# Carbon Emissions and the Bank-Lending Channel*

**Marcin Kacperczyk** **José-Luis Peydró**

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*Abstract*

We study how firm-level carbon emissions affect bank lending and, through this channel, real decisions in a sample of global firms with syndicated loans. Using bank-level commitments to carbon neutrality to measure changes in banks' green preferences, we show that firms with higher (lower) scope-1 emission levels borrowing from banks making commitments subsequently receive less (more) bank credit, even controlling for differences in their fundamentals. The bank decision to reallocate credit more likely reflects the bank preference for green rather than the response to an increased business risk. The reduction in bank lending to a brown sector lowers this sector's real investments but even then, these firms do not improve their environmental scores.

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* Marcin Kacperczyk: Imperial College London, CEPR, m.kacperczyk@imperial.ac.uk; José-Luis Peydró: Imperial College London, CEPR, jose.peydro@gmail.com. We thank for helpful comments seminar participants at Bank of Spain. We especially thank Andrea Fabiani and Win Monroe for excellent research assistance and numerous excellent suggestions.

## 1. Introduction

The battle against global warming is at the forefront of social and policy debates. A fundamental element to mitigate the climate problem is the reduction of carbon emissions, especially those of the private sector, a process that is often described as a transition from brown to green economy. Of special importance in the transition outcome is the financial sector, given its centrality in allocating resources to non-financial companies (NFCs), and its ability to impose costs on non-compliant companies either through price or quantity adjustments. Indeed, recent empirical evidence suggests that highly polluting firms face higher costs of capital, driven by bond and stock market investors (e.g., Bolton and Kacperczyk, 2020a, 2020b, 2021a; Krueger, Sautner, and Starks, 2020). If the expected costs are substantial, optimizing companies may decide to reduce their emissions. However, this argument implicitly assumes that alternative capital providers are not available or that other capital providers also impose similar costs. The banking sector is one of the most obvious alternatives which could possibly strengthen or weaken the disciplinary force. Understanding whether banks reinforce emission reduction, by actively cutting credit to brown firms and channeling credit towards green firms, or they provide an arbitrage opportunity for firms trying to avoid changes in their pollution activities, is therefore of utmost importance, a problem also highlighted by several policy makers (see, among others, Carney, 2015, and Lagarde, 2019).

In this paper, we shed light on this and related questions by looking at the sample of global firms that rely on bank credit and exhibit a rich cross-sectional variation in their carbon emission levels. As an empirical identification strategy of bank-level willingness to reduce brown lending, we exploit a cross-sectional variation among banks in their commitments, through the Science Based Targets Initiative (SBTi), to a well-defined path of reductions in carbon emissions, in line with the Paris Agreement goals. The extent to which such commitments are reflected in a more environmentally friendly distribution of credit across firms is unclear, though in the absence of sharp penalties and tight rules on lending to brown firms, commitments might be a tool for greenwashing, resulting in small or nil implications for the allocation of credit. Moreover, even if bank commitments are reflected in changes of their lending behavior, it is not clear whether firms could not secure their capital through other financial intermediaries and instruments, and hence continue polluting and investing.

In our first test, we examine whether firms associated with banks that decide to make commitments experience different financing outcomes conditional on their level of emissions. Our data cover the 2013-2018 period, consistent with the fact that these bank-level commitments happen after the Paris agreement. For identification, we take advantage of the staggered commitment to the SBTi-targets by financial institutions with large exposures in the syndicated loan market (these banks participate in 60% of the loans). This setting is suitable for estimating a triple difference-in-differences model, in which we compare outcomes across firms: i) before and after such bank commitments; ii) depending on whether firms have, or do not have, a (previously) established credit relationship with a committed bank; iii) conditional on whether a firm is relatively green, or brown, based on its level of greenhouse gas (GHG) emissions. Our results provide strong and robust evidence that committed banks affect credit outcomes, conditional on the level of firm emissions. Notably, the effect is visible in both low-emission (green) firms, which are allocated more credit, and in high-emission (brown) firms, which experience a reduction in total credit. After a bank commits to reducing carbon emissions, firms with higher ex-ante scope-1 (direct) emissions[1] and with ex-ante lending relationships with the committed bank (thereafter,

[1] Scope-1 greenhouse gas (GHG) emissions occur from sources that are controlled or owned by a firm. In our sample of firms, a standard deviation of the cross-section of scope-1 emissions equals 15.8 tons of $CO_2e$. The average level

committed firms) experience a relative reduction in total debt, compared to firms with the same levels of emissions but without ex-ante lending connections to the committed bank. The effect is economically significant with the difference in total debt of 6.4 pp per one standard deviation change in emissions. In turn, we do not find strong evidence of credit supply forces based on the variation in levels of scope-2 and scope-3 emissions. The distinction in results between the two types of emissions likely results from the fact that scope-1 emissions are easier to track and attribute to specific firm actions; hence, creditors find it easier to screen on such metrics.

We provide further evidence on the source of the financing effect using several additional tests. We first divide firm total debt into bank debt and non-bank debt and find that the effects for total debt are entirely driven by adjustments in bank debt, which suggests that the differences in leverage are a direct consequence of bank decisions rather than they are an outcome of an indirect channel in which banks affect the financial decisions of other market participants. We also find that banks are particularly responsive to firms with clear brown or green label, that is, firms located in the tails, both left and right, of the cross-sectional distribution of emissions. Further, the results survive a battery of robustness tests typical for the difference-in-differences setting. Specifically, we find that both the treatment and control groups follow similar trends prior to commitment episodes. Within non-committed firms, effects on bank lending are insignificant in all the periods. For committed firms, effects are only significant after their banks commit. We also find that both sets of companies are not very different along various firm-level observables. Finally, the results satisfy the test of selection of unobservables based on Oster (2019) and Altonji et al. (2005), that is, in the process of sequentially controlling for a large number of observables and different sets

of scope-1 emissions is close to 3.4 million tons of CO2e. Scope-2 emissions relate to the purchase of electricity (and steam and heat) and scope-3 emissions originate within the value chain in which a company operates.

of fixed effects (e.g., industry-time and firm fixed effects) that massively increase the R-squared, estimated effects remain very similar.

In our subsequent tests, we aim to shed more light on the underlying economic mechanism driving bank financing decisions. We consider two possible hypotheses. In the first one, banks cut credit to high-emission firms and channel credit to low-emission firms if they recognize that financial risk associated with their operations positively correlates with their emission activity. An alternative hypothesis is that committing banks make their credit decisions strictly based on their preferences for green assets. To distinguish between the two hypotheses, we control in our regressions for measures of credit risk dependent on the extent of leverage and underlying stock return volatility. The resulting estimates show that the preference channel retains its economic significance after controlling for financial risk, even though financial risk also exerts a meaningful effect on credit allocation.[2] From a quantitative perspective, after controlling for the risk channel, committed firms with one standard-deviation higher scope-1 emissions experience a credit cut by 5.1 pp (as compared to uncommitted firms), whereas the overall effect without controlling for firm risk is 6.4 pp. At the same time, committed firms with one standard-deviation higher risk experience a reduction in bank debt by 5.7 pp relative to equally risky uncommitted firms.

To provide a more nuanced view of the bank-lending channel, we next turn to loan-level data. Doing that allows us to separate effects that are driven by syndicated loans only from those that are possibly driven by other lending arrangements. At a broad level, the loan-level data allows us to include firm-time fixed effects in our regressions. Consequently, we can analyze lending

[2] More formally, we use (lagged by one quarter) rolling stock-return volatility, multiplied by firm financial leverage (debt over total assets), as a proxy for firm risk. In practice, we triple interact such firm risk measure with an indicator for whether a firm is committed and with a *post* indicator for whether a firm's lender has committed or not yet.

from committed vs. non-committed banks to the same firm at the same time, going in the direction of isolating a credit supply force. We find that adjustment through syndicate loans happens along the extensive margin: committed firms with high emission levels experience a relative decline in their frequencies of (syndicated) loan issuance. At the same time, we do not find a significant result on the intensive margin, that is, committed banks extend their syndicated loans as an in-or-out decision and do not partially ration the quantity of credit. This result further supports the preference story along the lines of the divestment channel we observe in capital markets (e.g., Hong and Kacperczyk, 2009). We also look at loan prices as another channel through which the credit channel could operate. While the extensive-margin result mechanically rules out the possibility of direct pricing effect by the committing banks, we do find that the average loan rates that the committed brown firms must pay still increases. This is possibly because other banks increase their costs for brown firms or because other types of bank loans become more expensive. Our results suggest that brown firms in which their banks commit are penalized by both the quantity and price forces.

Our results so far establish strong evidence of discrimination by committed banks applied to firms with high levels of emissions compared to those with low levels of emissions. While the increased cost of accessing credit may offer discipline to polluting firms, the ultimate question is whether such firms indeed respond to the market force. To answer this question, in a final set of tests, we investigate firm-level real effects, including environmental and operational effects. In the first set of tests, we evaluate the impact of credit pressure on firm leverage and investment decisions. Our estimates suggest that brown and committed firms undergo a process of deleveraging, characterized by shrinking asset size. A one-standard-deviation increase in ex-ante scope-1 emissions leads to 2 pp reduction in asset size. Business operations are likewise affected.

In a similar experiment, we find that *CAPEX* of brown firms goes down by 4.3 pp. We also consider employment and sales decisions. The results are slightly weaker for those outcomes.

