concern remains if such differences determine the treatment status or differentially affect brown lending. This is likely the case: For example, significant treatment banks are large by nature and thus more likely to maintain lending relationships with large firms which tend to belong to GHG-intensive industries. Or treatment banks are by nature more heavily regulated which perhaps makes them more susceptible to the “pro-green” policy environment, endogenously restricting their credit supply to brown firms.

To the extent that these differences are observable, they can be controlled for using the triple-dimension control variables $X_{j,b,t}$ in the baseline regression: $X_{j,b,t}$ captures the correlation of such observable characteristics with the respective bank’s tendency to extend credit to brown firms. Concerns arise when differences are unobservable, as the variation of the data does not allow to interact bank-quarter fixed-effects with the brown indicator. We acknowledge these limitations and provide a detailed discussion of observed effects in section 5.2. Moreover, robustness tests in section 6 restrict the sample of banks to the 3 smallest significant treated institutions and 3 largest less significant control institutions for each country. This aims at making the treatment and control banks more comparable, which allows to further determine how much results are driven by bank-level differences.

4.2.2 Other events

Recalling the timeline of the exercise in figure 1, the second clear identification concern is that other events occurring at the same time of the exercise could differentially affect treatment and control banks and firms. The most obvious example is the period of high inflation starting in Q3 2021 and having reached its peak in Q3 2022, exacerbated by the supply shock caused by the Russian invasion of Ukraine in February 2022. Given that energy costs in particular were rising at the time, it is plausible that firms belonging to GHG-intensive industries have been more adversely affected.

Again, effects stemming from events outside of the climate stress test are problematic if they affect treatment and control banks differently. On the one hand, considering treated banks' possible tendency to lend to brown firms, their susceptibility to high inflation would point to a potentially adverse reaction of treated banks' credit supply to brown firms. On the other hand, treated banks' regulatory status requires them to hold much more capital against possible losses and cash outflows, putting them in a relatively better position to supply credit to newly distressed brown firms. Of course, there are many more viewpoints and possible stories to consider here. In any case, the issue again boils down to unobservable differences between treatment and control banks possibly subjecting them to greater vulnerability to high inflation, and thereby affecting their credit supply to brown firms. As with selection effects, not much can be done to convincingly account for this beyond the detailed discussion of results and robustness checks.

4.2.3 Correlation between bank-specific loan demand and treatment

While we could partly account for bank-specific loan demand via firm times loan-type fixed effects, it may very well be that a brown firm borrowing from two different banks (one treated and the other not) might shift its credit demand to the non-treated bank upon learning about the treated bank's status. The bank-specific loan demand and treatment status may therefore be correlated. This is particularly the case if we assume that a brown firm expects a restriction in future credit supply if a bank participates in the climate stress test.

Is this assumption plausible? Generally, yes. However, we would argue that two key features of the ECB's exercise render this assumption less plausible. First, the list of participating banks is not public. Firms would therefore not know about their bank's participation without inquiring at the bank or using any other form of insider information. This cannot be ruled out, but we would refer to the second key feature to alleviate this concern: The lack of regulatory consequences of the exercise. Is it plausible to assume that brown firms would go beyond their usually short-term profit-driven considerations and adjust their loan demand if they know that whatever exercise their creditors are subjected to do not have material consequences for them? We would argue less so.

Evidently, some identification concerns can be invalidated more convincingly, while others remain open issues. We acknowledge the limitations of our setting and consider these concerns when interpreting the empirical results in section 5.

4.3 Data and sample

The empirical analysis requires data on bank credit supply as key outcome variable, bank and firm control data to control for relevant observables, and data on firm emissions. We obtain monthly loan data from *AnaCredit*, which contains loan-by-loan information on credit to companies and other legal entities extended by credit institutions and their foreign branches. *AnaCredit* contains highly granular credit and credit risk data, starting at loan amounts as small as 25,000 EUR. In particular, we obtain creditor and debtor identifiers, as well as respective outstanding loan amounts at monthly frequency, debtor and creditor-level country data, debtor-level NACE-based sector data, and instrument-level interest rate spreads and probabilities of default.

Using obtained creditor identifiers, we use *FINREP* and *COREP* as additional supervisory data sources for quarterly bank control data. *FINREP* (Financial Reporting) and *COREP* (Common Reporting) are standardized reporting frameworks used by financial institutions within the European Union to report their regulatory capital and financial information to supervisory authorities. In particular, we obtain balance sheet and income statement data including assets, liabilities, equity, income, and expenses from *FINREP*, while solvency ratios, leverage ratios and liquidity coverage ratios are obtained from *COREP*. Accordingly, we are able to control for a range of bank-level characteristics possibly influencing credit supply.

Finally, we link our debtor identifiers to *Orbis* for yearly firm control data. *Orbis* is a global database which has information on descriptive company information and fundamentals, particularly on assets, net income, return on assets and return on equity, liquidity ratios, and solvency ratios. In addition, we obtain annual firm-level emissions data from *ISS/Factset*, specifically on GHG emissions for Scope 1, 2, and 3, and respective GHG intensities. We can thus control for a variety of firm-level characteristics that might affect credit demand. Importantly, in addition to our comprehensive inclusion of controls, the granularity of our data on the bank-firm-time dimension allows us to absorb much of the variation driving credit supply and demand via firm-time and bank-time fixed-effects.

The final sample for the baseline analysis consists of ca. 1.2 million loans, extended to 12,242 distinct firms and originated by 259 banks. The banks consist of 101 (out of 104) participating significant treatment institutions and 158 (out of the largest 200) less significant control institutions as at 31 December 2021.[15] The timeframe runs from January 2020 until September 2023.

5 Results

5.1 Descriptive statistics

Table 1 presents summary statistics comparing treatment and control banks and their respective borrowers across several dimensions. The last column uses Imbens and Wooldridge (2009) normalized differences to test for significant differences between the groups.[16] Table 1 shows that there are significant differences between treatment and control banks, particularly in terms of their size, defaulted loans, net income, liquid assets, and the size of their borrowers. Treated banks also service significantly smaller firms. There are no significant differences when considering GHG emission intensity metrics or the ex-ante fraction of borrowers belonging to GHG-intensive industries.

It is not surprising that treatment and control banks differ so significantly given their inherent differences in nature as significant vs. less significant institutions. To address concerns whereby these differences could drive any observed effect in credit

15 Some banks drop out due to missing control data.

16 Estimations are able to balance covariates if normalized differences lie within a range of 25 percentage points around zero.

supply and demand, we construct the triple-dimension control variable $X_{j,b,t}$ to include all of the significantly different observable characteristics shown in table 1. In particular, we interact each time-varying bank-level variable with the firm-level brown indicator. This captures treatment banks' tendency to extend credit to brown firms differentially from control banks, where this tendency is driven by these observable characteristics.

5.2 Baseline results

Table 2 presents baseline regression results, starting with the baseline specification and progressively saturating it with different sets of fixed-effects. Column (9) is the most saturated specification, controlling for time-varying bank- and firm-level unobservables as well as time-varying bank-level observable characteristics possibly influencing brown lending via $X_{j,b,t}$ (included in "Controls"), and bank-firm relationships. The baseline results are robust to clustering standard errors on a bank-industry level (see table A1).

The coefficient on $PostAnn_t \times Treat_b \times Brown_j$ is consistently negative, although it turns significant only from column (5)-(8). The coefficient turns insignificant again once bank-firm relationships are accounted for in column (9). Table 2 generally indicates that the announcement of the climate stress test alone has not induced any significant differential reduction in lending between treated banks and brown firms. The insignificance in column (9) indicates that any prior significant effect is driven by unobservable bank-firm characteristics. That the coefficient turns less negative upon inclusion of bank-firm fixed-effects suggests a downward bias: It could be that treated banks have relatively stronger lending relationships with non-GHG-intensive industry firms, inducing higher loan volumes.[17]

The coefficient on $PostCST_t \times Treat_b \times Brown_j$ is consistently negative, turning insignificant only in column (6) upon inclusion of bank-quarter fixed-effects.[18] This

[17] This is intuitive considering that non-brown firms in the sample include financial corporations.

[18] The coefficient turns significant again in column (8) where firm fixed-effects have been replaced with firm-quarter fixed effects. However, this comes from the reduction in noise as can be seen in the reduced standard errors, as opposed to the removal of a firm-driven bias in the coefficient.

indicates that the results are driven by time-varying bank unobservable characteristics. Specifically, the downward bias across alternative specifications indicates that the treated banks from which brown firms obtain credit after the climate stress test on average extend reduced loan amounts in the quarters following the exercise across borrowers, independent of the climate stress test and firm brownness.[19] Interestingly, the bias seems to not be present when considering the credit brown firms obtain from treated banks upon announcement of the exercise. This suggests that the time-varying bank unobservable characteristic which drives loan reductions across borrowers in treated banks must have an enhanced effect in the post-exercise period.

At the first instance, the downward bias suggests that these banks extend smaller loan amounts to borrowers generally, and particularly so after July 2022. This is in line with the observation from table 1 that treated banks service significantly smaller firms, which likely require smaller loan amounts. What could make this bias particularly strong in the period after the exercise? For instance, it could be that the time of high inflation affecting small firms more adversely at the time could have induced treated banks to extend less credit to such firms after the exercise (see section 4.2.2). Note that there is also a negative correlation between being a large firm and the firm-level brown indicator in the data. This speaks to the possibility that newly vulnerable small firms are also more likely to be brown firms, which have arguably been more strongly affected by high energy cost inflation.

[19] The interpretation of the bias stems from the following (simplifed) omitted variable bias equation:

$$\beta^{FE1} = \beta^{FE2} + \beta^{log(LA)_{j,b,t}}_{log(\bar{L}A)_{b,t}} \times \frac{Cov(PostCST_t \times Treat_b \times Brown_j, log(\bar{L}A)_{b,t})}{Var(PostCST_t \times Treat_b \times Brown_j)}$$

whereby β^{FE1} represents the coefficient on $PostCST_t \times Treat_b \times Brown_j$ for column (5) and β^{FE2} represents the respective coefficient for column (6). As $\beta^{FE1} < \beta^{FE2}$, the bias term is negative. Note that the bank-quarter fixed-effect effectively demeans the outcome variable in the bank-quarter dimension, i.e. the "omitted variable" included in the FE is $log(\bar{L}A)_{b,t}$. $\beta^{log(LA)_{j,b,t}}_{log(\bar{L}A)_{b,t}}$ (i.e., the effect that the average loan amount banks extend at different points in time across borrowers has on any given loan amount to a borrower) is likely positive. There must therefore be a negative correlation between being a brown firm receiving credit from a treated bank after the climate stress test and the average loan size that these banks extend across borrowers in the quarters following the exercise.