The above results suggest that firms do respond to bank pressure. However, the question is whether they adjust their environmental performance consistent with the committed banks' preference. On the one hand, committed firms have significant incentives to become relatively greener, as this grants easier access to bank financing; on the other hand, the tightening of credit standards due to SBTi commitments might limit their ability to invest in green technology and it is costly to do so. Our findings provide a mixed picture. Indeed, we find that committed firms with higher emissions significantly improve their *E* (environmental) scores, although by just 10 pp as a response to the one-standard-deviation higher scope-1 emissions. At the same time, effects are insignificant for the non-environmental ESG metrics. When we decompose the *E*-score into its subcomponents, we do not find any evidence of a significant change in environmental expenditure and, crucially, in overall ex-post scope-1 emissions. Also, in the year after the shock, affected firms do not seem to make any more commitments regarding their own plans to reduce emissions in the future. Instead, what drives the improvement in environmental performance is better communication. Since such communication efforts do not lead to any changes in real emissions or plans to reduce them, they could simply reflect some form of greenwashing by such companies. Nonetheless, committed banks perceive these efforts as non-credible given that we still observe a significant credit pressure.

***Contribution to the literature***. Our paper contributes to the recent and flourishing literature on climate change and finance.[3] By now, there is a relatively large evidence that investors ask for a

[3] For a review of this literature, see Giglio, Kelly, and Stroebel (forthcoming).

premium to hold stocks of firms highly exposed to climate risk (e.g., because of high level of carbon emissions, as in Bolton and Kacperczyk, 2020a, 2020b, and 2021a), especially during periods in which climate risk is perceived to be higher (Engle et al., 2020; Choi, Gao, and Jiang, 2020). Similarly, corporate bonds issued by firms highly exposed to climate risk are found to grant lower future ex-post returns, amplified by perceptions of increased climate risk (Huynh and Xia, 2020; 2021). Our paper shows the bank channel for carbon risk.

The literature on the implications of climate change for banking is rather sparse. Few papers analyze how loan pricing responds to firm exposure to carbon risk through carbon emissions (Delis, de Greiff, and Ongena, 2019; Degryse et al., 2021; Ehlers, Packer, and de Greiff, 2021). Ginglinger and Moreau (2019) and Nguyen and Phan (2020) show that greater exposure to climate risk is associated to a reduction in corporate financial leverage. Reghezza et al. (2021) show bank lending is reduced using loan level data after the Paris Agreement. Our paper is the first to look at the ultimate implications of climate risk on the allocation of credit across firms with different levels of exposure to climate risk through carbon emissions and, importantly, we document that committed banks cut credit supply to firms that pollute relatively more, with significant firm-level real effects (e.g., firm investment). Moreover, the benefits for climate risk stem from the reallocation of credit supply towards green firms rather than brown more affected firms becoming more green.[4] Though we find an improvement in the *E*-score, consistent with the view that firms with high exposure to climate risks have incentives (such as higher market valuation) to take action to reduce such exposures (Pérez-González and Yun, 2013), our analysis also show that environmental expenditure does not drive such the adjustment in the *E*-score and

[4] Our definition of climate risk follows the concept of *transition risk*, resulting from firms' exposure to the process of transition to carbon neutrality. This is in contrast to *physical risk*, which results from damages due to climate disasters.

there is no reduction in (hard) firm-level carbon emission. Finally, our results suggest that corporate deleveraging is due to bank-lending channel, prompted by a change in banks' preferences towards lending to green-*vs*-brown firms, rather than a financial risk factor.

The rest of the paper is organized as follows. Section 2 presents our data and Section 3 describes our empirical strategy. We present our findings in Section 4 and next we briefly conclude.

## 2. Data

Our main analysis covers a sample of international firms for the period 2013-2018. The data we use result from merging the following sets: (i) syndicated lending relationships from Thomson Reuters Dealscan; (ii) firm-level GHG emissions from S&P Global Trucost; and (iii) firm-level information (e.g., firm output, investment, leverage, or return volatility) from Compustat Global. Information from Compustat Global are matched with Dealscan following the methodologies in Chava and Roberts (2008) for non-financial companies (NFCs) and Schwert (2018) for lenders. We matched Trucost data with the rest using ISIN. The combined data is a sample of 2112 firms, of which 631 firms have their headquarters located in the US, 348 in the European Union, 192 in the UK, and the remaining 941 firms are located elsewhere. We also use firm-level information from Capital IQ on firm-level bank vs. nonbank finance, from MSCI on ESG ratings, and on firm-level environmental expenditure from Refinitiv. We report all summary statistics in Table 1.

In our empirical strategy, we utilize the data on bank commitments, following the Science Based Targets initiative (SBTi).[5] For some tests, we also identify NFCs which directly commit to SBTi.

[5] Bolton and Kacperczyk (2021b) provide more details on the SBTi.

The SBTi is a joint initiative by Carbon Disclosing Project (CDP), the UN Global Compact, the World Wide Fund for Nature (WWF) (formerly named the World Wildlife Fund), and the World Resources Institute (WRI), whose purpose is to define and promote net-zero targets in line with the climate science. The overall goal of the initiative is to induce companies to commit to decarbonization pathways to increase the chance that global emissions can be reduced to a level that limits average temperature rise below 1.5°C. The SBTi now comprises just over 1000 companies in 60 countries, with a combined value of $20.5 trillion.[6] The SBTi commitments vary both in the choice of base year for emissions and the horizon of interim targets. To join the SBTi, a company must first sign a commitment letter stating that it will work to set a science-based emission reduction target. It then has 24 months to develop and submit a target for validation. Once the target has been validated it is disclosed.

Our sample includes 59 banks that belong to 11 bank holding groups that either committed or stated a target for emission reductions.[7] In general, banks commit in a staggered fashion. The first wave of commitments in our sample occurs in June of 2015 (2015Q2) with other important rounds of commitments in November 2015 and April 2016. We label each lender in Dealscan as committed, or not, depending on whether it eventually joins the SBTi, while also keeping track of the bank-specific commitment date. Formally, for each lender in our sample, we define two indicator variables: $Post_{b,t}$ is equal to one if bank $b$ has committed by quarter $t$, and $Committed_b = max_b(Post_{b,t})$, which is equal to one if bank $b$ ever commits to SBTi.

An important step in our analysis is establishing which firms are connected, through prior credit intakes, to banks which are committed to achieving green targets. For each NFC in our

[6] See, "From Ambition to Impact: Science Based Targets Initiative Annual Progress Report 2020."

[7] The list of lenders committed to SBTi comprises: ING Groep NV; Westpac Banking; Bancolombia SA; BNP Paribas; Société Générale; HSBC; BBVA; Standard Chartered; YES Bank; ABN Amro; Commercial International Bank Egypt.

sample, we compile the list of lenders in Dealscan the firm has (ex ante) borrowed from. For instance, the generic couple of firm *f* and bank *b* is defined as connected in quarter *t* if firm *f* has ever borrowed from bank *b* up to *t*, and defined as unconnected, otherwise. A firm is labelled as committed if at least one of its lenders is committed. Formally, let $B_f$ be the set of connected lenders of firm *f*. Then, $Post_{f,t} = max_{Bf}(Post_{b,t})$ takes a value of one from the date of the first commitment of firm *f*'s lenders, and zero before. Committed firms are those whose lenders eventually commit, that is, those for which the indicator variable $Committed_f = 1$.

The summary statistics in Table 1 suggest that 76.9% of the NFCs in our sample are connected to committed banks. This large share reflects the fact that committed banks are very active institutions in the syndicated loan market. To explore additional variation in lending arrangements, we define other variables to capture the strength of such relationship. First, we identify lead banks (or lead arrangers) in the syndicate (along the lines of Ivashina, 2009). Such institutions exert a prominent role in the issuance of syndicated loans, for example, they are primarily responsible for loan pricing, typically due to pre-existing stronger relationships with the borrower relative to the other banks in the syndicate. The resulting variable $LeadCommitted_f$ implies that the committed relationship involves at least one such lead bank for the firm; 56.2% of the firms have a lead-arranger committed to SBTi. Second, for our connection indicator, and its lead-bank counterpart, we construct an intensive-margin proxy, namely, the share of lenders ($\%Committed_f$) and the share of lead-arrangers (*%LeadCommitted*), out of the total number of firms' lenders committed to SBTi. On average, 15% and 12.8% of firms' total number of lenders involve committed banks and committed lead-banks, respectively. Third, based on the last two variables, we define high and low exposure to committed banks depending on whether the share of committed banks (and of lead arrangers) is above or below the sample median value.

For our analysis of emissions, we access, from Trucost, yearly firm-level GHG emissions. We mostly focus on scope-1 emissions, that is, direct greenhouse gas emissions that occur from sources that are controlled or owned by a firm. As the first bank commitment happens in 2015 and our aim is to rely on ex-ante measures of firm pollution, we start by building a GHG-exposure variable, given by the average firm-level scope-1 emissions over the period 2013-2014, expressed in tons of emissions and denoted as $S1_f$. Indeed, our sample features a highly heterogenous and skewed distribution for $S1_f$, as presented in Table 1. The average firm produces roughly 3.56 million tons of emissions (which is also close to the median level of emissions) per year. Moreover, a cross-sectional standard deviation of $S1_f$ equals 13.8 million tons. To deal with such a highly non-linear distribution of scope-1 emissions, for practical purposes, we take the natural logarithm of $S1_{f,pre}$, obtaining the relatively more normally distributed variable, $Log\text{-}S1_f$. Eventually, for easing the interpretation of our coefficients, we demean $Log\text{-}S1_f$ and the resulting demeaned variable is indicated as $\overline{Log\text{-}S1_f}$; its distribution is likewise described in Table 1.[8]

For some of our empirical tests we use financial variables, which we obtain from Compustat Global. The mean outstanding log-total debt in our sample of firms corresponds to $1276 million, with notable differences across firms: for instance, a firm in the 4th quartile of the debt distribution has a volume of total debt which is more than two times bigger than the one in the 1st quartile. On average, total debt amounts to roughly 30% of total assets, as is evident from the summary statistics for firm leverage (defined as total debt over total assets). In addition, from Capital IQ, we gather information on total bank debt, which, on average, equals 40% of total debt; the remaining fraction is predominantly bond-financed debt. Throughout our analysis, we apply

[8] Note that the mean of $Log\text{-}S1_{f,pre}$ is not exactly zero. This is due to the fact that when demeaning, we subtract the mean based on the firm-level distribution (that is, one observation per firm) instead of the in-sample distribution. The two distributions differ slightly because a small number of firms eventually exit our sample before 2018.

different firm-level controls, also predetermined at their 2013-2014 mean values, including a proxy for firm revenue growth and firm size (log of total assets). Moreover, we also proxy for firm-level default risk using a rolling-window stock-return volatility multiplied by firm financial leverage (debt over total assets). Firms exhibit notable heterogeneity in the risk, with an average risk factor of roughly 10.5 pp and an associated standard deviation of 8.6 pp.