The key conclusion to draw here is that selection effects as primary identification concern materially affect the differential impact of the climate stress test. It is hard to argue that the climate stress test has a clear causal effect in reducing brown lending considering that the coefficient turns progressively less negative. It is unlikely that the coefficient keeps its distance from the zero mark once the time-varying bank unobservables are more clearly accounted for in the robustness check in section 6.[20]

This conclusion is even clearer when considering figure 2, which shows the time-varying coefficients on the interaction of $Treat_b \times Brown_j$ with quarter dummies, relative to 2021 Q2. Figure 2 entails the most saturated specification taken from column (9) of table 2. It is evident that there is no significant effect upon announcement of the exercise in 2021 Q4, and only partial significance after the exercise has been completed after 2022 Q2. The lack of pre-trends visible in figure 2 suggests that whatever differential impact treated and control banks have experienced in the sample period appeared, or at least became stronger, after the initiation of the exercise.

5.3 Supply vs. demand results

Intensive margin

Table 3 presents intensive margin credit supply results. Columns (4)-(6) consider naive OLS regressions while columns (1)-(3) include firm fixed-effects to identify changes in credit supply, respectively. OLS regressions indicate that treated banks, and among those large firms in GHG-intensive sectors in particular, experienced significant increases in credit. Of course, these results are biased by the presence of differential firm credit demand, which is accounted for in the fixed-effects regressions. Accordingly, columns (1)-(3) show that within the same firm, upon participation in the exercise, treated banks extend significantly more credit, however, significantly less so to small brown firms. The

[20] In fact, if we include a triple-dimension control variable interacting time-varying firm size with the treatment indicator, the coefficient remains insignificant until column (7), after which any significance comes from noise reduction. This suggests that much of the bias is already accounted for using observable characteristics.

relative reduction upon participation is almost fully negated by being a large brown borrower. In effect, large brown borrowers do not experience a significant reduction in credit supply from treated banks compared to non-brown firms.

Comparing OLS to FE columns allows for useful insights: Not including firm fixed-effects introduces a positive bias in the $Treat_b \times Brown_j$ coefficient. This indicates that small brown firms receiving credit from treated banks on average exhibited large increases in credit at the time (driven by demand factors). Interestingly, the bias is slightly negative for the $Treat_b$ coefficient, meaning that overall firms receiving credit from treated banks on average exhibited decreases in credit demand at the same time. The source of demand-driven bias must therefore differentially affect brown firms. In particular small ones, as large brown firms do not exhibit differential changes in credit demand that introduce any bias in the OLS regression.

Extensive margin

Table 4 presents extensive margin credit supply results. Columns (1)-(3) consider whether an existing loan and as such an existing bank-firm relationship has been terminated in the post-period, differentiating between firm fixed-effects in columns (1) and (2) and naive OLS specifications in column (3). Columns (4)-(6) consider whether a new loan and as such a new bank-firm relationship has been formed in the post-period, again differentiating between firm fixed-effects and naive OLS specifications. Starting with the latter columns, i.e. loan entry in the post-period, we obtain close to zero coefficients on the relevant indicators. Generally, treated banks are significantly less likely to extend a new loan when considering the same firm. However, there is no differential effect for brown firms, regardless of their size.

When considering loan exits, columns (1) and (2) indicate that treated banks are significantly less likely to exit an existing loan within the same firm. This effect is even stronger if the existing loan is with a large brown borrower. When comparing OLS to FE results, there is again a biasing effect of credit demand on the extensive margin. Interestingly, the direction now hinges upon the size of the firm: Small brown firms receiving credit from treated banks exhibit on average less (demand-driven) exits of such banks. Conversely, large brown firms exhibit more. This is intuitive considering that large brown borrowers are likely to have more outside options to obtain credit.

Why are treated banks significantly less likely to exit an existing loan after participating in the exercise if the borrower is large and brown? There may be a bank-firm relationship component that depends on firm size that the baseline result did not control for: While treated banks may generally be less inclined to maintain lending relationships with firms after the climate stress test, this tendency may be more pronounced for the presumably less profitable, smaller clients. This preference could be bolstered by the latter being more adversely affected by macroeconomic developments at the time (e.g., high inflation). The story becomes more intuitive when again considering that treated banks lend to significantly smaller firms ex-ante (see table 1). This exposure perhaps makes them more susceptible to not wanting to lose more large clients, independent of their brownness.

5.4 Firm-level results

Table 5 presents firm-level effects that the participation in the exercise had on aggregate firm borrowing. The most saturated column (3) shows that higher exposure to treated banks led to significant increases in aggregate borrowing for large brown relative to small brown firms. Surprisingly, small brown firms with higher exposure to treated banks did not experience reduced aggregate borrowing, relative to non-brown firms.

Table 6 presents effects on additional real outcome variables, namely firms' default probability, profit margin, total assets, and scope 1 emission intensities. There is no meaningful impact of the exercise on firms' default probability, while small brown firms with greater exposure to treated banks exhibit significant reductions in profit margins. This reduction is significantly stronger for large brown firms. The latter also exhibit significant increases in their asset size relative to small brown firms across both tables. Perhaps most surprisingly, more heavily exposed small brown firms exhibit significant increases in scope 1 emission intensities, partially negating the respective significant decrease for non-brown firms. Large brown firms with greater exposure to the treatment exhibit an even stronger decrease in scope 1 emission intensities when compared to respective small brown firms, as well as to respective large firms in general.

How should we interpret these findings? In line with the previous section, table 5 indicates that large brown firms are in a relatively better position to substitute their credit. Results from table 6 are harder to reconcile with previous findings: It is surprising that "treated" small brown firms do not experience any adverse effect on their default probability after experiencing cuts in credit supply. On the other hand, table 5 also shows that small brown firms have not reduced aggregate borrowing, suggesting that they could substitute their foregone credit. However, if this story holds, it is less clear why both small and large brown firms see significant reductions in their profit margins. Scope 1 emission intensity outcomes are also puzzling, as large brown borrowers experience presumably less strong external pressure to adjust their "brownness" if they continue to obtain (or partly even increase) their credit. One could argue that small brown borrowers significantly increase their scope 1 emission intensities because the cut in credit from treated banks hinders them to actively transform the footprint of their business models (Fuchs et al., 2023). However, this story goes against their ability to substitute their credit in aggregate.

We can therefore not fully reconcile firm-level results with previously established findings. However, note that a clear identification of firm-level effects is not possible as there are multiple biases (on a bank- and firm-level) that cannot be controlled for. This is because the regression is done on the firm-level. While supply- and demand-driven biases could be more easily disentangled and interpreted in the preceding results, the present regressions can merely control for observable characteristics. We hence refrain from putting much weight on the observed effects.

6 Robustness

The empirical analysis has already indicated that the observed effects are likely coming from significant differences between treatment and control banks: Including time-varying bank fixed-effects eliminates the significant reduction in lending to brown firms upon participating in the climate stress test (see table 2). Regressions disentangling supply from demand are partly robust. However, they do not allow for the inclusion of bank fixed-effects, leaving them vulnerable to the same identification concern.

The following robustness test aims to alleviate this concern by rendering the treatment and control banks more comparable. The sample construction, empirical strategy, and respective results are outlined below.

6.1 Sample construction and empirical strategy

While table 1 has shown that treatment and control banks differ significantly across multiple variables, the most obvious difference is their size. There are multiple criteria for determining whether banks are considered significant, however, their size is the most readily available proxy in our setting.[21] Accordingly, our strategy to restrict the sample at hand to comparable treatment and control banks uses size-based country cut-offs: For each eurozone country, we restrict our sample to only the three smallest treated and three largest control banks based on their total asset size as at 2021 Q2 (i.e., right before the climate stress test was initiated).

The empirical strategy is to repeat the main analysis with the updated robustness sample. Accordingly, we rerun specifications (1)-(6). The sample now consists of ca. 160,000 loans, extended to 4,619 distinct firms and originated by 66 banks. The banks consist of 34 participating significant treatment institutions and 32 less significant con-

[21] According to the SSM Regulation and SSM Framework Regulation, significant banks must fulfil at least one of the following criteria: (i) the total value of its assets exceeds €30 billion; (ii) the bank is economically significant for the specific country or the EU econoour as a whole; (iii) the total value of its assets exceeds €5 billion and the ratio of its cross-border assets/liabilities in more than one other participating Member State to its total assets/liabilities is above 20%; (iv) it has requested or received funding from the European Stability Mechanism or the European Financial Stability Facility; (v) A supervised bank can also be considered significant if it is one of the three most significant banks established in a particular country.

trol institutions.

An ideal robustness check would test whether the observed effects, particularly for the supply vs. demand analysis, still hold when significant differences between treatment and control banks are removed. Table 7 shows that even when restricting our sample ex-ante, the setting does not allow for such an ideal robustness test: There are still significant differences between the treatment and control banks across several variables, albeit with reduced magnitudes. In particular, treatment banks are still significantly larger than control banks and lend to significantly smaller firms. Moreover, treatment banks now have significantly larger CET1 ratios.[22]

The lack of size homogeneity between treatment and control banks in the robustness sample indicates that the identification concerns will not be perfectly resolved. In particular, if the groups are still significantly different across known dimensions, they are also likely to be very different in their unobservable characteristics. Nonetheless, it will be quite indicative if the main results already prove sensitive to the current sample adjustment: If the established results no longer hold with only attenuated differences between treatment and control banks, there is likely no causal effect of participating in the climate stress test.

6.2 Results

Table 8 shows regression results equivalent to table 2 for the robustness sample, rerunning regression (1). As expected, the sign of the coefficient on $PostCST_t \times Treat_b \times Brown_j$ now is ambiguous across different specifications, only becoming negatively significant in the most saturated specification in column (9). In fact, the column (6) coefficient even becomes significantly positive upon the inclusion of bank-quarter fixed-effects, again pointing toward a downward bias driven by on average smaller loan amounts extended by treated banks. Table 8 confirms that previously established results are to a large part

[22] Note that we already control for this in the triple-dimension control variables.

driven by bank-level differences in the treatment and control groups.[23]

Figure 3 repeats the time-varying regression for the restricted sample. The time-varying coefficients are relatively less noisy and more robustly negative compared to figure 2. However, note again that the figure is based on column (9) as the most saturated specification, which remains significantly negative in the robustness test. Considering its lack of significance and partly positive sign in table 8, it is hard to argue that the significant reduction in brown lending is robust to different specifications.