For the analysis of real effects, we define the following variables. Sales and capital expenditure (*CAPEX*) are both expressed in log terms and measured at a quarterly frequency. Both variables display a large extent of variation. For instance, a firm in the 4$^{th}$ quartile of *CAPEX* (sales) has values 9 (6) times larger than a firm in the 1$^{st}$ quartile. We also use ESG scores and its *E* (environmental) subcomponent. These are obtained from MSCI. Both variables take on values ranging from 0 (worst) to 10 (best). For both variables, the average firm has a score close to 5 points, with a standard deviation being close to 2 points. In practice, the ESG score is computed as a weighted average of its three main subcomponents, which, in turn, are obtained as a weighted average of further (sub)subcomponents. For the *E* subcomponent, we additionally gather information on the underlying factors used for its computation, namely: climate change (resulting from firm performance in terms of, for example, carbon emissions, energy efficiency), natural resources (capturing firm contribution to water stress, biodiversity and land use, and the sourcing of raw material), pollution and waste (proxying for, for example, firms' toxic emissions and waste, product packaging), and environmental opportunities (assessing firms' awareness and ability to exploit opportunities in clean technologies, energy, and buildings). Finally, from Refinitiv, we also gather information on firm-level annual environmental expenditures. These represent a very small fraction of total assets, close to 1%.

Finally, to better dissect whether the adjustment in credit driven by bank commitments is demand or supply-driven, we conduct an analysis of syndicated loan issuance at the firm*committed-bank*time level. Our analysis is at the extensive margin,[9] that is, for each firm *f*, we construct a variable, $1(Issuance)_{f,c,t}$, tracking whether a firm *f* at time *t* issued a syndicated loan through a committed bank and/or through an uncommitted bank. Note that for firms ex-ante indebted with committed (or uncommitted) banks only, $1(Issuance)_{f,c,t}$ will have one observation per time period. In turn, for firms connected with both types of lenders, $1(Issuance)_{f,c,t}$ will have two observations per time-period. The variable suggests that syndicated loan issuance is overall very lumpy. Firms issue a loan (either through a committed or uncommitted bank) in just around 8% of the quarters.

## 3. Empirical Strategy

In our main empirical specifications, we investigate the implications of banks' commitments to SBTi for different firm-level outcome variables, $y_{f,t}$, given by debt variables (e.g., total debt, bank debt, and non-bank debt), real effects (e.g., sales, CAPEX), and environmental effects (e.g., environmental score, carbon emissions, environmental spending). Formally, we estimate the following triple difference-in-differences model:

[9] We also analyze the intensive margin for the granted loans.

$$y_{f,t}=\beta_1\overline{\text{Log-S1}}_f+\beta_2\text{Committed}_f+\beta_3\widetilde{\text{Post}}_t+\beta_4\overline{\text{Log-S1}}_f\text{Committed}_f+$$

$$\beta_5\widetilde{\text{Post}}_t{*}\overline{\text{Log-S1}}_f+\beta_6\text{Post}_{f,t}{*}\text{Committed}_f+\beta_7\text{Post}_{f,t}{*}\text{Committed}_f{*}\overline{\text{Log-S1}}_f+$$

$$\theta_1\text{Controls}_f+\text{FE}+e_{f,t} \quad (1)$$

In the above equation, $\widetilde{\text{Post}}_t$ is an indicator variable equal to one from 2015Q2, the first date in which banks commit to SBTi, onwards. We include this variable to control for secular changes in firm outcomes occurring, for example, due to the Paris agreement ratified in November 2015. Results are very similar if we set a post-Paris indicator variable to one for 2015:Q4 and after. Moreover, through the coefficient $\beta_5$, we also control for the possibility that firms with greater level of scope-1 emissions may have generated lower profitability after Paris agreement, thereby experiencing different dynamics in both debt and investment.

Given that $\overline{Log\text{-}S1}_f$ describes a demeaned exposure to climate risk through scope-1 emissions, the coefficient $\beta_6$ pins down the effect of being connected to a committed bank for a firm with average scope-1 emissions. The sign of this coefficient is ex ante uncertain, as bank commitment to green targets is expected to eventually result in cutting credit to firms with high (above average) scope-1 emissions. We expect the coefficient $\beta_7$ to be negative, at least in debt regressions: following commitment by connected banks, firms with above-average scope-1 emissions should suffer a reduction in total debt (and, importantly, bank debt).

There are other factors which may affect the evolution of debt, investment, and other left-hand side variables, beyond exposure to climate risk through GHG emissions. We try to control for them through a vector of firm-level controls, which includes predetermined revenue growth and log assets size. Both variables are fully interacted with the $Committed_f$ indicator and the *post* dummies. Additionally, FE represents a vector of fixed effects, which in the most robust version of the model includes time and firm-specific indicators: the former absorb any variation which is common across all firms; the latter take care of within-firm time-invariant (observed and unobserved) heterogeneity.[10] $e_{f,t}$ represent error terms, which we cluster at the firm level, in line with the fact that the key coefficient of interest is identified by firm-level heterogeneity (Cameron and Miller, 2015).

Equation (1) corresponds to a triple difference-in-differences model with staggered treatment across firms. The key identification assumption for consistently estimating our main coefficient of interest $\beta_7$ is that, absent bank commitment, connected and unconnected firms with comparable levels of scope-1 emissions would have experienced parallel dynamics in firm-level bank debt. Put differently, consistently estimating $\beta_7$ requires an augmented version of the parallel trend assumption to hold. The challenge with respect to a standard difference-in-differences model with common time-treatment is that, given staggered commitment (that is, treatment) across banks, there is not a single time period in which the treatment effect should materialize, thereby complicating the usual pre-*vs*-post comparisons.[11] In our setting, there are in practice two dates in which banks do commit, 2015Q2 and 2016Q2. Hence, we take the following approach. We estimate the equation below separately across committed and uncommitted firms:

$$y_{f,t}=\sum_{t\neq 2015Q1}\beta_t\overline{\text{Log-S1}}_f+\sum_{t\neq 2015Q1}\gamma_t \text{Controls}_f+\Gamma_t+\Gamma_f+u_{f,t} \quad (2)$$

[10] Under a version of the model including firm and time-fixed effects, the coefficients $\beta_1$, $\beta_2$, $\beta_3$, and $\beta_4$ in equation (1) are not identified. Note that firm pollution is observed before any commitment and is not time varying.
[11] For a formal explanation, see Goodman-Bacon (2021).

For uncommitted firms, that is, those with no connection to committed banks, $\beta_t$ should be generally insignificant. Differently, for committed firms, that is, those connected (through syndicated loans) with committed banks, $\beta_t$ may be negative after 2015Q2, with a potential effect also showing up in 2016Q2. $Controls_f$ include, as in equation (1), average revenue growth and asset size over the 2013-2014 period. $\Gamma_t$ and $\Gamma_f$ represent, respectively, time and firm-level fixed effects.

To investigate the debt mechanism, we first divide firm total debt into bank debt and non-bank debt. Second, we analyze the average loan rates that firms must pay. Third, we conduct a loan-level analysis. In particular, for the main loan-level analysis, we study adjustments along the extensive margin, that is, whether high-scope 1-emissions firms connected to committed banks experience a relative fall in the likelihood of being granted a syndicated loan. We study the evolution of a variable, $1(Issuance)_{f,c,t}$, with two observations per quarter $t$, a first one tracking whether the firm issued a syndicated loan through a committed bank, and a second one through an uncommitted bank. We take this approach, instead of investigating a firm*bank*time dataset (larger from a cross-sectional perspective) because syndicated loan issuance is quite lumpy. In fact, using our collapsed firm*(committed/uncommitted)-bank*time data, it turns out that firms issue a loan (either through a committed or uncommitted bank) in just about 8% of the observations. If we were to expand cross-sectionally our data to all connected banks, we would increase dramatically the sparsity of our data, with an exorbitant number of zeros (that is, firm*bank couples with no loan issuance).

We estimate the following regression model:

$$1(\text{Issuance})_{f,c,t}=\beta_1\overline{\text{Log-S1}}_f+\beta_2\text{Committed}_c+\beta_3\widetilde{\text{Post}}_t+\beta_4\overline{\text{Log-S1}}_f\text{Committed}_c+$$

$$\beta_5 \widetilde{Post_t} * \overline{Log\text{-}S1}_f + \beta_6 Post_{f,t} Committed_c + \beta_7 Post_{f,t} Committed_c * \overline{Log\text{-}S1}_f +$$

$$\theta_1 Controls_{f,c} + \Gamma_{firm*time} + e_{f,t} \qquad (3)$$

In this setting, $Committed_c$ varies at the level of committed/uncommitted banks.[12] The model is otherwise identical to that in equation (1). Nonetheless, we are able to control for relatively granular firm time-varying shocks through a vector of interacted firm and year fixed effects, $\Gamma_{firm*time}$. In practice, the identification of our coefficients of interest stems from the comparison of loan issuance for a given firm and in a given year, depending on bank commitment and on firms' GHG emissions.

## 4. Results

### 4.1 Firm-level Debt: Baseline Results

Table 2 reports findings for the estimation of equation (1), with (log) total debt as the dependent variable. We present results under progressively saturated versions of the model. In column 1, we do not include firm controls or fixed effects. In column 2, we augment the model with firm controls (fully interacted with both the post and firm-level commitment indicators). In columns 3 and 4, we add, one at a time, respectively, time-fixed effects –controlling for changes in firm debt which are common across all firms in our sample–and firm-fixed effects, taking care of firm-level time-invariant heterogeneity. Finally, in column 5, we integrate firm controls, time, and firm fixed effects. Across all specifications, the key coefficient of interest, $\beta_7$, describing the ex-post relative

[12] For each firm *f*, this variable is collapsed by analyzing the set of banks with pre-existing syndicated lending relationships. This implies that, for instance, firms connected to committed (or uncommitted) banks only will have one observation per quarter. Differently, companies connected to both types of lenders will have two observations per quarter.

total debt dynamics for committed firms with above average scope-1 emissions, is negative (close to -0.025) and statistically significant at conventional levels.

To gauge the economic magnitude of the described effect, we take as a reference the most robust version of the model in column 5. Following a lender's commitment, firms with a one standard deviation higher log-level of scope-1 emissions experience a relative decline in total debt by 6.4 pp, as compared to other firms without ex-ante lending relationships with committed banks. Notably, the described economic effect does not depend substantially on controls and fixed effects. Indeed, the magnitude of the coefficients swings across columns in a tight [6.4, 8.6] pp interval.

A relevant question is whether the described adjustments conditional on firm-level scope-1 emissions truly reflect a change in committed banks' environmental preferences, or whether they are driven by committed banks being more responsive to differences in risk, among firms with different levels of emissions. In the context of lending the primary source of firm-level risk of concern to lenders would be default risk. To distinguish between the two forces, in column 6, we analyze the impact of scope-1 emissions on total debt controlling for a proxy of firm-level default risk, defined as a (lagged) product of stock returns volatility and firm leverage. Our results indicate that relatively riskier firms connected to committed banks indeed experience a relative decline in total debt (by 5.7 pp in response to a one-standard-deviation increase in default risk), as compared to unconnected firms. Nonetheless, the preference-channel remains statistically and economically significant. From a quantitative perspective, after controlling for the risk channel, committed firms with a one-standard-deviation higher scope-1 emissions experience a credit cut of 5.1 pp (relative to uncommitted firms), whereas the overall effect without controlling for firm risk is 6.4 pp.

Next, in Table 3, we verify whether the adjustments in total debt are driven by bank debt or non-bank debt. Our hypothesis is that the relative decline in debt for firms with higher carbon emissions is due to bank commitment. Hence, under our hypothesis, we would expect larger reductions in bank debt than in non-bank debt. An additional possibility is that banks also affect the financial decisions of other market participants and hence we should also observe adjustments in the level of non-bank debt. Our results suggest that the decrease in total debt is mostly a consequence of the direct channel, in which banks are the main force of debt adjustment. We discuss these results below in more detail.

Since we can only dissect the fraction of debt financed by banks for a subset of the companies in our sample (from Capital IQ), we start by successfully replicating the baseline analysis for total firm debt of such firms in column 1. The results from estimating the model over this subsample of firms are qualitatively and quantitatively comparable to those in Table 3 for the larger sample of NFCs. In column 2, we estimate the same most robust version of equation (1) with bank debt as the dependent variable. Indeed, relative to unconnected firms, connected firms experience a reduction in bank debt if their scope-1 emissions are relatively larger. From an economic perspective, the decline amounts to 12.2 pp as a result of a one-standard-deviation increase in scope-1 emissions. In contrast, in column 3, we do not observe any statistically or economically significant adjustment for non-bank debt.

### 4.2 Firm-level Debt: Robustness

In this section, we provide further robustness to our difference-in-differences model. First, to understand whether the key identification assumption on parallel trends holds, we estimate equation (2) with bank-debt as a dependent variable. We plot the time-varying coefficients in Figure 1. For treated (connected) firms on the right-hand side of the figure, we observe a nonsignificant effect of scope-1 emissions on bank debt before the first date of commitment (2015Q2) and a negative effect thereafter, which is reassuringly more pronounced also in 2016Q2, that is, the quarter in which the second round of commitment takes place. In contrast, for the (unconnected) firms in the control group there is no significant bearing of scope-1 emissions on credit, neither before, nor after 2015q2.

In another test, we examine the differences between treated and control group based on a host of observables. We present the results from the balance test in Table 4 using both unadjusted and adjusted differences between the two groups. Our results indicate no significant differences across the two samples on most observables. The only visible difference is that in log(assets). Firms that are part of the treatment group are on average larger than those of the control group.

In Table 5, we also check whether our results hold using different proxies for a firm's connection to a committed bank. We also evaluate the role of possible non-linearities in carbon emissions. Our baseline findings, reported again in column 1 of Table 5, Panel A, are based on a definition of connection using the extensive margin, that is, a firm is connected to any bank that commits through ex-ante loans. In column 2, we substitute this measure with an intensive-margin one, namely the share of committed banks relative to the number of total lenders a firm is ex-ante indebted to. In this alternative specification, the main coefficient of interest remains statistically and economically significant. A one-standard-deviation increase in scope-1 emissions is associated with a reduction in credit by 4.4 pp for firms with one-standard-deviation higher share of committed lenders (17.8%). Next, in column 3, we condition extensive-margin connections on the committed lender being a lead arranger. The coefficient remains negative (though a bit smaller) but insignificant at conventional levels. In column 4, we replace the extensive-margin connection to committed lead arrangers with the share of committed lead arrangers. The results become statistically significant again, though the economic effect is slightly smaller (3.4 pp cut for firms with a one-standard-deviation greater scope-1 emissions and with a one-standard-deviation higher share of committed lead arrangers). The last two findings suggest that while committed lead arrangers may shield their borrowers from larger credit cuts (e.g., Bolton et al. 2016), being connected to them becomes binding if committed lead arrangers have a high enough weight in a firm's loans portfolio. Finally, in columns 5 and 6, we split exposures into high and low, depending on whether the share of committed lenders and of committed lead-arrangers, respectively, are above or below the sample median values. In both cases, a larger credit cut is driven by high exposures.

In Panel B, we examine whether discrimination by committed banks based on carbon emissions exhibits any nonlinearities. This test is motivated by the fact that the original (non-log-transformed) distribution of scope-1 emissions is highly skewed to the left, as highlighted in the data section. Formally, we split firms according to quintiles of the distribution of ex-ante scope-1 emissions and replace such quintile dummies with the $\overline{Log\text{-}S1_f}$ exposure variable. In Panel B of Table 5, the baseline group is given by the firms with the highest, top-quintile, emissions. We find that, in relative terms, these firms experience a reduction in total debt of 13.86%, as compared to firms with lower emission levels, especially the most environmentally friendly ones in the bottom-quintile emissions. The results in columns 2 and 3 further indicate that this effect can be entirely explained by the adjustment in bank debt. The effect for bank debt is particularly striking as the difference between highest and lowest-emission quintiles is a staggering 47%. Further, we can observe that the effect of emissions on debt is largely monotonic across quintiles, which supports the story of screening by banks based on total scope-1 emissions.

As a final robustness, we also perform a test for whether our estimates are potentially driven by self-selection along unobservables. Indeed, as we have argued in Table 4, connected and unconnected firms differ most notably on total assets. As such, differences in asset size may be symptomatic of differences along other dimensions that are not observed, for example, TFP. Nonetheless, given that the main coefficient in Table 2 is stable in different versions of the model this concern is unlikely to be relevant. This is particularly true as progressive saturation of the model with observable controls and different fixed effects implies an increase in R-squared by more than 60 pp moving from column 1 to 5 (Altonji et al., 2004). We formally verify this statement following Oster (2019). In practice, we assume that unobservables correlate with the treatment in the same way as observables (and fixed effects) do and fix an upper-bound for the ideal R-squared after controlling for all unobservables to one. Under these assumptions, the upper-bound for our coefficient of interest $\beta_7$ is -0.02013, which is strictly smaller than zero and also preserves its economic significance.

### 4.3 Loan-level Estimates

In this section, we report the results based on loan-level analysis. Table 6 reports results from the estimation of equation (3). Again, we consider progressively saturated versions of the model. Most notably, in column 5, we include a specification in which, on top of firm controls (fully interacted with the post and commitment dummies) and firm*year fixed effects, we include firm*committed bank fixed effects. Including this set of fixed effects ensures that we control for different baseline levels in the likelihood of issuing loans across committed and uncommitted banks.

Across all the specifications of the model, the coefficient of interest, loading on the triple interaction between the post and commitment indicators and firm scope-1 emissions is negative. Moreover, further including controls and fixed effects increases both the size of the coefficient and its statistical significance. Here, using firm*time fixed effects which proxy for firm-level time-varying unobserved shocks, including demand driven, is especially important as the strengthening of the coefficient suggests the credit adjustments are supply driven.[13] In the most robust version of the model in column 5, the effect is significant at the 5% level of statistical significance and economically meaningful. Connected firms with a one-standard-deviation higher scope-1 emissions experience a relative decline in the likelihood of issuing a new syndicated loan by roughly 90 basis points.[14] This is a large adjustment, equal to roughly 10% of the unconditional average frequency of loan issuance.

In the final test, we also verify that such loan-level effects hold at the firm level. That is, we estimate a model similar to the one in equation (1), using instead an indicator variable for whether a firm issued a loan or not in a given quarter as a left-hand side variable. This is a key step to establish that the adjustments in a firm-level bank debt are due to a reduced (credit-supply driven) ability to issue loans. Indeed, results in Table 7 suggest that this is the case (column 2), especially for firms with high shares of committed lenders (column 3).

### 4.4 Real Effects: Deleveraging and Investment

One of the main questions in our paper is whether the reduction in bank lending triggers any firm real adjustments. In particular, do banks provide necessary discipline to reduce firm emissions, or does the reduction in lending gets absorbed by firms in their other decisions? To shed more light on these questions, we start by investigating potential effects on firm deleveraging. We report the results in Table 8. For ease of comparison, in columns 1 and 2, we repeat the analysis using total

[13] As the estimated coefficient increases in absolute value with controls, the Oster (2019) test also implies a significant lower bound.