Tables 9 and 10 repeat the intensive and extensive margin supply vs. demand regressions for the participation period. Generally, when restricting the sample to more comparable treatment and control banks, significant credit supply adjustments using firm fixed-effects regressions no longer obtain. Table 9 is an exception and contains a surprising result: Large brown borrowers obtain significantly less credit from treated banks upon participation in the exercise. No equivalent effect is visible for their small and their non-brown counterparts. This result opposes table 3, where it was small brown borrowers which obtained differentially less credit from treated banks, and this credit reduction was compensated by being a large brown borrower.

What is driving this result? One notable difference when comparing tables 2 and 8 is that the introduction of firm-quarter fixed effects points to a positive bias coming from time-varying firm unobservable characteristics: The change in the coefficient from column (5) to (7) (and (8)) is quite sizeable and negative. This was not present in table 2. This means that brown firms receiving credit from treated banks after the climate stress test on average exhibit increased loan amounts ex-post across banks, regardless of their treatment status. Table 9 indicates that this bias is present mostly for large brown firms. This is intuitive as large firms are likely to obtain larger loan amounts. This finding at least partly explains why, once firm-level demand is controlled for, we observe a significant reduction in credit supply granted to large brown firms. In any

[23] Note that we also reran the regressions for all specifications on a country-level, i.e. only including the six treatment and control banks for each country in separate regressions. The results become more mixed and less straightforward to interpret across every country.

case, this remains the only significant supply-driven credit adjustment across the intensive and extensive margin robustness results. The fact that much of it is likely coming from removing a firm-level bias speaks to the lack of exogenously driven credit supply adjustment upon participating in the climate stress test.

Table 12 reruns firm-level total borrowing and real outcome regression analyses for the participation period. The most notable difference to the baseline analysis is that small brown borrowers now obtain differentially less credit in the aggregate, both upon announcement of and participation in the exercise: Table 11 exhibits a significantly negative coefficient of the $T\bar{reat}_f \times Brown_f$ interaction term. The credit supply reduction is more than overcompensated by being a large brown borrower, in line with the time-varying firm bias present in table 8. Table 12 is not very different from its baseline counterpart, except for the lack of significant effect on profit margins and emission outcomes.

Note that table 9 has actually shown that small brown borrowers do not obtain differentially less credit from treated banks. They therefore do not have to substitute any foregone credit, however, still obtain less in the aggregate (i.e., from both treated and control banks). While this is only suggestive, it could again point to ex-ante vulnerabilities to inflation risks materializing after the stress test. However, now the difference in the degree to which treated and control banks are exposed to such vulnerable small firms is smaller (see table 7), causing both groups to restrict credit supply.

Overall, the robustness test has shown that the established baseline results are highly dependent on sample characteristics, particularly the ex-ante differences between treatment and control banks. This finding is not surprising, as the baseline analysis has already shown some sensitivity to endogeneous characteristics of the setting, reflected via bank- and firm-level biases. Of course, one could still argue that the actual "shock" component of the exercise reflected in coefficients of the most saturated specification remains significantly negative. However, it is crucial to understand where any reaction from affected banks and firms originates. A disappearance or reversal of effects once bank- and firm-level endogeneity is controlled for points to (1) the need to potentially reconsider the test design and sample selection for future exercises, and (2) the question whether more stringent regulatory evaluations are needed to obtain meaningful adjustments in banks' credit business. We turn to the latter in the next section.

7 Bank-level scoring effects

In the final part of the analysis, we analyze whether banks' ex-post credit supply to brown firms depends on the score they obtain after the exercise. The ECB allocates a performance score to treated banks upon participation in the exercise, ranging from 1 to 4 (with 1 being the best). The scoring mechanism is of a relative nature: The score indicates the level of preparedness of individual banks in comparison with their peers. The distribution of the score can be taken from figure A5.

The empirical strategy is to repeat the baseline regression, whereby we restrict our sample to participating significant banks only, and the treatment indicator $BadScore_b$ is defined as receiving a "bad" score, namely an above median score. $BadScore_b$ is then equal to 1 for banks receiving a score above 2, 0 otherwise. Beyond the baseline result, we further analyze the credit response of banks differentiating by their individual scores: In addition to rerunning the baseline triple-difference regression using the $BadScore_b$ indicator as treatment, we interact the $PostAnn_t \times Brown_j$ and $PostCST_t \times Brown_j$ with score dummies for each score, where $Score_b = 1$ is the reference group. This is to better understand differential banks' responses based on their scoring category.

Table 13 shows the balanced table for the restricted sample, differentiating between banks receiving an above (bad-score banks) vs. below (good-score banks) median score as treatment and control group. As expected, the banks in the sample are now more comparable, only differing in that bad-score banks are significantly less profitable and have less liquid assets pre-treatment. As before, these differences are accounted for via their interaction with the $Brown_j$ dummy and inclusion as triple-dimension controls.

Table 14 reports results for the repeated baseline analysis, showing no differential response of bad scoring banks upon announcement of and participation in the exercise. Differentiating effects by individual scores as reported in table 15 allows for more useful insights: The results are much more robust to the inclusion of fixed-effects, whereby the coefficients on the scoring dummies interacted with $PostCST_t \times Brown_j$ remain significant throughout. This is particularly the case for the reference group, i.e. the best-performing banks. Table 15 therefore points to a selection effect: Best-performing banks are the ones to significantly reduce their credit supply to brown firms upon participating in the exercise. The other scoring groups exhibit a relatively significant increase in brown lending, fully negating the significant reduction of the best-performing banks as a reference group.

This is an important finding, suggesting that banks who are more advanced in measuring and managing climate-related risks more proactively take such risks into account in their lending decisions. Other banks who display deficiencies in their climate risk measurement and stress testing capabilities, and therefore arguably at managing this risk, do not seem to discriminate between brown respectively green borrowers in response to the stress test exercise.

8 Conclusion

Did the ECB's 2022 climate risk stress test affect bank credit supply to brown firms? This paper has shown that the significant reduction in credit supply stems from the tendency of treated significant institutions to extend less credit ex-post and to smaller firms ex-ante, relative to less significant control institutions. We conjecture that the climate stress test, by raising awareness and giving a sense of urgency to the industry, is likely to have acted as a catalyst for selected banks to better integrate climate risk considerations into their lending decisions. This seems to have been especially the case for the banks who are more advanced in their climate risk measurement capabilities (i.e., those banks who received higher performance scores in the exercise). In other words, those banks who were found to be better at measuring climate risks, appear to be more discriminative in their lending decisions after the exercise.

The results should be taken with due caution. Despite an extensive number of robustness checks and econometric controls, we cannot rule out that other factors have played a role in the lending dynamics we observe before and after the exercise. Indeed, the reduction in lending to brown firms has likely been bolstered by the broader adverse macroeconomic environment such firms had to face after the climate stress test. Moreover, the results are admittedly less robust when restricting the sample to more similar treatment and control banks.

There are two key policy implications that can be drawn from the findings of this paper. First, only by enhancing their capacity to properly measure and stress test climate risks banks will be able to properly manage the risks.[24] Climate stress tests and other supervisory activities aiming at fostering banks' climate risk management approaches are therefore instrumental in supporting the green transition. Importantly, properly understanding the risks related to climate risks also puts banks in a better position to support the financing of the green transition. Second, as the weaker performing banks in the climate stress test did not appear to significantly integrate climate risk considerations into their lending decisions in response to the exercise, continued emphasis for banks to align with supervisory expectations may be warranted. Both on the regulatory and supervisory side a range of measures are being implemented in order to induce the banking industry to more proactively measure and manage climate-related and environmental risks.

This paper demonstrated that the ECB 2022 climate risk exercise acted as a catalyst for selected banks to start better integrating climate risks into their risk management systems and business decisions. The stress test was a learning exercise with no capital

[24] This argument is emphasised in the 2024 ECB Supervision Blog by F. Elderson: You have to know your risks to manage them – banks' materiality assessments as a crucial precondition for managing climate and environmental risks.

implications. Therefore, the mixed results in terms of its impact on banks' lending decisions are not surprising. In the future, this underlines the importance of a strong and enforceable supervisory framework, including clearly defined expectations about climate stress testing and risk management more broadly, to ensure that banks more effectively integrate climate risks into their credit policies.

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Tables

Table 1: Comparison between treated and control banks (pre-treatment)

Variable	Treated		Control		Treated - Control
	Mean	SD	Mean	SD	Norm. diff.
Total assets (ln)	26.72	1.2	23.29	0.52	3.71*
Loans (defaulted) (ln)	22.62	1.27	18.72	1.11	3.26*
Net income (EUR mn)	577.0	2.1e+9	28.6	6.97e+7	0.37*
Liquid assets (ln)	25.07	1.16	21.42	0.74	3.75*
CET1 ratio	0.146	0.02	0.149	0.02	-0.15
Firm: S1 emission int. (ln)	2.08	2.47	2.04	2.38	0.02
Firm: S3 emissions int. (ln)	6.16	1.12	6.18	1.12	-0.02
Firm: Brown (sector dummy)	0.51	0.50	0.45	0.50	0.11
Firm: Total assets (ln)	19.11	1.93	19.75	1.96	-0.33*
Firm: Net income (EUR mn)	26.0	6.2e+8	75.4	5.13e+8	-0.09
Firm: ROA	1.73	6.66	2.08	6.61	-0.05
Firm: Liquidity ratio	1.13	1.57	1.24	1.08	-0.08

Notes: The table shows summary statistics of selected covariates separately for banks in the treatment group and the control group before 2021 Q3. For each bank group, the mean and standard deviation (SD) for each covariate is shown. The last column shows normalized differences as in Imbens and Wooldridge (2009), i.e. difference in means is normalized with the sum of variances. A star (*) indicates that the normalized difference is outside of the range ±0.25 (which serves as a rule of thumb).