[14] While we find significant effects for the extensive margin of loans, we find insignificant effects for the intensive margin of credit (volume).

debt and bank debt as dependent variables. In column 3, we use Firm Leverage as a dependent variable, defined as total debt over total assets. We find that committed firms with relatively higher scope-1 emissions experience a significant decrease in leverage. The magnitude of the adjustment is, nonetheless, quite small. A one-standard-deviation increase in scope-1 emissions implies a relative reduction in leverage for connected firms (as compared to unconnected ones) by just 60 basis points. This effect is small when compared to both the unconditional mean leverage in the sample (equal to 30%) and to the decrease in the numerator, that is, total debt, associated to the same variation in scope-1 emissions (6.4 pp). This result motivates our investigation of total assets as a separate dependent variable. We find that bank commitment is associated with a significant shrinkage in total assets for companies with high levels of scope-1 carbon emissions. Connected firms with a one-standard-deviation higher scope-1 emissions reduce the overall size of their balance sheets by roughly 2 pp.[15] When we decompose firm assets into their equity portion, we do not observe any significant variation in firm equity associated with bank commitment, as reported in column 5. This result implies that firms do not substitute debt finance with equity funds perhaps because equity finance is also relatively more expensive, as presented in Bolton and Kacperczyk (2020a, 2021a). Instead, our findings show that bank commitments are associated with deleveraging by firms with relatively higher carbon emissions.

Another dimension of firm behavior we consider are business operations, such as CAPEX, sales, and employment. We present the results in Table 9. We find mixed results with respect to the three measures. On the one hand, we observe a significant cut in firm investments, as proxied

[15] A back of the envelope calculation suggests that the overall decline in leverage is roughly in line with the described magnitudes of the adjustment in the numerator (total debt) and denominator (total assets). Note, in fact, that, as for any ratio, we can write the first derivative of leverage with respect to $log\text{-}S1_f$ as the first derivative of the numerator (debt) minus the first derivative of the denominator (assets), multiplied by leverage itself. This corresponds to (-0.064 + 0.02) *leverage. For a firm with average leverage close to 30%, this translates in a 40-bps decline in leverage.

by (log) CAPEX (see column 1). Connected firms with a one-standard-deviation higher scope-1 emissions reduce their *CAPEX* by 4.3 pp (as compared to unconnected NFCs). On the other hand, we find insignificant effects for firm employment (in column 2) and sales (in column 3). Overall, while the investment result is consistent with the deleveraging effect in that lower asset base requires less investment, it may also imply that tightened credit standards reduce the ability of high-emission firms to finance investments needed to improve their green technology. We turn to studying the environmental effects in the next section.

### 4.5 Environmental Performance: Emissions, ESG Metrics, and Expenditures

The underlying premise of bank commitments is their disciplinary effect on emission production. A simple adjustment cost mechanism would imply that banks that redirect lending towards greener companies should incentivize brown firms' adoption of cleaner technologies. After we have established a decline in lending for NFCs with higher scope-1 emissions and an increase in lending to firms with lower scope-1 emissions, we aim to understand whether the brown companies indeed adjust their operations and technologies to become relatively greener.

To evaluate this mechanism, we consider a host of regressions in which the dependent variables measure firms' environmental performance along different dimensions. We present the collective findings in Table 10. As a first test, we examine whether connected firms reduce their scope-1 emissions. Our dependent variable is one-year ahead scope-1 emissions measured on an annual basis. While the results suggest that the average connected firm reduces scope-1 emissions significantly by 35 pp, we do not observe an additional marginal effect for firms with relatively higher scope-1 emissions.

Next, we consider a broadly defined ESG score as a dependent variable. The results in column 2 also show no relevant treatment effect, that is, connected firms with higher emissions do not seem to improve their ESG metrics. However, when we look specifically at the *E* component of the ESG score, which tracks environmental performance at the firm level, in column 3, we find some statistical differences. Connected firms with a one-standard-deviation higher scope-1 emissions improve their *E*-scores by roughly 10 pp (as compared to firms with similar level of emissions but without connection to committed banks). Still, the result is relatively small economically, given that the *E*-score varies between 0 and 10. In contrast, we do not observe any significant adjustment in environmental expenditures, neither when it is measured in logs (column 4), not when it is rescaled by total assets (column 5). This variable is however available for just a very small subset of firms and hence our results should be interpreted with caution. In column 6, we further analyze whether affected firms increase their usage of renewable energy. We find no significant result. Finally, since adjustment of environmental performance may be a slow process, we study whether affected firms at least express their willingness to commit to future emission reduction, using SBTi commitments of NFCs. Again, we do not find a statistically significant incidence in this type of efforts.

As a final step of our analysis, we dig deeper into more granular drivers of the improvement in the *E*-factor. The results are presented in Table 11. For ease of interpretation, we begin by reporting, in columns 1—4, the results related to the overall ESG score and to the *E* (environmental score), *S* (social score), and *G* (governance score), respectively. Only the *E*-factor displays a significant change (improvement) for affected firms. In the subsequent tests, we use different subcomponents of the *E-score*, defined by MSCI, as our left-hand-side variable. We do not find any improvement for affected firms in terms of their climate change mitigation efforts (column 5), waste reduction through a revision of product packaging policies (column 6), or carbon emissions (column 8). If anything, firms also perform worse in terms of their usage of natural resources (column 7). The only small improvement observed in the *E*-factor results from a mixed improvement in the awareness of affected firms about environmental opportunities (e.g., related to clean technology). Whether this effect reflects a changed corporate perspective on environmental problem or is a manifestation of greenwashing is difficult to confirm using our data. Combined with the significant reduction in bank debt it seems that the latter may be a more likely explanation of firm policies, which the banks in fact do not find credible.

Overall, our findings suggest that connected firms become at least partly aware of climate-related issues, but despite that, in the short term, they do not materially improve their environmental performance. An interesting twist to the story is whether tightening of financial constraints could be the reason why brown firms do not improve their environmental performance.

## 5. Conclusions

One of the most relevant questions in the current debate on climate policies is whether financial sector can provide some discipline to spur improvement in environmental performance of the corporate sector. We analyze this problem in the context of the commercial banking sector. Using global data for the period of 2013-2018 and bank commitments as a form of changes in attitudes to green finance, we find strong and robust evidence that committed banks redirect lending towards greener firms and cut credit supply to brown firms. We find that the cut in total debt is entirely driven by adjustments in bank debt. For bank debt, effects, both positive and negative, are especially strong in the extreme quintiles of the emission distribution. Using loan-level data, we also find that adjustment through syndicate loans happens along the extensive margin: brown and committed firms experience a relative decline in the frequency of (syndicated) loans issuance. Further, the average loan rates that the committed brown firms must pay increase. The bank lending channel operates more as a bank preference channel for green attitude rather than as an adjustment to changing business risk, though the latter also plays some role. In sum, our results suggest that corporate deleveraging is due to bank-lending channel, prompted by a change in banks' preferences towards lending to green-*vs*-brown firms, rather than a risk factor.

Finally, we gauge whether affected firms improve their environmental profile. On one hand, committed firms have significant incentives to become relatively greener, as this grants easier access to bank financing; on the other hand, the tightening of credit standards due to SBTi commitments might limit their ability to invest in green technology and it is costly to do so. The findings provide a somewhat mixed picture. Indeed, we find that committed and brown firms improve the environmental component of the ESG score significantly. However, the economic effects are small and, importantly, we do not find any evidence of significant change in environmental expenditure and crucially in overall ex-post scope-1 emissions. In consequence, our results suggest that the benefits for climate risk stem from the reallocation of credit supply towards green firms (cutting also credit to brown firms) rather than brown (more affected) firms becoming greener.

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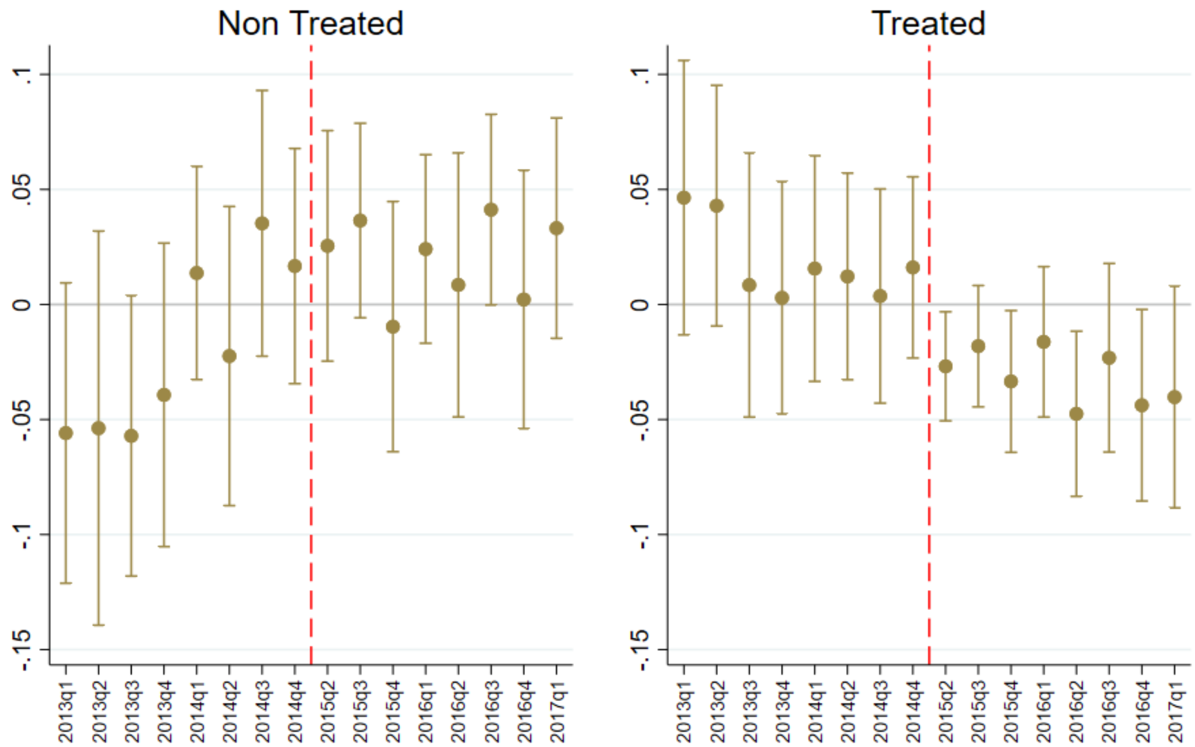
Figure 1. Bank Lending: Parallel Trends




This figure plots time-varying coefficients from a regression with log one plus bank lending as a dependent variable and the primary variable of interest: ex ante log emissions demeaned interacted with an indicator for each time period. 2015Q1 is the omitted base level and is one period before any bank commits to reducing emissions, indicated by the dashed red line. The left panel shows firms that never borrow from a bank that commits and right panel shows firms that do borrow from at least one bank that commits. The regressions include firm and time fixed effects and controls for predetermined total assets and revenue growth averaged over 2013 and 2014 interacted with the date indicators.

Tables

## Table 1: Summary Statistics

This table reports summary statistics for the variables used in the analysis. The sample period is 2013-2018. Ex ante variables are averaged over 2013 and 2014.

| VARIABLES | (1) N | (2) mean | (3) sd | (4) p25 | (5) p50 | (6) p75 |
|---|---|---|---|---|---|---|
| Log-S1$_{ft}$ | 8,691 | 11.78 | 2.538 | 10.03 | 11.55 | 13.42 |
| Log-S1$_f$ (ex-ante) | 2,112 | 11.78 | 2.538 | 10.04 | 11.54 | 13.39 |
| $\overline{\text{Log-S1}}_f$ (ex-ante, demeaned) | 2,112 | -3.22e-09 | 2.538 | -1.737 | -0.238 | 1.608 |
| Committed$_f$ | 41,450 | 0.769 | 0.421 | 1 | 1 | 1 |
| Post$_{f,t}$ * Committed$_f$ | 41,450 | 0.383 | 0.486 | 0 | 0 | 1 |
| Lead Post$_{f,t}$ * Committed$_f$ | 41,450 | 0.266 | 0.442 | 0 | 0 | 1 |
| % Post$_{f,t}$ * Committed$_f$ (ex-ante) | 41,450 | 0.0785 | 0.148 | 0 | 0 | 0.118 |
| % Lead Post$_{f,t}$ * Committed$_f$ (ex-ante) | 41,450 | 0.0638 | 0.145 | 0 | 0 | 0.0339 |
| Committed$_f$ (% committed) | 41,450 | 0.150 | 0.178 | 0 | 0.107 | 0.200 |
| Committed$_f$ (lead committed) | 41,450 | 0.562 | 0.496 | 0 | 1 | 1 |
| Committed$_f$ (% lead committed) | 41,450 | 0.128 | 0.184 | 0 | 0.0625 | 0.194 |
| Committed$_f$ (high % committed) | 41,450 | 0.349 | 0.477 | 0 | 0 | 1 |
| Committed$_f$ (low % committed) | 41,450 | 0.361 | 0.480 | 0 | 0 | 1 |
| Committed$_f$ (high % lead committed) | 41,450 | 0.257 | 0.437 | 0 | 0 | 1 |
| Committed$_f$ (low % lead committed) | 41,450 | 0.261 | 0.439 | 0 | 0 | 1 |
| Log Total Debt | 41,450 | 7.152 | 1.543 | 6.230 | 7.369 | 8.387 |
| Log Bank Debt + 1 | 32,844 | 5.367 | 2.468 | 4.581 | 6.041 | 7.232 |
| Log Non-Bank Debt + 1 | 32,844 | 5.503 | 2.885 | 4.047 | 6.488 | 7.818 |
| Leverage | 41,450 | 0.304 | 0.155 | 0.202 | 0.307 | 0.375 |
| Log Assets | 41,450 | 8.534 | 1.169 | 7.747 | 8.623 | 9.598 |
| Log Equity | 40,318 | 7.471 | 1.157 | 6.757 | 7.600 | 8.496 |
| Risk | 37,641 | 10.55 | 8.606 | 5.218 | 7.845 | 12.23 |
| Revenue growth (ex-ante) | 2,112 | 0.0540 | 0.233 | -0.0496 | 0.0202 | 0.0905 |
| Log Assets (ex-ante) | 2,112 | 8.384 | 1.205 | 7.582 | 8.455 | 9.444 |
| Log Capital Expenditures | 38,120 | 3.723 | 1.550 | 2.762 | 3.932 | 5.148 |
| Log Sales | 37,926 | 6.638 | 1.210 | 5.850 | 6.787 | 7.797 |
| Log Employment | 7,734 | 2.477 | 1.113 | 1.668 | 2.463 | 3.485 |
| Interest Expense | 36,951 | 0.0123 | 0.00775 | 0.00833 | 0.0106 | 0.0141 |
| Loan Issuance | 39,039 | 0.0898 | 0.286 | 0 | 0 | 0 |
| ESG | 31,687 | 4.730 | 1.163 | 4 | 4.700 | 5.500 |
| Environmental Score | 31,687 | 5.149 | 2.209 | 3.500 | 4.900 | 6.500 |
| Social Score | 31,687 | 4.482 | 1.758 | 3.400 | 4.500 | 5.600 |
| Governance Score | 31,685 | 5.585 | 2.085 | 4.100 | 5.500 | 7 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Climate Change Score | 29,269 | 6.411 | 2.881 | 4.400 | 6.700 | 9 |
| Natural Resource Score | 24,623 | 4.992 | 2.486 | 3.300 | 4.700 | 6.500 |
| Waste Management Score | 24,016 | 5.498 | 2.577 | 3.600 | 5.500 | 7.600 |
| Environmental Opportunity Score | 13,420 | 4.579 | 1.561 | 3.400 | 4.400 | 5.700 |
| Carbon Emissions Score | 26,614 | 6.929 | 2.706 | 5.300 | 7.200 | 9.500 |
| Log Environmental Expenditure + 1 | 1,962 | 4.082 | 2.954 | 1.902 | 3.621 | 5.720 |
| Environmental Expenditure / Total Assets | 1,961 | 0.875 | 8.431 | 0.000783 | 0.00360 | 0.0240 |
| Renewable Use | 35,112 | 0.506 | 0.500 | 0 | 1 | 1 |
| Firm Commitments | 41,450 | 0.00914 | 0.0952 | 0 | 0 | 0 |

## Table 2: The Effects of Bank Commitment on Total Firm Debt

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variable is log total debt. In columns (1) through (5) the primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. Each column adds controls and more stringent fixed effects. Column (6) includes firm risk, defined as stock volatility times leverage. ***p<.01, **p<.05, *p<.1

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | | | Total Debt | | | |
| $Post_{f,t}$ * $Committed_f$ * $\overline{\text{Log-S1}}_f$ | -0.027786* | -0.032313** | -0.031327** | -0.025477*** | -0.024014*** | -0.019162** |
| | (0.016655) | (0.012946) | (0.012985) | (0.008177) | (0.008188) | (0.007575) |
| $Post_{f,t}$ * $Committed_f$ | 0.313085*** | 0.094554 | 0.059311 | 0.176433 | 0.117986 | 0.292254 |
| | (0.037466) | (0.277427) | (0.278606) | (0.222047) | (0.222679) | (0.203518) |
| $\widetilde{Post}_t$ * $\overline{\text{Log-S1}}_f$ | -0.022071* | 0.000900 | 0.000007 | -0.003347 | -0.004936 | -0.001413 |
| | (0.012501) | (0.010758) | (0.010765) | (0.008102) | (0.008120) | (0.007463) |
| Treated * $\overline{\text{Log-S1}}_f$ | -0.052864** | -0.016541 | -0.016881 | | | |
| | (0.025963) | (0.018809) | (0.018819) | | | |
| $\widetilde{Post}_t$ | -0.039191 | 0.729832*** | | 0.445915** | | |
| | (0.027807) | (0.256963) | | (0.191540) | | |
| $Committed_f$ | 0.355131*** | -1.027485** | -1.018895** | | | |
| | (0.061938) | (0.410459) | (0.410318) | | | |
| $\overline{\text{Log-S1}}_f$ | 0.362887*** | 0.051541*** | 0.051905*** | | | |
| | (0.021640) | (0.016522) | (0.016528) | | | |
| $Risk_{f,t}$ | | | | | | 0.047664*** |
| | | | | | | (0.004469) |
| $Post_{f,t}$ * $Committed_f$ * $Risk_{f,t}$ | | | | | | -0.006667*** |
| | | | | | | (0.002121) |
| $\widetilde{Post}_t$ * $Risk_{f,t}$ | | | | | | -0.011347*** |
| | | | | | | (0.001875) |
| $Committed_f$* $Risk_{f,t}$ | | | | | | -0.003405 |
| | | | | | | (0.005161) |
| Observations | 41,450 | 41,450 | 41,450 | 41,450 | 41,450 | 37,627 |
| R-squared | 0.306596 | 0.704369 | 0.705459 | 0.904226 | 0.905337 | 0.921224 |
| Econ effect per 1sd | -.074 | -.086 | -.083 | -.068 | -.064 | -.051 |
| Firm Controls | No | Yes | Yes | Yes | Yes | Yes |
| Time FE | No | No | Yes | No | Yes | Yes |
| Firm FE | No | No | No | Yes | Yes | Yes |