Table 2: Log loan amounts (Cluster: Bank)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treat_b$	−1.31***	−1.17***		−0.81***					
	(0.14)	(0.15)		(0.13)					
$PostAnn_t$	−0.01	−0.01	−0.11**	−0.26***	−0.22***				
	(0.05)	(0.06)	(0.05)	(0.08)	(0.05)				
$PostCST_t$	−0.64***	−0.69***	−0.65***	−0.63***	−0.60***				
	(0.06)	(0.05)	(0.05)	(0.05)	(0.04)				
$Brown_j$	−0.26***	0.96	0.76						
	(0.08)	(0.82)	(0.60)						
$Treat_b \times PostAnn_t$	−0.01	−0.01	0.08	0.21***	0.18***		0.21***		
	(0.06)	(0.06)	(0.05)	(0.08)	(0.05)		(0.03)		
$Treat_b \times PostCST_t$	0.72***	0.73***	0.70***	0.69***	0.67***		0.35***		
	(0.06)	(0.06)	(0.05)	(0.04)	(0.03)		(0.04)		
$Treat_b \times Brown_j$	0.07	−0.11	0.02	0.06	0.17	0.16	0.13	0.10	
	(0.15)	(0.13)	(0.14)	(0.17)	(0.12)	(0.12)	(0.11)	(0.11)	
$PostAnn_t \times Brown_j$	0.03	0.04	0.09	0.40***	0.27***	0.26***			
	(0.06)	(0.06)	(0.06)	(0.13)	(0.07)	(0.07)			
$PostCST_t \times Brown_j$	0.24***	0.26***	0.15***	0.15***	0.13**	0.03			
	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)	(0.04)			
$PostAnn_t \times Treat_b \times Brown_j$	−0.07	−0.05	−0.05	−0.34**	−0.22***	−0.22***	−0.16***	−0.10***	−0.03
	(0.08)	(0.08)	(0.07)	(0.13)	(0.08)	(0.08)	(0.04)	(0.03)	(0.02)
$PostCST_t \times Treat_b \times Brown_j$	−0.27***	−0.23***	−0.15**	−0.18***	−0.17***	−0.05	−0.15***	−0.05***	−0.05***
	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)	(0.05)	(0.03)	(0.02)	(0.01)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Bank FE	No	No	Yes	No	Yes	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes	No	No	No
Bank-Quarter FE	No	No	No	No	No	Yes	No	Yes	Yes
Firm-Quarter FE	No	No	No	No	No	No	Yes	Yes	Yes
Bank-Firm FE	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.02	0.03	0.09	0.48	0.50	0.51	0.54	0.55	0.86
Num. obs.	1237037	1163543	1163543	1163543	1163543	1163543	1163543	1163543	1163543
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the main triple DiD regression results following equation 1, whereby each column progressively saturates the regression with fixed-effects. The outcome variable of each column is the log of the loan amounts outstanding between bank b and firm j in month t. $PostAnn_t$ is equal to 1 in the period following the announcement of the climate stress test, 0 otherwise. $PostCST_t$ is equal to 1 in the period following participation in the climate stress test, 0 otherwise. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. Controls include triple-dimension interactions between the firm-level $Brown_j$ dummy and bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios, respectively. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 3: Credit demand vs. supply — Intensive margin (Participation)

	Δ Log loan amount					
	FE			OLS		
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
$(Intercept)$				-0.44^{***}	-0.19^{***}	-0.19^{***}
				(0.03)	(0.04)	(0.04)
$Treat_b$	0.39^{***}	0.31^{***}	0.29^{***}	0.42^{***}	0.25^{***}	0.23^{***}
	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
$Treat_b \times Brown_j$		-0.16^{***}	-0.15^{***}		-0.01	-0.01
		(0.04)	(0.04)		(0.02)	(0.02)
$Treat_b \times Brown_j \times Large_j$		0.15^{***}	0.14^{***}		0.15^{***}	0.15^{***}
		(0.05)	(0.05)		(0.04)	(0.03)
Fixed effects	Firm	Firm	Firm	-	-	-
Firm controls	-	-	-	No	No	Yes
Bank controls	No	No	Yes	No	No	Yes
Number of observations	839168	489327	482052	839168	489327	482052
R-squared	0.37	0.38	0.38	0.01	0.01	0.01
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the bank lending intensive margin regression results following Khwaja and Mian (2008) for the participation period. Columns (1)-(3) report firm fixed-effects results of equation 3 and are therefore run on the sample of firms that borrow from multiple banks. Columns (4)-(6) report OLS results of equation 2. The outcome variable of each column is the change in the log of the loan amounts outstanding between bank b and firm j in the period before and after the implementation of the climate stress test. This means that all observations from January 2020 to December 2021 for a given loan are time-averaged into one, and all observations from August 2022 to September 2023 are time-averaged into one. The outcome variable is the difference between the two. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. $Large_j$ is equal to 1 for firms with above-median values for log total assets as at 31 December 2021, 0 otherwise. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Bank controls include bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 4: Credit demand vs. supply — Extensive margin (Participation)

Dependent variable	Exit?			Entry?		
	FE (1)	FE (2)	OLS (3)	FE (4)	FE (5)	OLS (6)
$(Intercept)$			0.45^{***}			0.02^{***}
			(0.03)			(0.00)
$Treat_b$	-0.02^{***}	-0.02^{***}	-0.03	-0.01^{***}	-0.01^{***}	0.00
	(0.01)	(0.01)	(0.03)	(0.00)	(0.00)	(0.00)
$Treat_b \times Brown_j$		0.01	-0.11^{***}		-0.00	-0.00^{***}
		(0.01)	(0.01)		(0.00)	(0.00)
$Treat_b \times Brown_j \times Large_j$		-0.02^{**}	0.08^{***}		-0.00	0.00
		(0.01)	(0.01)		(0.01)	(0.00)
Fixed effects	Firm	Firm	-	Firm	Firm	-
Firm controls	-	-	Yes	-	-	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	1122267	727675	727675	1122267	727675	727675
R-squared	0.76	0.83	0.03	0.31	0.33	0.01
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the bank lending extensive margin regression results following Khwaja and Mian (2008) for the participation period. The regressions examine how the participation in the climate stress test affects exit and entry of firms (from borrowing). Columns (1)-(3) look at exit by including all loans that were outstanding at the time of the climate stress test implementation in January 2022. For a given loan, "exit" is classified as 1 if the loan is not renewed at some point during the post-period, i.e. after July 2022. Columns (1) and (2) use firm fixed-effects following equation 5 and therefore limit the sample to only firms that were borrowing from multiple banks before the climate stress test implementation. Column (3) runs naive OLS regressions following equation 4. Columns (4)-(6) look at entry and include all loans given out after the climate stress test implementation. For a given loan, "entry" is classified as 1 if the loan was made for the first time in the post-period, i.e. after July 2022. Columns (4) and (5) use firm fixed-effects following equation 5 and therefore limit the sample to only firms that were borrowing from multiple banks after the climate stress test implementation. Column (6) runs naive OLS regressions following equation 4. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. $Large_j$ is equal to 1 for firms with above-median values for log total assets as at 31 December 2021, 0 otherwise. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Bank controls include bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 5: Firm-level aggregate lending outcomes (Participation)

Dependent variable	ΔLog aggregate loan size		
	OLS (1)	OLS (2)	OLS (3)
(Intercept)	-0.57^{***}	-0.29^{***}	-0.08
	(0.04)	(0.04)	(0.07)
$\bar{Treat}_j$	0.58^{***}	0.36^{***}	0.26^{***}
	(0.05)	(0.05)	(0.09)
$\bar{Treat}_j \times Brown_j$		0.09^{***}	-0.04
		(0.02)	(0.02)
$\bar{Treat}_j \times Large_j$			0.05
			(0.10)
$\bar{Treat}_j \times Brown_j \times Large_j$			0.20^{***}
			(0.03)
Bank controls	No	Yes	Yes
Firm controls	No	Yes	Yes
Number of observations	933700	469320	469320
R-squared	0.01	0.01	0.01

Notes: Following Khwaja and Mian (2008), these regressions examine the effect of the climate stress test participation on the total borrowing across all banks of firms following equation 6. The logs of all loans at a point in time from any of the banks for a given firm are summed to compute the aggregate firm-level loan size. Each column uses the change in the log of the aggregate firm-level loan size in the period before and after the implementation of the climate stress test as outcome variable. This means that all aggregate firm-level loan size observations from January 2020 to December 2021 are time-averaged into one, and all aggregate firm-level loan size observations from August 2022 to September 2023 are time-averaged into one. The outcome variable is the difference between the two. $\bar{Treat}_j$ is a firm-level formulation of the treatment indicator, which captures how many of a firm's loans are on average extended by treated banks ex-ante (i.e., we take the average of the bank-level treatment indicator per firm before the implementation of the climate stress test in January 2022). We use the same logic in constructing firms' exposure to observable bank controls, which consider bank total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 6: Firm-level real outcomes (Participation)

	ΔDefault probability	ΔProfit margin	ΔLog(Total assets)	ΔLog(S1 emission int.)
Dependent variable	OLS	OLS	OLS	OLS
	(1)	(2)	(3)	(4)
(Intercept)	-0.00	1.34***	0.11***	0.44**
	(0.01)	(0.47)	(0.01)	(0.19)
$\bar{Treat}_j$	0.02**	0.41	0.00	-0.76***
	(0.01)	(0.68)	(0.02)	(0.21)
$\bar{Treat}_j \times Brown_j$	0.00	-0.79***	0.00	0.24***
	(0.01)	(0.21)	(0.01)	(0.06)
$\bar{Treat}_j \times Large_j$	0.01	-0.01	-0.02*	0.37*
	(0.01)	(0.83)	(0.01)	(0.21)
$\bar{Treat}_j \times Brown_j \times Large_j$	0.00	-0.47*	0.04***	-0.29***
	(0.01)	(0.28)	(0.01)	(0.06)
Bank controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Number of observations	43607	380633	380633	92758
R-squared	0.01	0.03	0.04	0.02

Notes: Following Khwaja and Mian (2008), these regressions examine the effect of the climate stress test participation on firm outcomes following equation 6, specifically on their default probability, profit margin, log of total assets, and log of scope 1 emission intensities. Each column uses the change in the respective outcome variables in the period before and after the implementation of the climate stress test. This means that for each outcome variable, all aggregate firm-level observations from January 2020 to December 2021 are time-averaged into one, and all aggregate firm-level observations from August 2022 to September 2023 are time-averaged into one. The final outcome variables are the difference between the two. $\bar{Treat}_j$ is a firm-level formulation of the treatment indicator, which captures how many of a firm's loans are on average extended by treated banks ex-ante (i.e., I take the average of the bank-level treatment indicator per firm before the implementation of the climate stress test in January 2022). I use the same logic in constructing firms' exposure to observable bank controls, which consider bank total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 7: Comparison between treated and control banks (pre-treatment) - Robustness

Variable	Treated		Control		Treated - Control
	Mean	SD	Mean	SD	Norm. diff.
Total assets (ln)	25.31	0.94	23.80	0.53	1.98*
Loans (defaulted) (ln)	21.02	0.96	19.46	1.46	1.26*
Net income (EUR mn)	353.7	5.76e+8	30.5	1.08e+8	0.77*
Liquid assets (ln)	23.80	0.91	22.29	0.64	1.91*
CET1 ratio	0.166	0.022	0.145	0.034	0.74*
Firm: S1 emissions (ln)	1.86	2.48	1.42	2.52	0.18
Firm: S3 emissions (ln)	6.11	1.15	6.20	1.00	-0.08
Firm: Brown (sector dummy)	0.510	0.50	0.512	0.50	-0.01
Firm: Total assets (ln)	18.74	2.37	19.33	1.79	-0.28*
Firm: Net income (EUR mn)	49.9	5.43e+8	53.1	5.81e+8	-0.01
Firm: ROA	2.18	6.95	2.19	7.03	-0.00
Firm: Liquidity ratio	1.12	1.02	1.13	1.05	-0.00

Notes: The table shows summary statistics of selected covariates separately for the robustness sample of the smallest three treated and the largest three control banks per country as at 30 June 2021, before 2021 Q3. For each bank group, the mean and standard deviation (SD) for each covariate is shown. The last column shows normalized differences as in Imbens and Wooldridge (2009), i.e. difference in means is normalized with the sum of variances. A star (*) indicates that the normalized difference is outside of the range ±0.25 (which serves as a rule of thumb).

Table 8: Log loan amounts (Cluster: Bank) - Robustness

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treat_b$	−2.00***	−1.72***		−0.94***					
	(0.39)	(0.41)		(0.23)					
$PostAnn_t$	−0.05	−0.13	−0.13	−0.07	−0.13				
	(0.12)	(0.14)	(0.11)	(0.09)	(0.09)				
$PostCST_t$	−0.46***	−0.70***	−0.55***	−0.40***	−0.40***				
	(0.16)	(0.14)	(0.10)	(0.10)	(0.10)				
$Brown_j$	−0.53**	−1.01	−0.21						
	(0.27)	(0.69)	(0.68)						
$Treat_b \times PostAnn_t$	0.11	0.09	0.14	0.08	0.16*		0.27***		
	(0.15)	(0.16)	(0.13)	(0.09)	(0.09)		(0.10)		
$Treat_b \times PostCST_t$	0.55***	0.69***	0.56***	0.44***	0.46***		0.23***		
	(0.18)	(0.15)	(0.12)	(0.10)	(0.10)		(0.07)		
$Treat_b \times Brown_j$	0.11	−0.17	−0.64**	−0.66**	−0.52*	−0.54***	−0.47*	−0.55**	
	(0.35)	(0.40)	(0.30)	(0.30)	(0.23)	(0.24)	(0.25)	(0.26)	
$PostAnn_t \times Brown_j$	−0.01	0.18*	0.14	0.06	0.16	0.18*			
	(0.10)	(0.11)	(0.09)	(0.11)	(0.10)	(0.10)			
$PostCST_t \times Brown_j$	0.12	0.28**	0.18	0.06	0.03	−0.15			
	(0.16)	(0.13)	(0.12)	(0.13)	(0.12)	(0.12)			
$PostAnn_t \times Treat_b \times Brown_j$	−0.07	−0.15	−0.13	−0.04	−0.12	−0.08	−0.26**	−0.14**	−0.10**
	(0.13)	(0.17)	(0.13)	(0.14)	(0.13)	(0.15)	(0.10)	(0.06)	(0.04)
$PostCST_t \times Treat_b \times Brown_j$	−0.12	0.02	0.02	0.09	0.08	0.27**	−0.02	0.06	−0.04**
	(0.19)	(0.18)	(0.16)	(0.14)	(0.13)	(0.10)	(0.09)	(0.06)	(0.02)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Bank FE	No	No	Yes	No	Yes	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes	No	No	No
Bank-Quarter FE	No	No	No	No	No	Yes	No	Yes	Yes
Firm-Quarter FE	No	No	No	No	No	No	Yes	Yes	Yes
Bank-Firm FE	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.07	0.06	0.15	0.67	0.68	0.71	0.71	0.72	0.92
Num. obs.	164112	126490	126490	126490	126490	126490	126490	126490	126490
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the main triple DiD regression results following equation 1, using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021, whereby each column progressively saturates the regression with fixed-effects. The outcome variable of each column is the log of the loan amounts outstanding between bank b and firm j in month t. $PostAnn_t$ is equal to 1 in the period following the announcement of the climate stress test, 0 otherwise. $PostCST_t$ is equal to 1 in the period following participation in the climate stress test, 0 otherwise. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. Controls include triple-dimension interactions between the firm-level $Brown_j$ dummy and bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios, respectively. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 9: Credit demand vs. supply — Intensive margin (Participation) - Robustness

	Δ Log loan amount					
	FE			OLS		
Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)
$(Intercept)$				-0.31^{***}	-0.16^{**}	-0.21^{***}
				(0.08)	(0.06)	(0.07)
$Treat_b$	0.24^{***}	0.11	0.14	0.31^{***}	0.07	0.16^{**}
	(0.08)	(0.14)	(0.13)	(0.11)	(0.09)	(0.08)
$Treat_b \times Brown_j$		0.17	0.20		-0.00	0.02
		(0.14)	(0.14)		(0.06)	(0.05)
$Treat_b \times Brown_j \times Large_j$		-0.25^{***}	-0.29^{***}		0.14	0.11
		(0.10)	(0.08)		(0.09)	(0.08)
Fixed effects	Firm	Firm	Firm	-	-	-
Firm controls	-	-	-	No	No	Yes
Bank controls	No	No	Yes	No	No	Yes
Number of observations	120148	63005	61670	120148	63005	61670
R-squared	0.66	0.73	0.74	0.01	0.00	0.02
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the bank lending intensive margin regression results following Khwaja and Mian (2008) for the participation period, using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021. Columns (1)-(3) report firm fixed-effects results of equation 3 and are therefore run on the sample of firms that borrow from multiple banks. Columns (4)-(6) report OLS results of equation 2. The outcome variable of each column is the change in the log of the loan amounts outstanding between bank b and firm j in the period before and after the implementation of the climate stress test. This means that all observations from January 2020 to December 2021 for a given loan are time-averaged into one, and all observations from August 2022 to September 2023 are time-averaged into one. The outcome variable is the difference between the two. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. $Large_j$ is equal to 1 for firms with above-median values for log total assets as at 31 December 2021, 0 otherwise. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Bank controls include bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 10: Credit demand vs. supply — Extensive margin (Participation) - Robustness

	Exit?			Entry?		
Dependent variable	FE (1)	FE (2)	OLS (3)	FE (4)	FE (5)	OLS (6)
$(Intercept)$			0.32^{***}			0.03^{***}
			(0.06)			(0.01)
$Treat_b$	-0.03^{***}	0.00	0.07	-0.01	-0.02	0.00
	(0.01)	(0.02)	(0.07)	(0.01)	(0.01)	(0.01)
$Treat_b \times Brown_j$		0.01	-0.04		-0.01	-0.01^{**}
		(0.02)	(0.03)		(0.01)	(0.01)
$Treat_b \times Brown_j \times Large_j$		-0.03	-0.01		0.02	0.01
		(0.02)	(0.04)		(0.01)	(0.01)
Fixed effects	Firm	Firm	-	Firm	Firm	-
Firm controls	-	-	Yes	-	-	Yes
Bank controls	Yes	Yes	Yes	Yes	Yes	Yes
Number of observations	152044	90231	90231	152044	90231	90231
R-squared	0.85	0.91	0.02	0.59	0.71	0.01
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the bank lending extensive margin regression results following Khwaja and Mian (2008) for the participation period, using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021. The regressions examine how the participation in the climate stress test affects exit and entry of firms (from borrowing). Columns (1)-(3) look at exit by including all loans that were outstanding at the time of the climate stress test implementation in January 2022. For a given loan, "exit" is classified as 1 if the loan is not renewed at some point during the post-period, i.e. after July 2022. Columns (1) and (2) use firm fixed-effects following equation 5 and therefore limit the sample to only firms that were borrowing from multiple banks before the climate stress test implementation. Column (3) runs naive OLS regressions following equation 4. Columns (4)-(6) look at entry and include all loans given out after the climate stress test implementation. For a given loan, "entry" is classified as 1 if the loan was made for the first time in the post-period, i.e. after July 2022. Columns (4) and (5) use firm fixed-effects following equation 5 and therefore limit the sample to only firms that were borrowing from multiple banks after the climate stress test implementation. Column (6) runs naive OLS regressions following equation 4. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. $Large_j$ is equal to 1 for firms with above-median values for log total assets as at 31 December 2021, 0 otherwise. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Bank controls include bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 11: Firm-level aggregate lending outcomes (Participation) - Robustness

Dependent variable	ΔLog aggregate loan size		
	OLS (1)	OLS (2)	OLS (3)
(Intercept)	-0.34^{***}	-0.26^{***}	-0.22^{**}
	(0.11)	(0.08)	(0.12)
$\bar{Treat}_j$	0.42^{***}	0.19^{*}	0.37^{***}
	(0.16)	(0.10)	(0.14)
$\bar{Treat}_j \times Brown_j$		0.11	-0.17^{***}
		(0.08)	(0.05)
$\bar{Treat}_j \times Large_j$			-0.28^{**}
			(0.14)
$\bar{Treat}_j \times Brown_j \times Large_j$			0.56^{***}
			(0.08)
Bank controls	No	Yes	Yes
Firm controls	No	Yes	Yes
Number of observations	129397	63998	63998
R-squared	0.01	0.02	0.03

Notes: Following Khwaja and Mian (2008), these regressions examine the effect of the climate stress test participation on the total borrowing across all banks of firms following equation 6, using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021. The logs of all loans at a point in time from any of the banks for a given firm are summed to compute the aggregate firm-level loan size. Each column uses the change in the log of the aggregate firm-level loan size in the period before and after the implementation of the climate stress test as outcome variable. This means that all aggregate firm-level loan size observations from January 2020 to December 2021 are time-averaged into one, and all aggregate firm-level loan size observations from August 2022 to September 2023 are time-averaged into one. The outcome variable is the difference between the two. $\bar{Treat}_j$ is a firm-level formulation of the treatment indicator, which captures how many of a firm's loans are on average extended by treated banks ex-ante (i.e., I take the average of the bank-level treatment indicator per firm before the implementation of the climate stress test in January 2022). I use the same logic in constructing firms' exposure to observable bank controls, which consider bank total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 12: Firm-level real outcomes (Participation) - Robustness

Dependent variable	ΔDefault probability OLS (1)	ΔProfit margin OLS (2)	ΔLog(Total assets) OLS (3)	ΔLog(S1 emission int.) OLS (4)
(Intercept)	0.02***	0.31	0.10***	0.12*
	(0.01)	(0.77)	(0.02)	(0.09)
$Treat_f$	-0.02***	1.80	0.05	-0.03
	(0.01)	(1.17)	(0.04)	(0.18)
$Treat_f \times Brown$	0.01**	0.40	-0.05**	0.14
	(0.00)	(0.32)	(0.02)	(0.38)
$Treat_f \times Large$	-0.05	-3.28	-0.13***	-0.10
	(0.04)	(2.46)	(0.03)	(0.24)
$Treat_f \times Brown \times Large$	-0.01	-1.68	0.03	-0.16
	(0.01)	(2.11)	(0.03)	(0.33)
Bank controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Number of observations	23936	86335	86335	29525
R-squared	0.03	0.05	0.06	0.10

Notes: Following Khwaja and Mian (2008), these regressions examine the effect of the climate stress test participation on firm outcomes following equation 6, specifically on their default probability, profit margin, log of total assets, and log of scope 1 emission intensities, using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021. Each column uses the change in the respective outcome variables in the period before and after the implementation of the climate stress test. This means that for each outcome variable, all aggregate firm-level observations from January 2020 to December 2021 are time-averaged into one, and all aggregate firm-level observations from August 2022 to September 2023 are time-averaged into one. The final outcome variables are the difference between the two. $\bar{Treat}_j$ is a firm-level formulation of the treatment indicator, which captures how many of a firm's loans are on average extended by treated banks ex-ante (i.e., I take the average of the bank-level treatment indicator per firm before the implementation of the climate stress test in January 2022). I use the same logic in constructing firms' exposure to observable bank controls, which consider bank total assets, defaulted loans, net income, liquid assets, and CET1 ratios at a quarterly frequency. Firm controls include firm-level total assets, net income, return on assets and liquidity ratios at a yearly frequency. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 13: Comparison between bad and good score treated banks (pre-treatment)

Variable	Bad-score		Good-score		Bad - Good
	Mean	SD	Mean	SD	Norm. diff.
Total assets (ln)	26.62	1.14	26.86	1.14	-0.21
Loans (defaulted) (ln)	22.57	1.21	22.66	1.32	-0.07
Net income (EUR mn)	216.26	1.93e+9	874.83	2.19e+9	-0.32*
Liquid assets (ln)	24.88	1.18	25.22	1.11	-0.30*
CET1 ratio	0.147	0.02	0.144	0.02	0.14
Firm: S1 emission int. (ln)	2.07	2.09	2.52	2.42	-0.01
Firm: S3 emissions int. (ln)	6.17	1.13	6.15	1.11	0.01
Firm: Brown (sector dummy)	0.52	0.50	0.50	0.50	0.04
Firm: Total assets (ln)	18.88	1.77	19.00	1.59	-0.07
Firm: Net income (EUR mn)	27.69	6.59e+8	24.68	5.88e+8	0.00
Firm: ROA	1.80	6.67	1.68	6.64	0.02
Firm: Liquidity ratio	1.09	1.25	1.16	1.78	-0.04

Notes: The table shows summary statistics of selected covariates separately for bad-score and good-score banks in the treatment sample before 2021 Q3. Bad-score banks are defined as achieving equal to or above median scores from 1-4 (1 being the best), i.e. scores of 3 or 4. Good-score banks achieved below median scores, i.e. scores of 1 or 2. For each bank group, the mean and standard deviation (SD) for each covariate is shown. The last column shows normalized differences as in Imbens and Wooldridge (2009), i.e. difference in means is normalized with the sum of variances. A star (*) indicates that the normalized difference is outside of the range ±0.25 (which serves as a rule of thumb).

Table 14: Log loan amounts (Cluster: Bank) - Score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Bad_b	0.04	0.04		−0.22*					
	(0.17)	(0.18)		(0.11)					
$PostAnn_t$	0.04	0.04	0.05	−0.04	−0.04				
	(0.04)	(0.04)	(0.03)	(0.04)	(0.04)				
$PostCST_t$	0.03	0.02	0.06	−0.00	−0.01				
	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)				
$Brown_j$	−0.10	−0.17	0.14						
	(0.15)	(0.18)	(0.17)						
$Bad_b \times PostAnn_t$	−0.12**	−0.11**	−0.13**	−0.01	−0.01		−0.03		
	(0.06)	(0.05)	(0.05)	(0.05)	(0.05)		(0.05)		
$Bad_b \times PostCST_t$	−0.08	−0.09	−0.14**	−0.02	−0.01		−0.02		
	(0.07)	(0.07)	(0.07)	(0.05)	(0.04)		(0.05)		
$Bad_b \times Brown_j$	−0.20	−0.17	−0.10	0.05	0.03	0.01	−0.03	−0.03	
	(0.23)	(0.24)	(0.23)	(0.17)	(0.15)	(0.14)	(0.15)	(0.14)	
$PostAnn_t \times Brown_j$	−0.09	−0.09	−0.07	0.02	0.06	0.04			
	(0.07)	(0.07)	(0.07)	(0.06)	(0.06)	(0.05)			
$PostCST_t \times Brown_j$	−0.04	−0.02	−0.07	−0.02	−0.05	−0.05			
	(0.08)	(0.09)	(0.07)	(0.06)	(0.04)	(0.03)			
$PostAnn_t \times Bad_b \times Brown_j$	0.12	0.13	0.11	0.02	−0.02	−0.02	0.03	0.02	0.02
	(0.09)	(0.09)	(0.08)	(0.06)	(0.06)	(0.05)	(0.05)	(0.04)	(0.02)
$PostCST_t \times Bad_b \times Brown_j$	0.03	0.03	0.07	−0.02	0.02	0.04	0.00	0.02	−0.01
	(0.09)	(0.09)	(0.07)	(0.06)	(0.05)	(0.04)	(0.04)	(0.04)	(0.02)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Bank FE	No	No	Yes	No	Yes	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes	No	No	No
Bank-Quarter FE	No	No	No	No	No	Yes	No	Yes	Yes
Firm-Quarter FE	No	No	No	No	No	No	Yes	Yes	Yes
Bank-Firm FE	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.01	0.01	0.06	0.49	0.51	0.51	0.55	0.55	0.85
Num. obs.	1049699	1038236	1038236	1038236	1038236	1038236	1038236	1038236	1038236
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the main triple DiD regression results following equation 1 for the bad vs. good score treated sample, whereby each column progressively saturates the regression with fixed-effects. The outcome variable of each column is the log of the loan amounts outstanding between bank b and firm j in month t. $PostAnn_t$ is equal to 1 in the period following the announcement of the climate stress test, 0 otherwise. $PostCST_t$ is equal to 1 in the period following participation in the climate stress test, 0 otherwise. Bad_b is equal to 1 if the loan is from a bank that has received an equal or above median score (i.e. above 2), 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. Controls include triple-dimension interactions between the firm-level $Brown_j$ dummy and bank-level net income and liquid assets, respectively. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Table 15: Log loan amounts (Cluster: Bank) - Individual Score

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$PostAnn_t \times Brown_j$	0.15	0.19	0.09	0.07	−0.01	0.10	−0.19	−0.09	0.11***
	(0.17)	(0.16)	(0.18)	(0.12)	(0.14)	(0.11)	(0.14)	(0.12)	(0.03)
$PostAnn_t \times Brown_j \times 1\{Score_b = 2\}$	−0.23	−0.27	−0.14	−0.06	0.05	−0.13	0.14	0.02	−0.18***
	(0.17)	(0.18)	(0.18)	(0.09)	(0.11)	(0.12)	(0.13)	(0.15)	(0.03)
$PostAnn_t \times Brown_j \times 1\{Score_b = 3\}$	−0.14	−0.15	−0.04	−0.05	0.03	−0.05	0.15	0.10	−0.11***
	(0.18)	(0.18)	(0.20)	(0.12)	(0.15)	(0.12)	(0.14)	(0.13)	(0.03)
$PostAnn_t \times Brown_j \times 1\{Score_b = 4\}$	−0.24	−0.27	−0.24	−0.06	−0.05	−0.24	0.09	−0.10	−0.18***
	(0.27)	(0.27)	(0.29)	(0.19)	(0.23)	(0.22)	(0.22)	(0.24)	(0.05)
$PostCST_t \times Brown_j$	−0.24***	−0.25***	−0.23***	−0.27***	−0.25***	−0.21***	−0.26***	−0.19***	−0.15***
	(0.07)	(0.07)	(0.06)	(0.06)	(0.06)	(0.04)	(0.07)	(0.05)	(0.04)
$PostCST_t \times Brown_j \times 1\{Score_b = 2\}$	0.22**	0.25**	0.19**	0.26***	0.21***	0.17***	0.24***	0.13**	0.12**
	(0.11)	(0.11)	(0.07)	(0.08)	(0.07)	(0.05)	(0.07)	(0.06)	(0.05)
$PostCST_t \times Brown_j \times 1\{Score_b = 3\}$	0.25***	0.26***	0.24***	0.24***	0.23***	0.19***	0.24***	0.15**	0.10**
	(0.08)	(0.08)	(0.08)	(0.06)	(0.07)	(0.04)	(0.08)	(0.06)	(0.05)
$PostCST_t \times Brown_j \times 1\{Score_b = 4\}$	0.14	0.17	0.17	0.21*	0.21**	0.27**	0.19	0.18	0.14**
	(0.17)	(0.17)	(0.16)	(0.11)	(0.11)	(0.12)	(0.13)	(0.14)	(0.06)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Bank FE	No	No	Yes	No	Yes	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes	No	No	No
Bank-Quarter FE	No	No	No	No	No	Yes	No	Yes	Yes
Firm-Quarter FE	No	No	No	No	No	No	Yes	Yes	Yes
Bank-Firm FE	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.01	0.01	0.06	0.49	0.51	0.51	0.55	0.55	0.85
Num. obs.	1049699	1038236	1038236	1038236	1038236	1038236	1038236	1038236	1038236
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Notes: This table reports the main triple DiD regression results following equation 1 for the treated sample, differentiating by CST scores whereby each column progressively saturates the regression with fixed-effects. The outcome variable of each column is the log of the loan amounts outstanding between bank b and firm j in month t. $PostAnn_t$ is equal to 1 in the period following the announcement of the climate stress test, 0 otherwise. $PostCST_t$ is equal to 1 in the period following participation in the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. $Score_b$ is a bank-level variable ranging from 1-4, with 1 being the best. The best performing banks are therefore the reference group. The table omits the display of lower-dimensional coefficients. Controls include triple-dimension interactions between the firm-level $Brown_j$ dummy and bank-level net income and liquid assets, respectively. Clustered standard errors at the bank-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

Figures

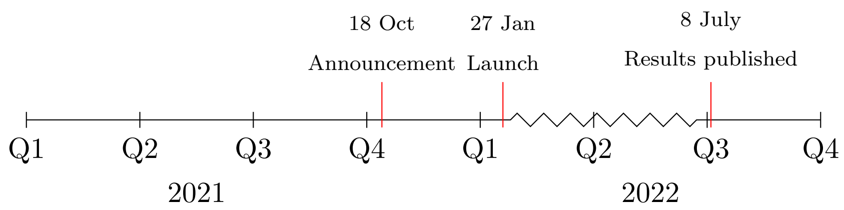


Figure 1: Timeline

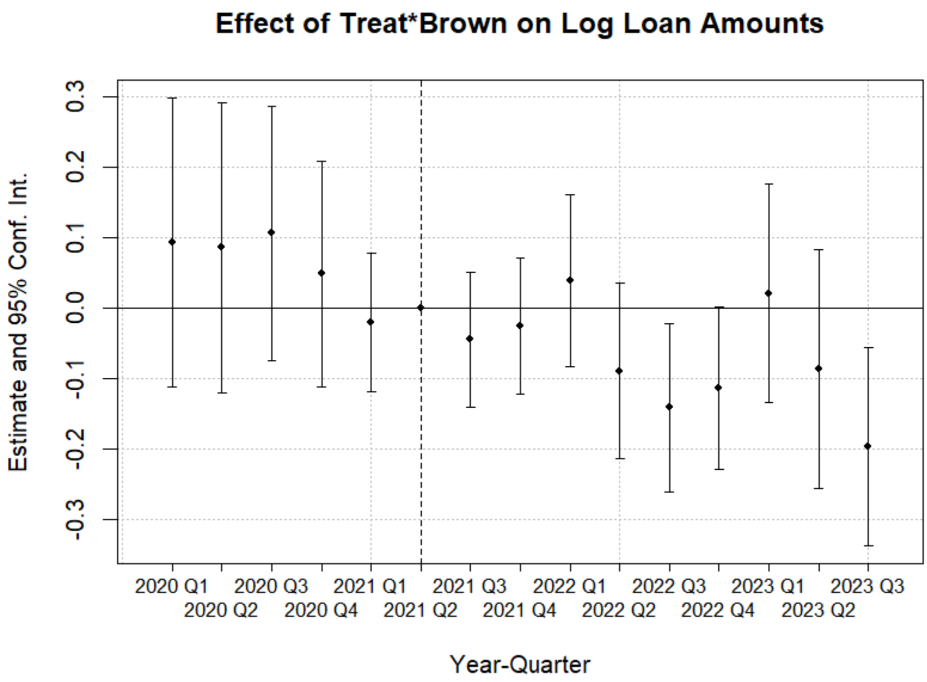


Figure 2: Time-varying coefficients

This figure reports time-varying coefficients on the interaction of $Treat_b \times Brown_j$ with respective year-quarter dummies, following the most saturated specification of equation 1. The figure therefore displays the time-varying equivalent to column (9) of table 2. The reference period is 2021 Q2, i.e. right before the announcement of the climate stress test at the end of 2021 Q3. The announcement period is 2021 Q4, and the participation period is 2022 Q1 - 2022 Q2. The confidence intervals are at the 95% level.

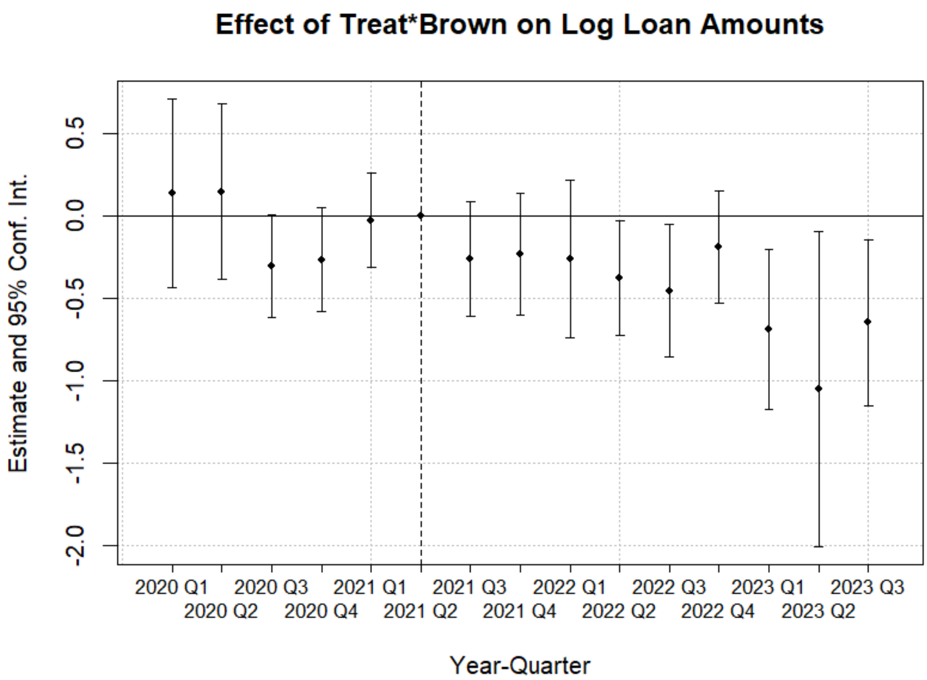


Figure 3: Time-varying coefficients - Robustness

This figure reports time-varying coefficients on the interaction of $Treat_b \times Brown_j$ with respective year-quarter dummies, following the most saturated specification of equation 1 and using the robustness sample of the three smallest treated and three largest control banks per country as at 30 June 2021. The figure therefore displays the time-varying equivalent to column (9) of table 8. The reference period is 2021 Q2, i.e. right before the announcement of the climate stress test at the end of 2021 Q3. The announcement period is 2021 Q4, and the participation period is 2022 Q1 - 2022 Q2. The confidence intervals are at the 95% level.

Appendix

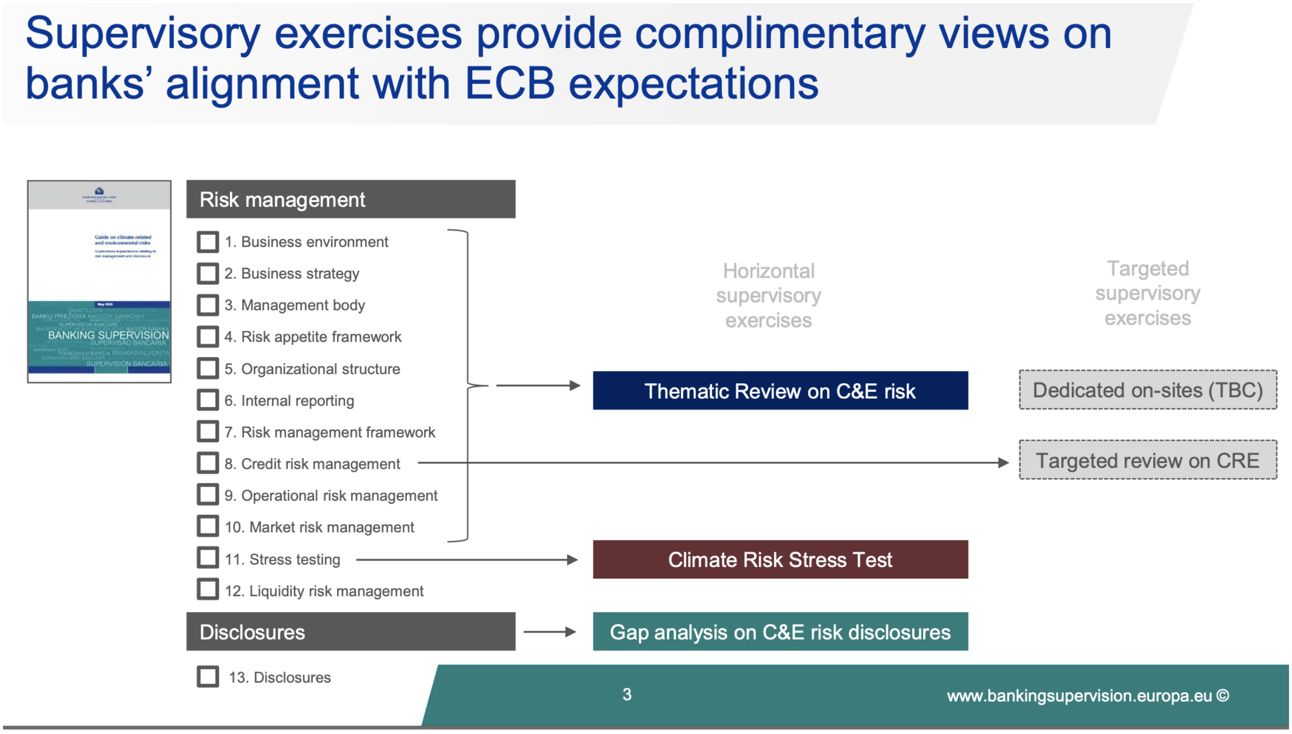


Figure A1: Climate risk at the ECB

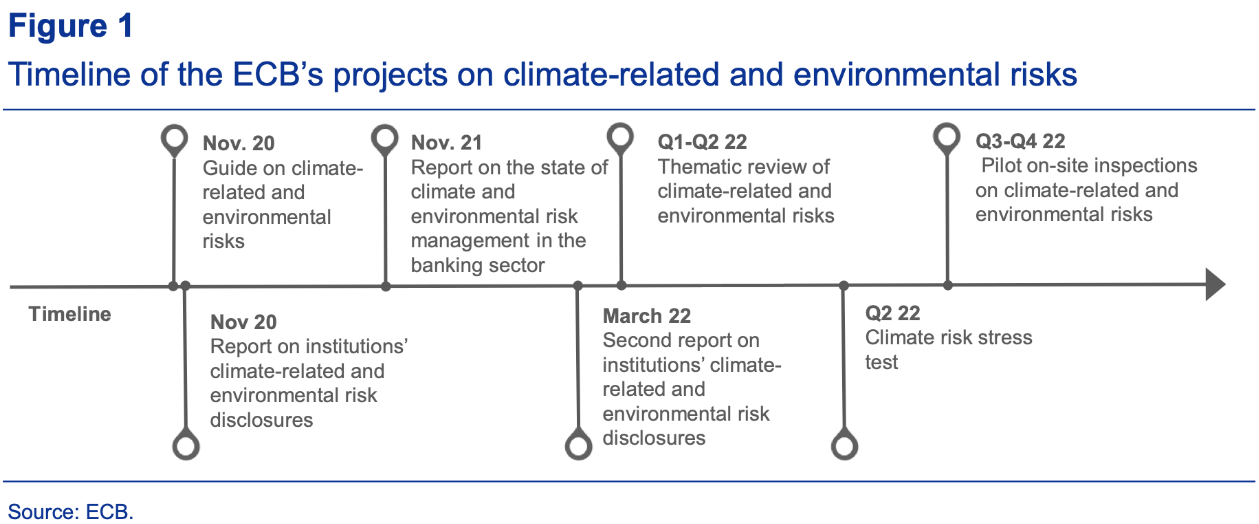


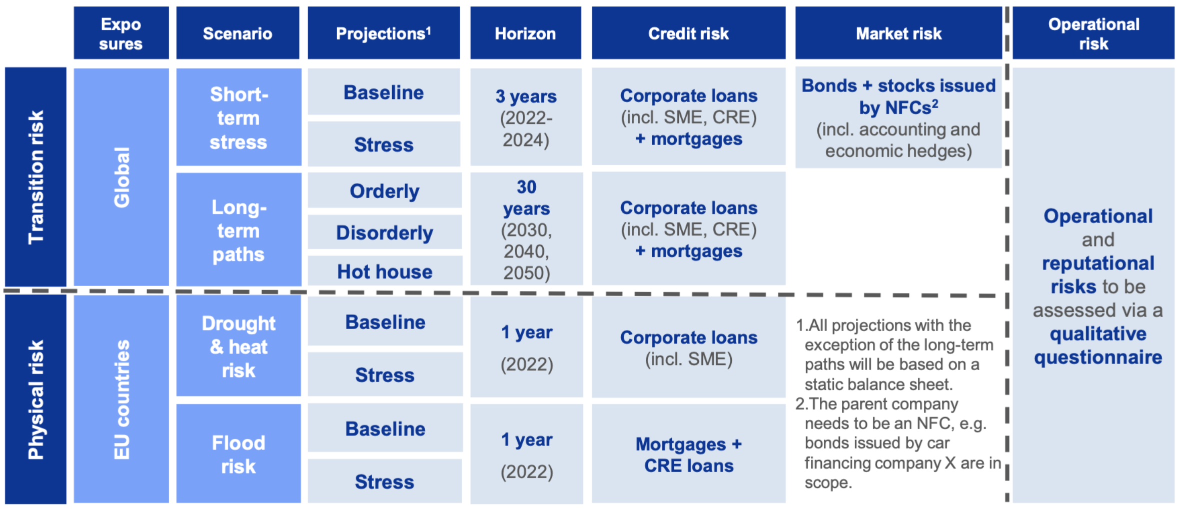
Figure A2: Climate risk at the ECB - Timeline

NACE industrial sector	NACE industrial sector description
A01	Crop and animal production, hunting and related service activities
A02-A03	Forestry and logging; Fishing and aquaculture
B	Mining and quarrying
C10-C12	Manufacture of food products, beverages and tobacco products
C13-C18	Manufacture of textiles; Manufacture of wearing apparel; Manufacture of leather and related products; Manufacture of wood and of products of wood and cork, except furniture; Manufacture of articles of straw and plaiting materials; Manufacture of paper and paper products; Printing and reproduction of recorded media
C19	Manufacture of coke and refined petroleum products
C20	Manufacture of chemicals and chemical products
C21-C22	Manufacture of basic pharmaceutical products and pharmaceutical preparations; Manufacture of rubber and plastic products
C23	Manufacture of other non-metallic mineral products
C24-C25	Manufacture of basic metals; Manufacture of fabricated metal products, except machinery and equipment
C26-C28	Manufacture of computer, electronic and optical products; Manufacture of electrical equipment; Manufacture of machinery and equipment not elsewhere classified
C29-C30	Manufacture of motor vehicles, trailers and semi-trailers; Manufacture of other transport equipment
C31-C33	Manufacture of furniture; Other manufacturing; Repair and installation of machinery and equipment
D	Electricity, gas, steam and air conditioning supply
E36-E39	Water collection, treatment and supply; Sewerage; Waste collection, treatment and disposal activities; Materials recovery; Remediation activities and other waste management services
F	Construction
G45-47	Wholesale and retail trade and repair of motor vehicles and motorcycles; Wholesale trade, except of motor vehicles and motorcycles; Retail trade, except of motor vehicles and motorcycles
H49	Land transport and transport via pipelines
H50	Water transport
H51	Air transport
H52-H53	Warehousing and support activities for transportation; Postal and courier activities
L	Real estate activities

Figure A3: List of industries

Chart 2

Module 3 scenarios and risk dimensions



Source: ECB, climate risk stress test 2022, methodology, October 2021.
Notes: CRE stands for commercial real estate; NFC stands for non-financial corporation; SMEs stands for small and medium-sized enterprises.

Figure A4: Module 3

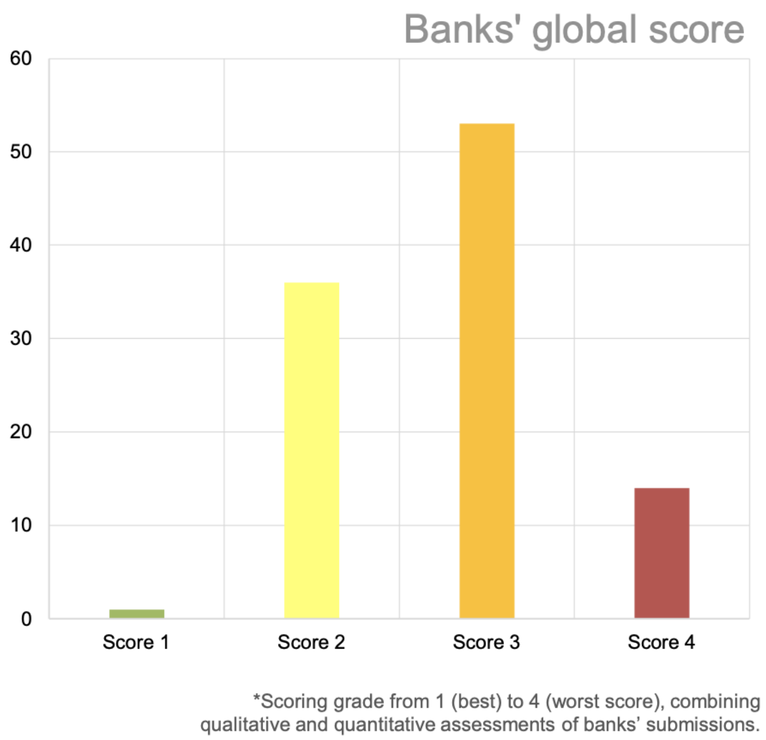


Figure A5: Score

Table A1: Log loan amounts (Cluster: Bank, Industry)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$Treat_b$	−1.31***	−1.17***		−0.81***					
	(0.12)	(0.19)		(0.19)					
$PostAnn_t$	−0.01	−0.01	−0.11***	−0.26***	−0.22***				
	(0.05)	(0.05)	(0.04)	(0.09)	(0.04)				
$PostCST_t$	−0.64***	−0.69***	−0.65***	−0.63***	−0.60***				
	(0.07)	(0.07)	(0.05)	(0.08)	(0.06)				
$Brown_j$	−0.26*	0.96	0.76						
	(0.15)	(0.83)	(0.53)						
$Treat_b \times PostAnn_t$	−0.01	−0.01	0.08**	0.21***	0.18***		0.21***		
	(0.04)	(0.05)	(0.04)	(0.08)	(0.04)		(0.02)		
$Treat_b \times PostCST_t$	0.72***	0.73***	0.70***	0.69***	0.67***		0.35***		
	(0.04)	(0.04)	(0.03)	(0.07)	(0.05)		(0.03)		
$Treat_b \times Brown_j$	0.07	−0.11	0.02	0.06	0.17*	0.16*	0.13	0.10	
	(0.13)	(0.17)	(0.12)	(0.21)	(0.09)	(0.10)	(0.09)	(0.09)	
$PostAnn_t \times Brown_j$	0.03	0.04	0.09	0.40***	0.27***	0.26***			
	(0.06)	(0.06)	(0.06)	(0.13)	(0.08)	(0.09)			
$PostCST_t \times Brown_j$	0.24***	0.26***	0.15**	0.15*	0.13*	0.03			
	(0.07)	(0.08)	(0.06)	(0.08)	(0.06)	(0.05)			
$PostAnn_t \times Treat_b \times Brown_j$	−0.07	−0.05	−0.05	−0.34***	−0.22***	−0.22**	−0.16***	−0.10***	−0.03*
	(0.06)	(0.06)	(0.07)	(0.13)	(0.08)	(0.10)	(0.02)	(0.03)	(0.02)
$PostCST_t \times Treat_b \times Brown_j$	−0.27***	−0.23***	−0.15***	−0.18**	−0.17**	−0.05	−0.15***	−0.05**	−0.05***
	(0.06)	(0.06)	(0.05)	(0.09)	(0.07)	(0.06)	(0.03)	(0.02)	(0.01)
Controls	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Quarter FE	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Bank FE	No	No	Yes	No	Yes	No	Yes	No	No
Firm FE	No	No	No	Yes	Yes	Yes	No	No	No
Bank-Quarter FE	No	No	No	No	No	Yes	No	Yes	Yes
Firm-Quarter FE	No	No	No	No	No	No	Yes	Yes	Yes
Bank-Firm FE	No	No	No	No	No	No	No	No	Yes
Adj. R^2	0.02	0.03	0.09	0.48	0.50	0.51	0.54	0.55	0.86
Num. obs.	1237037	1163543	1163543	1163543	1163543	1163543	1163543	1163543	1163543
Clustering	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.	Bank, Ind.

Notes: This table reports the main triple DiD regression results following equation 1, whereby each column progressively saturates the regression with fixed-effects. The outcome variable of each column is the log of the loan amounts outstanding between bank b and firm j in month t. $PostAnn_t$ is equal to 1 in the period following the announcement of the climate stress test, 0 otherwise. $PostCST_t$ is equal to 1 in the period following participation in the climate stress test, 0 otherwise. $Treat_b$ is equal to 1 if the loan is from a bank that has been subject to the climate stress test, 0 otherwise. $Brown_j$ is equal to 1 if borrowing firms belong to the list of GHG-intensive industries as pre-defined in the exercise, 0 otherwise. Controls include triple-dimension interactions between the firm-level $Brown_j$ dummy and bank-level total assets, defaulted loans, net income, liquid assets, and CET1 ratios, respectively. Clustered standard errors at the bank- and industry-level are in parentheses: $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

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