Table 3: The Effects of Bank Commitment on Bank Debt and Non-Bank Debt

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are log total debt or log one plus bank debt and log one plus non-bank debt. The primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. ***p<.01, **p<.05, *p<.1

| VARIABLES | (1) Total Debt | (2) Bank Debt | (3) Non-Bank Debt |
|---|---|---|---|
| $Post_{f,t}$ * $Committed_f$ * $\overline{\text{Log-S1}}_f$ | -0.021475*** | -0.045625* | -0.004968 |
| | (0.007257) | (0.023655) | (0.021828) |
| $Post_{f,t}$ * $Committed_f$ | 0.184968 | -0.155845 | 0.206685 |
| | (0.239169) | (0.475702) | (0.493342) |
| $\widetilde{Post_t}$ * $\overline{\text{Log-S1}}_f$ | -0.007374 | -0.004587 | -0.012032 |
| | (0.006602) | (0.018698) | (0.020001) |
| Observations | 32,828 | 32,828 | 32,828 |
| R-squared | 0.912666 | 0.745594 | 0.801383 |
| Econ effect per 1sd | -.057 | -.122 | -.013 |
| Firm Controls | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |

Table 4: Balance Test

The value displayed for t-tests are the differences in the means across the groups. Standard errors are clustered at the firm level. ***p<.01, **p<.05, *p<.1

| | Not Committed | | Committed | | t-test Difference | Normalized difference |
|---|---|---|---|---|---|---|
| **Variable** | **N/[Clusters]** | **Mean/SE** | **N/[Clusters]** | **Mean/SE** | **(1)-(2)** | **(1)-(2)** |
| Log-S1 | 1647 | 11.050 | 7044 | 11.956 | -0.906*** | -0.357 |
| | [395] | [0.126] | [1425] | [0.067] | | |
| Log-S1 (residualized) | 1647 | -0.003 | 7044 | -0.055 | 0.051 | 0.025 |
| | [395] | [0.101] | [1425] | [0.055] | | |
| $\overline{\text{Log-S1}}$ (ex-ante, demeaned) | 632 | -0.460 | 1480 | 0.196 | -0.656*** | -0.258 |
| | [632] | [0.098] | [1480] | [0.066] | | |
| $\overline{\text{Log-S1}}$ (ex-ante, demeaned, residualized) | 632 | 0.104 | 1480 | -0.045 | 0.149 | 0.075 |
| | [632] | [0.074] | [1480] | [0.053] | | |
| Log Bank Debt + 1 | 7638 | 5.561 | 25206 | 5.308 | 0.253** | 0.103 |
| | [588] | [0.093] | [1405] | [0.062] | | |
| Leverage | 9561 | 0.287 | 31889 | 0.310 | -0.022*** | -0.144 |
| | [632] | [0.006] | [1480] | [0.004] | | |
| Log Assets | 9561 | 8.036 | 31889 | 8.683 | -0.647*** | -0.553 |
| | [632] | [0.051] | [1480] | [0.029] | | |
| Risk | 8380 | 28.314 | 29261 | 34.878 | -6.565 | -0.025 |
| | [584] | [5.708] | [1377] | [4.535] | | |

## Table 5: The Effects of Bank Commitment on Total Firm Debt – Robustness

**In Panel A**, the sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are log total debt. The primary variables of interest are several measures of firm exposure to committed banks. Column (1) uses an indicator equal to one if any bank that the firm has a relationship with has committed. Column (2) uses the number of banks that have committed as a fraction of the total number of banks a firm has a relation with. Column (3) uses an indicator if any lead bank has committed. Column (4) uses the fraction of lead banks that have committed. Columns (5) and (6) use indicators as to whether a firm has above or below the median fraction of committed banks. Each of these is interacted with ex ante log emissions demeaned. **In Panel B**, the sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are log total debt or log one plus bank debt and log one plus non-bank debt. The primary variables of interest are an indicator for when a firm has a relation with a bank that has committed and an indicator for each quintile of ex ante log emissions, with the quintile 1 being the lowest, and quintile 5 the highest. Quintile 5 is omitted as the baselevel. ***p<.01, **p<.05, *p<.1

### Panel A: Alternative Proxies of Commitment

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| VARIABLES | Total Debt | | | | | |
| Commit Measure | I(Any Bank Commits) | %Committed Banks | I(Lead Commits) | %Committed Lead | Above/below median %Committed Banks | Above/below median %Committed Lead |
| $\text{Post}_{f,t}$ *$\text{Committed}_f$ * $\overline{\text{Log-S1}}_f$ | -0.024014*** | -0.093725*** | -0.010178 | -0.071809** | | |
| | (0.008188) | (0.033102) | (0.009093) | (0.032792) | | |
| $\text{Post}_{f,t}$*$\text{Committed}_f$ | 0.117986 | -1.305176** | 0.216759 | -0.840769 | | |
| | (0.222679) | (0.537944) | (0.254531) | (0.548636) | | |
| $\widetilde{\text{Post}}_t$ * $\overline{\text{Log-S1}}_f$ | -0.004936 | -0.009875 | -0.015379** | -0.013942** | -0.007004 | -0.016386** |
| | (0.008120) | (0.007086) | (0.007352) | (0.006615) | (0.008272) | (0.007375) |
| Commit Share $\text{High}_f$ * $\overline{\text{Log-S1}}_f$ | | | | | -0.029125*** | -0.017349† |
| | | | | | (0.010980) | (0.012170) |
| Commit Share $\text{Low}_f$ * $\overline{\text{Log-S1}}_f$ | | | | | -0.016342* | -0.001899 |
| | | | | | (0.009060) | (0.010661) |
| Commit Share $\text{High}_f$ | | | | | -0.647325** | -0.429841 |
| | | | | | (0.284397) | (0.311560) |
| Commit Share $\text{Low}_f$ | | | | | 0.377655 | 0.674715 |
| | | | | | (0.310938) | (0.455270) |
| Observations | 41,450 | 41,450 | 41,450 | 41,450 | 41,450 | 41,450 |
| R-squared | 0.905337 | 0.905210 | 0.905180 | 0.905157 | 0.905488 | 0.905358 |
| Econ effect per 1sd | -.064 | -.044 | -.027 | -.034 | - | - |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes |

Panel B: Non-Linearities in Carbon Emissions

| VARIABLES | (1)<br>Total Debt | (2)<br>Bank Debt | (3)<br>Nonbank Debt |
|---|---|---|---|
| $Post_{f,t}$ *$Committed_f$ * Quintile 1 | 0.138618** | 0.474173** | 0.014359 |
| | (0.061178) | (0.197333) | (0.182255) |
| $Post_{f,t}$ *$Committed_f$ *Quintile 2 | 0.192361*** | 0.203736 | 0.287315* |
| | (0.054961) | (0.165603) | (0.159117) |
| $Post_{f,t}$ *$Committed_f$ * Quintile 3 | 0.113401** | -0.018428 | 0.188040 |
| | (0.048498) | (0.166055) | (0.141700) |
| $Post_{f,t}$ *$Committed_f$ * Quintile 4 | 0.011189 | -0.107660 | 0.222196 |
| | (0.045262) | (0.148789) | (0.150365) |
| Committedf | 0.064127 | -0.438775 | 0.072964 |
| | (0.253016) | (0.524062) | (0.533237) |
| $\widetilde{Post_t}$* Quintile 1 | 0.058740 | -0.087736 | 0.106368 |
| | (0.052815) | (0.139144) | (0.163310) |
| $\widetilde{Post_t}$ * Quintile 2 | -0.000599 | 0.082633 | -0.105404 |
| | (0.045981) | (0.119509) | (0.140540) |
| $\widetilde{Post_t}$* Quintile 3 | 0.011438 | 0.002838 | -0.073921 |
| | (0.043449) | (0.117004) | (0.125830) |
| $\widetilde{Post_t}$ * Quintile 4 | -0.025937 | 0.066539 | -0.244025* |
| | (0.029519) | (0.114918) | (0.127184) |
| Observations | 32,828 | 32,828 | 32,828 |
| R-squared | 0.912859 | 0.746010 | 0.801608 |
| Firm Controls | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |

Table 6: The Effects of Bank Commitment on the Likelihood of Issuing Syndicate Loans (Loan-level Estimates)

The sample period is 2013-2018. We report the results of the pooled regression with standard errors double clustered at the firm and main-bank-time level. The dependent variable is an indicator equal to one if the firm took out a loan from a bank that commits or a bank that does not commit. The primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the bank has committed yet or not. Each column adds controls and more stringent fixed effects. ***p<.01, **p<.05, *p<.1, †<p.13

| VARIABLES | (1)<br>1(Loan) | (2)<br>1(Loan) | (3)<br>1(Loan) | (4)<br>1(Loan) | (5)<br>1(Loan) |
|---|---|---|---|---|---|
| $Post_{f,t}$ * $Committed_c$ * $\overline{\text{Log-S1}}_f$ | -0.000918 | -0.001335† | -0.001515* | -0.002383† | -0.003569** |
| | (0.000797) | (0.000864) | (0.000815) | (0.001508) | (0.001711) |
| $Post_{f,t}$ * $Committed_c$ | -0.008340 | -.0099095 | 0.003386 | .3220423 | 0.342296 |
| | (0.096133) | (.1116458) | (0.120403) | (0.248489) | (.263151) |
| $\widetilde{Post}_t$ * $\overline{\text{Log-S1}}_f$ | 0.000516 | 0.000906 | 0.000546 | 0.001034 | 0.001126 |
| | (0.000963) | (0.001001) | (0.001059) | (0.001021) | (0.001046) |
| $Committed_c$ * $\overline{\text{Log-S1}}_f$ | -0.000505 | -0.000222 | -0.000217 | -0.000238 | |
| | (0.000684) | (0.000699) | (0.000655) | (0.000702) | |
| $\widetilde{Post}_t$ | -0.081131* | -0.024270 | -.0222399 | -0.085935* | -.079602 |
| | (0.045901) | (0.045078) | (.0454808) | (0.047530) | (0.050186) |
| $Committed_c$ | -0.085670 | -.1328254 | -0.142786 | -0.152114* | |
| | (0.080365) | (.0856923) | (0.087407) | (0.092086) | |
| $\overline{\text{Log-S1}}_f$ | -0.000823 | | | | |
| | (0.000847) | | | | |
| Observations | 65,078 | 65,078 | 62,192 | 65,031 | 65,031 |
| R-squared | 0.006718 | 0.037058 | 0.038708 | 0.053190 | 0.070361 |
| Econ effect per 1sd | -.002 | -.004 | -.004 | -.006 | -.009 |
| Firm Controls | Yes | Yes | Yes | Yes | Yes |
| Bank Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | No | Yes | Yes | - | - |
| Year FE | No | Yes | Yes | - | - |
| Sector-Year FE | No | No | Yes | - | - |
| Firm-Year FE | No | No | No | Yes | Yes |
| Firm-Committed Bank FE | No | No | No | No | Yes |

Table 7: The Effects of Bank Commitment on the Likelihood of Issuing Syndicate Loans (Firm-level Estimates)

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variable is an indicator equal to one if the firm took out a loan. The primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. Each column adds controls and more stringent fixed effects. ***p<.01, **p<.05, *p<.1, †<p.13

| | (1) | (2) | (3) |
|---|---|---|---|
| VARIABLES | | I(Loan) | |
| Commit Measure | I(Any Bank Commits) | I(Any Bank Commits) | % Committed Banks |
| $Post_{f,t}$ * $Committed_f$ * $\overline{\text{Log-S1}}_f$ | -0.001313 | -0.003573† | -0.017296** |
| | (0.002058) | (0.002346) | (0.007152) |
| $Post_{f,t}$ * $Committed_f$ | -0.036689 | -0.000311 | -0.229196* |
| | (0.039462) | (0.046506) | (0.118040) |
| $\widetilde{Post}_t$ * $\overline{\text{Log-S1}}_f$ | -0.000953 | 0.001215 | 0.000934 |
| | (0.001953) | (0.002154) | (0.001892) |
| $Committed_f$* $\overline{\text{Log-S1}}_f$ | 0.000698 | | |
| | (0.002581) | | |
| $\widetilde{Post}_t$ | 0.026333 | | |
| | (0.037264) | | |
| $Committed_f$ | 0.055502 | | |
| | (0.048636) | | |
| $\overline{\text{Log-S1}}_f$ | 0.000698 | | |
| | (0.002603) | | |
| Observations | 39,039 | 39,028 | 39,028 |
| R-squared | 0.006810 | 0.076405 | 0.075895 |
| Econ effect per 1sd | -.003 | -.01 | - |
| Firm Controls | Yes | Yes | Yes |
| Time FE | No | Yes | Yes |
| Firm FE | No | Yes | Yes |

## Table 8: Real Effects: Deleveraging

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are log one plus bank debt, log total debt, leverage (debt over total assets), log total asserts, and log equity. The primary variable of interest is the interaction of ex-ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. ***p<.01, **p<.05, *p<.1

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| VARIABLES | Bank Debt | Total Debt | Leverage | Assets | Equity |
| $Post_{f,t}$ * $Committed_f$ * $\overline{\text{Log-S1}}_f$ | -0.054523** | -0.026864*** | -0.002433** | -0.008075** | 0.000146 |
| | (0.025261) | (0.008708) | (0.001200) | (0.003967) | (0.006007) |
| $Post_{f,t}$ * $Committed_f$ | -0.223237 | 0.097761 | 0.031714 | 0.136436 | 0.096496 |
| | (0.477445) | (0.222258) | (0.026243) | (0.086296) | (0.125774) |
| $\widetilde{Post_t}$ * $\overline{\text{Log-S1}}_f$ | 0.000260 | -0.005666 | -0.000241 | -0.007750** | -0.006699 |
| | (0.018432) | (0.008522) | (0.001068) | (0.003475) | (0.005088) |
| Observations | 32,828 | 41,450 | 41,450 | 41,450 | 40,316 |
| R-squared | 0.745613 | 0.905367 | 0.827560 | 0.972200 | 0.926749 |
| Econ effect per 1sd | -.138 | -.068 | -.006 | -.02 | 0 |
| Firm Controls | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes |

Table 9: Real Effects: CAPEX, Sales and Employment

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are log capital expenditures, log sales, and log employment. Log employment is only available annually and so is one year ahead. The primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. ***p<.01, **p<.05, *p<.1

| VARIABLES | (1) Capex | (2) Sales | (3) $\text{Employment}_{t+1}$ |
|---|---|---|---|
| $Post_{f,t}$ * $Committed_f$ * $\overline{\text{Log-S1}}_f$ | -0.016102** | 0.001264 | -0.004614 |
| | (0.007987) | (0.004033) | (0.003328) |
| $Post_{f,t}$ * $Committed_f$ | -0.057014 | 0.013032 | 0.082330 |
| | (0.176607) | (0.102766) | (0.067054) |
| $\widetilde{Post}_t$ * $\overline{\text{Log-S1}}_f$ | -0.019796** | -0.025055*** | -0.008904*** |
| | (0.007895) | (0.003515) | (0.003058) |
| Observations | 38,106 | 37,922 | 7,649 |
| R-squared | 0.889488 | 0.962620 | 0.983909 |
| Econ effect per 1sd | -.043 | .003 | -.012 |
| Frequency | Q | Q | Y |
| Firm Controls | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes |

Table 10: Scope-1 Emissions, ESG Score, and Environmental Expenditures

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables are Log Scope 1 emission in the next year, the MSCI ESG score, the environmental sub-score, log environmental expenditures in the next year, environmental expenditures in the next year normalized by total assets, an indicator for whether the firm uses renewable energy, and an indicator for whether the firm itself has committed to reducing emissions. For the ESG and Environmental scores, higher is considered "better" from an ESG perspective. The primary variable of interest is the interaction of ex ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. ***p<.01, **p<.05, *p<.1

| VARIABLES | (1)<br>Log-S1$_{t+1}$ | (2)<br>ESG Score | (3)<br>Env Score | (4)<br>Env Exp$_{t+1}$ | (5)<br>Env Exp$_{t+1}$/TA | (6)<br>Renewable | (7)<br>Committed |
|---|---|---|---|---|---|---|---|
| $Post_{f,t}$ * $Committed_f$ * $\overline{Log\text{-}S1}_f$ | -0.000267 | 0.008959 | 0.036235** | -0.016145 | -0.039195 | 0.000500 | -0.000342 |
| | (0.012214) | (0.010368) | (0.018379) | (0.032979) | (0.096206) | (0.004630) | (0.001249) |
| $Post_{f,t}$ * $Committed_f$ | -0.355541* | -0.031557 | 0.424572 | -0.002870 | 0.562182 | 0.064190 | -0.072430*** |
| | (0.200091) | (0.210578) | (0.433195) | (0.599974) | (1.126328) | (0.083559) | (0.025429) |
| $\widetilde{Post_t}$ * $\overline{Log\text{-}S1}_f$ | -0.030971*** | 0.044174*** | 0.014001 | -0.037427 | -0.094248* | -0.008940** | -0.002145** |
| | (0.011287) | (0.010677) | (0.016804) | (0.025164) | (0.056671) | (0.003942) | (0.000960) |
| Observations | 8,633 | 31,668 | 31,668 | 1,911 | 1,911 | 35,112 | 41,450 |
| R-squared | 0.969906 | 0.845531 | 0.856806 | 0.966963 | 0.736150 | 0.842053 | 0.355540 |
| Econ effect per 1sd | -.001 | .024 | .097 | -.043 | -.104 | .001 | -.001 |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Table 11: ESG Score Subcomponents

The sample period is 2013-2018. We report the results of the pooled regression with standard errors clustered at the firm level. The dependent variables provide more information about firms' ESG performance. Column (1) is the overall ESG score, column (2) is the environmental score. Column (3) is the social score. Column (4) is the governance score. Columns (5) to (9) are elements within the environmental score focused on climate, natural resources, waste management, carbon usage, and environmental opportunities. Higher scores are considered "better" from an ESG perspective. The primary variable of interest is the interaction of ex-ante log emissions demeaned and an indicator for if the firm has a relation to a bank that has committed. ***p<.01, **p<.05, *p<.1

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| VARIABLES | ESG | Env Score | Soc Score | Gov Score | Climate | Natural Res | Waste | Carbon | Env Opps |
| $Post_{f,t}$ * $Committed_f$ * $\overline{Log\text{-}S1}_f$ | 0.008959 | 0.036235** | 0.013780 | 0.007442 | 0.028564 | -0.042912* | -0.010501 | -0.010249 | 0.073176*** |
| | (0.010368) | (0.018379) | (0.019152) | (0.024213) | (0.027702) | (0.025179) | (0.019920) | (0.026165) | (0.022019) |
| $Post_{f,t}$ * $Committed_f$ | -0.031557 | 0.424572 | -0.303427 | -0.394068 | 0.483688 | -0.333657 | -0.755065 | 0.798631 | 0.713411 |
| | (0.210578) | (0.433195) | (0.357121) | (0.499908) | (0.644104) | (0.588016) | (0.498163) | (0.596354) | (0.504627) |
| $\widetilde{Post_t}$ * $\overline{Log\text{-}S1}_f$ | 0.044174*** | 0.014001 | -0.033116 | -0.039935 | -0.027283 | -0.130389*** | -0.173053*** | -0.051186** | 0.047196** |
| | (0.010677) | (0.016804) | (0.020170) | (0.027676) | (0.024921) | (0.025832) | (0.020335) | (0.024795) | (0.021049) |
| Observations | 31,668 | 31,668 | 31,668 | 31,666 | 29,247 | 24,570 | 23,933 | 26,582 | 13,413 |
| R-squared | 0.845531 | 0.856806 | 0.760652 | 0.596703 | 0.859468 | 0.800882 | 0.851921 | 0.877359 | 0.802693 |
| Econ effect per 1sd | .024 | .097 | .037 | .02 | .076 | -.114 | -.028 | -.027 | .195 |
| Firm Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Firm FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Time FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |