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Essays in Financial Economics

Preface

Banks play a special role in the financial system. According to classical banking theory, they help reduce informational asymmetries and serve as liquidity providers. Banks can, at least partially, lower transaction costs that result from information frictions between investors and firms and thereby alleviate firms' funding constraints (Diamond, 1984). Moreover, banks create liquidity on their balance sheets by financing comparably illiquid assets with relatively liquid liabilities (Diamond and Dybvig, 1983). Integrating credit and liquidity provision functions, banks have been the object of numerous studies on financial intermediation.

A particular focus in recent years has been on banks' behavior as well as on the consequences of their actions for the real economy when hit by adverse shocks. Following the global financial crisis, financial shocks that originate from within the financial sector have received wide attention (Cingano et al., 2016; Chodorow-Reich, 2014; Khwaja and Mian, 2008; Paravisini, 2008; Paravisini et al., 2015; Schnabl, 2012). However, banks are also subject to numerous non-financial shocks, which are the focus of this thesis.

Paper 1 investigates how banks change their credit supply after a shock to the salience of transition risk that arises from moving to a greener and more sustainable economy.[1] Following an increase in public awareness of firms' transition risks, financial market participants may update their prevailing perceptions of these risks and act accordingly. We show that lending changes in the aftermath of such an event depending on whether firms can benefit or lose from stricter environmental regulations as well as on the ex-ante stringency of the regulatory landscape the borrowers operate in. Stringency proxies for heterogeneous expectations about future environmental regulation across countries as well as the materiality of the financial risks (benefits) that firms are exposed to (Carbone et al., 2021; Ehlers et al., 2021; Krueger et al., 2020). Only in countries in which existing environmental regulations are relatively stringent, banks supply more

[1]For a detailed definition of transition risk, see Basel Committee on Banking Supervision (2021).

funding to firms that will benefit from them. Conversely, firms that are likely hurt by regulation receive more credit if located in less stringent environments or if linked to banks with a portfolio tilted toward lending to negatively impacted firms. Thus, the effect of transition risk on banks' lending depends on the interaction of how firms will be affected by regulation, the existing regulatory landscape the firms operate in, and banks' own exposure to firms' regulatory risks via their loan portfolios.

Paper 2 studies how banks' management of transition risk interacts with corporate loan securitization. After a political event that lowered the risk of new environmental policies being introduced, we find that banks alter the securitization of loans granted to firms that exhibit higher transition risks. While these loans were more likely to be sold off before, they become more likely to remain on banks' balance sheets after the shock. This effect is more pronounced if banks impose covenants in the loan contract. This could suggest that banks consider that political circumstances may change in the future altering the performance of these loans. Evaluating which banks engage in lower securitization of higher transition risk loans, we identify that it is, in particular, banks that have low or no preferences for sustainable lending as well as domestic lenders that are likely to respond more strongly to local political events.

Papers 1 and 2, thus, contribute to the discussion on the role of banks in the transition toward a greener and more sustainable economy. Banks are seen as critical for this process given their central position in allocating resources through their intermediation function as well as their ability to impose costs via quantity and price adjustments. Paper 1 sheds light on whether and how banks account for transition risk in their lending decisions. Paper 2 highlights an alternative channel of how banks manage transition risk, i.e. securitization. This channel is of relevance as banks may be limited in their willingness to account for transition risk in their lending terms and securitization markets are of very large sizes. Moreover, it is crucial for regulators and supervisors to know, who in the financial system ultimately carries the risk. This knowledge is a precondition for designing appropriate policies to address climate-related risks to financial stability. Both papers have in common that they draw attention to how different bank characteristics influence the management of transition risk. A finding that should be taken into consideration when future regulatory actions are mapped out.

Moving away from shocks in the context of the green transition, Paper 3 analyses how banks adjust lending in response to the dismantling of trade barriers. Increased import competition adversely affects non-financial corporations (e.g., Autor et al., 2013) and subsequently feeds through to banks via their lending relationships. This work shows that banks reduce lending the more they are affected by the liberalization of trade. Importantly, it uncovers large heterogeneity in banks' reactions depending on their sectoral specialization. Banks shield the industries in which they specialize. While I find evidence that banks' reductions in credit in response to the trade shock have adverse real effects, lending specialization dampens the negative impact on firm outcomes. These findings provide valuable input for accounting the gains from trade liberalization and therefore allow for a more informed design of such policies. Moreover, they shed light on the complex implications of lending specialization.

All three papers use the same data as their main foundation: syndicated loan data provided by Thomson Reuters LPC's DealScan. This dataset is rich enough to answer pressing questions in the fields of corporate finance and banking. In combination with its commercial availability, it has therefore been employed by a whole array of highly influential papers. They explore fundamental topics such as asymmetric information and loan pricing (Ivashina, 2009; Sufi, 2007), the nature and determinants of relationship lending (Bharath et al., 2011; Schwert, 2018), as well as the effect of credit market shocks on firm outcomes (Chodorow-Reich, 2014; Correa et al., 2021). A key feature of the usage of this database is the multitude of options to define sample and lending outcomes. This feature does not only leave the researcher with a large degree of discretion regarding which option to take but also raises many questions on how to make appropriate sampling and definition decisions.

Therefore, Paper 4 scrutinizes the results of an established empirical setting across a variety of DealScan specifications, which we identified to be the most commonly used in the literature. The results paint a somewhat positive picture. Estimates are robust across many choices while we highlight modifications that appear to be especially relevant for the conclusions drawn. In this vein, this study corroborates the sampling choices made in Papers 1 to 3 but also provides insights to other researchers on how specific data decisions might affect coefficient estimates as well as presents structured guidance on possible scrutiny tests.

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Paper 1:

Climate Change-Related Regulatory Risks and Bank Lending

Climate Change-Related Regulatory Risks and Bank Lending*

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Abstract

We analyze how firms' climate change-related regulatory risks affect banks' lending. Exploiting the Paris Agreement as a shock that raises awareness of regulatory risks, we find that effects depend on how borrowers will be affected by regulation as well as the stringency of the existing regulatory environment where firms are located. Firms that benefit from regulation receive more credit only if located in more stringent regulatory environments. Conversely, firms hurt by regulation receive more credit if located in less stringent environments or if linked to banks with a portfolio tilted toward lending to negatively impacted firms.

JEL classification: G21, Q51, Q58

Keywords: Climate change, climate risk, bank lending, Paris Agreement

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1 Introduction

Climate change poses a substantial threat to the global economy and makes transitioning toward a more sustainable and greener future a first-order challenge. To overcome this challenge, many jurisdictions have started to introduce regulation to align short-term profit-maximizing decisions of firms with long-term interests of society (Tenreyro and De Silva, 2021). Hence, firms face regulatory risks related to climate change. While some can be negatively impacted by the introduction of regulation - for example, due to increasing operating and input costs - others may benefit - for instance, due to subsidies. Firms will need external capital to undertake the massive investments required to manage these risks and adjust to the transition (UNEP, 2011). Banks can play a central role in providing the necessary funding and in setting the incentives for a green transformation. Hence, understanding how lending adjusts to regulatory risks, and which firms receive financing, is essential.

We analyze how firms' regulatory risks affect banks' lending. Our results show that effects depend on whether firms stand to benefit or lose from the introduction of environmental regulation. Interestingly, we find that, overall, firms that may be negatively impacted by regulation experience a relative increase in credit volumes. This result is rationalized when taking the stringency of the existing regulatory environment in which borrowers are located into account. Stringency proxies for heterogeneous expectations about future environmental regulation across countries as well as the materiality of the financial risks (benefits) that firms are exposed to (Carbone et al., 2021; Ehlers et al., 2021; Krueger et al., 2020). Only in environments with low regulatory stringency, where expectations and associated financial risks are likely to be lower, firms that can be negatively impacted receive relatively more credit. In contrast, where regulatory stringency is high, firms that can benefit receive relatively more credit. Thus, the effect of regulatory risks on lending patterns depends on the interaction of firms' exposure to these risks and the stringency of existing regulation.

Our paper centers around the 2015 Paris Agreement as a shock that raises market participants' awareness of firms' regulatory risks (Bolton and Kacperczyk, 2022; Degryse et al., 2021; Delis et al., 2021; Kruse et al., 2020; Monasterolo and De Angelis, 2020; Seltzer et al., 2022). Specifically, the Paris Agreement is the first comprehensive agreement at the global level to coordinate actions to tackle climate change and limit global warming below 2°C. Previous research documents the impact of this shock on the pricing of transition risk in different markets. We argue that it may also have impacted credit volumes.

The effect on credit may vary depending on firms' exposure to regulatory risks. Certain firms stand to lose from the introduction of stricter environmental regulation, as it can negatively influence operating costs, earnings, and cash flows as well as loss probabilities (Campiglio, 2016; Huang et al., 2018). We refer to them as *negatively exposed firms*. Banks may reduce credit supply to these firms as they become more aware of the negative impact regulation may have on their outcomes. Alternatively, banks may lend more to negatively exposed firms either to profit from still un-internalized negative externalities (Reghezza et al., 2022) or to support their transition (Engle et al., 2020; Faccini et al., 2021). Meanwhile, some firms may benefit from the introduction of regulation due to e.g., the provision of subsidies (Holburn, 2012). We refer to these firms as *positively exposed firms*. Following an increase in awareness about potential benefits, banks may lend more. However, several factors, such as policy uncertainty and the existing financial regulatory regime, may still act as a barrier to lending to positively exposed firms (Campiglio, 2016; D'Orazio and Popoyan, 2019; Holburn, 2012).

The impact of the Paris Agreement may at the same time differ across the participating nations (Carbone et al., 2021). Existing regulatory stringency can be a qualifying factor for the impact of this event on banks' lending responses and may lead to different hypotheses being realized across jurisdictions. The more stringent the regulatory environment, the more material the financial risks or benefits that firms face in the respective jurisdiction (Ehlers et al., 2021; Krueger et al., 2020). Hence, banks may also consider the stringency of existing regulation in their lending decisions.

We exploit a difference-in-differences (DID) setting to evaluate how credit changes depending on firms' regulatory risks following the Paris Agreement. Given the high uncertainty around whether an agreement can be achieved at the Paris Summit and the surprisingly ambitious extent of the final outcome, it seems unlikely that this event is anticipated (Obergassel et al., 2015; PIK, 2015; Seltzer et al., 2022). To implement our research design, we employ a measure of firms' regulatory risks by Sautner et al. (2022). The measure is constructed using textual analysis of quarterly earnings conference calls. It reports the proportion of conversations during the conference call that is centered on regulatory topics related to climate change as well as its sentiment. The measure, therefore, captures a forward-looking view from within the firm rather than a historical record of the current business model as measures that focus on carbon emissions do. To classify firms in our sample, we calculate their average exposure over the pre-shock period and define firms with, on average, positive (negative) exposure as positively (negatively) exposed firms and assign firms with an average exposure of zero to the control group. We, therefore, investigate relative changes in lending to negatively and positively exposed firms with respect to the control group.

By raising banks' as well as firms' awareness of regulatory risks, the Paris Agreement may have impacted not only supply but also demand for credit conditional on firms' exposure. Demand for credit of positively exposed firms may change as the Paris Agreement may alter the attractiveness of alternative funding sources or the balance between risks and expected returns for these firms (Alessi et al., 2021; Holburn, 2012). Demand of negatively exposed firms may adjust to finance their transition or to conserve borrowing capacity to hedge future liquidity needs (Kovacs et al., 2021; Nguyen and Phan, 2020). In order to identify supply-side effects and control for changes in firms' demand for credit, we introduce borrowers' industry-location-size-time (ILST) fixed effects à la Degryse et al. (2019). This approach absorbs changes in demand that are homogeneous within industry-location-size-time clusters. To control for potential heterogeneous demand shifts within clusters linked to firms' exposure, we additionally introduce firm controls that relate to credit demand. As this approach does not lead to significantly different results, we are reassured that our preferred specification is likely to capture supply-side effects. Nevertheless, results from the baseline specification may be biased by residual demand changes. In further tests, we exploit heterogeneity across lenders, which allows us to isolate supply effects by including firm-time fixed effects. Firm-time fixed effects absorb observable and unobservable time-varying borrower characteristics, including loan demand.

We combine data on firms' exposure with granular loan-level information covering syndicated lending to an international sample of firms between 2010 and 2019. In the syndicated loan market, it is plausible to assume that borrowers are aware of their regulatory exposure and banks acting as lead arrangers are at least able to judge whether firms would benefit or lose from policy intervention. First, this is due to the financial significance of the material risks faced by borrowers if environmental regulation is eventually introduced (Ehlers et al., 2021). Second, the reputational damage associated with a failure in due diligence when assessing a loan incentivizes lead arrangers to conduct proper ex-ante screening and monitoring (Sufi, 2007). Last, this market comprises mostly large companies that tend to be less opaque (Gopalan et al., 2011).

Our results can be summarized as follows: Overall, negatively exposed firms are granted more credit relative to non-exposed firms after the Paris Agreement. This is in line with the hypotheses that banks cream off profits from negative externalities not yet being internalized or increasingly finance the transition of these firms. As conjectured, the stringency of the existing regulatory environment appears to be a qualifying factor for the impact of this event on firms. Only in environments with low regulatory stringency, negatively exposed firms are granted 29% more relatively to non-exposed firms after the Paris Agreement. This corresponds to US$ 20 million more at the bank-firm pair level. In contrast, where stringency is high, positively exposed firms receive 80% or US$ 56 million more. This is in line with banks supplying more credit to positively exposed firms in environments where the materiality of potential benefits for these firms is higher.

These findings are mirrored for the two largest regions in our sample, United States and Europe, that are characterized by, respectively, a less and more stringent regulatory environment. We provide evidence that the diverging results between the United States and Europe are not driven by alternative events such as the election of Donald Trump, differences in the financing structure of firms across regions, or fluctuations in energy prices. Moreover, we show that when considering within-region variation in regulatory stringency the impact of the Paris Agreement on negatively exposed firms aligns in less (more) stringent localities within the United States and Europe. Confirming previous findings in the literature, we show that the Paris Agreement also affects prices (Delis et al., 2021). While loan spreads are relatively higher for negatively exposed firms both in the United States and in Europe, the increase is significantly higher in Europe.

We also investigate to what extent bank characteristics shape lending to exposed firms. Certain characteristics may lead banks to face different incentives when adjusting their lending after the Paris Agreement. Following the Summit, banks may also update their beliefs about their own, indirect exposure to firms' regulatory risks stemming from their lending portfolio. This may create incentives particularly for negatively exposed banks to either diversify their portfolio or protect the value of their legacy positions (Degryse et al., 2022; Diamond, 1984). We build a new measure of banks' indirect exposure to investigate this. Based on previous literature, we also consider the role of systemic importance, preferences for green lending, and banks' locations. Systemically important banks may underrate firms' regulatory risks as they are more protected from financial losses due to higher capital requirements to too-big-to-fail guarantees (Beyene et al., 2021). Banks' stated preferences for sustainable lending may also influence credit supply decisions, as they may lead to adjustments in a more sustainable way (Degryse et al., 2021). Finally, banks' locations can play a role as banks may be better able to track or predict regulatory risk domestically. We find heterogeneity in banks' credit supply depending on their type when lending to firms. In Europe, negatively exposed, significant, and European banks appear to increase credit supply relatively more to negatively exposed firms. This investigation highlights that even in stringent environments negatively exposed firms receive more funding when connected to particular banks.

Our work contributes to the recent literature on the awareness of transition risks in the financial sector. Evidence on investors' reactions suggests the existence of a risk premium on stock returns, divestments from firms or industries with higher transition risks, as well as the creation of shareholder value by mitigating these risks (Boermans and Galema, 2019; Bolton and Kacperczyk, 2021; Ceccarelli et al., 2020; Chava, 2014; Fernando et al., 2017; Krueger et al., 2020; Sautner et al., 2022). The literature on whether and how banks consider transition risks in their lending decisions is growing rapidly. Evidence suggests that firms holding more fossil fuel reserves or with higher carbon emissions are charged higher interest rates, while firms that voluntarily disclose environmental data receive preferential terms (Chava, 2014; Degryse et al., 2021; Delis et al., 2021; Ehlers et al., 2021). Previous papers also consider changes in credit volumes in response to firms' transition risks by using carbon emissions or exposure to green technology disruptions as proxies (Degryse et al., 2022; Kacperczyk and Peydró, 2021; Reghezza et al., 2022).

Rather than capturing a historical record of the current business model, as measures that focus on carbon emissions do, the measure of firms' risks employed in this study provides a forward-looking view of key firm stakeholders on their own exposure. This allows us to properly identify firms that could be negatively impacted by regulatory interventions, as well as those that could benefit from them. Observing changes in credit toward both groups can provide a more complete picture of how banks' behavior interacts with the need to transition.

A sub-strand of papers evaluates how certain bank characteristics affect lending outcomes differentially. Beyene et al. (2021) show that significant banks are willing to provide cheaper and more financing to fossil fuel firms. Degryse et al. (2021) outlines that green banks, in particular, lend to green firms at preferential terms, while Kacperczyk and Peydró (2021) find that a change in banks' stated preferences for sustainable lending leads to reductions in credit supply to high emitting firms. Degryse et al. (2022) propose that banks' asset overhang affects credit supply decisions due to incentives to protect the value of existing positions from green technology disruptions. Building on their argument, we contribute to the literature by constructing a measure of banks' portfolio exposure to firms' regulatory risks and showing that it impacts their lending behavior. We further show whether significance, preferences, as well as location play a role in credit supply adjustments to both groups of exposed firms.

2 The Paris Agreement and its impact on credit

The Paris Agreement, signed in December 2015, aims to coordinate actions among 196 nations to mitigate climate change by limiting global warming below 2°C. The Paris Summit was accompanied by intensive media attention characterizing it as a landmark accord and marks the first comprehensive agreement at a global level to address climate change (Kruse et al., 2020).

We argue that this event has raised public awareness of transition risks and shifted market participants' prevailing perceptions of these risks (Bolton and Kacperczyk, 2022; Degryse et al., 2021; Kruse et al., 2020). Survey evidence on institutional investors by Krueger et al. (2020) points to the recent increased attention to climate risks in investment decisions. Investors adjust their investments not only because of the belief that climate risks can have significant financial implications for firms but also because of a shift in the preferences of clients and managers. Banks, analogous to institutional investors, are exposed to the same shifts in knowledge, attitudes, and perceptions of climate change-related risks. Hence, banks may update their beliefs about these risks and adjust how they allocate credit accordingly.[1]

The way banks may adjust lending conditional on firms' exposure is a priori unclear as there are several possible and, at times contrasting incentives that may drive banks' decision-making. When lending to *positively exposed firms*, banks may now consider more that these firms could benefit from the introduction of legislation, owing to their business model or increasing public support, and supply more funding. However, several factors, such as policy uncertainty and the existing financial regulatory regime, may still act as barriers to lending to positively exposed firms (Campiglio, 2016; D'Orazio

[1] While this event arguably shifted banks' perceptions of firms' regulatory risks, we argue that it should not have had an impact on banks' direct regulatory risks. The political discussion at the time of the Paris Agreement was not yet about regulating the financial system. The Bank of England was the first central bank and supervisor in 2018 to publish a report to set supervisory expectations for banks on the management of climate-related financial risks, covering governance, risk management, scenario analysis, and disclosure. Moreover, the EU Taxonomy Regulation, a classification system establishing a list of environmentally sustainable economic activities in Europe, was only introduced in June 2020.

and Popoyan, 2019; Holburn, 2012). When lending to *negatively exposed firms*, banks may reduce credit supply as they are now more aware that firms may face challenges in repaying loans or exhibit higher probabilities of default, as future regulation can decrease earnings and cash flow as well as reduce the value of assets present in firms' balance sheets (Campiglio, 2016; Huang et al., 2018). Alternatively, banks may lend more to negatively exposed firms. This can have two potential but contrasting reasons. On the one hand, banks may want to increase lending to these firms before regulation is actually introduced, thereby benefiting from the fact that negative externalities are not yet internalized (Reghezza et al., 2022). On the other hand, banks may lend more to those negatively exposed firms that have a strategy or potential to transition to a more sustainable business model (Engle et al., 2020; Faccini et al., 2021).

At the same time, the Paris Agreement may have increased firms' own awareness of the regulatory risks they face. As a result thereof, firms' demand for credit may be different conditional on their exposure. Positively exposed firms may borrow less as alternative sources of financing became more cost attractive (i.e. green bonds (Alessi et al., 2021)) or they may borrow more as reduced policy uncertainty alters the balance between risks and expected returns encouraging more investments (Holburn, 2012). Negatively exposed firms may increase demand in order to meet future compliance costs and finance the adaptation of their business model. Alternatively, they may reduce demand to conserve borrowing capacity to hedge future liquidity needs, due to e.g., fines or litigation (Kovacs et al., 2021; Nguyen and Phan, 2020).

Further aspects need to be discussed related to the use of the Paris Agreement in our setting. First, the fact that an agreement was reached was in itself not an assured outcome. A series of failures to reach a global climate treaty preceded the Paris Agreement, creating "a virtual consensus among academics, who have argued that UN talks cannot succeed" (Dimitrov, 2016, p. 8). Mere weeks before the conclusion of the negotiations, high-level European officials warned that the outcome of the negotiation process was highly uncertain (Seltzer et al., 2022). Second, the extent of the Agreement with regard to the number of nations signing it as well as in terms of the ambitious goals set forward was largely unexpected (Obergassel et al., 2015; PIK, 2015; Seltzer et al., 2022). It represents the first time that all nations, including both China and India, committed to actions against climate change on an international level. Moreover, the goals set out were considered much more ambitious than previously expected. Nevertheless, we conduct several robustness checks to illustrate that anticipation effects do not drive our results in Section 8.

3 Data and summary statistics

3.1 Data and measurement

Loan-level data We retrieve detailed loan-level information from Thomson Reuters LPC's DealScan, which covers syndicated loans. It encompasses information on lending volumes, the date of origination, maturity, as well as lender and borrower identities. Data are aggregated using the ultimate parent-level information from DealScan for both banks and firms. We start with all active facilities between 2010 and 2019. The start of the sample period is determined by the need to exclude effects stemming from the global financial crisis. The sample ends in 2019 to avoid the influence of the economic crisis following the COVID-19 outbreak. We exclude firms in the financial sector (SIC codes between 6000 and 6999) from the sample.

We convert facility volumes to millions of US dollars if applicable utilizing the spot exchange rate that DealScan provides at loan origination. Following De Haas and Van Horen (2013), we allocate loan shares according to the breakdown provided by DealScan or, if this information is missing, we distribute the facility amount equally among all syndicate members.

Loans in DealScan are generally granted by a syndicate of banks among which one or more can act as lead arrangers and have a more active role in the setting up and negotiation of the loan. As standard in the literature, we restrict the sample to include only lead arrangers, which we define in a manner similar to Chakraborty et al. (2018).[2] Lead arrangers can be expected to be aware of a firm's regulatory exposure, at least to the extent of being able to judge whether it would benefit or lose from policy intervention. Large loan sizes, long maturities, reputational damage, and economical costs associated with a failure in due diligence incentivize lead banks to conduct proper ex-ante screening and monitoring (Gopalan et al., 2011). Moreover, the syndicated loan market comprises mostly large companies which tend to be less opaque. Furthermore, Ehlers et al. (2021) document the financial significance of material risks faced by borrowers if climate change-related regulation is introduced. Hence, lead arrangers are expected to be aware of regulatory risks and are shown to be pricing them (Delis et al., 2021).

Given that DealScan captures loan information only at the time of origination,

[2] We consider lead arrangers lenders classified as: "Admin Agent", "Lead bank", "Lead arranger", "Mandated lead arranger", "Mandated arranger", and lenders denoted with a "yes" for lead arranger credit.

we use loan proportions to construct a stock variable proxying the outstanding loan volume between each bank-firm pair with the aim to obtain granular credit-level data (Chakraborty et al., 2018; Doerr and Schaz, 2021). Hence, each loan enters a bank's loan book from the time of its origination until it matures.[3] We aggregate outstanding loan volumes in each quarter for each pair such that our level of observation is the bank-firm-quarter. Mueller et al. (2022) show that their results are robust to various data preparation choices in DealScan. In the Internet Appendix Table IA1, we illustrate that the same holds in our case when considering loan issuances only at origination, alternative lead arranger definition, the inclusion of only real syndicates, as well as common loan types.

Firm-level climate change exposure We rely on a new database by Sautner et al. (2022), who construct a detailed measure of regulatory risk at the firm level. It initially covers more than 10,000 publicly listed firms from 34 countries. The authors base their work on the transcripts of conversations between management, financial analysts, and other market participants in quarterly earnings conference calls. Earnings calls are major corporate events during which material aspects of a firm's current and future developments are discussed. The measure captures the proportion of the conversation during the conference call centered on regulatory topics related to climate change as well as its sentiment. Specifically, the measure is constructed as follows:

$$\text{CCExposure}_{f,t} = \frac{1}{\mathrm{B}_{f,t}} \sum_{b}^{\mathrm{B}_{f,t}} (1[b \in \mathbb{C}]) \times \sum_{b}^{\mathrm{B} \in S} \tau(b) \tag{1}$$

where $\mathbb{C}$ is a set of bigrams developed on the basis of text analysis that captures regulatory shocks related to climate change, $b = 0, 1, ..., B_{f,t}$ are the bigrams of firm f in quarter t, $1[.]$ is an indicator function, S encompasses sentences containing $b = 0, 1, ..., B_{f,t}$, and $\tau(b)$ assigns sentiment to each b. The set of bigrams $\mathbb{C}$ is taken to the conference call of firm f in quarter t to count their frequency of occurrence. The total count is scaled by the total number of bigrams in the call while taking into account different call durations. The first part of the product captures the relative frequency with which related bigrams occur in the conference call transcripts of a firm. The second part of the product assigns sentiment to each bigram with

[3] This construction assumes that loans are not repaid before maturity.

$$\tau(b) = \begin{cases} 1 & \text{if } b \text{ has a positive tone} \\ -1 & \text{if } b \text{ has a negative tone} \\ 0 & \text{otherwise.} \end{cases} \tag{2}$$

Hence, $\mathit{CCExposure}_{f,t}$ can be negative, positive or zero.[4] *Negatively exposed firms* are firms for which regulatory topics or developments constitute bad news as they can negatively influence firms' operating costs, earnings, and cash flows as well as relate to an increased loss probability (Campiglio, 2016; Huang et al., 2018). An example of a negatively exposed firm in our sample is GenOn Energy, which, e.g., discusses the costs associated with compliance with the Maryland Healthy Air Act in its Q1 2012 conference call.

A positive exposure, in turn, suggests that the firm expects to benefit from regulatory developments or at least considers them as good news for its business. These policies might correct the relative cost disadvantages of greener business models, either by providing subsidies to greener technologies or by increasing the operating costs of more polluting competitors (Holburn, 2012). We refer to these firms as *positively exposed firms*. An example of a positively exposed firm in our sample is Fortum Oyj, which, e.g., discusses that the future development strategy of the firm will be even more targeted towards renewable energy in Q2 2015. Finally, we consider firms in our sample with zero exposure as not exposed to climate change-related regulatory risks and employ them as our control group. In our empirical analysis, we rely on the average pre-shock exposure of each firm - $\overline{\mathit{CCExposure}}_f$ - to construct indicators for whether a firm is negatively ($\overline{\mathit{CCExposure}}_f < 0$) or positively ($\overline{\mathit{CCExposure}}_f > 0$) exposed. We employ indicator variables as banks should at least be able to judge whether a firm would benefit or lose from policy intervention due to the financial significance of regulatory risks (Ehlers et al., 2021). Nevertheless, we show in the Internet Appendix that using $\overline{\mathit{CCExposure}}_f$ does not lead to qualitatively different results. Moreover, the use of pre-shock averages reduces endogeneity concerns.

The use of this dataset to capture firms' exposure to regulatory risks has three main advantages. First, we are able to identify not only firms that could be negatively impacted by regulatory interventions but also those who could benefit from them. This is contrariwise to carbon emission data that do not allow for the differentiation be-

[4] This corresponds to $\mathit{CCSentiment}^{Reg}$ from the database by Sautner et al. (2022). In this paper, the authors provide further information on the specific bigrams underlying the exposure measure. Data was downloaded on April 12th, 2021.

tween 'good' and 'bad' emissions.[5] Second, this measure captures a forward-looking view of key stakeholders in the firm rather than a historic record of the current business model as measures that focus on carbon emissions or fossil fuel reserves do. It reflects an internal evaluation of the firms' exposure rather than an outsider estimate based on observable factors. The stakeholders in a firm have access to more intangible information, such as the future direction the firm plans to take. Third, it suffers less from selection bias because earnings conference calls are a common practice and take place on a regular basis for large firms. This is in contrast to carbon emission data or environmental, social, and governance (ESG) reports that are mostly provided voluntarily.

A key concern regarding the measure may relate to greenwashing efforts by management. Greenwashing refers to the "selective disclosure of positive information about a company's environmental or social performance, without full disclosure of negative information on these dimensions, so as to create an overly positive corporate image" (Lyon and Maxwell, 2011, p. 9). However, conference calls are less prone to greenwashing than annual or ESG reports. Even if management evades climate change topics or selectively addresses only positive achievements, financial analysts are actively involved in the calls and can participate in the discussions (Hollander et al., 2010). This is reflected in the difference between exposures based on the presentation and Q&A sessions separately. In the latter part, climate change-related topics are generally discussed in a more negative way (Sautner et al., 2022). Nevertheless, we conduct further tests to ensure that greenwashing does not affect our results in Section 8.

Another possible concern related to this measure in our setting is that it may be endogenous with respect to banks' credit supply choices. This would be the case if, for instance, a new loan leads to firms' exposure becoming less negative or more positive. While bank lending may have an impact on firms' exposure in the long term, we calculate the correlation between having received a loan and firms' average exposure one year after the loan. The correlation is 0.006. Thus, we find no systematic change in firms' exposure immediately after receiving a new loan.

Firm and bank characteristics We first merge the exposure data to Worldscope and, if missing in Worldscope, to Orbis using firms' ISIN numbers. This also allows obtaining characteristics, names, and locations of firms in the data by Sautner et al. (2022).

[5] Emissions are considered 'good' when contributing to the transition to a greener economy by e.g. increasing energy efficiency (Seltzer et al., 2022).

Following Almeida et al. (2004), we account for mergers and acquisitions by excluding firm-quarters with annual asset or sales growth exceeding 100%. We require total assets to be non-zero and non-negative. All variables are winsorized at the 1st and 99th percentiles.

We then use a string matching approach to identify the firms that borrow in DealScan. We link the two datasets based on firm name, ticker, country, and city, and manually approve each non-perfect match. From the 11,461 firms included in the data by Sautner et al. (2022), we arrive at 3,245 firms that borrow in the syndicated loan market, for which we have exposure information as well as firm characteristics. Further sampling (e.g., dropping financial firms or requiring non-missing information on SIC codes and location) leads to a final number of 2,096 firms in our regressions.

To saturate several descriptive statistics and further regressions with bank characteristics, we add bank-level information from Wordscope using a link file provided by Schwert (2018). We then expand on this by linking the two datasets based on bank name, ticker, country, and city, and manually confirm all non-perfect matches. This process delivers bank-level characteristics for a subset of 277 lenders in our baseline sample.

3.2 Descriptive statistics

Table 1 contains the definitions of all variables used in this analysis, and Table A1 in the Appendix comprises the corresponding summary statistics.

[Table 1]

Industry variation To illustrate the underlying variation of the data by Sautner et al. (2022) that serves as the basis of our analysis, after omitting firms with zero exposure, we aggregate firms' pre-shock exposure at one-digit industry level separately for the group of positively and negatively exposed firms in Figure 1. It illustrates that the degree to which firms consider themselves to be positively affected is much lower than the extent to which firms expect to lose from future regulation. However, this is not surprising given the form that future regulation related to climate change is likely to take. Comparing negatively exposed firms across industries, it is, in particular, transportation, mining, and manufacturing that face high negative regulatory risks related to climate change. Comparing positively exposed firms across industries, agriculture, manufacturing, and services exhibit the largest potential to benefit from regulation. On average, all industries are negatively exposed. Nevertheless, within industry variation is significant, as we observe positively, negatively, and non-exposed firms within each industry (except for construction). In line with Sautner et al. (2022), we find that 75% of the variation in firms' exposure in our sample is not explained by industry, country, or time fixed effects.

[Figure 1]

Firm exposure variation Figure 2 shows the distribution of the average pre-shock exposure, $\overline{\textit{CCExposure}}$, at the firm level. In line with the expectation that more firms are likely to be negatively affected than to benefit from regulatory intervention, we observe approximately three times as many negatively exposed firms as positively exposed ones. Roughly 7% of firms are positively exposed, while close to 20% are negatively exposed. Furthermore, the average negative exposure of firms is higher than the average positive exposure.

[Figure 2]

Figure IA1 in the Internet Appendix furthermore illustrates the distribution of positively and negatively exposed firms across the three regions in our sample. This distribution is relatively similar for the two exposed groups, with around 75% of exposed firms located in the United States, around 17% in Europe, and the remaining in rest of the world (ROW).

4 Identification

4.1 Empirical specification

We employ a DID design to identify how credit changes after the Paris Agreement, depending on firms' exposure to climate change-related regulatory risks:

$$\begin{aligned} \mathrm{y}_{b,f,t} = {} & \beta_1 \mathrm{Positive}_f \times \mathrm{Post}_t + \beta_2 \mathrm{Negative}_f \times \mathrm{Post}_t \\ & + \zeta_{b,f} + \zeta_{j,l,s,t} + \zeta_{b,t} + \varepsilon_{b,f,t}. \end{aligned} \tag{3}$$

The dependent variable is the natural logarithm of outstanding credit between bank b and firm f in quarter t. $\textit{Positive}_f$ is a binary variable assuming a value of one if a firm has a positive exposure to regulatory risks, and zero otherwise. It is constructed on the basis of $\overline{CCExposure}_f$, which is the average pre-shock exposure of firm f. Correspondingly, $Negative_f$ is equal to one if a firm has a negative exposure to regulatory risks, and zero otherwise. Hence, the comparison group always comprises firms with zero exposure. $Post_t$ divides the sample into a pre- and post-shock period. The cut-off point is the last quarter of 2015, as the Paris Agreement was signed in December of that year.

To isolate the effect of the Paris Agreement on quantities lent to positively and negatively exposed firms respectively, our empirical specification needs to absorb bank-, firm-, and pair-specific factors that influence loan outcomes, other shocks that might lead to changes in bank's credit supply, and potential changes in firms' credit demand. We saturate the equation with bank-firm fixed effects ($\zeta_{b,f}$) to capture differences across firms and banks that are constant over time as well as unobservable time-invariant characteristics that influence loan outcomes of each bank-firm pair, such as relationship or distance. Furthermore, we introduce bank-time fixed effects ($\zeta_{b,t}$) to control for shocks to banks' characteristics that could lead to changes in credit supply. This may also absorb changes in banks' lending behavior in response to the Paris Agreement that affect all groups of firms equally. However, it does not control for how banks alter their lending differentially after the Paris Agreement depending on the exposed group of firms under study conditional on existing credit supply.

While it is common in the banking literature to control for changes in firm demand via the inclusion of firm-time fixed effects, our empirical setup does not allow their inclusion as they subsume the interaction terms of interest. To nevertheless make sure that changes in firms' demand for financing do not influence our estimations, we use borrowers' ILST fixed effects ($\zeta_{j,l,s,t}$) in the fashion of Degryse et al. (2019). Industry fixed effects are at the two-digit SIC code level, location fixed effects are at three-digit postal codes for the United States, at the NUTS1 level in Europe, and the country level in ROW. Size bins are based on deciles of firms' total assets averaged over the pre-shock period. This approach will absorb changes in credit demand driven by the Paris Agreement that are homogeneous within industry-location-size-time clusters. However, changes in credit demand driven by the Paris Agreement may be heterogeneous within these clusters depending on firms' exposure. Therefore, we additionally rely on time-varying firm controls that importantly relate to demand for credit in a later specification. As their inclusion does not lead to significant changes in the estimated effects, we are reassured that baseline results are likely to isolate supply-side effects. In addition, $\zeta_{j,l,s,t}$ implicitly capture any macroeconomic developments that affect all banks and firms in the sample. The single terms $Positive_f$, $Negative_f$, and $Post_t$ are absorbed by the fixed effects. $\varepsilon_{b,f,t}$ is the idiosyncratic error term.

Ultimately, we identify changes in credit volumes to exposed firms relative to the control group of non-exposed firms induced by the Paris Agreement. The variation we estimate rests on within-bank changes in credit supply to positively (negatively) exposed firms. From this follows that β_1 illustrates how loan volumes granted to positively exposed firms change compared to firms with zero exposure after the shock. Correspondingly, β_2 outlines the changes in lending to negatively exposed firms compared to the group of firms with zero exposure.

4.2 Parallel trends

The validity of any DID design crucially depends on the assumption that the treatment and control groups would follow the same trend in the absence of treatment. To provide evidence that this assumption holds in our setting, we report the pre-shock averages of various loan, firm, and bank characteristics for each group of firms. This includes negatively and positively exposed firms as well as firms with zero exposure. Table 2 shows normalized differences by treatment status in the fashion of Imbens and Wooldridge (2009). A difference smaller than $\pm$ 0.25 indicates no significant difference between the groups and the adequateness of linear estimation methods.

[Table 2]

Importantly, loan growth of each group of firms is sufficiently equal, as apparent in Panel A. Irrespective of their exposure to regulatory shocks, firms exhibit similar trends before the Paris Agreement as illustrated in Panel B. Similarly, Panel C shows that banks that lend to the three groups do not follow statistically different trends in the pre-shock period. To confirm this finding, we replicate Table 2 to display differences of the top quartile of treated firms relative to the group of non-exposed firms in Table IA2 in the Internet Appendix.

Figure 3 shows further evidence that the parallel trends assumption holds for our baseline results, i.e. $Negative \times Post$ is positive and significant. First, Panel (a) displays yearly treatment coefficients for negatively exposed firms. We interact *Negative* with a full set of year dummies using 2015 as a reference. We find that yearly treatment effects are not significant before 2015. Hence, this exercise does not provide any indication that parallel trends are absent.

[Figure 3]

Second, we use placebo tests to establish that 'treatment' effects are not observable in the absence of our shock. Panel (b) plots the estimates for $Negative \times Post$ and 95% confidence intervals for regressions in which we define 12 placebo events between Q1 2005 and Q4 2007. We find insignificant effects in each placebo regression, indicating that our results are indeed driven by the Paris Agreement in 2015.

Last, we test our baseline specification on several propensity-score-matched samples finding congruous results (see Panel (c)). We estimate a probit model for the probability that a firm is either negatively or positively exposed using the following characteristics to match: ROA, equity ratio, R&D inv. ratio, capital exp. ratio, and sales ratio. We then employ the estimated coefficients to compute the propensity score and create a matched sample of treated and control firms with the closest propensity scores using one-to-one matching with no replacement, nearest neighbor, and three nearest neighbors.

Overall, there is no evidence that firms develop differently over the pre-shock period nor that they are linked to banks that develop differently.

5 The effect of regulatory risks on credit

We now turn to the results on how credit volumes granted to exposed firms relatively to non-exposed firms change following the Paris Agreement. Table 3 displays the results from estimating Equation (3) with standard errors clustered at bank level. Columns (1) to (4) sequentially introduce the fixed effects structure outlined in Section 4.1, with Column (4) reporting the results of our preferred specification. Neither controlling for time-invariant bank, firm as well as bank-firm pair characteristics in Column (2) nor for shocks to bank characteristics that might affect overall credit supply in Column (3) leads to qualitative changes in the estimated coefficients. In Column (4), we incorporate ILST fixed effects that absorb confounding changes in firms' demand for credit. Once controlling for changes in demand, we no longer identify significant changes in quantities granted to positively exposed firms after the Paris Agreement compared to non-exposed firms (i.e. β_1 is not significant). β_2, in turn, is now positive and statistically significant and outlines that negatively exposed firms receive 17% more credit after the Paris Agreement compared to non-exposed firms. Negatively exposed firms experience a relative increase in lending quantities despite the increased awareness of the potentially negative impacts of regulation on these firms. This is in line with the hypothesis that banks lend more to them to exploit the free ride on the negative externalities that are not yet internalized (Reghezza et al., 2022). Alternatively, our results could hint that banks supply relatively more credit to those negatively exposed firms that have a strategy and the potential to transition to a greener business model (Engle et al., 2020; Faccini et al., 2021).

[Table 3]

Next, Column (5) includes time-varying firm variables lagged by one quarter to ensure that the results are not driven by residual changes in firms' demand for credit related to the Paris Agreement. We introduce characteristics that importantly relate to firms' demand for credit. This encompasses firms' return on assets as a measure of their profitability, the ratio of common equity to total assets to proxy their capital structure, R&D expenditure to total assets to capture innovative activities, capital expenditure to total assets to control for investment decisions, and sales to total assets as this closely relates to firms' liquidity.[6] The inclusion of these controls may be partially correlated with changes in negatively or positively exposed firms' demand for credit induced by the Paris Agreement. For instance, firms' sales ratio may be considered to quite directly capture the effect of changes in consumer preferences due to the Paris Agreement on firms' demand for credit. The introduction of these controls does not lead to significant changes in the estimated coefficients as can be seen by comparing Columns (5) and (6), where Column (6) reports the results of estimating the baseline specification on the sample for which we have firm controls. This suggests that the effect observed in Column (4) may rather be driven by changes in banks' supply to negatively exposed firms.

5.1 Heterogeneity across regions

In our setting, it is important to consider that the impact of the Paris Agreement may be different across participating nations reflecting heterogeneous expectations about environmental regulation across countries. The stringency of the existing regulatory environment where the firm is located can be a proxy for both these expectations as well as for the materiality of the financial risks (benefits) that negatively (positively) exposed firms face (Carbone et al., 2021; Ehlers et al., 2021; Krueger et al., 2020). Banks may then also consider the stringency of the regulatory environment in which

[6] We set missing R&D expenditures to zero (Bena et al., 2017).

their borrowers are located when allocating credit. The more stringent the regulatory environment, the more material the financial risks or benefits that firms face. In Columns (1) and (2) of Table 4, we, therefore, split the countries in our sample at the median depending on the stringency of the regulatory environment related to climate change policies before the Paris Agreement. To rank countries, we employ the 2014 Climate Change Performance Index (CCPI) from Germanwatch (Beyene et al., 2021; Delis et al., 2021; Ehlers et al., 2021).[7]

[Table 4]

The sample split illustrates that regulatory stringency appears to be a qualifying factor for the impact of this event on firms. Only in countries with low regulatory stringency, we find that quantities given to negatively exposed firms are higher after the Paris Agreement compared to the control group (Column (1)). Negatively exposed firms receive 29% more credit. This corresponds to US$ 20 million more at the bank-firm pair level.[8] In contrast, we do not observe a significant change in credit volumes extended to negatively exposed firms that are located in more stringent regulatory environments (Column (2)). In these jurisdictions instead, positively exposed firms obtain relatively more credit. They receive 80% more, which corresponds to US$ 56 million more at the bank-firm pair level.[9] This is in line with banks supplying more credit to positively exposed firms in environments where the materiality of potential benefits for these firms is higher.

To further zoom into differences in how the Paris Agreement impacted credit volumes, we look separately at large regions within our sample. In Columns (3), (4), and (5), we distinguish between firms located in the United States, Europe, and ROW.[10] The results for US firms are in line with the estimated coefficients for relatively less stringent environments while for European firms we observe changes in credit volumes largely in accordance with the results for more stringent ones. This is not surprising as Europe encompasses predominantly countries, which are characterized by more stringent regulations. In ROW, we do not observe significant changes in credit volumes,

[7] Detailed information on the Climate Change Performance Index is retrieved from: https://germanwatch.org/sites/default/files/publication/10407.pdf.

[8] The change in average loan volumes between the pre- and post-shock periods is US$ 69.6 million. We show the product of this change and the coefficient, i.e., 0.288× US$ 69.6 million.

[9] We show the product of the change in pre- to post- average loan volumes and the coefficient, i.e., 0.803× US$ 69.6 million.

[10] We would have liked to be more granular and divide ROW into smaller sub-regions. However, limitations in data coverage prevented us from doing so.

potentially due to the grouping of both high and low regulatory risk countries as well as low sample size. Hence, we drop ROW from further investigations.

These results might also reflect the political contexts surrounding and following the Paris Agreement. Although the United States was very active in facilitating the Agreement, 2016 was an election year making imminent policy efforts rather unlikely. Furthermore, following the election of Donald Trump as President of the United States in November 2016, his administration signaled and actively pursued a deregulating agenda to scale back or eliminate federal climate mitigation and adaptation measures.[11] This culminated in June 2017 with the Trump administration formally announcing the withdrawal from the Paris Agreement. Conversely, the European Union, in particular, was seen to have quickly finalized legislative processes ratifying the Agreement and was expected to meet 2030 climate targets in 2016 (Dröge, 2016).

It could be argued that the diverging results for US and European firms following the Paris Agreement do not depend on the regulatory environment but rather on alternative events such as the election of Donald Trump, differences in the financing structure of firms across regions, or fluctuations in energy prices. In Section 8, we provide evidence that this is not the case. Nevertheless, we exploit variation in environmental stringency within our two regions to confirm that the regulatory environment is an important qualifying factor in determining the impact of the Paris Agreement. To this end, we rely on variation across states in the United States and across European countries. We classify US states using *Adaption* and *Democratic*. *Adaption* is an indicator for the 13 US states that have climate adaption plans in place before the adoption of the Paris Agreement (Kovacs et al., 2021).[12] *Democratic* takes on the value of one if Democrats had control of state legislatures after the 2014 election. While this applies to eleven states, there is no perfect overlap with the *Adaption* indicator. Already in the years preceding the Paris Agreement, the two main political parties in the United States took an increasingly polarized stance on climate change, with the Democratic Party supporting the introduction of mitigating regulation and the Republican Party opposing this process (Brewer, 2012). For Europe, we continue to use the 2014 CCPI to create an indicator for European countries with an index above the median.

Columns (1) and (2) in Table 5 show that the stringency of the initial, local regu-

[11] The Climate Deregulation Tracker run by the Sabin Center for Climate Change Law lists 176 deregulating actions in climate law taken by the Trump administration (Sabin Center for Climate Change Law, 2021)

[12] To which states this is applicable, is retrieved from: https://www.georgetownclimate.org/adaptation/plans.html.

latory environment plays a significant role in determining the credit volumes for negatively exposed firms in the United States. These firms receive lower credit volumes if located in states that have more stringent climate policies, but relatively more in states with a more relaxed regulatory environment. Column (3) shows that similar effects can be observed in the European sample, where the stringency of the local regulatory environment also qualifies the effect on lending to negatively exposed firms.[13] Hence, this confirms that regulatory stringency can lead to different changes in lending after the Paris Agreement across jurisdictions. This is reflected in the fact that when considering the local regulatory environment, the impact of the Paris Agreement for negatively exposed firms aligns in the United States and Europe.

[Table 5]

So far we have investigated how lending changes following the Paris Agreement. Nevertheless, existing literature documents that this shock also has an impact along the pricing margin. For instance, Delis et al. (2021) find that increasing spreads are charged to firms holding more fossil fuel reserves while Degryse et al. (2022) document discounted lending rates for firms voluntarily disclosing environmental data. Table 6 shows the results of estimating Equation (3) with *Loan Spread* as the dependent variable for the sample of US firms (Column (1)) and European firms (Column (2)). We find that banks price negative firms' regulatory exposure more after the Paris Agreement both in the United States and in Europe. In the United States, however, the relative increase is 8 basis points, significantly less than in Europe where these firms are subject to an added 34 basis points. Mirroring the results on loan volume, we also find differences in the impact of the Paris Agreement depending on regulatory stringency when considering this alternative lending margin. Banks appear to be pricing the risk of negatively exposed firms more in environments where the materiality of financial risks for these firms is higher.

[Table 6]

6 The role of banks' exposure and other bank heterogeneity

The Paris Agreement led to significant changes in lending volumes and prices. However, our investigation so far disregarded potential heterogeneity in banks' behavior. Certain

[13] Due to small sample size, we are not able to estimate the differential effect of regulatory stringency for volumes obtained by positively exposed firms in Europe.

characteristics may lead banks to adjust their lending differentially following the Paris Agreement. Understanding how banks respond to these risks is important as banks can play a central role in not only setting the incentives for a green transformation but also in providing the necessary funding to achieve it (UNEP, 2011). To this end, we extend the empirical specification in Equation (3) to include an interaction with indicators for each bank characteristic considered. This extension allows now for the inclusion of firm-time fixed effects which fully absorb changes in firms' demand. In this setting, we no longer can estimate β_1 and β_2. Instead, our estimates capture changes in banks' credit supply due to the Paris Agreement depending on firms' exposure and the bank trait considered.

First, we consider that banks are themselves, albeit indirectly, exposed to regulatory risks related to climate change via their loan portfolios. To construct a measure that captures banks' own exposure to regulatory risks, we rely on financial institutions being predominantly exposed to regulatory risks due to the financial activities they undertake (Giuzio et al., 2019). A large part of these activities encompasses the provision of credit to the real economy. Hence, banks are exposed through their lending to firms that are subject to regulatory risks.[14] Data availability restricts our sample to lending on the syndicated loan market, which is, however, a substantial part of banks' lending activities.[15] Similarly to Federico et al. (2020) and Mueller (2020), we use banks' syndicated loan portfolios to construct a proxy for their exposure as follows:

$$\text{Bank Exposure}_b = \sum_{f=1}^{N_b} \left(\frac{\text{lending}_{b,f}}{\text{lending}_b} \times \overline{\mathit{CCExposure}_f} \right). \tag{4}$$

Thus, bank b's exposure is defined as the sum of all firms' ($f = 1, ..., N_b$) pre-shock average share of lending to total lending weighted by their average pre-shock exposure to regulatory risks, $\overline{\mathit{CCExposure}_f}$. N_b are the number of firms that bank b is connected to. Correspondingly, banks' exposure can take on negative or positive values if a bank predominantly lends to negatively or positively exposed firms, respectively. Alternatively, it can be zero if a bank only lends to firms with zero exposure over the pre-shock period. For visualization, banks' exposure is scaled up 10^3 because of the small original values. Different degrees of initial bank exposure may be related to the

[14] This abstracts from the fact that banks could also be exposed to climate-related risks via their invested capital, other types of lending, or shareholder activism.

[15] Our portfolio measure represents around 4.4% of banks' total assets, this is slightly below the 6% measure in Doerr and Schaz (2021) as we require data availability on firms' exposure.

banks' business models, specialization or diversification choices, and location (Blickle et al., 2021; Doerr and Schaz, 2021).

We focus on banks that are negatively exposed. Figure 4 highlights that the majority of banks are negatively exposed, and only approximately 6% are positively exposed. While this corresponds only to a small number of banks, it is unsurprising that only very few banks predominantly lend to the few positively exposed firms. Furthermore, 21% of the banks exhibit zero exposure to regulatory risks as they lend predominantly to firms that have, on average, zero exposure over the pre-shock period.

[Figure 4]

Another reason to focus on banks' negative exposure is related to the fact that we consider banks' indirect exposure to firms' regulatory risks. If regulation is indeed introduced and, as a consequence, a firm defaults on its debt, lenders to the firm are negatively affected by the default. However, if a firm benefits from regulation, its lenders do not directly benefit from the regulation's positive impact on the firm's outcomes.

Negatively exposed banks can face different incentives when allocating credit. They may diversify their portfolios and reduce their exposure by either (or both) lending more to positively exposed firms or less to negatively exposed firms. This hypothesis rests on the predictions of the classical banking theory and empirical evidence that diversification reduces risks and is associated with many other benefits, such as improved performance (Diamond, 1984; Rossi et al., 2009; Tabak et al., 2011). Alternatively, negatively exposed banks might be reluctant to lend more to positively exposed firms to prevent (or at least delay) a devaluation of legacy positions and protect the credit value of the firms already in their books (Degryse et al., 2022). To this end, they might even increasingly support more negatively exposed incumbent clients either to protect them or to finance their transition (Engle et al., 2020).

Table 7 shows the results separately for firms located in the United States in Column (1) and Europe in Column (2). We do not find any differential effect of banks' exposure in the sample of US firms. In Europe, the positive coefficient for the triple interaction $Negative \times Post \times NegBank$ indicates that the more negatively a bank is exposed through its loan portfolio, the more it increases credit supply to negatively exposed firms relative to non-exposed firms after the Paris Agreement. This is economically meaningful as this corresponds to 26% more credit given to negatively exposed firms by a bank at the 90th percentile of the $NegBank$ distribution.[16] Thus, this provides evidence for a differential response to regulatory risks depending on banks' exposure. However, negatively exposed banks do not lend differently from non-negatively exposed banks to positively exposed firms.

[Table 7]

Hence, it does not appear to be the case that negatively exposed banks attempt to diversify their portfolios. These results instead partially align with the hypothesis of Degryse et al. (2022), although revealing a more nuanced picture. While we do find evidence that banks that are negatively exposed increase funding to their incumbent clients, our results deviate from the proposition that these banks do not support green firms, which could threaten the stability of incumbent clients in the same industry and location. This would have implied a negative and significant coefficient for $Positive \times Post \times NegBank$, which we do not find.

Besides banks' own exposure, other bank characteristics could matter in determining changes in credit supply after the Paris Agreement. We consider banks' systemic importance, preferences, and locations. To investigate the role of banks' importance, we create an indicator for whether a bank is classified as a globally systemically important bank (GSIB) in 2014, $GSIB$. These banks are subject to higher capital requirements, and therefore better protected from financial losses stemming from firms' regulatory risks. Moreover, too-big-to-fail guarantees might also lead GSIBs to underrate these risks (Beyene et al., 2021). While we do not identify differential behaviors from GSIBs lending in the United States (Column (2)), we do observe that these banks lend relatively more than less significant institutions to negatively exposed firms in Europe (Column (6)). This is consistent with GSIBs discounting firms' negative regulatory exposure in Europe.

Banks' existing preferences for sustainable lending might also determine how banks adjust their credit supply following the Paris Agreement. Banks with green preferences may adjust lending in a more sustainable way or at least refrain from supplying credit in a less sustainable manner. To investigate the role of preferences, we construct an indicator of banks' public commitments to lend sustainably, $UNEP$. We consider membership in the United Nations Environment Programm Initiative (UNEP FI) before the Paris Agreement following Degryse et al. (2021) and Delis et al. (2021).[17]

[16] A bank at the 90th percentile has a negative exposure of 0.093.

[17] The data is hand-collected from the official website: http://www.unepfi.org/members/ (accessed on July 20, 2021)

Columns (3) and (7) show that, only when lending to European firms, committed banks do adjust their lending differentially. However, when horse-racing all bank traits simultaneously this effect vanishes (see Table IA4 in the Internet Appendix). Revealed preferences for sustainable lending practices do not appear to be important drivers of credit supply decisions after the Paris Agreement. One should, however, consider that stated preferences (i.e. participation in UNEP FI) might underestimate the impact of true preferences, as banks might participate in this initiative merely to appear green.

One more aspect to consider in this setting is that banks might have a "home bias" in their lending decisions that could lead to different considerations about firms' regulatory exposure when lending to firms in the same country relative to firms abroad (Giannetti and Laeven, 2012a; Giannetti and Laeven, 2012b).[18] In our context, banks might be better able to track or predict regulatory risk domestically than in foreign countries. Alternatively, one could also speculate that banks might be more likely to want to appear green domestically and/or increase lending to negatively exposed firms abroad. We investigate whether domestic banks adjust their lending differentially from foreign ones. We include an interaction with $Home$, an indicator variable for US (European) banks. The evidence in Column (4) suggests that US banks do not adjust credit supply differently from other banks when lending within their region. European banks instead do lend relatively more to negatively exposed European firms compared to banks from other regions (Column (8)). This could be in line with the hypothesis that they better track or predict regulatory risk domestically.

To sum up, we find that there is evidence for heterogeneity in banks' credit supply decisions depending on bank type when lending to European firms. In Europe, negatively exposed banks, GSIBs, and European banks appear to increase credit supply relatively more to negatively exposed firms. This investigation highlights that even in stringent environments negatively exposed firms receive more funding when connected to particular types of banks.

7 Changes in credit as a hindering or facilitating factor in the transition

Transitioning towards a more sustainable economy is going to require massive investments to allow firms to reduce their carbon footprints and adapt their business models (UNEP, 2011). By investigating whether and how the Paris Agreement leads to changes

[18] Evidence shows that when faced with adverse economic shocks lenders adjust their credit supply differentially towards domestic and foreign firms in a way that is inconsistent with a "flight to quality" assumption.

in credit volumes, we can first provide evidence on whether increased awareness of regulatory risks leads to shifts in credit that support or hinder the transition. Moreover, there has been considerable discussion during the COP26 meeting in November 2021 and in regulatory circles in the last few years around whether and how to regulate financial market participants in order to facilitate a transition to a more sustainable economy. In a second step, we, therefore, provide some initial understanding of how different bank types consider these risks in adjusting their behavior and discuss how this interacts with the need to transition to a greener economy. We do this by taking a closer look at the type of negatively exposed firms toward which credit is directed.

The baseline results for European firms show changes in lending following the Paris Agreement which appear to support the transition, as positively exposed firms receive more credit. However, results for US firms do not present a clear-cut answer to the question of whether changes in credit volumes hinder or support the transition. Increased credit volumes granted to negatively exposed firms could be interpreted as hindering if this credit is not directed toward supporting firms in their transition toward a more sustainable business model. We, therefore, take a closer look at negatively exposed firms and consider the degree of negative exposure as an indicator of their capacity to transition. The underlying rationale is that firms that have only a low negative exposure might have higher potential or fewer difficulties in adapting their business model to new regulation (De Haas et al., 2021; Sautner et al., 2022). Thus, if increases in credit volumes are concentrated among negatively exposed US firms that are more likely to transition, this could be evidence in favor of a shift in credit that supports the transition in the United States.

Panel A in Table 8 presents the results. $VeryNegative$ is equal to one for the top quartile of negatively exposed firms, and zero otherwise. $LessNegative$ indicates the bottom three quartiles. Hence, in both cases, we still compare them to non-exposed firms. In the United States, both groups of negatively exposed firms receive relatively more credit (Column (1)). Thus, we do not find that banks increase lending, in particular, for those firms that have a higher likelihood to green their business model successfully. In Table IA5 in the Internet Appendix, we also consider other potential proxies for firms' ex-ante likelihood to transition (investments in R&D, capital expenditures, and green patenting activity) and find no contradicting evidence (De Haas et al., 2021; Sautner et al., 2022).

[Table 8]

Column (2), in turn, displays the results for European firms when allowing for different degrees of negative exposure. These results provide anew evidence that changes in credit volumes in Europe following the Paris Agreement support the transition. We observe that less negatively exposed firms receive relatively more credit while very negatively exposed firms get relatively less.

Given the key role that is assigned to banks in the context of financing the transition, we now take a closer look at the behavior of certain bank types. In the previous section, we observe that negatively exposed banks, GSIBs, and European banks appear to increase credit supply relatively more to negatively exposed firms in Europe. This could be evidence that the actions of certain bank types are hindering the transition, again if the increased support is not directed towards helping firms transition. We, therefore, take another look at banks' characteristics to investigate whether the observed increases in credit supply to negatively exposed firms are hindering or supporting the transition in Europe.

To this end, we introduce interactions with bank characteristics as in Table 7 while distinguishing between $VeryNegative$ and $LessNegative$ exposed firms. The results of this exercise are presented in Panel B of Table 8. In line with results from Section 6, we do not find that bank characteristics matter for credit supply decisions after the Paris Agreement in the United States (Columns (1) to (4)). In Europe, we find that negatively exposed banks and GSIBs do not increase lending relatively more to firms that are more likely to transition (Columns (5) and (6)). Hence, their behavior may represent an obstacle to the transition in Europe. European banks, in turn, lend more only to less negatively exposed firms (Column (8)). This is evidence for home banks supporting the transition in Europe.

In summary, the evidence that we can provide suggests changes in credit may be hindering the transition in the United States while supporting it in Europe. On the role of banks' traits in this setting, our findings point toward negatively exposed banks and systemically important ones adjusting their credit supply in a way that could be an obstacle to the transition in Europe. This might be of interest for future policy considerations.

8 Robustness checks

2016 US presidential election A potential concern with the findings from Section 5 is that the relative increase in credit volumes for negatively exposed firms in the United

States may be entirely driven by the election of President Trump or his deregulation agenda with regard to environmental regulation. In Column (1) in Table A2, we now show that this result can already be observed in the period after the Paris Agreement during the Obama Administration (Q4 2014 until Q3 2016). This leads to estimates which are qualitatively similar, although smaller in economic magnitude which is in line with low expectations for imminent policy efforts before the upcoming election. In Column (2), we estimate larger coefficients in economic terms when excluding this first period from the estimation and using only observations following the Trump election in the post-shock period. Thus, there does seem to be an exacerbation of the trends observed following the presidential election of 2016. In Column (3), we show that our findings do not vary if we consider the withdrawal announcement in Q2 2017 as the beginning of the post-shock period as this represents a credible signal that future climate policy to comply with the Paris Agreement was unlikely in the United States. The announcement could also have had an impact on Europe by undermining the credibility of the Paris Agreement. However, we see that the increase in lending to positively exposed firms only becomes stronger with time (Column (4)).

Firms' ratings and energy Next, we test that our results are not driven by differences in the financing structure across regions or fluctuations in energy prices. US firms differ from European ones in their financing structure, as the majority of US firms has access to the corporate bond market. This could be an alternative explanation for our diverging results across the two regions. If this were the case, we would expect to see similar effects in a sub-sample of US and European firms that are more comparable. We utilize the existence of a long-term issuer rating by Standard & Poor's to distinguish firms based on their access to the corporate bond market (Schwert, 2018). Columns (1) and (5) in Table A3 show that, using homogeneous subsets of non-rated firms in the United States and in Europe, we continue to find diverging results consistent with our original ones. Moreover, we also do not find statistically significant differences between the estimated coefficients in the non-rated and rated sample of US firms (Columns (1) and (2)). Thus, our results do not seem to be driven by differential access to other sources of financing. We cannot do the equivalent exercise for European firms as the number of rated firms is not large enough for estimation. Moreover, we do not find evidence that our results are driven by the energy sector or fluctuations in oil prices. Our results remain unchanged when we drop energy firms (Columns (3) and (6)) and industries heavily dependent on oil (Columns (4) and (7)).

Timing and location of regulation In Table A5, we test whether our results are driven by a shift in credit toward a shorter time horizon, particularly in the United States, by reducing the sample to loans with a longer maturity. The salience of regulatory risks may be higher with regard to loans with longer maturity, as in the long term, it is more difficult to predict whether regulation will be introduced. However, once we restrict the sample to loans that have a minimum maturity of two years, Columns (1) and (5) qualitatively show the same results. Another aspect to consider in our context is firms' locations. For example, regulation to curb carbon emissions is likely introduced at the location where emissions are generated and not at the headquarter level. Our data for regulatory risks are, however, at the headquarter level, and we treat loans from subsidiaries as if they originate from the parent firm. To test that this is not confounding our results, we run our estimations on a sample that excludes loans from foreign subsidiaries. Columns (2) and (6) confirm the results for the United States and Europe, respectively.

Anticipation effects While we have put forward arguments that anticipating the Paris Agreement seems questionable, we formally ensure that anticipation effects do not drive our findings. Therefore, we exclude observations from Q2 2014 to Q3 2015 in the creation of $\overline{CCExposure}$. This ensures that corroborating events such as reform proposals related to climate change by the Obama administration in the summer of 2014 or the endorsement of the UN Sustainable Development Goals in early 2015 do not influence our measure of regulatory risks. Columns (3) and (7) in Table A5 demonstrate that the results are qualitatively the same.

Greenwashing efforts To ensure that greenwashing efforts by management do not bias our results, we illustrate that our findings hold when looking at a sub-sample of firms, which previous literature has shown to be less likely to greenwash. Greenwashing can be deterred by intense scrutiny. A particular instance of a firm being subject to intensified scrutiny is when it is cross-listed, that is, listed at, at least, one international stock exchange in addition to a listing at the domestic exchange. Exposure to foreign investors and regulators dissuades firms from engaging in greenwashing (Del Bosco and Misani, 2016; Yu et al., 2020). Column (4) in Table A5 display the results from estimating Equation (3) for the sub-sample of US firms, which are listed at multiple exchanges. We are encouraged that our results are not driven by greenwashing, as we find similar results in this sub-sample. This test can not be performed on the sub-sample of European firms because the remaining variation within ILST clusters is not sufficient for estimation. However, when we relax the location definition in the clusters, we can confirm the result for Europe, too.

Alternative exposure measures and control group In Table A4, we use two alternative approaches to create the exposure measure. First, we use a cumulative exposure measure over the pre-shock period as the basis on which *Positive* and *Negative* are consequently constructed (Columns (1) and (4)). Second, we drop firms for which we do not have at least four consecutive observations in the pre-shock period to construct $\overline{CCExposure}$ (Columns (2) and (5)). Furthermore, we drop all firms with zero exposure from the sample, such that we can directly compare equilibrium quantities between positively and negatively exposed firms. This leads to $Negative \times Post$ dropping from Equation (3) and β_1 identifying relative differences in lending between positively and negatively exposed firms after the Paris Agreement. The results in Columns (3) and (6) confirm that even in direct comparison, negatively US firms obtain more lending after the shock compared to positively exposed US firms. The opposite holds true for European firms.

Further robustness Further checks are available in an Internet Appendix in Tables IA6 to IA13. First, we illustrate that results employing $\overline{CCExposure}$ instead of *Negative* and *Positive* align. Nevertheless, it is harder to understand which type of firm drives the results and to grasp the magnitudes of the effects from this specification. Second, we replicate parts of Table 3 by sequentially introducing our fixed effects structure for the sub-samples of US and European firms. Third, we employ different clustering schemes at the firm, location, bank-firm, and bank-time level. Fourth, we sequentially relax how we construct ILST fixed effects, as this should deliver more variation at the expense of more precision in controlling for other shocks that could affect firms' general demand for credit, as well as use bank controls instead of bank-time. Fifth, we test that the inclusion of loan characteristics (maturity and spread) as controls does not affect the results materially. In unreported results, we drop each firm, bank, and industry sequentially from the regression to verify that our results are not driven by a particular firm, bank, and industry.

Banks' negative exposure In Table A6, we show that results are not driven by outliers in *NegBank* or by how we define it. In Columns (1) and (3), we winsorize *NegBank* at the 1st percentile. In Columns (2) and (4), we employ *Bank Exposure* instead of *NegBank*.

In anticipation of the Paris Summit, banks may have changed their portfolio compositions to adjust exposure to certain firms or sectors. To show that anticipation effects do not drive the results on banks' exposure in this context, we exclude observations from Q2 2014 to Q3 2015 in the construction of banks' and firms' exposure. Columns (1) and (3) in Table A7 illustrates that results are virtually unchanged.

Loan securitization poses another challenge for our empirical strategy. If banks sell off the loans after origination, they may not or at least less be concerned about firms' regulatory risks. This may imply that *NegBank* is not adequately capturing the exposure of banks' loan portfolios. However, our data preparation process largely mitigates this concern, as our sample encompasses only lead arrangers. They typically retain a fraction of the loans on their balance sheets (Benmelech et al., 2012). Nevertheless, Blickle et al. (2020) outlines that in around 12% of all loans lead banks still sell off their entire loan shares. To therefore fully address this issue, we identify loans that are especially likely to be sold off and exclude them from the sample as well as from the construction of banks' exposure. This applies, in particular, to Term B loan.[19] Columns (2) and (4) in Table A7 show that our results hold.

9 Conclusion

This paper provides an assessment of how regulatory risks related to climate change affect banks' lending. We exploit the Paris Agreement as a shock to awareness of firms' regulatory risks. We investigate how credit supply changes depending on whether borrowers will be affected positively or negatively by the introduction of regulation. To do so, we rely on detailed, loan-level information between 2010 and 2019 for an international sample of banks and firms enriched with a firm-level measure of regulatory risks.

The effects of the Paris Agreement on credit volumes are not only a function of firms' exposure but also of the stringency of the existing regulatory environment in which firms are located. Stringency proxies for both expectations about environmental regulations as well as for the materiality of the financial risks (benefits) that negatively (positively) exposed firms face. In environments with low regulatory stringency, nega-

[19] Blickle et al. (2020) also highlight that if lead banks sell their shares, they do so shortly after origination.

tively exposed firms experience a relative increase in credit supply. In contrast, when regulatory stringency is high, it is positively exposed firms that receive relatively more. This finding is mirrored in the results for the two largest regions in our sample, United States and Europe, which are characterized by, respectively, a less and more stringent regulatory environment. Thus, banks react to the Paris Agreement by supplying more credit to negatively (positively) exposed firms in environments where the materiality of financial risks (benefits) is lower (higher) for these firms.

We next investigate heterogeneity in banks' behavior as certain characteristics may lead banks to adjust their lending differentially following the Paris Agreement. We create a measure of banks' own, indirect exposure to regulatory risks based on their lending portfolio. Furthermore, we look at banks' significance, their preferences for sustainable lending, as well as their locations. We find that there is evidence for heterogeneity in banks' credit supply decisions depending on bank type when lending to European firms. In Europe, negatively exposed, significant, and European banks appear to increase credit supply relatively more to negatively exposed firms. This investigation highlights that even in stringent environments negatively exposed firms receive more funding when connected to particular types of banks.

Our paper contributes to the literature on the current role of the financial sector in mitigating transition risks: We identify the effect of regulatory risks related to climate change on lending to both firms that can benefit and those that might be hurt by the introduction of regulation. Considering a measure of firms' regulatory risks that captures a forward-looking view from within the firm allows differentiating between positively and negatively exposed firms. Observing changes in credit toward both groups can provide a more complete picture of how banks' behavior interacts with the need to transition. Moreover, we add to the literature by constructing a measure of banks' portfolio exposure and investigating how this affects banks' credit supply choices.

There are a few limitations in our setup that should be kept in mind when generalizing our results. We cannot speak as to aggregate effects, our findings are in relative changes. The DiD design does not allow quantifying whether, overall, exposed firms experience an increase or decline in lending from all lenders. Moreover, external validity is limited as the syndicated loan market comprises mostly larger banks and firms, therefore these results might not reflect effects in other lending markets nor with regard to smaller firms and banks. Nevertheless, results in this market matter as syndicated loans represent an important source of financing for non-financial firms as well as a significant proportion of banks' total lending (Doerr and Schaz, 2021).

This project has important policy implications for the scope of banking regulation in fostering the transition to a greener economy. We provide some initial understanding of how different bank types consider firms' regulatory risks when changing credit supply and how these changes might hinder or sustain the transition. In particular, negatively exposed and systemically important banks in Europe appear to behave in a way that may hinder the transition. Thus, these results highlight aspects in which future regulatory action may be needed

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Tables and figures

Table 1: Variable definitions

Variable name	Description
Loan volume	Outstanding loan volume in US$ million between bank b and firm f in quarter t
Loan maturity	Average maturity of outstanding loans in months between bank b and firm f in quarter t
Loan spread	Average spread of outstanding loans in basis points over Libor between bank b and firm f in quarter t
Post	An indicator for whether the Paris Agreement was already adopted or not
Firm characteristics	
Positive	An indicator for whether a firm has a positive average pre-shock exposure to regulatory risks
Negative	An indicator for whether a firm has a negative average pre-shock exposure to regulatory risks
VeryNegative	An indicator for the top quartile of negatively exposed firms
LessNegative	An indicator for the bottom three quartiles of negatively exposed firms
CCExposure	Relative frequency with which bigrams that capture climate change-related regulation shocks are mentioned along with positive and negative tone words
ROA	Ratio of net income to total assets
Equity ratio	Ratio of common equity to total assets
R&D inv. ratio	Research and development expenditures divided by total assets
Capital exp. ratio	Ratio of capital expenditures (additions to fixed assets) to total assets
Sales ratio	Ratio of net sales to total assets
Bank characteristics	
NegBank	The absolute value of a bank's exposure if its exposure is negative and zero otherwise
Bank Exposure	A bank's loan share to firm f weighted by firm f's exposure to regulatory risks averaged across all firms a bank lends to
GSIB	An indicator for whether a bank is classified as a globally systemically important bank in 2014
UNEP	An indicator for membership in the United Nations Environmental Programme Finance Initiative before the Paris Agreement
Home	An indicator for banks lending within their own region
ROA	Net income divided by total assets
Equity ratio	Common equity divided by total assets
Retained earnings	Retained earnings divided by total assets
Short-term funding	Ratio of current liabilities to total assets
Country or state characteristics	
Adaption	An indicator for whether a US state had finalized climate a adaption plan before the adoption of the Paris Agreement
Democratic	An indicator for whether the Democratic Party controls US state legislatures after the 2014 election
High CCPI	An indicator for whether a European country had a 2014 climate change performance index above the median

Table 2: Parallel trends

	Negative		*Zero exposure*		*Positive*		*Neg - No*	*Pos - No*
	Mean	SD	Mean	SD	Mean	SD	Normalized diff.	
Panel A: Bank-firm level								
Δ Loan volume	0.200	0.686	0.179	0.664	0.178	0.682	0.022	-0.001
Δ Loan spread	0.049	0.249	0.037	0.219	0.030	0.237	0.036	-0.023
Δ Loan maturity	0.025	0.127	0.016	0.106	0.020	0.123	0.052	0.024
Panel B: Firm level								
Δ Total assets	0.132	0.195	0.141	0.229	0.127	0.221	-0.029	-0.044
Δ ROA	-0.266	2.205	-0.097	2.430	-0.389	2.232	-0.052	-0.088
Δ Equity ratio	-0.035	0.474	-0.024	0.660	-0.021	0.627	-0.013	0.003
Δ R&D inv. ratio	-0.115	0.588	-0.061	0.480	-0.025	0.343	-0.070	0.062
Δ Capital exp. ratio	0.189	0.887	0.321	1.085	0.261	0.979	-0.094	-0.041
Δ Sales Ratio	0.012	0.079	0.020	0.098	0.021	0.083	-0.057	0.012
Panel C: Bank-firm level								
Δ Total assets	0.032	0.058	0.035	0.057	0.030	0.057	-0.035	-0.057
Δ ROA	-0.087	0.843	-0.055	0.808	-0.096	0.848	-0.027	-0.034
Δ Equity ratio	0.070	0.059	0.069	0.057	0.068	0.057	0.011	-0.010
Δ Retained earnings	0.619	1.672	0.515	1.506	0.737	1.922	0.046	0.091
Δ Short-term debt ratio	0.412	1.375	0.323	1.208	0.350	1.263	0.049	0.016

Note: This table shows summary statistics of relevant loan characteristics at the bank-firm level in Panel A, firm characteristics at the firm level in Panel B, and bank characteristics at the bank-firm level in Panel C for each of the three subgroups of firms in the analysis: negatively exposed, non-exposed, and positively exposed firms. The last two columns report normalized differences between negatively exposed and non-exposed firms in Column (8) and positively exposed and non-exposed firms in Column (9). All means are constructed over the pre-shock period between Q1 2010 and Q3 2015 and winsorized at the 1st and 99th percentiles. All variables are average annual percentage changes.

Table 3: The effect of regulatory risks on outstanding credit

	(1) No	(2) Bank-firm	(3) Bank-time	(4) ILST	(5) Firm controls	(6) Sample with controls
Positive	0.368***					
	(0.051)					
Positive × Post	-0.226***	-0.159***	-0.153***	0.161	-0.000	0.024
	(0.040)	(0.022)	(0.022)	(0.129)	(0.114)	(0.127)
Negative	0.225***					
	(0.029)					
Negative × Post	0.021	-0.005	0.009	0.171**	0.502***	0.516***
	(0.028)	(0.018)	(0.018)	(0.066)	(0.088)	(0.093)
Post	0.336***	0.195***				
	(0.026)	(0.039)				
Observations	336,257	336,257	336,257	336,257	230,681	230,681
Bank-firm FE	No	Yes	Yes	Yes	Yes	Yes
Bank-time FE	No	No	Yes	Yes	Yes	Yes
ILST FE	No	No	No	Yes	Yes	Yes
Firm controls	No	No	No	No	Yes	No
Adjusted R^2	0.023	0.842	0.918	0.921	0.926	0.926
Number of banks	307	307	307	307	265	265
Number of firms	2,096	2,096	2,096	2,096	1,800	1,800
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. In Columns (2) to (4), bank-firm, bank-time, as well as industry-location-size-time fixed effects are introduced sequentially. In Column (5), the estimation is saturated with lagged time-varying firm characteristics: return on assets, equity ratio, R&D inv. ratio, capital exp. ratio and sales ratio. Column (6) shows results for the sub-sample for which firm characteristics are available. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: The importance of local regulatory risk

	(1) Low regulatory stringency	(2) High regulatory stringency	(3) USA	(4) Europe	(5) ROW
Positive × Post	-0.123	0.803***	-0.251	0.795***	0.005
	(0.133)	(0.221)	(0.185)	(0.216)	(0.089)
Negative × Post	0.288***	0.149	0.319***	0.133	0.122
	(0.071)	(0.122)	(0.081)	(0.112)	(0.099)
Observations	215,103	109,443	180,399	102,596	49,845
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.921	0.921	0.901	0.911	0.929
Number of banks	206	195	121	164	189
Number of firms	1740	313	1553	292	247
Clustering	Bank	Bank	Bank	Bank	Bank

Note: This table explores lending changes following the Paris Agreement, as specified in Equation (3) across different jurisdictions and regions. Columns (1) to (2) are estimated on the sub-sample of firms located in jurisdictions with low and high regulatory stringency, respectively. Countries in the sample are sorted using the CCPI from 2014 and divided at the median. Columns (3) to (5) are estimated on the sub-samples of US, European, and ROW firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the period following the announcement of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Within-region regulatory risk

	(1) USA	(2) USA	(3) Europe
Indicator for Stringent:	Adaption	Democratic	High CCPI
Positive × Post	-0.211	-0.181	0.426**
	(0.223)	(0.205)	(0.190)
Positive × Post × *Stringent*	-0.085	-0.288	
	(0.287)	(0.308)	
Negative × Post	0.409***	0.375***	0.552***
	(0.097)	(0.091)	(0.196)
Negative × Post × *Stringent*	-0.410***	-0.335***	-0.793***
	(0.119)	(0.120)	(0.206)
Observations	180,399	180,399	100,087
Bank-firm FE	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes
Adjusted R^2	0.901	0.901	0.912
Number of banks	121	121	163
Number of firms	1,553	1,553	281
Clustering	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement and how they relate to within-region variation in regulatory stringency. We estimate Equation (3) for the sub-sample of US and European firms respectively with an added interaction with an indicator for stringency. The dependent variable is the log of outstanding credit. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the period following the announcement of the Paris Agreement. In Column (1), the indicator for stringency *Adaption* indicates whether US states finalized a climate adaption plan before the Paris Agreement. In Column (2), *Democratic* indicates in which US states Democrats controlled the state's legislative chambers after the election in 2014. In Column (3), *High CCPI* indicates countries in Europe with a CCPI above the European median. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Effect on firms' costs of funding

	(1) USA	(2) Europe
Positive × Post	-18.274	0.928
	(12.145)	(25.144)
Negative × Post	8.286*	31.134***
	(4.717)	(6.905)
Observations	177,030	101,135
Bank-firm FE	Yes	Yes
Bank-time FE	Yes	Yes
ILST FE	Yes	Yes
Adjusted R^2	0.956	0.959
Number of banks	118	164
Number of firms	1,536	288
Clustering	Bank	Bank

Note: This table explores changes in credit spreads following the Paris Agreement, as specified in Equation (3) for the sub-sample of US and European firms respectively. The dependent variable is the average loan spread at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the period following the announcement of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: The role of banks' own exposure and other bank heterogeneity

	USA				Europe			
	(1) Exposure	(2) Signif.	(3) Pref.	(4) Loc.	(5) Exposure	(6) Signif.	(7) Pref.	(8) Loc.
Positive × Post × NegBank	0.637				-0.162			
	(1.210)				(1.925)			
Negative × Post × NegBank	0.404				2.785***			
	(0.706)				(0.824)			
Positive × Post × GSIB		-0.045				-0.017		
		(0.054)				(0.081)		
Negative ×Post × GSIB		0.022				0.090**		
		(0.032)				(0.044)		
Positive × Post × UNEP			0.013				0.125*	
			(0.062)				(0.075)	
Negative × Post × UNEP			-0.023				0.039	
			(0.031)				(0.041)	
Positive × Post × Home				-0.070				0.150
				(0.061)				(0.102)
Negative × Post × Home				-0.014				0.095*
				(0.036)				(0.057)
Observations	177,702	177,702	177,702	177,702	102,483	102,483	102,483	102,483
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.908	0.908	0.908	0.908	0.914	0.914	0.914	0.914
Number of banks	119	119	119	119	163	163	163	163
Number of firms	1,454	1,454	1,454	1,454	289	289	289	289
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how certain bank characteristics affect how banks adjust credit supply following the Paris Agreement. To this end, the baseline regression specification is augmented with an interaction with a specific bank characteristic. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. In Columns (1) and (5), $NegBank_b$ takes on the value of bank b's exposure if $Bank\ Exposure_b$ is negative and takes a value of zero if bank b's exposure is zero or positive. In Columns (2) and (6), $GSIB_b$ is an indicator for whether the bank is classified as a GSIB in 2014. In Columns (3) and (7), $UNEP_b$ is an indicator for banks' membership in the UNEP FI before the Paris Agreement. In Columns (4) and (8), $Home_{b,c}$ is an indicator for banks lending within their region. Each specification includes bank-firm, bank-time, as well as firm-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

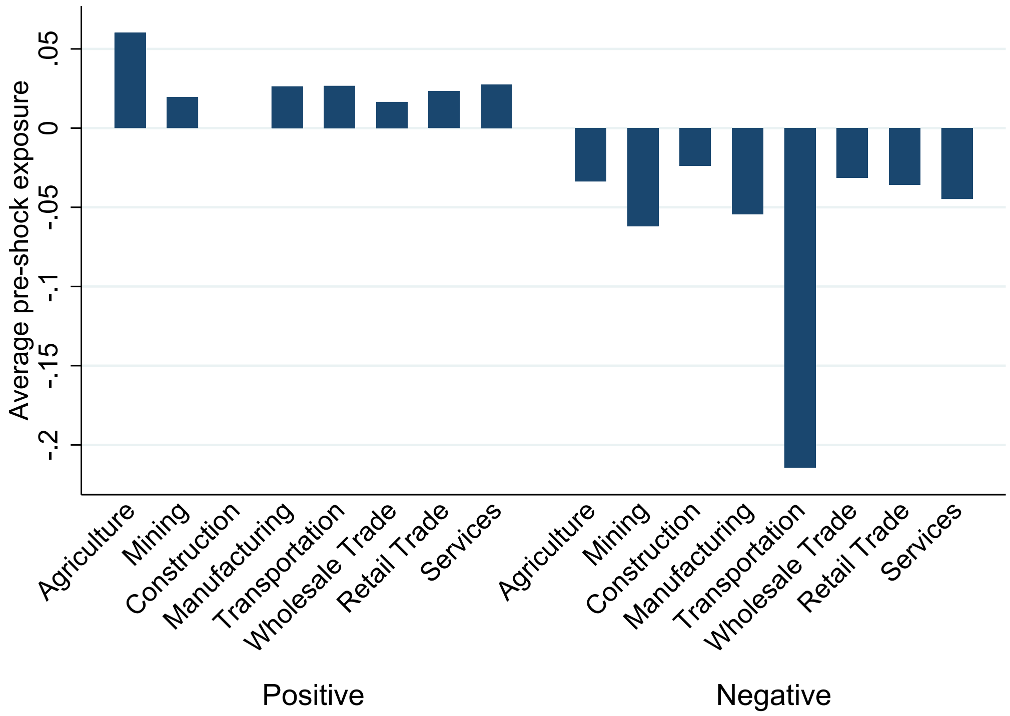
Table 8: Lending and firms' likelihood to transition

Panel A: Baseline effects	(1) USA	(2) Europe
Positive × Post	-0.253	1.164***
	(0.186)	(0.248)
LessNegative × Post	0.307***	0.503***
	(0.080)	(0.150)
VeryNegative × Post	0.411**	-0.400***
	(0.163)	(0.078)
Observations	180,399	102,596
Bank-firm FE	Yes	Yes
Bank-time	Yes	Yes
ILST FE	Yes	Yes
Adjusted R^2	0.901	0.911
Number of banks	121	164
Number of firms	1,553	292
Clustering	Bank	Bank

Panel B: Bank heterogeneity	USA				Europe			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Indicator for Bank Type:	NegBank	GSIB	UNEP	Home	NegBank	GSIB	UNEP	Home
Positive × Post × *Type*	0.627	-0.045	0.013	-0.070	-0.162	-0.017	0.126*	0.148
	(1.204)	(0.054)	(0.062)	(0.061)	(1.923)	(0.080)	(0.074)	(0.101)
LessNegative × Post × *Type*	1.583	0.035	-0.009	0.035	2.796***	0.067	0.007	0.098*
	(1.009)	(0.039)	(0.042)	(0.041)	(0.927)	(0.047)	(0.044)	(0.057)
VeryNegative × Post × *Type*	-1.086	-0.013	-0.051	-0.102	2.969**	0.123**	0.068	0.072
	(1.161)	(0.059)	(0.073)	(0.071)	(1.190)	(0.062)	(0.060)	(0.070)
Observations	177,913	177,913	177,913	177,913	104,022	104,022	104,022	104,022
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.908	0.908	0.908	0.908	0.913	0.913	0.913	0.913
Number of banks	119	119	119	119	163	163	163	163
Number of firms	1,459	1,459	1,459	1,459	297	297	297	297
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank	Bank

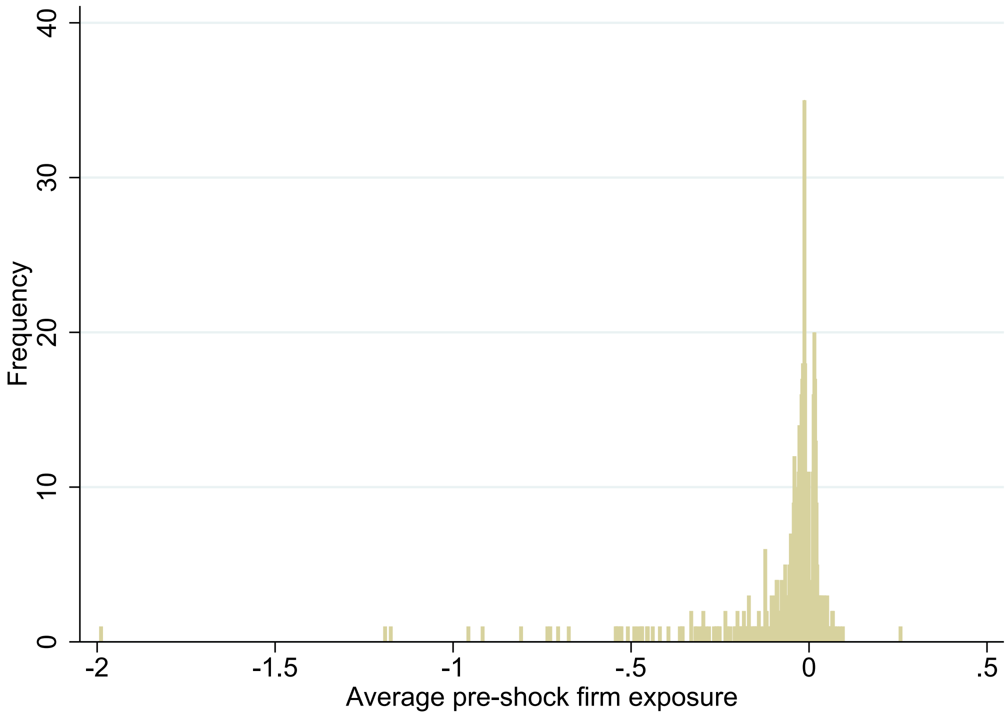
Note: This table investigates changes in lending volumes following the Paris Agreement depending on firms' degree of negative exposure as a proxy for firms' ex-ante likelihood to transition. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $VeryNegative_f$ indicates if firm f has a negative exposure above the 75th percentile of the negative exposure distribution. $LessNegative_f$ indicates if firm f has a negative exposure below the 75th percentile. $Post_t$ indicates the time period after the adoption of the Paris Agreement. In Panel A, each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. In Panel B, each specification includes bank-firm, bank-time, as well as firm-time fixed effects. In each column in Panel B, the main specification is augmented with an interaction with a specific bank characteristic. $NegBank_b$ takes on the value of bank b's exposure if $Bank\ Exposure_b$ is negative and takes a value of zero if bank b's exposure is zero or positive. $GSIB_b$ is an indicator for whether the bank is classified as a GSIB in 2014. $UNEP_b$ is an indicator for banks' membership in the UNEP FI before the Paris Agreement. *Home* is an indicator for banks lending within their region. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Industry distribution of firms' exposure



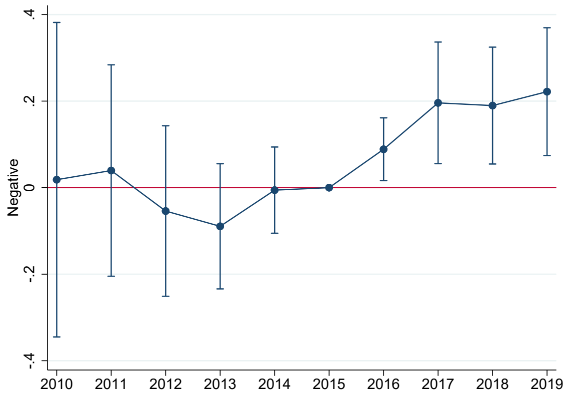
Note: This figure shows the distribution of firms' ex-ante exposure, $\overline{CCExposure}$ averaged at the 1-digit industry level separately for negatively and positively exposed firms. It is constructed as in Equation (1) and averaged over the pre-shock period at firm level. For visualization, exposure is scaled up 10^3 because of the small original values. Non-exposed firms are not included.

Figure 2: The distribution of ex-ante firms' exposure

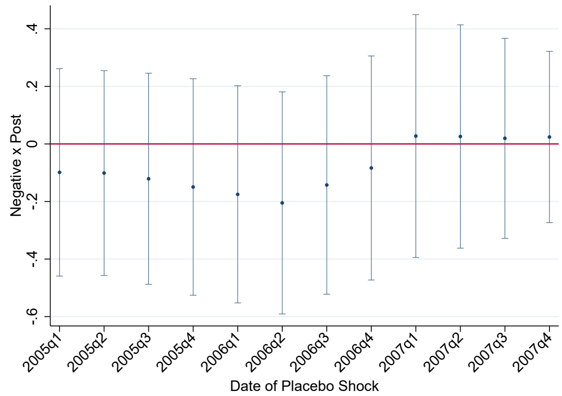


Note: This figure shows the distribution of firms' ex-ante exposure, $\overline{CCExposure}$ at firm level. It is constructed as in Equation (1) and averaged over the pre-shock period. For visualization, exposure is scaled up 10^3 because of the small original values. The bin size is 0.002 and non-exposed firms are not included in the graph to better observe the distribution of exposed firms. The baseline sample includes 1,534 firms with an exposure of zero, 414 negatively exposed firms, and 148 positively exposed firms.

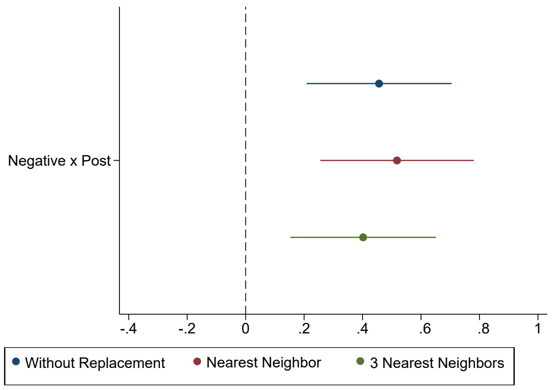
Figure 3: Parallel trends



(a) Effect over time



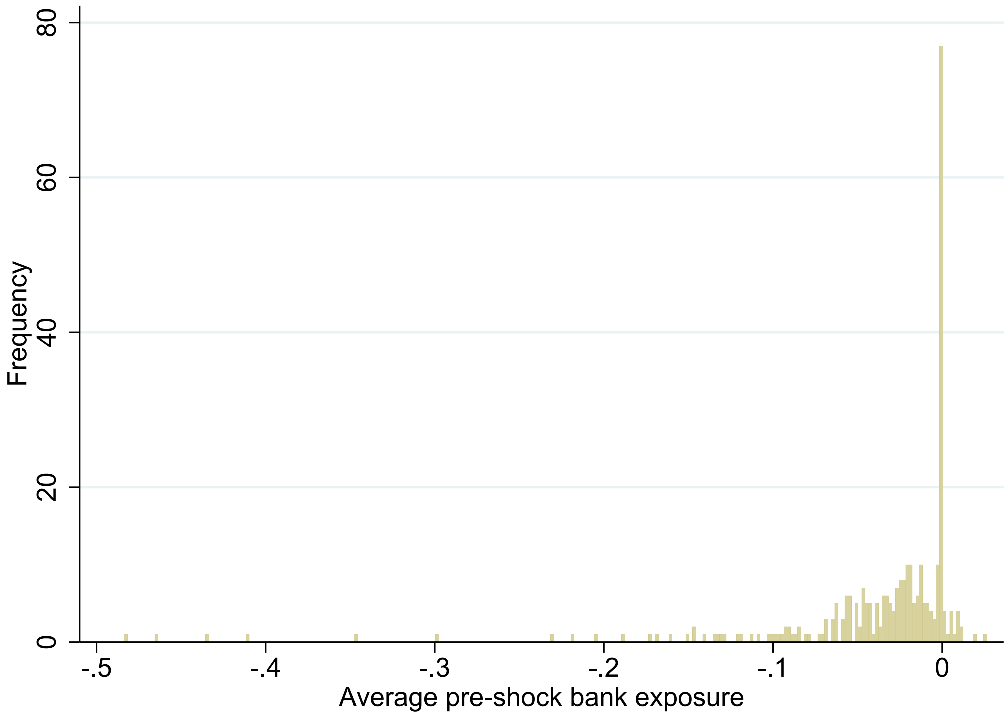
(b) Placebo tests



(c) Propensity score matching

Note: This figure shows three tests of the parallel trend assumption in the main regression specification. Panel (a) shows yearly treatment coefficients in accordance with Column (4) in Table 3 but interacting $Negative_f$ with a full set of year dummies using 2015 as a reference. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. Panel (b) illustrates the results of several placebo tests in which the shock is simulated to hit at different points in time. For each sub-sample, the estimated coefficient for $Negative_f \times Post_t$ and 95% confidence bands are plotted for twelve alternative placebo shocks in each quarter between Q1 2005 and Q4 2007. For each placebo test, we use a sample of bank-firm level observations for the banks and firms in our baseline sample for a period that predates the time frame employed in our analysis (Q1 2002 to Q2 2008). Panel (c) presents the results of estimating the baseline specification on a sub-sample of matched firms using propensity score matching and one-to-one matching with no replacement, with the closest neighbor, or with the three closest neighbors. For the matching, we employ as observables average pre-shock firm measures of ROA, equity ratio, R&D inv. ratio, capital exp. ratio, and sales ratio. In Table IA3 in the Internet Appendix, we report the first stage results. The specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects.

Figure 4: The distribution of ex-ante banks' exposure



Note: This figure shows the distribution of banks' average *ex-ante* exposure to the Paris shock, *Bank Exposure*, at the bank level. It is constructed as in Equation (4). The bin size is 0.001 and the highest bar is not the one including the non-exposed, it includes the marginally negatively exposed banks (i.e. from -0.001 to 0 not included). The sample includes 65 banks with an exposure of zero.

Appendix

Table A1: Summary statistics

	Mean	SD	P25	Median	P75
Panel A: Bank-firm-quarter level					
Loan volume	291.906	527.349	63.926	141.959	312.390
Loan spread	192.612	126.485	101.250	167.500	262.000
Loan maturity	64.284	32.179	56.000	60.000	66.000
Panel B: Firm level					
Positive	0.071	0.256	0.000	0.000	0.000
Negative	0.198	0.398	0.000	0.000	0.000
VeryNegative	0.147	0.355	0.000	0.000	0.000
LessNegative	0.050	0.218	0.000	0.000	0.000
CCExposure	-0.018	0.070	-0.009	0.000	0.000
Total assets (bio)	11.35	30.003	1.023	2.828	8.427
ROA	4.078	7.537	1.798	4.601	7.702
Equity ratio	40.307	17.414	27.611	39.663	52.425
R&D inv. ratio	0.442	0.910	0.000	0.000	0.394
Capital exp. ratio	0.434	0.514	0.127	0.276	0.537
Sales ratio	23.656	16.898	12.186	19.622	30.143
Panel C: Bank-firm level					
Bank exposure	-0.044	0.058	-0.064	-0.030	-0.003
GSIB	0.085	0.279	0.000	0.000	0.000
UNEP	0.170	0.376	0.000	0.000	0.000
USBank	0.169	0.376	0.000	0.000	0.000
EuropeanBank	0.391	0.489	0.000	0.000	1.000
Total assets (bio)	413.50	660.50	48.20	126.10	402.90
Equity ratio	7.832	3.003	5.538	7.321	10.109
ROA	1.064	0.582	0.684	0.981	1.441
Retained earnings	4.034	2.661	1.976	3.659	5.709
Short-term funding	9.945	6.929	4.452	8.595	14.522

Note: This table provides summary statistics for relevant variables at bank-firm-quarter level in Panel A, at firm level in Panel B, and at bank-firm level in Panel C. All means are constructed over the full sample period. Ratios are provided in percent. See Table 1 for variable definitions.

Table A2: 2016 US presidential election

	USA			Europe
	(1) Obama period	(2) Trump period	(3) After announcement	(4) After announcement
Positive ×Post	-0.194	-0.341*	-0.340	1.753***
	(0.159)	(0.201)	(0.225)	(0.185)
Negative × Post	0.189***	0.386***	0.428***	0.124
	(0.071)	(0.104)	(0.113)	(0.123)
Observations	104,443	159,125	148,105	86,734
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.916	0.899	0.898	0.907
Number of banks	107	120	120	163
Number of firms	1,429	1,550	1,550	292
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how the election of Donald Trump and the US withdrawal announcement interact with how lending changes following the Paris Agreement, as specified in Equation (3) in the US as well as in Europe. Columns (1) to (3) are estimated on the sub-sample of US firms. Column (4) is estimated on the sub-sample of European firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the period following the announcement of the Paris Agreement. In Column (1), the post-shock period ends in Q3 2016 limiting the time period to the last quarter of the Obama administration. In Column (2), the period between Q4 2015 and Q3 2016 is left out of the analysis. Hence, the impact of the treatment is estimated only during the Trump Administration. In Columns (3) and (4), the period between Q4 2015 and Q1 2017 is left out of the analysis. Hence, the impact of the treatment is estimated on the period following the United States' announcement of withdrawal from the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Firms' ratings and energy

	(1) USA Non-rated	(2) USA Rated	(3) USA Wo/energy	(4) USA Wo/highoil	(5) Europe Non-rated	(6) Europe Wo/energy	(7) Europe Wo/highoil
Positive × Post	-0.663***	-0.151	-0.097	-0.251	0.428**	1.052***	0.792***
	(0.203)	(0.225)	(0.334)	(0.185)	(0.190)	(0.131)	(0.216)
Negative × Post	0.361***	0.426***	0.455***	0.319***	-0.236***	0.130	0.129
	(0.085)	(0.097)	(0.164)	(0.080)	(0.072)	(0.113)	(0.112)
Observations	26,962	140,848	149,106	163,924	87,601	86,815	89,585
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.909	0.901	0.906	0.899	0.908	0.906	0.911
Number of banks	56	115	114	117	155	146	156
Number of firms	243	1,151	1,340	1,415	237	266	256
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Columns (1) and (5) include only non-rated firms. Column (2) includes only rated US firms. Columns (3) and (6) include only non-energy firms. Columns (4) and (7) exclude sectors with high oil consumption. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Alternative exposure measures and control group

	(1) USA Cum	(2) USA 4Seq	(3) USA Exposed	(4) Europe Cum	(5) Europe 4Seq	(6) Europe Exposed
Positive × Post	-0.251	-0.256	-0.924***	0.795***	0.796***	0.645***
	(0.185)	(0.185)	(0.254)	(0.216)	(0.222)	(0.189)
Negative × Post	0.319***	0.300***		0.133	0.134	
	(0.081)	(0.081)		(0.112)	(0.123)	
Observations	180,399	179,540	55,288	102,596	102,159	41,262
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.901	0.900	0.899	0.911	0.911	0.910
Number of banks	121	120	68	164	163	112
Number of firms	1,553	1,543	403	292	287	90
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. Both variables are constructed on the basis of a cumulative exposure measure in Columns (1) and (4). In Columns (2) and (5), we exclude firms for which we do not have 4 consecutive observations in the pre-shock period to construct their exposure. $Post_t$ indicates the time period after the adoption of the Paris Agreement. In Columns (3) and (6), non-exposed firms are dropped. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Timing and location of regulation, anticipation effects, and greenwashing

	(1) USA Timing	(2) USA Location	(3) USA Anticipation	(4) USA Greenwash	(5) Europe Timing	(6) Europe Location	(7) Europe Anticipation
Positive × Post	-0.268	-0.195	-0.251	-0.088	0.872***	0.530***	0.795***
	(0.182)	(0.190)	(0.185)	(0.331)	(0.210)	(0.174)	(0.216)
Negative × Post	0.290***	0.335***	0.319***	0.563***	0.171	0.111	0.133
	(0.081)	(0.072)	(0.081)	(0.117)	(0.107)	(0.122)	(0.112)
Observations	178,651	171,482	180,399	80,938	102,239	95,568	102,596
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.905	0.899	0.901	0.898	0.913	0.903	0.911
Number of banks	117	118	121	89	163	159	164
Number of firms	1,543	1,522	1,553	522	290	291	292
Clustering	Bank	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). In Columns (1) and (5), the estimation is conducted on a sub-sample encompassing only loans with a minimum maturity of two years. In Columns (2) and (6), the baseline regression is estimated on a sub-sample that excludes loans by subsidiaries that are not located in the same country as the parent. In Columns (3) and (7), exposures are constructed on the basis of $\overline{CCExposure_f}$ that, in contrast to the baseline, rests on a shortened pre-shock period ending in Q1 2014. In Column (4), estimation is conducted only on the sub-sample of US firms that are listed at multiple exchanges. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Winsorized and continuous bank exposure

	(1) USA	(2) USA	(3) Europe	(4) Europe
Positive × Post × NegBank	0.637		-0.162	
	(1.210)		(1.925)	
Negative × Post × NegBank	0.404		2.785***	
	(0.706)		(0.824)	
Positive × Post × Bank Exposure		-0.640		0.162
		(1.208)		(1.911)
Negative × Post × Bank Exposure		-0.404		-2.785***
		(0.706)		(0.823)
Observations	177,702	177,702	102,483	102,483
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.908	0.908	0.914	0.914
Number of banks	119	119	163	163
Number of firms	1,454	1,454	289	289
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how banks adjust credit following the Paris Agreement differentially depending on their own exposure to the shock. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. $NegBank_b$ takes on the value of bank b's exposure if $Bank\ Exposure_b$ is negative and takes a value of zero if bank b's exposure is zero or positive, the absolute value of the exposure is used to simplify the interpretation. $Bank\ Exposure_b$, in turn, is bank b's average pre-shock share of lending weighted by firms' exposure. In Columns (1) and (3), $NegBank_b$ is winsorized at the 1st percentile. Each specification includes bank-firm, bank-time, as well as firm-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Bank: Anticipation effects and securitization

	(1) USA Wo/14-15	(2) USA Wo/termB	(3) Europe o/14-15	(4) Europe Wo/termB
Positive × Post × NegBank	0.637	1.317	-0.162	0.016
	(1.210)	(1.160)	(1.925)	(1.706)
Negative × Post × NegBank	0.404	0.432	2.785***	2.503***
	(0.706)	(0.575)	(0.824)	(0.714)
Observations	177,702	167,027	102,483	101,813
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.908	0.920	0.914	0.917
Number of banks	119	116	163	162
Number of firms	1,454	1,430	289	296
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how banks adjust credit following the Paris Agreement differentially depending on their own exposure to the shock. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. $NegBank_b$ takes on the value of bank b's exposure if $Bank\ Exposure_b$ is negative and takes a value of zero if bank b's exposure is zero or positive, the absolute value of the exposure is used to simplify the interpretation. In Columns (1) and (3), Q2 2014 until Q3 2015 are excluded from the construction of banks' and firms' exposure. In Columns (2) and (4), we exclude loans that are likely to be securitized. Each specification includes bank-firm, bank-time, as well as firm-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Internet appendix

Table IA1: DealScan particularities

	(1) New issuan.	(2) Alt. lead	(3) Real synd.	(4) Common types
Positive × Post	0.048	0.112	0.074	0.195
	(0.037)	(0.139)	(0.156)	(0.126)
Negative × Post	0.057**	0.179***	0.206***	0.162**
	(0.028)	(0.063)	(0.067)	(0.065)
Observations	51,287	323,165	332,631	331,091
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	
Adjusted R^2	0.559	0.918	0.923	0.925
Number of banks	325	290	300	303
Number of firms	1,789	2,091	2,008	2,070
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Column (1) is estimated on the original DealScan structure. In Column (2), we use an alternative lead arranger definition. Column (3) excludes loans that are de facto no syndicate. Column (4) encompasses only common loan types in DealScan, i.e. credit lines and term loans. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects, except for Column (1) which rests on relaxed ILT fixed effects to allow for sufficiently large clusters for estimation. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA2: Parallel trends for the top quartile of exposed firms

	Top quartile Negative		*Zero exposure*		*Top quartile Positive*		*Neg - No*	*Pos - No*
	Mean	SD	Mean	SD	Mean	SD	Normalized diff.	
Panel A: Bank-firm level								
Δ Loan volume	0.226	0.708	0.179	0.664	0.096	0.621	0.049	-0.091
Δ Loan spread	0.056	0.263	0.037	0.219	0.031	0.216	0.057	-0.020
Δ Loan maturity	0.025	0.129	0.016	0.106	0.022	0.109	0.054	0.040
Panel B: Firm level								
Δ Total assets	0.120	0.235	0.141	0.229	0.114	0.225	-0.064	-0.084
Δ ROA	-0.369	1.535	-0.097	2.430	-0.728	2.008	-0.094	-0.200
Δ Equity ratio	0.024	0.416	-0.024	0.660	-0.177	0.726	0.062	-0.155
Δ R&D inv. ratio	0.076	0.281	0.037	0.408	0.152	0.344	0.079	0.216
Δ Capital exp. ratio	0.140	0.723	0.321	1.085	0.518	1.737	-0.138	0.097
Δ Sales Ratio	0.007	0.085	0.020	0.098	0.041	0.118	-0.093	0.141
Panel C: Bank-firm level								
Δ Total assets	0.038	0.049	0.038	0.047	0.032	0.047	0.009	-0.076
Δ ROA	-0.062	0.361	-0.041	0.364	-0.062	0.255	-0.042	-0.048
Δ Equity ratio	0.061	0.050	0.060	0.046	0.062	0.048	0.015	0.037
Δ Retained earnings	0.358	0.862	0.270	0.657	0.419	1.163	0.081	0.112
Δ Short-term debt ratio	0.504	1.475	0.257	1.047	0.312	1.130	0.137	0.036

Note: This table complements Table 2 by restricting the exposed groups to the top quartile of negative and positively exposed firms. Summary statistics of relevant loan characteristics at the bank-firm level in Panel A, firm characteristics at the firm level in Panel B, and bank characteristics at the bank-firm level in Panel C for each of the three subgroups of firms in the analysis: negatively exposed, non-exposed, and positively exposed firms. The last three columns report normalized differences between negatively exposed and non-exposed firms in Column (8) and non-exposed and positively exposed firms in Column (9). All means are constructed over the pre-shock period between Q1 2010 and Q3 2015. All variables are average annual percentage changes.

Table IA3: Propensity score matching

Panel A: First-stage propensity score matching	
	Treated (either Positive or Negative)
ROA	0.007
	(0.005)
Equity ratio	-0.002
	(0.002)
R&D inv. ratio	-0.117***
	(0.033)
Sales ratio	-0.007***
	(0.002)
Capital exp. ratio	-0.014
	(0.052)
Constant	-0.290**
	(0.092)
Number of firms	1,714

Panel B: Baseline on matched sample			
	(1) No replacement	(2) Nearest neighbor	(3) 3 nearest neighbors
Positive × Post	-0.029	0.190	-0.030
	(0.179)	(0.189)	(0.183)
Negative × Post	0.456***	0.518***	0.402***
	(0.126)	(0.133)	(0.126)
Observations	176,179	157,657	223,210
Bank-firm FE	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes
Adjusted R^2	0.919	0.916	0.919
Number of banks	232	222	258
Number of firms	937	826	1,243
Clustering	Bank	Bank	Bank

Note: This table presents the results of estimating Equation (3) on a sub-sample of matched firms using propensity score matching. Panel A presents the results of estimating a probit regression with the dependent variable $Treated_f$ and average pre-shock firm characteristics as regressors. For this test, we employ average pre-shock firm measures of ROA, equity Ratio, R&D inv. ratio, sales ratio, and capital exp. ratio. $Treated_f$ is equal to one if the firm was either positively or negatively exposed in the pre-shock period. We employ the estimated coefficients from this first-stage model to compute the propensity score for each observation in our sample and then match each treated firm with a control firm: one-to-one with no replacement in Column (1), with the closest neighbor in Column (2), or with the three closest neighbors in Column (3). Columns (1), (2), and (3) in Panel B report the baseline regression results estimated on each respective sample. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA4: The role of banks' own exposure and other bank heterogeneity: Horse race

	(1) USA	(2) Europe
Positive ×Post × NegBank	0.959	1.469
	(1.223)	(2.338)
Negative ×Post × NegBank	0.246	3.524***
	(0.550)	(0.746)
Positive × Post × GSIB	-0.064	-0.069
	(0.056)	(0.089)
Negative × Post × GSIB	0.027	0.090**
	(0.029)	(0.042)
Positive × Post × UNEP	-0.015	0.134
	(0.074)	(0.086)
Negative× Post ×UNEP	-0.049	0.003
	(0.043)	(0.040)
Positive × Post ×Home	-0.077	0.129
	(0.071)	(0.095)
Negative × Post ×Home	-0.031	0.163***
	(0.044)	(0.050)
Observations	177,702	102,483
Bank-firm FE	Yes	Yes
Bank-time FE	Yes	Yes
Firm-time	Yes	Yes
Adjusted R^2	0.908	0.915
Number of banks	119	163
Number of firms	1,454	289
Clustering	Bank	Bank

Note: This table explores how certain bank characteristics affect how banks adjust credit supply following the Paris Agreement. We conduct a horse race between all bank characteristics considered in Section 6. To this end, the baseline regression specification is augmented with interactions with the following bank characteristic and the inclusion of firm-time fixed effects. $NegBank_b$ takes on the value of bank b's exposure if $Bank\ Exposure_b$ is negative and takes a value of zero if bank b's exposure is zero or positive. $GSIB_b$ is an indicator for whether the bank is classified as GSIB in 2014. $UNEP_b$ is an indicator for banks' membership in the UNEP FI before the Paris Agreement. $Home_{b,c}$ is an indicator for banks lending within their region. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as firm-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA5: Alternative proxies for ex-ante likelihood to transition

	(1) USA	(2) USA	(3) USA	(4) USA
Positive × Post	-0.390**	-0.380*	-0.503*	-0.495*
	(0.177)	(0.211)	(0.266)	(0.273)
Negative × Post	0.354***	0.536***	0.485***	0.499***
	(0.105)	(0.155)	(0.181)	(0.176)
Post × High R&D	0.149			
	(0.179)			
Negative × Post × High R&D	-0.381***			
	(0.126)			
Post × High cap. exp.		0.038		
		(0.101)		
Negative × Post × High cap. exp.		-0.409*		
		(0.221)		
Post × High green pat. ratio			0.221	
			(0.176)	
Negative × Post × High green pat. ratio			-0.194	
			(0.265)	
Post × High green pat. n.				0.041
				(0.203)
Negative × Post × High green pat. n.				-0.097
				(0.240)
Observations	172,733	172,733	102,295	102,295
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.903	0.903	0.900	0.900
Number of banks	120	120	98	98
Number of firms	1,505	1,505	859	859
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement depending on alternative proxies of US firms' ex-ante likelihood to transition: R&D expenditures, capital expenditures, and green patenting activity. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. *High R&D*$_f$ and *High cap. exp.*$_f$ are indicators for average R&D inv. ratio or capital exp. ratio in the pre-shock period in the top quartile. *High green pat. ratio*$_f$ is an indicator for firms in the top quartile of the distribution by the ratio of green patents over all patents in the pre-shock period. *High green pat. n.*$_f$ is an indicator for firms in the top quartile of the distribution by N. of green patents in the pre-shock period. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA6: Continuous exposure measures

	(1) USA	(2) USA	(3) Europe	(4) Europe
$\overline{\text{CCExposure}}$ × Post	-1.604***		1.100*	
	(0.414)		(0.641)	
$\overline{\text{CCExposure}}$Positive × Post		-10.913		54.399***
		(8.062)		(14.948)
$\overline{\text{CCExposure}}$Negative × Post		1.497***		-0.534
		(0.380)		(0.578)
Observations	180,399	180,399	102,596	102,596
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.900	0.900	0.911	0.911
Number of banks	121	121	164	164
Number of firms	1,553	1,553	292	292
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. Instead of $Positive_f$ and $Negative_f$, we employ the continuous variable $\overline{CCExposure}_f$ in Columns (1) and (3) and the continuous $\overline{CCExposure}Positive_f$ and $\overline{CCExposure}Negative_f$ in Columns (2) and (4). $\overline{CCExposure}Positive_f$ takes on the value of $\overline{CCExposure}_f$ if it is positive and zero otherwise. $\overline{CCExposure}Negative_f$ takes on the absolute value of $\overline{CCExposure}_f$ if it is negative and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA7: Fixed effects cascades: USA

	(1) No	(2) Bank-firm	(3) Bank-time	(4) ILST
Positive	-0.096*			
	(0.050)			
Positive × Post	0.002	-0.112***	-0.124***	-0.251
	(0.044)	(0.025)	(0.026)	(0.185)
Negative	0.165***			
	(0.036)			
Negative × Post	0.065**	0.082***	0.059***	0.319***
	(0.031)	(0.021)	(0.018)	(0.081)
Post	0.367***	0.381***		
	(0.026)	(0.021)		
Observations	180,399	180,399	180,399	180,399
Bank-firm FE	No	Yes	Yes	Yes
Bank-time FE	No	No	Yes	Yes
ILST FE	No	No	No	Yes
Adjusted R^2	0.034	0.801	0.896	0.901
Number of banks	121	121	121	121
Number of firms	1,553	1,553	1,553	1,553
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3) for the sub-samples of US firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. bank-firm, bank-time, as well as industry-location-size-time fixed effects are introduced sequentially from Column (2) onward. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA8: Fixed effects cascades: Europe

	(1) No	(2) Bank-firm	(3) Bank-time	(4) ILST
Positive	0.532***			
	(0.052)			
Positive × Post	-0.102*	-0.146***	-0.161***	0.795***
	(0.054)	(0.047)	(0.050)	(0.216)
Negative	0.211***			
	(0.033)			
Negative × Post	0.086**	0.009	0.011	0.133
	(0.034)	(0.024)	(0.027)	(0.112)
Post	0.072**	-0.069***		
	(0.028)	(0.022)		
Observations	102,596	102,596	102,596	102,596
Bank-firm FE	No	Yes	Yes	Yes
Bank-time FE	No	No	Yes	Yes
ILST FE	No	No	No	Yes
Adjusted R^2	0.025	0.811	0.905	0.911
Number of banks	164	164	164	164
Number of firms	292	292	292	292
Clustering	Bank	Bank	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3) for the subsamples of European firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. bank-firm, bank-time, as well as industry-location-size-time fixed effects are introduced sequentially from Column (2) onward. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA9: Clustering: USA

	(1)	(2)	(3)	(4)
Positive × Post	-0.251	-0.251	-0.251	-0.251
	(0.163)	(0.224)	(0.194)	(0.169)
Negative × Post	0.319*	0.319***	0.319**	0.319***
	(0.169)	(0.094)	(0.150)	(0.089)
Observations	180,399	180,399	180,399	180,399
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.901	0.901	0.901	0.901
Number of banks	121	121	121	121
Number of firms	1,553	1,553	1,553	1,553
Clustering	Firm	Location	Bank-firm	Bank-time

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3) for the subsample of US firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered as indicated in the last line of the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA10: Clustering: Europe

	(1)	(2)	(3)	(4)
Positive × Post	0.795***	0.795**	0.795***	0.795***
	(0.257)	(0.321)	(0.246)	(0.276)
Negative × Post	0.133	0.133	0.133	0.133
	(0.214)	(0.294)	(0.214)	(0.116)
Observations	102,596	102,596	102,596	102,596
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
ILST FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.911	0.911	0.911	0.911
Number of banks	164	164	164	164
Number of firms	292	292	292	292
Clustering	Firm	Location	Bank-firm	Bank-time

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3) for the subsample of European firms. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered as indicated in the last line of the table. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA11: Alternative fixed effects: USA

	(1) SIC1	(2) State	(3) Year	(4) Bank controls
Positive × Post	-0.184***	-0.028	-0.069	-0.292
	(0.066)	(0.090)	(0.055)	(0.186)
Negative × Post	0.239***	0.191***	0.099***	0.350***
	(0.068)	(0.052)	(0.025)	(0.097)
Observations	180,399	180,388	180,399	147,924
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	No
ILST FE	Yes (SIC1)	Yes (State)	Yes (Year)	Yes
Adjusted R^2	0.894	0.892	0.899	0.899
Number of banks	121	121	121	74
Number of firms	1,553	1,552	1,553	1,480
Clustering	Bank	Bank	Bank	Bank

Note: This table explores alternative fixed effects structures to support the robustness of the baseline results for the United States. The regression is estimated as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. We relax the construction of the industry-location-size-time fixed effects using 1-digit instead of 2-digit SIC codes in Column (1), using location at the state level instead of at three-digit postal codes in Column (2), and using year instead of quarter in Column (3). In Column (4), we use lagged, time-varying bank controls (total assets, ROA, equity ratio, retained earnings, and short-term funding) instead of bank-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA12: Alternative fixed effects: Europe

	(1) SIC1	(2) Country	(3) Year	(4) Bank controls
Positive × Post	0.480**	0.464***	0.277***	0.450***
	(0.217)	(0.103)	(0.056)	(0.170)
Negative × Post	0.000	0.001	0.024	0.207
	(0.087)	(0.102)	(0.045)	(0.151)
Observations	102,596	102,596	102,596	56,505
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	No
ILST FE	Yes (SIC1)	Yes (Country)	Yes (Year)	Yes
Adjusted R^2	0.904	0.907	0.896	0.909
Number of banks	164	164	164	83
Number of firms	292	292	292	273
Clustering	Bank	Bank	Bank	Bank

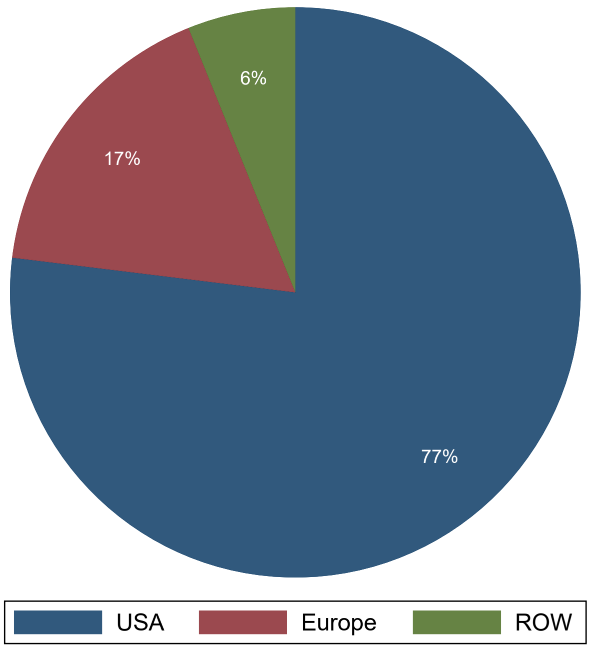
Note: This table explores alternative fixed effects structures to support the robustness of the baseline results for Europe. The regression is estimated as specified in Equation (3). The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. We relax the construction of the industry-location-size-time fixed effects using 1-digit instead of 2-digit SIC codes in Column (1), using location at the country level instead of at NUTS1 level in Column (2), and using year instead of quarter in Column (3). In Column (4), we use lagged, time-varying bank controls (total assets, ROA, equity ratio, retained earnings, and short-term funding) instead of bank-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table IA13: Loan controls

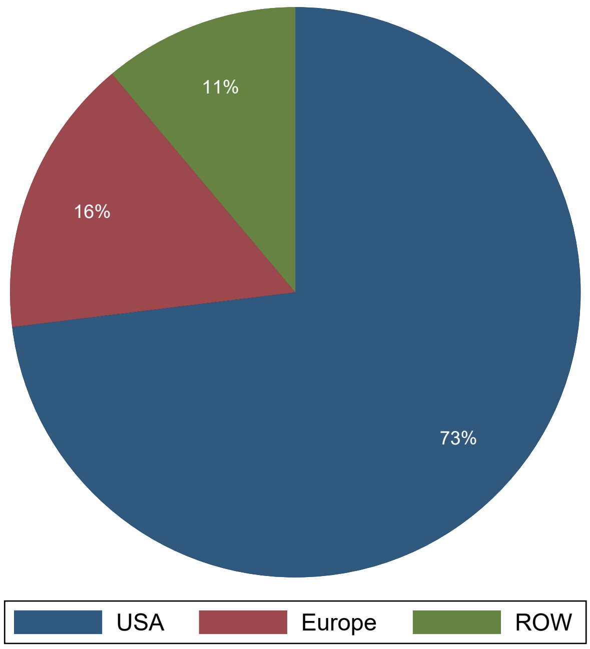
	(1) USA	(2) Europe
Positive × Post	-0.247	0.531***
	(0.176)	(0.133)
Negative × Post	0.310***	0.051
	(0.078)	(0.095)
Loan maturity	-0.011***	-0.007***
	(0.002)	(0.001)
Loan spread	0.000	0.001**
	(0.000)	(0.000)
Observations	177,030	101,135
Bank-firm FE	Yes	Yes
Bank-time FE	Yes	Yes
ILST FE	Yes	Yes
Adjusted R^2	0.903	0.916
Number of banks	118	164
Number of firms	1,536	288
Clustering	Bank	Bank

Note: This table explores how lending changes following the Paris Agreement, as specified in Equation (3). The baseline regression is expanded to include controls for loan characteristics such as the average spread and maturity at the bank-firm-quarter level. The dependent variable is the log of outstanding credit at bank-firm-quarter level. $Positive_f$ assumes a value of one if firm f has a positive exposure to regulatory risks and zero otherwise. $Negative_f$ assumes a value of one if firm f has a negative exposure to regulatory risks and zero otherwise. $Post_t$ indicates the time period after the adoption of the Paris Agreement. Each specification includes bank-firm, bank-time, as well as industry-location-size-time fixed effects. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure IA1: Distribution of exposed firms across regions



(a) Distribution of positively exposed firms



(b) Distribution of negatively exposed firms

Note: This figure shows the distribution of positively and negatively exposed firms across the three regions in our sample. Positively exposed firms are firms where $Positive_f$ takes on a value of one. Negatively exposed firms are firms where $Negative_f$ takes on a value of one. Firms' exposure is constructed as in Equation (1) and averaged over the pre-shock period at firm level.

Paper 2:

The Color of Corporate Loan Securitization

The Color of Corporate Loan Securitization*

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Abstract

We examine whether banks manage firms' climate transition risks via corporate loan securitization. Results show that banks are more likely to securitize loans granted to firms that become more carbon-intensive. The effect is more pronounced if banks have a lower willingness to adjust loan terms. Exploiting the election of Donald Trump as an exogenous shock that lowers transition risk, we show that banks respond by a lower securitization of loans given to firms that become more carbon-intensive. This is mainly driven by banks that have no or low preferences for sustainable lending and domestic lenders.

JEL classification: G21, G28, K11

Keywords: Climate change, securitization, syndicated lending, Trump election, risk shifting

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1 Introduction

How do banks manage firms' climate risks? A growing literature focuses on how banks incorporate firms' physical risks related to climate change such as exposures to natural disasters into their lending and risk-shifting decisions (Koetter et al., 2020; Murfin and Spiegel, 2020; Nguyen et al., 2022; Ouazad and Kahn, 2022; Schüwer et al., 2018). However, a fundamental element to mitigate climate risk is not only about adjusting lending to borrowers affected by climate change but also disciplining and incentivizing firms to reduce carbon emissions and make greener investments. This process is often referred to as the transition to a low-carbon economy. As main providers of credit, financial institutions, and especially banks, play a crucial role in this transition.

Unlike physical risk which is more quantifiable, transition risk is much harder for banks to manage as the implied costs depend on the highly uncertain pace of transition. Nevertheless, several studies ask whether banks account for firms' transition risks in their lending decisions and find that banks change credit supply accordingly and price the risks into loan contracts (Chava, 2014; Ivanov et al., 2021; Kacperczyk and Peydró, 2021; Mueller and Sfrappini, 2022). One question left unanswered, however, is whether and how banks manage transition risk via other channels, especially when they are limited in their ability or willingness to adjust credit conditions.

In this paper, we investigate whether banks manage firms' transition risks via corporate loan securitization. Since one can infer firms' transition risks from their decarbonization efforts (Bolton and Kacperczyk, 2022), we use changes in firms' carbon intensities to capture exposures to transition risk. Our main results are as follows: First, banks are more likely to sell loans that are given to firms that become more carbon-intensive over time. Second, banks specialized in lending to brown industries are even more likely to securitize these higher transition risk (HTR) loans as they have a lower willingness to adjust loan terms. Third, exploiting an exogenous shock that lowers transition risk, we show that banks cut back on securitizing HTR loans. This is, in particular, driven by banks that demonstrate no or low preferences for sustainable lending and domestic lenders.

Understanding how banks actively manage firms' transition risks via securitization is important for three main reasons. First, as some lenders may not be able to account for transition risk through adjustments in credit conditions, it is crucial to understand alternative ways of how banks manage this risk. While this is also possible via e.g. credit derivatives, the massive size of securitization markets makes it a first-order focus of investigation. Second, while loan amount, interest rate, and maturity are equilibrium outcomes from the negotiation process between firms and banks, the decision to securitize loans can be unilaterally taken by the bank (Ivanov et al., 2021). Hence, studying securitization allows obtaining additional evidence on banks' expectations about the effect of a green transition on firms. Third, regulators concerned about transition risk and its potential impact on financial stability may be interested in which financial market participants actually carry this risk. Since several central banks around the world started to examine climate-related capital requirements such as the European Central Bank and the Bank of England, it may be important for policymakers to know whether and how banks shift risk off their balance sheets to avoid any underestimation in the real level of transition risk.

We start with an analysis that relates banks' securitization intentions and changes in firms' emission intensities augmented by a rich fixed effects structure. The analysis rests on loan-level data from Thomson Reuters LPC's DealScan for US firms between 2013 and 2019 and carbon emissions data from Refinitiv. We construct whether lead arrangers are likely to securitize loans by observing whether participants in the syndicate at the time of origination include a manager of a Collateralized Loan Obligation (CLO) (Benmelech et al., 2012; Blickle et al., 2020; Bord and Santos, 2015). While this approach does not directly capture whether a loan is actually securitized, it allows for identifying loans that are more likely to be securitized. We categorize loans granted to firms with increasing carbon intensity over time as HTR loans and loans granted to firms that experience no change in their emission intensities or even decrease them as lower transition risk (LTR) loans. Changes in firms' carbon intensities capture the rate of firms' decarbonization efforts and their shift away from or into higher future emissions.[1]

We find evidence of a positive relation between banks' securitization intentions and firms' transition risks. Banks are 5 percentage points more likely to sell loans that carry HTR to third parties compared to LTR loans. Given that we identify 66% of the 3,673 loans in our sample as intended for securitization, the association between loan sales and transition risk is economically meaningful. To better understand how banks choose between adjusting credit conditions and securitization, we consider banks' business models, as reflected in sectoral specialization and market share. If a bank is

[1] While firms' level of emissions is a proxy for their long-run exposure to transition risks, firms that keep on emitting more may be considered even riskier due to the growing discrepancy to reach a net-zero target (Bolton and Kacperczyk, 2022).

specialized in lending to brown industries, we document that HTR loans have an even higher likelihood to be securitized but are not associated with higher spreads. This can be rationalized by specialized banks having an interest in shielding their borrowers to protect information advantages acquired (De Jonghe et al., 2020) and, thus, having a lower willingness to adjust loan terms. In contrast, if a bank has a high market share in lending to brown industries, we show that HTR loans have a lower likelihood to be securitized but higher spreads. This is in line with banks having higher bargaining powers to price risk and extract rents if they exhibit higher market shares.

In the next step, to establish causality, we exploit an exogenous shock that lowers the risk that new environmental policies are introduced and even increases the possibility of existing policies being rolled back, i.e. the election of Donald Trump on November 8, 2016. We rely on several aspects related to Trump's election that allow for causal identification of how firms' transition risks and banks' securitization intentions relate using a difference-in-differences (DiD) approach. First, the outcome of the 2016 US election was unexpected and Trump won only by a small margin. Second, Trump's election cannot be considered as the continuation of the existing trend of tightening environmental policy but rather as a shift toward a rollback of existing policies. We argue that Trump's election triggered a reduction in transition risk. Therefore, if banks view securitization as a tool to manage their exposure to firms' transition risks, a decline in these risks would lead banks to securitize fewer HTR assets.

Our results confirm this hypothesis and show that banks adapt to lower transition risk. After Trump's election, banks are 10 percentage points more likely to retain HTR loans on their balance sheets compared to a control group of LTR loans. This effect is more pronounced when banks impose covenants in the loan contract. With covenants serving as an important monitoring tool, we interpret this result as banks taking into account that the performance of HTR loans may change if Trump is not re-elected and the new government favors stricter environmental regulations. Shedding more light on which banks have higher intentions to retain HTR loans after Trump's election, we find that it is, in particular, banks that have no or low expressed preferences for sustainable lending as well as domestic lenders, which are likely to respond more strongly to the election outcome.

Our paper relates to several strands of literature. First, we speak to the debate on how banks account for firms' climate transition risk in their lending decisions. Recent evidence suggests that banks incorporate transition risk into their loan pricing: Firms with higher carbon emissions or fossil fuel reserves pay higher interest rates while firms that disclose environmental information receive more favorable terms (Chava, 2014; Degryse et al., 2021; Delis et al., 2021). Furthermore, banks seem to account for transition risk in their loan volumes (Kacperczyk and Peydró, 2021; Mueller and Sfrappini, 2022; Reghezza et al., 2022). Mueller and Sfrappini (2022) show that banks lend more to firms that are likely to benefit from the introduction of environmental regulations while Kacperczyk and Peydró (2021) find that firms with high carbon emissions receive less funding after banks committed to lending sustainable. Ivanov et al. (2021) show that carbon pricing policies lead high-emission firms to face not only higher interest rates but also shorter loan maturities and lower access to permanent forms of bank financing. In contrast to these papers, we are the first to document how banks account for transition risk via securitization.

Second, we add to the relatively sparse literature on how banks deal with climate risk via securitization. Ouazad and Kahn (2022) illustrate that lenders are more likely to approve mortgages that can be securitized in areas that recently suffered from natural disasters. Similarly, Nguyen et al. (2022) outline that lenders are more likely to not price sea-level rise risk when mortgages under consideration can be securitized. Moreover, the findings by Keenan and Bradt (2020) show that, in particular, locally concentrated lenders reduce their exposures to physical risk by selling high-risk loans in secondary markets. As this strand of literature so far considers physical climate risk and the securitization of mortgages, our contribution is focusing on the implications of transition risk for the securitization of corporate loans.

Third, we contribute to the literature on the determinants and consequences of banks' securitization activities. Early literature theoretically predicts when banks sell loans on secondary markets (Gorton and Pennacchi, 1995; Pennacchi, 1988). Empirical works show that banks sell off high credit risk loans when they have to hold costly equity capital against their credit exposures (Parlour and Winton, 2013) and when loan purchasers do not price the risk correctly (McGowan and Nguyen, 2022). We depart from this literature by focusing on how banks choose between securitization and adjusting loan terms when they face climate transition risks.

2 Institutional background and derivation of hypotheses

2.1 Banks' management of transition risk

We aim to investigate whether banks manage transition risk via securitization. Theories offer some predictions for our prior. Under the traditional "originate-to-hold" banking business model, agency theory perspective suggests that lenders may limit their exposure through their lending activities when there is a misalignment between the risk management goals of lenders and profit maximization goals of borrowers (Armstrong et al., 2010; Shleifer and Vishny, 1997). In the case of transition risk, lenders may have different climate-related goals than their borrowers and may expect them to undertake actions to reduce pollutants and comply with environmental policies. In contrast, borrowers may focus on their financial performance and make business decisions such as investing in high-profit but pollution-intensive projects (Jung et al., 2018). These projects can be risky because they involve the externalization of pollutants Agency theory predicts that if these pollution-intensive projects are successful, shareholders will benefit whereas creditors bear most of the cost if these projects fail. To avoid this issue, lenders can reduce their exposure to firms' transition risks by adjusting loan contracts accordingly. Empirical evidence consistently documents that transition risk is, at least to some degree, accounted for in banks' pricing and quantity decisions (Chava, 2014; Kacperczyk and Peydró, 2021; Mueller and Sfrappini, 2022; Ivanov et al., 2021).

However, if banks operate an "originate-to-distribute" business model, instead of imposing stricter loan contracts or reducing credit supply, they can use securitization as an alternative way to mitigate their exposures to transition risk. Theoretically, a bank decides to securitize a loan when the fee generated from the sale of that loan is greater than the interest income that the loan brings minus the expected loss if the borrower defaults. Thus, if the fee income from securitization and the interest income does not change, any changes in the riskiness of borrowers should provoke banks to adjust their securitization decisions.

Contemporary theories on loan sales offer some insights into banks' securitization behaviors. Parlour and Winton (2013) suggest that capital requirements, which force lenders to hold costly equity capital against their credit exposures, lead to a benign motive that banks sell off high credit risk loans. Gorton and Pennacchi (1995) model a bank's choice between selling and holding loans and show that moral hazard associated with loan securitization could be reduced if banks hold a certain fraction of a loan. Empirically, McGowan and Nguyen (2022) show that banks transfer credit risk through securitization when loan purchasers do not price the risk correctly. These predictions on the determinants of loan sales have implications beyond banks' monitoring of credit risk. For instance, in the case of transition risk, banks could be more active in the securitization market to reduce their exposure to this risk. We formulate the first hypothesis as follows:

H1: Banks are more likely to securitize HTR loans compared to LTR loans.

A natural question that arises is why banks do not mitigate their transition risk exposure only by changing credit supply or interest rates. The key answer lies in banks' business models, as reflected in their sectoral specialization and market share. While the former measures how important a certain industry is for a bank, the latter proxies how important a bank is for a certain industry (De Jonghe et al., 2020). Lending specialization allows banks to build up expertise and superior knowledge to attract new customers, select better projects, and issue loans of higher quality (Blickle et al., 2021; Giometti and Pietrosanti, 2019). In our setting, banks that are specialized in lending to industries with poor environmental performance may be more reluctant to reduce their exposure to transition risk through the lending channel to protect the information advantage acquired. These banks, therefore, may consider alternative ways to shift risk, for instance, through securitizing loans.

In contrast, when having a higher market share in such industries, banks may use the resulting market power to extract higher rents from their customers (De Jonghe et al., 2020; Sharpe, 1990). In our setting, this may imply that banks that have a larger market share in lending to industries with poor environmental performance have a better bargaining position to incorporate transition risk into lending rates but not securitize HTR loans as much as other banks. These considerations lead us to formulate the following hypotheses:

H2a: Banks are <u>more</u> likely to securitize but <u>less</u> likely to raise lending rates of HTR loans when they are <u>more specialized</u> in lending to brown industries.

H2b: Banks are <u>less</u> likely to securitize but <u>more</u> likely to raise lending rates of HTR loans when they have <u>higher market shares</u> in lending to brown industries.

2.2 Banks' management of transition risk and Trump's election

As it is challenging to identify a causal relationship between banks' securitization intentions and firms' transition risks, we exploit the election of Donald Trump in 2016 to overcome this challenge. Similar to Ilhan et al. (2021) and Ramelli et al. (2021), we consider this an exogenous event that lowered transition risk. Throughout the 2016 electoral campaign, Donald Trump expressed opposing views on climate policy to his opponent by, for instance, proposing to dismantle the Clean Power Plan and leave the

Paris Agreement. Hilary Clinton, in turn, was expected to continue to tighten environmental regulations. With most polls predicting a victory for Clinton, Trump winning the election was a big surprise. Already a month after the election, Scott Pruitt, a climate skeptic, was appointed as the head of the Environmental Protection Agency. Some further notable examples of how Trump's election affected climate policies are, among others, the withdrawal of the United States from the Paris Agreement in 2017 and the replacement of the Clean Power Plan with the Affordable Clean Energy Rule in 2019.

Using data from RepRisk, a data provider specializing in collecting ESG-related news for both private and public firms, we corroborate the findings by Faccini et al. (2021) and show a decline in firms' transition risks after Q4 2016.[2] We calculate the monthly fraction of environmental news that mentions environmental-related risks for firms scaled by the total of all news of firms that RepRisk collected between 2013 and 2020. This environmental-related news usually reveals how much firms are affected by climate change and climate-related regulations. Figure 1 confirms Faccini et al. (2021) and our conjecture that transition risk is lower after Trump's election. The fraction of bad environmental news declined by almost 5 percentage points (or 6% compared to the mean of the whole sample).

[Figure 1]

Since our discussion of existing theories and empirical evidence show that banks may securitize loans to transfer transition risk to loan purchasers (Gorton and Pennacchi, 1995; McGowan and Nguyen, 2022; Parlour and Winton, 2013), one would expect that any shocks that change the level of transition risk would have an impact of loan securitization. As Trump's election has reversed market participants' expectations about future US environmental policies, we can formulate our third hypothesis as follows:

> *H3: HTR loans have a lower likelihood to be securitized after Trump's election compared to LTR loans.*

[2] Since RepRisk screens daily over 80,000 media, stakeholder, and third-party sources, including print and online media, NGOs, government bodies, regulators, think tanks, newsletters, social media (e.g., Twitter), and blogs, for news related to firms' ESG practices, it is plausible to treat the data as unbiased and representative.

3 Data and descriptive statistics

3.1 Data sources

Loan-level information Our main source of information is Thomson Reuters LPC's DealScan, which covers syndicated loans. It provides detailed loan-level information such as lender and borrower identities, date of origination, maturity, spread, and loan volume. Data is aggregated at the ultimate parent level for both lenders and borrowers. We retrieve all facilities between 2013 and 2019 issued to US firms and exclude firms in the financial sector (SIC codes between 6000 and 6999) from the sample.

Facility volumes are converted to millions of US dollars if applicable utilizing the spot exchange rate that DealScan provides at loan origination. Following De Haas and Van Horen (2013), we allocate loan shares according to the breakdown provided by DealScan or, if this information is missing, we distribute the loan amount equally among all syndicate members.

As syndicated loans are predominantly granted by a syndicate of lenders, we follow Ivashina (2009) to identify lead arranger(s) and proceed with excluding participants from the sample.[3] Unlike other participants in the syndicate, lead arrangers play an active role in setting up and negotiating loans, and thus, are more informed about firms' environmental performance.

The presence of CLOs in the syndicate Mortgage securitization has been well known for decades but corporate loan securitization has become common only since the early 2000s, following the development of secondary markets for collateralized loan obligations (CLOs). Following Benmelech et al. (2012) and Wang and Xia (2014), we identify securitization-inclined loans as loans with at least one CLO among the syndicate participants at the time of loan origination. The CLO or collateral manager, often an investment management company, usually structures a CLO by acquiring tranches of syndicated loans, managing the structure and rating of the deal, and then issuing securities to investors. Through the interaction in the syndication process and other related services like underwriting, lead banks, compared to other participants, have better information and access to secondary markets. As a result, they can easily sell parts of their shares to the CLO in the syndicate (Benmelech et al., 2012; Blickle et al.,

[3] Ivashina (2009) defines the administrative agent to be the lead bank if available. If not, lenders that act as agent, arranger, book runner, lead arranger, lead bank, or lead manager are defined to be lead arranger(s).

2020; Bord and Santos, 2015; Bozanic et al., 2018; Drucker and Puri, 2009; Guo and Zhang, 2020; Paligorova and Santos, 2016).

We identify loans that include CLOs via a two-step procedure. First, we check whether the lenders' type is "Inst. Invest. CDO" and whether participants' names include "CLO", "CDO", or "obligation" (Beyhaghi et al., 2019). Second, we match participant names to a list of CLO managers active in global CLO deals that we manually from Fitch Ratings and various other sources. Relying only on information at loan origination might under-report CLO presence and might lead to a misclassification of loans. However, recent evidence shows that if lead arrangers sell off their shares, they do so shortly after origination (Blickle et al., 2020; Lee et al., 2019).

While our securitization measure does not directly capture whether a loan is actually securitized but rather the intention of lead arrangers to securitize, it suits the purpose of our analyses well. First of all, as Parlour and Plantin (2008) predict, banks' access to the securitization market may be sufficient to change their monitoring incentives, even without actual loan securitization. Second, although we are not able to observe how much of its share the lead bank actually sells off, contrary to previous beliefs that the lead arranger retains a significant stake in the loan, Bord and Santos (2015) illustrate that in nearly 50% of the loans in which a CLO is present among the syndicate participants, lead banks sell off their share completely. Moreover, Blickle et al. (2020) show that lead arrangers sell off their entire share in at least 12% of all loans.

Firms' environmental profiles We define HTR loans as loans given to firms with a worsening environmental profile. To evaluate firms' environmental profiles, we retrieve information on US firms' emissions between 2013 and 2019 from Refinitiv. This initially encompasses 3,408 firms. To account for size differences across firms, we calculate firms' emission intensities which equal total carbon emissions divided by total assets. Later on, in robustness checks, we also use firms' emissions, Refinitiv's ESG scores and Sautner et al. (2022)'s climate regulatory risk measurements to evaluate different aspects of firms' transition risks.

To test the first and second hypotheses, we calculate yearly changes in emission intensity for each firm and classify HTR loans as loans given to firms with positive changes in emission intensity. For our third hypothesis, to make sure that we capture the causal effect of Trump's election on securitization, we define HTR loans using the pre-shock information only. Specifically, we identify HTR loans as loans given to firms that increase their emission intensity between 2013 and 2015. Thus, we only include firms in our analysis if they have information on emissions in 2013 and 2015. This results in a sample of 765 firms. The drop in the number of firms is a consequence of it not being mandatory to report carbon emissions in 2013 or 2015. Hence, our sample likely contains firms with relatively low levels of emissions as they are probably more willing to disclose this information. This is another reason to focus on the change in emissions instead of the level.

As there is no common identifier between DealScan and Refinitiv, we hand-match the data via name and ticker. We can match 529 firms to DealScan, which corresponds to 70% of firms for which emission data is available for 2013 and 2015.

Other firm and bank characteristics We retrieve quarterly data on firm characteristics from Worldscope. We require firms to have non-negative, non-zero total assets. Furthermore, to control for mergers and acquisitions, we exclude observations where asset growth is larger than 100% (Almeida et al., 2004). All variables are winsorized at the 1st and 99th percentiles. As Refinitiv and Worldscope encompass firms' ISINs, we can combine information by merging via ISIN and date. Out of the 529 firms that result from the overlap of Refinitiv and DealScan, we can retrieve firm characteristics for 380 firms.

To zoom into heterogeneous effects across bank characteristics at a later stage of the analysis, we further collect quarterly bank-level information between 2013 and 2019 from Compustat. As there is no common identifier between DealScan and Compustat, we first obtain GVKEYs from Schwert (2018). We then check the names of the unmatched banks and manually identify ISIN codes. By doing so, we can obtain characteristics for all of the 81 banks in our final sample.

3.2 Descriptive statistics

Table 1 shows variable definitions and Table 2 reports summary statistics of our main variables. The baseline sample consists of 3,673 loan observations, 66% of our loans have at least one CLO in the syndicate. 40% of loans are granted to firms that increased their emission intensity between 2013 and 2015.

[Table 1 and Table 2]

Figure 2 illustrates the distribution of firms that exhibit an increase in their emission intensity over the pre-shock period across industries. Several important facts can be noted: First, there are firms to which this implies in each industry. Second, there is variation across industries. Construction and manufacturing as well as wholesale and retail trade have the largest share of firms with increasing emission intensity (43%). Transportation has the lowest share with 26%. Overall, 41% of the 380 firms in our sample exhibit an increase in their emission intensity over the pre-shock period.

[Figure 2]

To get a first understanding of how banks' securitization intentions and firms' transition risks relate, we calculate the share of HTR and LTR loans that are likely to be securitized. 69% of HTR loans are intended for securitization while 63% of LTR loans are likely to be securitized. This provides a first albeit descriptive hint that our first hypothesis may hold.

4 Empirical strategy

4.1 Research design for the link between securitization and transition risk

To test our first hypothesis, we start with a simple econometric model that focuses on the relationship between banks' securitization intentions and changes in firms' environmental performance. We estimate the following linear probability model (LPM)[4]:

$$\begin{aligned}\text{Securitization-Inclined}_{l,b,f,t} = {} & \beta_1 \text{HTR Loan (Yrly)}_{f,t} \\ & + \gamma_1 \mathrm{L}_{l,b,f,t} + \gamma_2 \mathrm{F}_{f,t-1} \\ & + \zeta_{b,f} + \zeta_{b,t} + \zeta_l + \eta_l + \varepsilon_{l,b,f,t}.\end{aligned} \tag{1}$$

Securitization-Inclined$_{l,b,f,t}$ takes on a value of one if loan l to firm f in quarter t is granted by bank b that is part of a syndicate which includes a CLO as a participant, and 0 otherwise. *HTR Loan (Yrly)*$_{f,t}$ takes on a value of one if firm f emitted more carbon emissions relative to its total assets in t than in $t-1$. $L_{l,b,f,t}$ is a vector of loan controls that includes the log of the loan volume, maturity, and spread. $F_{f,t-1}$ is a vector of firm controls encompassing size, return on assets, equity ratio, and capital expenditures to total assets. These variables are included as their first lags.

[4] We use a LPM estimated via OLS over a probit model due to its simplicity in interpretation and model specification given the various multi-way fixed effects. We make sure to address two potential shortcomings of LPMs by using standard errors robust to heteroskedasticity and verifying that predicted values are within the unit interval (Wooldridge, 2010). Out of the 3,673 fitted probabilities, 99.9% are within the unit interval.

We include bank-firm fixed effects ($\zeta_{b,f}$) to absorb time-invariant bank and firm characteristics as well bank-firm-specific factors. For example, if banks always securitize loans given to certain firms because they believe that these firms are always riskier than others rather than due to changes in firms' environmental performance, the bank-firm fixed effects will capture this. Bank-time fixed effects ($\zeta_{b,t}$) are introduced to control for shocks at the bank level that could lead to overall changes in banks' securitization intentions. Last, we also integrate loan type (ζ_l) as well as loan purpose fixed effects (η_l) to ensure that our results do not reflect differences in loan contracts. $\varepsilon_{l,b,f,t}$ is the idiosyncratic error term. The main coefficient of interest is β_1 which identifies whether banks are more likely to securitize loans given to firms whose emission intensities increase.

The use of bank-time fixed effects partially solves the issue of omitted variables that could affect banks' credit supply. However, our estimated β_1 could still capture other firm or loan characteristics that make these loans more marketable in the secondary market. We take one step further and complement our OLS analysis by using a propensity score matching strategy described by Heckman et al. (1998) where we use the observable loan and firm characteristics used in Equation (1) to estimate the predicted probability of being a higher transition risk loan using a probit model. Doing so ensures that our average treatment effect reflects purely how banks intend to securitize loans in response to changes in firms' emission intensities. Other papers that use this approach include Drucker and Puri (2009) and Bharath et al. (2011).

Next, we test our second hypothesis by estimating the following LPM:

$$\begin{aligned}\text{Securitization-Inclined}_{l,b,f,j,t} = \; & \beta_1 \text{HTR Loan (Yrly)}_{f,t} \\ & + \beta_2 \text{HTR Loan (Yrly)}_{f,t} \times \text{Brown Specialization}_{b,j,t} \\ & + \beta_3 \text{Brown Specialization}_{b,j,t} \\ & + \beta_4 \text{HTR Loan (Yrly)}_{f,t} \times \text{Brown Market Share}_{b,j,t} \\ & + \beta_5 \text{Brown Market Share}_{b,j,t} \\ & + \gamma_1 \text{L}_{l,b,f,t} + \gamma_2 \text{F}_{f,t-1} \\ & + \zeta_i + \zeta_c + \zeta_{b,f} + \zeta_{b,t} + \varepsilon_{l,b,f,j,t}.\end{aligned} \tag{2}$$

All variables are the same as in Equation (1) except *Brown Specialization*$_{b,j,t}$ and *Brown Market Share*$_{b,j,t}$. We measure the level of brown specialization for each bank as the share of loan volume that bank b grants to brown industry j that firm f is a part of relative to bank b's total loan volume, where a brown industry is one with an

ESG score lower than the top 75th percentile of ESG scores of all industries (Gantchev et al., 2022).[5] *Brown Market Share*$_{b,j,t}$ is the share of bank b's credit granted to brown industry j relative to total loans granted by all banks to this industry.

Our main coefficient of interest is now β_2 and β_4. β_2 indicates whether banks are more likely to securitize HTR risk loans when they are more specialized in lending to brown industries. β_4 shows how banks securitize HTR loans when they have more bargaining power in lending relationships with brown borrowers.

To illustrate the choice between transition risk pricing and transition risk shifting, we replicate Equation (2) using *Loan Spread*$_{l,b,f,j,t}$ as a dependent variable. When banks depend on lending to brown industries, they may reward firms that pollute more over time by reducing loan spreads for these borrowers. At the same time, they may shift transition risk off balance sheets using securitization. In contrast, when banks hold large market shares in brown industries and can easily adjust loan spreads, one may expect to see the pricing of transition risk in lending contracts.

4.2 Research design for the effect of Trump's election on the link between securitization and transition risk

4.2.1 Difference-in-differences specification

In the next step, we turn to a difference-in-differences (DiD) estimation to establish a causal relationship between banks' securitization intentions and changes in firms' environmental performance. Using the election of Donald Trump as the president of the United States in Q4 2016 as an exogenous shock to expectations about environmental policy, we estimate:

$$\begin{aligned}\text{Securitization-Inclined}_{l,b,f,t} = {} & \theta \text{HTR Loan}_f \times \text{Trump}_t \\ & + \iota_1 \text{L}_{l,b,f,t} + \iota_2 \text{F}_{f,t-1} \\ & + \kappa_{b,f} + \kappa_{b,t} + \kappa_l + \lambda_l + \epsilon_{l,b,f,t}.\end{aligned} \tag{3}$$

All variables are the same as in Equation (2), except for *HTR Loan*$_f$ and *Trump*$_t$. The former takes on a value of one if firms' emission intensities increased over the pre-shock period, that is between 2013 and 2015. This definition reduces endogeneity concerns because we will not capture changes in banks' securitization intentions due to changes in firms' behaviors that may result from lower transition risk due to Trump's election.

[5] Our result also holds if we define brown specialization using the median split in ESG scores of industries.

The latter takes on a value of one from Q4 2016 onward which corresponds to the quarter Trump was elected and zero otherwise. Since the fixed effects structure is the same as in Equation (2), we do not observe the coefficient on $HTR\ Loan_f$ because it is absorbed by bank-firm fixed effects. The coefficient on $Trump_t$ is absorbed by bank-time fixed effects. $\epsilon_{l,b,f,t}$ is the idiosyncratic error term. Our main coefficient of interest is θ which identifies whether changes in the probability to securitize HTR loans after the election compared to LTR loans.

4.2.2 Parallel trends

Even though previous papers rely on the election of Trump as an exogenous shock (Ilhan et al., 2021; Ramelli et al., 2021), we still need to test whether the parallel trend assumption holds for our DiD approach to be valid. Specifically, for loans to firms with decreasing or unchanged carbon intensity to serve as a valid counterfactual in our setup, there must be no divergence in the development of treatment and control firms in the absence of treatment. To address this issue formally, we implement an approach by Imbens and Wooldridge (2009) and test if loans issued to the treatment and control groups were comparable prior to Trump's election and if firms' characteristics followed a similar trend. In addition, to underline that our results from estimating Equation (3) are not driven by the fact that treatment and control firms are connected to banks that develop differently, we compare the evolution of banks that lend to the treatment and control groups before the election.

Table 3 shows normalized differences by treatment status. As suggested by Imbens and Wooldridge (2009), an absolute normalized difference smaller than 0.25 indicates that there is no significant difference between treatment and control groups. The securitization probability of loans given to treated firms is higher than the one of loans granted to firms in the control group. However, the difference in the securitization probability between these two groups is sufficiently low as the normalized difference is 0.14, much smaller than the 0.25 rule of thumb. Furthermore, loan volumes, interest rates, as well maturities are sufficiently equally between the treatment and control groups prior to Trump's election.

[Table 3]

Similarly, we cannot find evidence that there is a significant difference in the development of firms in the treatment and control groups when considering the annual percentage change in their return on assets, equity ratio, or ratio of capital expenditures to total assets. However, firms with increasing emission intensity grow slightly more than the counterfactual. Considering bank characteristics, we do not find any statistically significant difference in how banks connected to the treatment and control groups develop when considering their size, capital ratio, return on assets, and the share of deposits to total assets.

Figure 3 confirms the picture that emerged from considering normalized differences by displaying quarterly treatment coefficients. We interact *HTR Loan* with a set of quarter dummies using Q3 2016 as the reference. We find that quarterly treatment effects are not significant before Trump's election. However, we find a negative effect of HTR loans on securitization after the election. Hence, this exercise does not indicate that parallel trends are absent.

[Figure 3]

Last, we utilize placebo tests to establish that treatment effects are not observable in the absence of our shock. Figure 4 plots estimates for *HTR Loan* $\times$ *Trump* and 95% confidence intervals for regressions in which we define eight placebo events between Q1 2014 and Q4 2015. We find insignificant effects in each placebo regression.

[Figure 4]

5 Baseline results

5.1 Banks' securitization intentions and firms' transition risks

Table 4 reports the results from estimating Equation (1) and thus for our first hypothesis. Standard errors are clustered at the bank level. In Column (1), we perform the estimation only with loan controls but without any fixed effects. We include bank-firm fixed effects in Column (2) and bank-time fixed effects in Column (3). We add firm characteristics in Column (4). Column (5) is saturated with bank-firm, bank-time, loan type, and loan purpose fixed effects and exhibits our preferred specification. Overall, the coefficient on *HTR Loan (Yrly)* is positive and ranged between 0.04 and 0.06. It turns significant as soon as bank-firm fixed effects are introduced in Column (2). This implies that HTR loans are 4 to 6 percentage points more likely to be securitized compared to LTR loans, holding all else constant. We also consider the economic magnitude to be meaningful. Compared to the mean of *Securitization-Inclined* for LTR loans, which is 0.62, the effect is equivalent to an increase of 8% (5/62*100 = 8%). Thus, this finding is in line with our first hypothesis and indicates that secondary loan sales play an important role in banks' transition risk management.

[Table 4]

Column (6) reports the results from the propensity score matching approach applied to ensure we indeed capture the impact of changes in firms' environmental performance and not the influence of other loan or firm characteristics that make these loans more marketable in the secondary loan market. We use firm characteristics such as firm size, return on assets, equity ratio, capital expenditure ratio, and loan characteristics such as loan amount, spread, and maturity to compute the propensity score of HTR loans (treated loan) (Table A1). Next, we match each treated loan to a LTR loan granted which has a similar propensity score using the one-to-one non-replacement approach.

We find that the coefficient on *HTR Loan (Yrly)* is still positive, significant, and of similar magnitude compared to previous results. The result strongly suggests that financially similar loans from financially similar firms can get securitized at a different intensity depending on firms' transition risks.[6]

5.2 The choice between risk pricing and risk shifting

This section presents an analysis that helps us to better understand the mechanism behind banks' management of transition risk using securitization. As banks can also adjust exposure to firms' transition risks through changes in credit conditions (Ivanov et al., 2021; Mueller and Sfrappini, 2022; Kacperczyk and Peydró, 2021; Roncoroni et al., 2021), an important question that arises is how do banks choose between pricing and shifting transition risk through the secondary market for loan sales.

We exploit two cases where banks may choose risk shifting over risk pricing or vice versa: Banks' specialization in brown industries and banks' market shares in brown industries. To test Hypotheses 2a and 2b, we estimate Equation (2). Table 5 displays the results. Column (1) illustrates that β_2 is positive and significant. It shows that it is banks that are specialized in brown industries that drive the securitization of LTR loans. In particular, when brown specialization increases by one standard deviation (26.25), banks are 8 percentage points (26.25*0.003*100) more likely to securitize HTR

[6] Table A2 in the Appendix shows that our results hold if we use yearly changes in emission intensity based on Scope 1 or Scope 2 emissions to capture firms' transition risks.

loans. Using the mean of *Securitization-Inclined* among LTR loans, the effect is equivalent to 13%. Column (2) supports our hypothesis that when specialized banks use securitization to shift transition risk off their balance sheets, they may not price risk accordingly in loan spreads. We even find that specialized banks offer lower interest rates for HTR loans compared to LTR loans. One standard deviation increase in brown specialization is associated with a 7 basis points (26.25*(-0.271)) decline in loan spreads of HTR loans compared to LTR loans.

[Table 5]

In the case of banks having high brown market shares, the findings are the opposite. β_4 in Column (1) is negative and statistically significant. One standard deviation increase in brown market share (2.44) relates to a 4 percentage points (or 6%) decline in the probability of a HTR loan being securitized. Instead, it leads to a 19 basis points increase in loan spreads of a HTR loan compared to a LTR loan (Column (2)). This result supports Hypothesis 2b and illustrates that banks choose to price transition risk into loan contracts instead of shifting the risk to third parties when they have more bargaining power over their borrowers.

6 The effects of Trump's election

6.1 Banks' securitization intentions, firms' transition risks, and Trump's election

Our evidence so far indicates that banks manage their exposure to transition risk using securitization. We now turn to banks' securitization intentions when there is an exogenous shock to transition risk. The election of Donald Trump as president of the United States in November 2016 drastically reduced expectations of tightening environmental policies. We expect that this event would lead banks to securitize fewer HTR assets, consistent with the findings by (Ramelli et al., 2021) that show that carbon-intensive firms benefit in terms of their stock market performance after the election.

We estimate Equation (3) and report results in Table 6. Column (1) estimates a reduced form with only loan controls but no fixed effects. The coefficient for *HTR Loan* is positive and statistically significant. This implies that loans granted to firms with increasing emission intensity have a higher likelihood to be securitized before the 2016 election compared to the control group. This mirrors the results from our analysis in Section 5.1. *HTR Loan* $\times$ *Trump*, in turn, is negative and statistically significant. This illustrates that the probability of securitization is lower for HTR loans after the 2016 election compared to LTR loans. We now introduce the outlined fixed effects structure sequentially from Column (2) onward and observe that the sign or the magnitude of θ remains similar across columns. In our preferred specification in Column (5), the propensity to securitize loans granted to firms with increasing emission intensity is 10 percentage points lower.

[Table 6]

Our results imply that banks adapt how they manage transition risk. Once transition risk is lower, they are more likely to retain loans from firms with an increasingly poorer environmental performance on their balance sheet and more likely to securitize loans from firms that improve their environmental performance after the election. Putting it differently, Trump's election may lead to banks' balance sheets carrying higher transition risk than before. However, if banks expect that the Trump administration may not be elected in the subsequent term, they may worry that these HTR loans perform worse when the next government favors promoting new climate policies. Thus, they may have an incentive to monitor these HTR loans when they hold them on their balance sheets. We, therefore, examine the role of banks' monitoring efforts in the next step.

6.2 Monitoring efforts

Following Bharath et al. (2011) and Wang and Xia (2014), we use loan covenants to measure banks' monitoring efforts. Rajan and Winton (1995) argue that loan covenants increase banks' incentives to monitor because they allow banks to renegotiate when firms' risks and performance change. Specifically, when borrowers' financial conditions deteriorate, covenant violations allocate control rights to lenders so that they can collect additional information and deter borrowers from excessive risk-taking. We test for the monitoring effort channel by examining whether the propensity to securitize HTR loans after Trump's election changes depending on covenant design.

To investigate this channel, we additionally interact *HTR Loan* $\times$ *Trump* with a binary indicator, *Covenant* that identifies whether a least one covenant is included in the loan contract. Column (1) in Table 7 shows the results of this exercise for the full sample while we differentiate between financial and net worth covenants in Columns (2) and (3) respectively. The coefficient on *HTR Loan* $\times$ *Trump* $\times$ *Covenant* is negative and significant in all 3 columns. Hence, banks are even less likely to securitize HTR loans compared to the control group after Trump's election if there is at least one covenant in the loan contract. This suggests that while HTR loans are seen as carrying lower transition risk and are more likely to be kept on the balance sheet after Trump's election, this often relates to more monitoring efforts via the imposition of covenants.

[Table 7]

We view our results as an indication that banks insure themselves in case the Trump administration does not win the subsequent term leading to the possibility that the performance of HTR loans may change by having control rights through covenant restrictions. As a matter of fact, Biden won the 2020 presidential election and on his first day in office, he reverted Trump's decision to leave the Paris Agreement.

6.3 Heterogeneous effects across banks' characteristics

In this section, we dive into the heterogeneity in our findings across banks' characteristics. This allows shedding light on who are the banks that display the observed securitization intentions. Perhaps one question that arises is whether green banks' intentions to securitize change differently after Trump's election compared to non-green banks. Previous literature highlights the role that banks' green preferences play in their decision-making (Degryse et al., 2021; Kacperczyk and Peydró, 2021). Understanding whether green banks respond differently to a shock to firms' transition risks can shed light on whether they really care about the transition toward a greener economy or they only use climate awareness as a tool to create an image of social responsibility. Furthermore, we explore how several other bank characteristics interact with banks' management of transition risks such as their location, capitalization, and size.

Do green banks behave differently? Previous evidence points towards green banks giving preferential terms to green firms and reducing credit to brown borrowers. Following this line of thought, green banks' securitization intentions may change differentially depending on firms' emission intensities after Trump's election. Therefore, we first collect information on banks' ESG scores before Trump's election and define banks with ESG scores higher than the median as green banks. We report estimation results from the sub-sample of banks with ESG scores higher than the median in Column (1) of Table 8, lower than the median in Column (2), and with no ESG score in Column (3). Gantchev et al. (2022) imply that when a firm does not have an ESG score, it is an indication of not being concerned about ESG issues as much as other firms. The results strongly support that non-green banks, that is banks that have a low or no ESG score, are more likely to keep HTR loans on their balance sheets after Trump's election. In contrast, we do not find that this applies to banks with high ESG scores.

[Table 8]

Next, we employ an alternative approach to identify green banks by following Degryse et al. (2021). Banks are considered to be green if they joined the United Nations Environmental Programme (UNEP) Finance Initiative before Trump's election. Column (4) shows the results for the sub-sample of banks that did join before while Column (5) illustrates the findings for the sub-sample of banks that joined only after the election or not at all. This exercise highlights that it is banks that did not join the UNEP initiative before the election that drive the results.[7]

Thus, irrespective of how we define green banks, the picture that presents itself here implies that green banks do not exhibit a lower propensity to retain HTR loans on their balance sheets after the election.

Other bank characteristics Zooming into the importance of other bank characteristics, we first investigate the role of banks' locations. The idea is that non-US banks may not respond to the US election outcome as much as US banks. In particular, EU banks may have different perspectives given that the European Union quickly ratified the 2015 Paris Agreement and continued to focus environmental policies between 2016 and 2019 on decarbonizing the economy (in contrast to US policies). In Columns (1) and (2) of Table 9, we estimate Equation (3) for US and non-US banks separately. The results confirm our expectation and show that it is US banks that are less likely to securitize loans granted to firms with increasing emission intensities after the election. In contrast, non-US banks are not less likely to securitize these loans but rather more likely.

[Table 9]

[7] In unreported results, we repeat this exercise while disregarding the time when banks joined the initiative to capture the fact that some banks may have green preferences before they officially decide to join. The results are unchanged.

Next, we consider how banks' capitalization interacts with the link between the propensity to securitize and firms' environmental performance. It may be the case that it is less capitalized banks driving the results as they have less room for maneuver in terms of their capital position that allows them to take climate risk considerations into account irrespective of the regulatory environment (Reghezza et al., 2022). Moreover, theories of loan sales focus on the role of bank capital (Gorton and Pennacchi, 1995; Pennacchi, 1988) as banks with lower levels of capital tend to securitize loans more to reduce risk-weighted assets and capital requirements. For this analysis, we split our sample into banks with capital ratios above (Column (3)) and below the median (Column (4)). The results suggest that it is both high and low-capital banks exhibiting a lower propensity to securitize loans with higher transition risk. Hence, we do not find any heterogeneity in terms of banks' capitalization.

Last, we look into the role of banks' size. Large banks are often subject to higher capital requirements due to which they are better protected from losses related to transition risk. Hence, these banks may be even less concerned than other banks about transition risk after the 2016 election (Beyene et al., 2021). More generally, larger banks have better access to secondary markets, are better informed, and thus may be more responsive in their securitization decisions, consistent with the view that bank size is a proxy for diversification and efficiency of Demsetz and Strahan (1997). Therefore, we estimate Equation (3) separately for the sub-sample of banks with sizes above (Column (5)) and below (Column (6)) the median. However, the results do not confirm our conjecture. Both large and small banks demonstrate a lower intention to securitize loans granted to firms with increasing emission intensity after Trump is elected.

7 Robustness checks

Alternative measurement of firms' transition risks Table A3 demonstrates that our results are robust to alternative ways in which firms' transition risks are defined or measured. In Column (1), we compare the securitization propensity of loans that are granted to firms, whose increase in the ratio of emissions over total assets ratio is larger than the bottom third quartile over the pre-shock period with all other firms. The measure used in the baseline corresponds roughly to a divide at the median. In Column (2), we base *HTR Loan*$\times$ *Trump* on the change in emissions over the pre-shock period (not emission intensity). In Column (3), we construct our indicator variable on the average yearly change in emission intensity over the pre-shock period.

Moreover, Bajic et al. (2021) point out that carbon emissions may not reflect all associated transition risk of borrowers and that firm-level carbon emissions could be inconsistent across different data sources. Therefore, we employ two other measurements to capture firms' transition risks: ESG scores collected from Refinitiv (Column (4)) and a proxy for firms' regulatory risks related to climate change from Sautner et al. (2022) (Column (5)). ESG scores incorporate a more forward-looking view on firms' environmental performance as they rest on e.g., investments or investment plans as well as on the adaption of emission targets or climate change frameworks. The proxy by Sautner et al. (2022) has the advantage that it captures a view from within firms as it is based on the conversation around regulatory topics related to climate change in quarterly earnings conference calls between board members of firms, financial analysts, and other stakeholders. For both alternative measures, we create binary indicators that take on a value of one when a firm sees a worsening in its ESG score or an increase in its regulatory risk over the pre-shock period.

Anticipation and alternative clustering While anticipating the election of Donald Trump as the president of the United States seems highly unlikely, we outline that results are qualitatively unchanged when we drop the year 2016 from the regression (Column (1) in Table A4). In the same vein, we also exclude the year 2019 from the regression to illustrate that our results are not driven by expectations about the next presidential election. Furthermore, we demonstrate that our results are robust to clustering at the bank-time level (Column (3)), bank-firm level (Column (4)), and bank-industry level (Column (5)).

Sampling and data preparation choices In Table A5, we ensure that our results do not depend on sampling or data preparation choices made. We, therefore, exclude borrowers from the public sector (Column (1)) and the energy sector (Column (2)). In Column (3), we use facility volumes as a control variable instead of loan volumes. In Column (4), the sample encompasses only facilities that have a single lead arranger. In Column (5), we employ an alternative lead arranger definition (Chakraborty et al., 2018).

Confounding factors Trump's election did not only shift expectations of future environmental policy but also of other policy fields. Prospective changes in these other fields are shown to affect firms differentially depending on their characteristics (Wagner et al., 2018). To ensure that our effects indeed capture the relative impact of firms' environmental performance on banks' securitization intentions and are not driven by the differential effect of the election of Trump due to other characteristics, we introduce additional interaction terms with relevant firm characteristics and *Trump* in Table A6.

In Column (1), we include an interaction term with firms' income tax rates as Trump's election implied lower corporate taxes. In Column (2), we employ an indicator for whether a firm is part of the tradeable sector as Trump announced stricter trade policies (Wagner et al., 2018). In Column (3), an interaction with firms' governance scores is introduced to capture that Trump implied financial deregulation generally would impact firms to different degrees depending on their corporate governance (Ramelli et al., 2021). Our results are qualitatively unaffected by these checks. In Column (4), we introduce another control variable, firms' financial constraints proxied by the size-age-index a la Hadlock and Pierce (2010). This should reduce concerns that changes in firms' emission intensities only capture firms' financial constraints. Firms could increase their emission intensities because they do not have sufficient funding to change their emission path. However, the inclusion of firms' financial constraints does not change our results nor do we find a correlation between firms becoming more carbon intensity and receiving a loan.

Alternative specifications To alleviate the concern that our findings depend on the specific empirical strategy selected, we run several alternative specifications and report results in Table A7. In Column (1), we alter how the dependent variable is defined. We now identify loans likely to be securitized by whether they are Term B loans. In Column (2), we add industry-time fixed effects to absorb any industry-specific shocks that could bias our results. In Column (3), the sample is collapsed into a single pre- and post-shock period (Bertrand et al., 2004). In Columns (4) and (5), we check whether our results are driven by firms that are located in stricter regulatory environments by estimating the regression separately for firms located in US states that had emission targets in place before the 2016 election and those that did not. We do not find that this drives our results.

Falsification test One critical concern with respect to our findings would be that we simply capture general changes in banks' securitization intentions that are unrelated to changes in transition risk. We rule out this alternative channel by running Equation (3) on a sample of syndicated loans granted to EU firms. While the United States opted out of the Paris Agreement, the EU still focuses its climate policies on decarbonization. Thus, if our hypotheses hold, one should not observe any contraction in the propensity to securitize after Trump's election for EU firms.

We follow the same data collection procedure as for the sample of US firms. As a result, we have information on 63 firms getting loans from 93 banks between Q1 2013 and Q4 2019. Table A8 shows that there is no evidence that loans to EU firms that see an increase in their emission intensity have a statistically significantly lower likelihood to be securitized after 2016.

8 Conclusion

This paper presents novel evidence that banks use securitization to manage their exposure to firms' transition risks. We document a sizable increase in the probability of loans granted to firms with a worsening environmental performance to be securitized, especially when banks may not be willing to reduce credit supply to their borrowers. We find that depending on banks' business models, they choose between risk pricing through increasing loan spreads or risk shifting through securitization.

Considering an exogenous shock that lowers transition risk, the election of Donald Trump, we show that banks adapt their management of transition risk. HTR loans have a lower likelihood to be securitized after the election compared to LTR loans. However, banks seem to ensure against potential changes in US climate policies as this effect is even stronger when covenants are incorporated into the loan contracts. Zooming into which banks are driving these effects, we highlight that it is, in particular, US banks as well as lenders that have no or low preferences for sustainable lending that display a decreased intention to securitize HTR loans.

Our findings provide important insights for the design of future environmental policies directed at banks. As banks can manage transition risk using securitization, policymakers should be aware of this fact when designing climate-related capital and liquidity requirements. Policymakers need to understand who is actually carrying the risk involved and how much skin in the game banks have to make sure to not underestimate banks' exposure. Our results provide a first understanding of whether banks intend on shifting transition risk off their balance sheets via securitization as well as why they use this channel and do not adjust credit conditions. A limitation of our approach is that we can say little about how much skin in the game banks maintain as we do not observe how much of their share banks are willing to sell or how these vary when transition risk changes. This is a promising avenue for future research.

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Tables and figures

Table 1: Variable definitions

Variable name	Description	Source
Loan characteristics		
Securitization-Inclined	Dummy that is equal to one if there is at least one participant in the syndicate at loan origination that is a CLO and zero otherwise	Fitch Ratings
Loan Maturity	Loan maturity in years	DealScan
Loan Spread	Spread in basis points over Libor	DealScan
Loan Amount	Loan amount in US$ million	DealScan
Trump	A dummy variable that takes on a value of one between 2016 Q4 and 2019 Q4 and zero otherwise	
Brown Specialization	Share of a bank's loan volume granted to a particular brown industry, where a brown industry is one with an ESG score lower than the top 75th percentile, relative to the bank's total loan volume granted.	DealScan, Refinitiv
Brown Market Share	Share of a bank's loan volume granted to a particular brown industry relative to the total loan volume granted by all banks to this industry	DealScan, Refinitiv
Covenant	Dummy that equals one if a bank includes at least one covenant in the loan contract at origination and zero otherwise	DealScan
Firm characteristics		
HTR Loan (Yrly)	Dummy that is equal to one if the annual change in firms' emission intensities is positive and zero otherwise	Refinitiv
HTR Loan	Dummy that is equal to one if the change in firms' emission intensities between 2013 and 2015 is positive and zero otherwise	Refinitiv
Much HTR Loan	Dummy that is equal to one if the change in firms' emission intensities between 2013 and 2015 is larger than the 75th percentile and zero otherwise	Refinitiv
$\overline{\text{HTR Loan (Yrly)}}$	Dummy that is equal to one if the average yearly change in emissions to total assets ratio over the pre-shock period is larger than zero and zero otherwise	Refinitiv
Lower ESG Score	Dummy that is equal to one if firms' ESG scores decreased over the pre-shock period and zero otherwise	Refinitiv
Higher Rrisk	Dummy that is equal to one if firms' regulatory risks increased over the pre-shock period and zero otherwise	Sautner et al. (2022)
Firm Total Assets	Total assets in billion US Dollars	Worldscope
Firm Size	Log of total assets	Worldscope
Firm ROA	Net income divided by total assets	Worldscope
Firm Equity	Common equity divided by total assets	Worldscope
Firm Capex	Capital expenditures divided by total assets	Worldscope
Firm Tax Rate	Income tax rate	Worldscope
Tradeable	Dummy that is equal to one if firms belong to the tradeable sector and zero otherwise	DealScan
Firm SA Index	Size-age index defined in accordance with Hadlock and Pierce (2010)	Worldscope
Target	Dummy that is equal to one if firms are located in states that had emission targets in place before the 2016 election	NCSL
Bank characteristics		
Joined UNEP	Dummy that is equal to one if banks joined the UNEP FI before the 2016 election	UNEP FI
High Bank ESG Score	Dummy that is equal to one if banks have an ESG score above the median and zero if banks have a score below the median or no score	Refinitiv
US Bank	Dummy that is equal to one if banks have their headquarters in the United States	Compustat

Table 1: Variable definitions

Variable name	Description	Source
High Bank Capital	Dummy that is equal to one if banks have a pre-shock capital ratio above the median and zero otherwise	Compustat
High Bank Size	Dummy that is equal to one if banks' pre-shock size is larger than the median and zero otherwise	Compustat
Bank Total Assets	Total assets in US$ billion	Compustat
Bank Size	Log of total assets	Compustat
Bank Equity	Total equity divided by total assets	Compustat
Bank ROA	Income before tax divided by total assets	Compustat
Bank Deposit	Total deposits divided by total assets	Compustat

Table 2: Summary statistics

	N	Mean	SD	p25	p50	p75
Securitization-Inclined	3,673	0.66	0.48	0.00	1.00	1.00
Loan Amount	3,673	1.36	1.87	0.36	0.80	1.53
Facility Amount	3,673	13.74	22.57	3.80	8.03	16.03
Loan Maturity	3,673	4.44	1.59	3.83	5.00	5.00
Loan Spread	3,673	174.30	102.58	112.50	150.00	225.00
Covenant	3,673	0.46	0.50	0.00	0.00	1.00
Trump	3,673	0.46	0.50	0.00	0.00	1.00
Brown Specialization	3,673	26.25	27.45	0.00	24.96	41.87
Brown Market Share	3,673	2.44	2.46	0.00	2.20	4.97
HTR Loan (Yrly)	3,673	0.35	0.48	0.00	0.00	1.00
HTR Loan	3,673	0.40	0.49	0.00	0.00	1.00
Firm Total Assets	3,673	23.22	28.37	5.15	11.59	31.85
Firm ROA	3,673	6.21	7.94	3.41	5.50	8.78
Firm Equity	3,673	27.42	25.64	17.92	29.50	42.26
Firm Capex	3,673	1.27	1.20	0.45	0.98	1.73
Much HTR Loan	3,673	0.28	0.45	0.00	0.00	1.00
$\overline{\text{HTR Loan (Yrly)}}$	3,673	0.40	0.49	0.00	0.00	1.00
Lower ESG Score	3,673	0.38	0.49	0.00	0.00	1.00
Higher Rrisk	3,631	0.16	0.37	0.00	0.00	0.00
Firm Tax Rate	2,846	26.56	12.82	18.05	27.55	34.85
Tradeable	3,673	0.43	0.49	0.00	0.00	1.00
Firm G Score	3,662	0.60	0.19	0.47	0.61	0.75
Firm SA Index	3,673	-0.52	5.17	-3.05	1.19	3.21
Target	3,673	0.18	0.39	0.00	0.00	0.00
Joined UNEP	3,673	0.31	0.46	0.00	0.00	1.00
High Bank ESG Score	3,673	0.15	0.36	0.00	0.00	0.00
US Bank	3,673	0.71	0.45	0.00	1.00	1.00
High Bank Capital	3,392	0.43	0.50	0.00	0.00	1.00
High Bank Size	3,456	0.50	0.50	0.00	0.00	1.00
Bank Size	3,433	1905.35	698.49	1673.98	2104.53	2449.60
Bank Equity	3,373	8.59	2.24	7.94	9.02	10.30
Bank ROA	2,749	0.21	0.13	0.13	0.23	0.30
Bank Deposits	2,745	55.90	10.27	52.55	55.89	63.56

Note: This table reports descriptive statistics for the variables used in the main empirical analysis. The baseline sample consists of 3,673 loan observations that are granted to US borrowers between 2013 to 2019. All continuous variables are winsorized at the 1st and 99th percentile. Table 1 provides detailed variable definitions.

Table 3: Parallel trends

	Treated		*Control*		*Treated - Control*
Variable	Mean	SD	Mean	SD	Normalized diff.
Loan characteristics					
Securitization-Inclined	0.723	0.448	0.622	0.485	0.15
Loan Amount	1.181	1.593	1.326	1.850	-0.06
Loan Spread	203.741	125.698	172.398	101.514	0.19
Loan Maturity	4.448	1.580	4.608	1.468	-0.07
Firm characteristics					
ΔFirm Size	2.399	11.443	6.635	7.672	-0.31
ΔFirm ROA	-19.589	108.796	-5.525	66.020	-0.11
ΔFirm Equity	-0.983	41.597	-3.077	30.142	0.04
ΔFirm Capex	9.759	26.797	7.486	17.851	0.07
Bank characteristics					
ΔBank Size	-0.447	5.307	-0.183	4.857	-0.04
ΔBank Equity	4.113	3.923	3.844	3.362	0.05
ΔBank ROA	-54.992	43.165	-55.044	44.587	0.00
ΔBank Deposits	2.603	2.193	2.398	1.966	0.07

Note: This table reports statistics of relevant co-variates over the pre-shock period (Q1 2013 to Q3 2016) dividing the sample between treated and control firms. Treated firms exhibit an increase in their emissions to total asset ratio over the pre-shock period while control firms do not emit more. The last column reports normalized differences between the treatment and control groups. An absolute difference smaller than 0.25 indicates no significant difference between the groups. Firm and bank characteristics are reported as annual percentage changes (in %).

Table 4: The propensity to securitize and firms' transition risks

	(1)	(2)	(3)	(4)	(5)	(6) Matching
HTR Loan (Yrly)	0.038	0.036*	0.041*	0.054**	0.052**	0.060**
	(0.025)	(0.019)	(0.024)	(0.021)	(0.020)	(0.024)
Loan Spread	0.000	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan Maturity	0.033***	0.022**	0.020**	0.020**	0.036***	0.032**
	(0.007)	(0.011)	(0.010)	(0.009)	(0.011)	(0.014)
Ln(Loan Amount)	0.007	-0.037***	-0.040***	-0.041***	-0.039***	-0.053***
	(0.014)	(0.008)	(0.007)	(0.007)	(0.007)	(0.009)
Firm Size				-0.026	-0.002	0.057*
				(0.025)	(0.024)	(0.031)
Firm ROA				0.006***	0.005***	0.006**
				(0.002)	(0.002)	(0.002)
Firm Equity				0.000	0.000	-0.000
				(0.001)	(0.001)	(0.002)
Firm Capex				0.002	-0.002	-0.006
				(0.017)	(0.018)	(0.032)
Observations	3,673	3,673	3,673	3,673	3,673	2,596
Bank-firm FE	No	Yes	Yes	Yes	Yes	Yes
Bank-time FE	No	No	Yes	Yes	Yes	Yes
Loan type FE	No	No	No	No	Yes	Yes
Loan purpose FE	No	No	No	No	Yes	Yes
Adjusted R^2	0.019	0.634	0.629	0.632	0.637	0.632
Number of banks	81	81	81	81	81	58
Number of firms	380	380	380	380	380	329
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores the link between banks' securitization intentions and firms' transition risks as specified in Equation (1). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *HTR Loan (Yrly)* takes on a value of one if a firm increases its yearly emissions to total assets ratio and zero otherwise. Loan and firm controls as well as fixed effects are introduced sequentially from Columns (1) to (5). Column (6) uses a matched sample of loans granted to firms with worsening environmental profiles and loans granted to firms without a deterioration in their profiles. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Brown specialization and market share

	(1) Securitization-Inclined	(2) Loan Spread
HTR Loan (Yrly)	0.038	1.735
	(0.034)	(6.763)
HTR Loan (Yrly) × Brown specialization	0.003***	-0.271**
	(0.001)	(0.118)
Brown specialization	0.001***	0.113
	(0.000)	(0.117)
HTR Loan (Yrly) × Brown Market Share	-0.020***	2.162**
	(0.004)	(1.081)
Brown Market Share	-0.015***	-7.997*
	(0.004)	(4.041)
Observations	3,673	3,673
Loan controls	Yes	Yes
Firm controls	Yes	Yes
Bank-firm FE	Yes	Yes
Bank-time FE	Yes	Yes
Loan type FE	Yes	Yes
Loan purpose FE	Yes	Yes
Adjusted R^2	0.639	0.731
Number of banks	81	81
Number of firms	380	380
Clustering	Bank	Bank

Note: This table explores how the link between banks' securitization intentions and firms' transition risks varies depending on banks' specialization and market share in brown industries as specified in Equation (2). *Securitization-Inclined* and *Loan Spread* are the dependent variables. *HTR Loan (Yrly)* takes on a value of one if a firm increased its yearly emissions to total assets ratio and zero otherwise. *Brown Specialization* is the share of loan volume granted to to the respective brown industry relative to the total loan volume granted by the bank. *Brown Market Share* is the share of loan volume granted to the respective brown industry relative to the total loan volume granted by all banks to that industry. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: The propensity to securitize, firms' transition risks, and Trump's election

	(1)	(2)	(3)	(4)	(5)
HTR Loan	0.100***				
	(0.031)				
Trump	0.039	0.014			
	(0.030)	(0.019)			
HTR Loan × Trump	-0.109**	-0.052**	-0.073**	-0.085***	-0.095***
	(0.045)	(0.025)	(0.028)	(0.030)	(0.027)
Loan Spread	0.000	0.000	0.000	0.000	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Loan Maturity	0.035***	0.022**	0.021**	0.021**	0.037***
	(0.006)	(0.010)	(0.010)	(0.009)	(0.011)
Ln(Loan Amount)	0.008	-0.037***	-0.040***	-0.041***	-0.039***
	(0.014)	(0.008)	(0.007)	(0.007)	(0.007)
Firm Size				-0.036	-0.012
				(0.026)	(0.025)
Firm ROA				0.005***	0.005***
				(0.002)	(0.002)
Firm Equity				0.000	0.000
				(0.001)	(0.001)
Firm Capex				0.002	-0.002
				(0.018)	(0.018)
Observations	3,673	3,673	3,673	3,673	3673
Bank-firm FE	No	Yes	Yes	Yes	Yes
Bank-time FE	No	No	Yes	Yes	Yes
Loan type FE	No	No	No	No	Yes
Loan purpose FE	No	No	No	No	Yes
Adjusted R^2	0.022	0.633	0.629	0.632	0.637
Number of banks	81	81	81	81	81
Number of firms	380	380	380	380	380
Clustering	Bank	Bank	Bank	Bank	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. Loan and firm controls as well as fixed effects are introduced sequentially. Firm controls are included as their first lags. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: Monitoring

	(1) All covenants	(2) Financial	(3) Net worth
HTR Loan × Trump	-0.065**	-0.068**	-0.087***
	(0.027)	(0.028)	(0.026)
HTR Loan × Trump × Covenant	-0.068*	-0.062*	-0.445***
	(0.035)	(0.032)	(0.140)
Covenant	0.087***	0.091***	-0.702***
	(0.023)	(0.024)	(0.027)
HTR Loan × Covenant	-0.052*	-0.053	1.230***
	(0.028)	(0.032)	(0.437)
Trump × Covenant	-0.071**	-0.080***	0.364***
	(0.027)	(0.026)	(0.131)
Observations	3,673	3,673	3,673
Loan controls	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes
Adjusted R^2	0.638	0.638	0.638
Number of banks	81	81	81
Number of firms	380	380	380
Clustering	Bank	Bank	Bank

Note: This table explores how the effect of firms' transition risks on banks' securitization intentions after Trump's election interacts with banks' monitoring efforts. The dependent variable is *Securitization-Inclined* which identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increases its emissions to total asset ratio over the pre-shock period and zero otherwise. *Covenant* is a dummy that equals one if a bank includes at least one covenant in the loan contract at origination and zero otherwise. In Column (1), both financial and net worth covenants are considered. In Column (2), only financial covenants are considered. In Column (3), only net worth covenants are considered. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Do green meet green?

	(1)	(2)	(3)	(4)	(5)
	Bank ESG scores			Joined UNEP	
	High	Low	No	Before	After or No
HTR Loan × Trump	0.105	-0.080***	-0.116***	-0.115	-0.094***
	(0.099)	(0.004)	(0.028)	(0.117)	(0.014)
Observations	548	387	2,738	1,144	2,529
Loan controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.839	0.725	0.594	0.711	0.613
Number of banks	11	10	60	27	54
Number of firms	66	84	336	118	351
Clustering	Bank	Bank	Bank	Bank	Bank

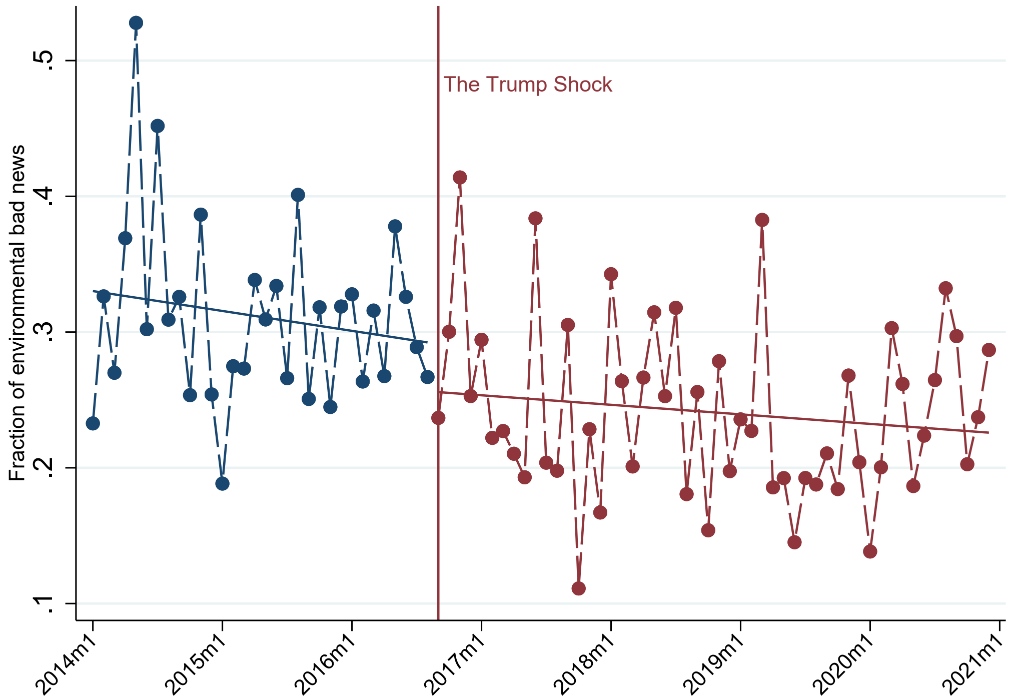
Note: This table explores how the effect of firms' transition risks on banks' securitization intentions after Trump's election depends on banks' green preferences. *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. The table splits the sample into green and non-green banks by banks' pre-shock average ESG scores (Columns (1) to (3)) and their membership in the UNEP FI before 2016 (Columns (4) and (5)). Column (1) encompasses only banks that have a score above the median. Column (2) encompasses only banks that have a score below the median. Column (3) encompasses only banks that have no ESG score. Column (4) encompasses only banks that joined the UNEP FI before Trump's election and Column (5) that did not join at all or only after the election. Firm controls are included as their first lag and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9: Other bank characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	Bank location		Bank capital		Bank size	
	US	Non-US	High	Low	High	Low
HTR Loan × Trump	-0.113***	0.115*	-0.110**	-0.077**	-0.093***	-0.123*
	(0.024)	(0.064)	(0.044)	(0.030)	(0.011)	(0.072)
Observations	2,613	1,060	1,455	1,936	1,713	1,742
Loan controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.601	0.831	0.646	0.621	0.571	0.716
Number of banks	24	57	16	25	5	52
Number of firms	369	86	241	230	295	182
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

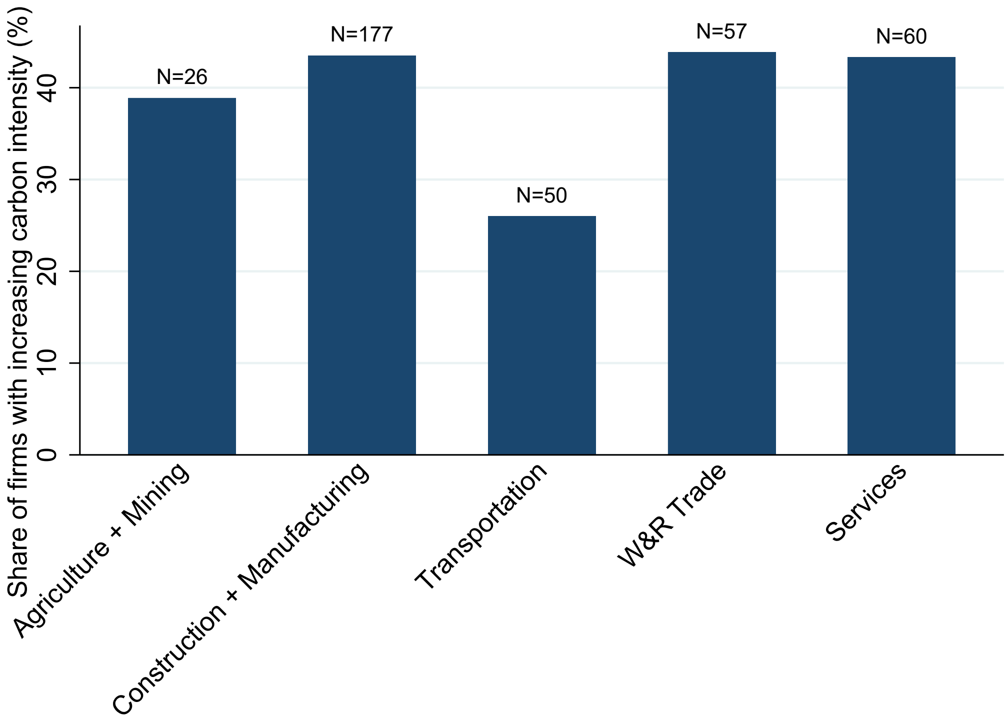
Note: This table explores how the effect of firms' transition risks on banks' securitization intentions after Trump's election depends on banks' location, capital, and size. *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. In Columns (1) and (2), the sample is split into US and non-US banks. In Columns (3)/(4), the sample is split into banks' average pre-shock equity ratio being above/ below the median. In Columns (5)/(6), the sample is split into banks' average pre-shock size being above/ below the median. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Fraction of bad environmental news before and after Trump's election



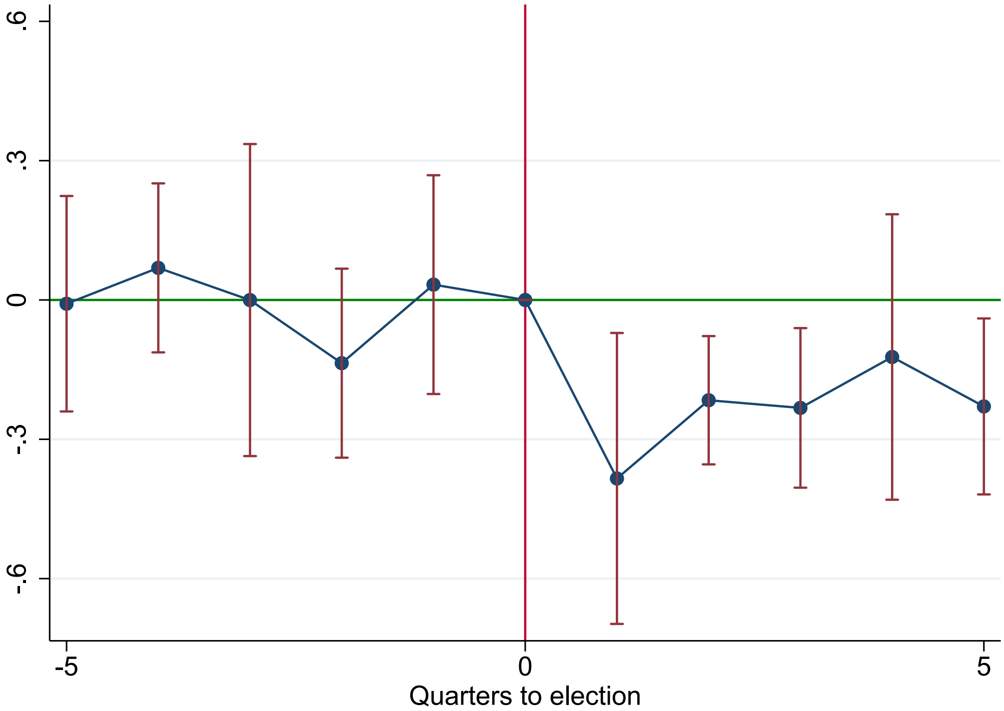
Note: This figure illustrates the percentage of bad environmental news over total ESG news for US firms on a monthly basis between January 2014 and January 2021. The data is from RepRisk, a leading data provider, that screens daily over 80,000 media, stakeholder, and third-party sources as well as social media for news related to firms' ESG practices since 2007.

Figure 2: Share of firms with increasing carbon intensity



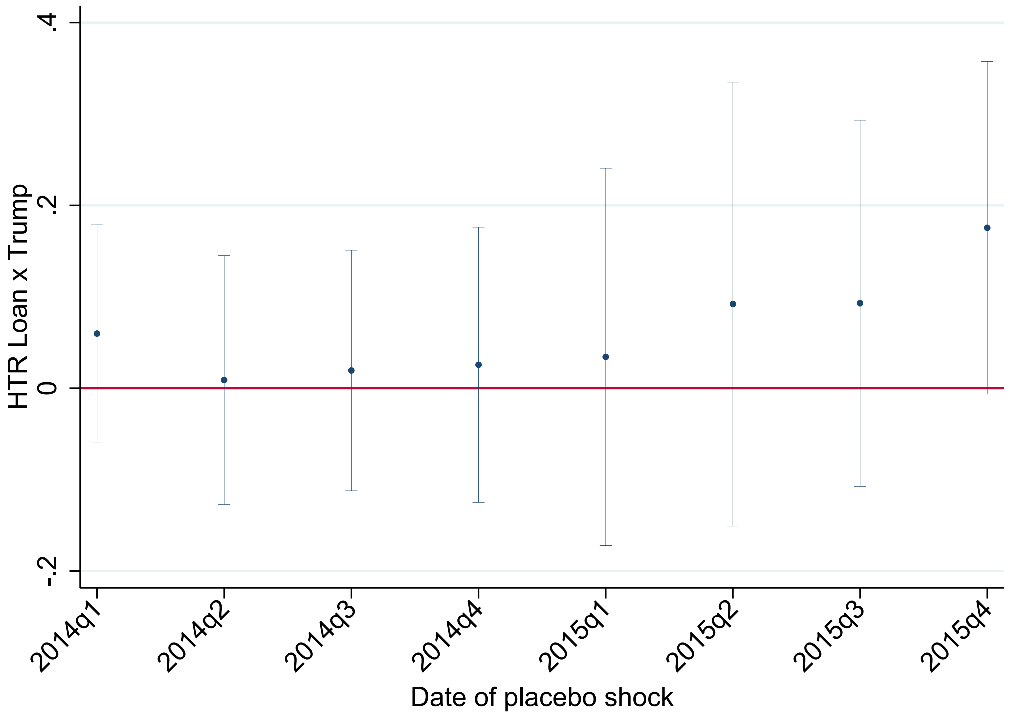
Note: This figure illustrates the distribution of firms with worsening environmental profiles within as well as across industries. The share of firms with increasing carbon intensities indicates how many firms per industry experience an increase in their emissions to total assets ratio over the pre-shock period. N displays the total number of firms per industry.

Figure 3: Dynamic effects



Note: This figure illustrates the quarterly treatment effects for five quarters before and five quarters after Trump's election. To this end, we estimate Equation (3) but interact *HTR Loan* with a set of quarter dummies using Q4 2016, the quarter in which Trump is elected, as the reference. *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. 90% confidence intervals are displayed.

Figure 4: Placebo exercise



Note: This figure illustrates the results of several placebo tests in which the shock under study is simulated to take place at different points in time during the pre-shock period from Q1 2013 until Q3 2016. For each test that is simulated to take place in each quarter between Q1 2014 and Q4 2015, the estimated coefficient $HTR\ Loan \times Trump$ and 95% confidence bands are plotted.

Appendix

Table A1: First-stage propensity matching

	(1) HTR Loan (Yrly)
Firm Size	0.006
	(0.021)
Firm ROA	-0.013***
	(0.003)
Firm Equity	0.001
	(0.001)
Firm Capex	-0.071***
	(0.019)
Loan Spread	0.002***
	(0.000)
Loan Maturity	-0.010
	(0.014)
Ln(Loan Amount)	0.057***
	(0.017)
Observations	3,673
Pseudo R^2	0.0321
Number of banks	81
Number of firms	380

Note: This table shows the first stage of the propensity matching reported in Column (6) in Table 4. For each loan, we estimate the propensity score of granting a loan to a firm with increasing emission intensities conditional on borrowers' and loan characteristics using a probit model. We then match each treated loan to a set of loans granted to firms with no deterioration in their environmental performance, which have similar propensity score matching using one-to-one non-replacement. *HTR Loan (Yrly)* takes on a value of one if a firm increases its yearly emissions to total assets ratio and zero otherwise. Firm controls are included as their first lags. Standard errors are reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: The propensity to securitize and firms' transition risks - by scopes

	(1)	(2)	(3)
HTR Loan (Yrly Scope 1)	0.034***		
	(0.011)		
HTR Loan (Yrly Scope 2)		0.055**	
		(0.027)	
HTR Loan (Yrly Scope 3)			0.006
			(0.041)
Observations	3,673	3,673	3,673
Loan controls	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes
Adjusted R^2	0.631	0.631	0.631
Number of banks	81	81	81
Number of firms	380	380	380
Clustering	Bank	Bank	Bank

Note: This table explores the link between the propensity to securitize and firms' transition risks as specified in Equation (1). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *HTR Loan (Yrly Scope 1) or (Yrly Scope 2, 3)* takes on a value of one if a firm increases its yearly scope 1 (scope 2, scope 3) emissions to total assets ratio and zero otherwise. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: Alternative measurements

	(1)	(2)	(3)	(4)	(5)
Much HTR Loan × Trump	-0.155***				
	(0.037)				
HTR Loan × Trump		-0.089*			
		(0.053)			
$\overline{\text{HTR Loan (Yrly)}}$ × Trump			-0.095***		
			(0.027)		
Lower ESG Score × Trump				-0.082***	
				(0.025)	
Higher Rrisk × Trump					-0.078*
					(0.039)
Observations	3,673	3,673	3,673	3,673	3630
Loan controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.638	0.637	0.637	0.636	0.632
Number of banks	81	81	81	81	81
Number of firms	380	380	380	380	374
Clustering	Bank	Bank	Bank	Bank	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *Much HTR Loan* takes on a value of one if a firm's ratio of emissions over total assets increased more than that of the bottom third quartiles over the pre-shock period and zero otherwise. In Column (2), *HTR Loan* is defined based on the change in emissions instead of emission intensity. $\overline{\textit{HTR Loan (Yrly)}}$ is defined on the average annual change over the pre-shock period. In Column (4), we use changes in firms' ESG scores as a proxy for firms' transition risks instead of changes in emission intensities. In Column (5), we use changes in a measure of regulatory risk instead of changes in emission intensity. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Anticipation and alternative clustering

	(1) Wo/2016	(2) Wo/2019	(3) Cluster	(4) Cluster	(5) Cluster
HTR Loan × Trump	-0.106***	-0.087**	-0.095***	-0.095***	-0.095***
	(0.020)	(0.034)	(0.029)	(0.029)	(0.024)
Observations	3,256	3,090	3,673	3,673	3673
Loan controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.655	0.640	0.637	0.637	0.637
Number of banks	80	74	81	81	81
Number of firms	371	354	380	380	380
Clustering	Bank	Bank	Bank-time	Bank-firm	Bank-ind

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. In Column (1), the year 2016 is dropped from the regression. In Column (2), the year 2019 is dropped from the regression. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered as indicated and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Sampling and data preparation choices

	(1) Wo/SIC8	(2) Wo/energy	(3) Facility	(4) Single lead	(5) Alter. lead
HTR Loan × Trump	-0.091***	-0.102**	-0.099***	-0.066***	-0.083***
	(0.024)	(0.048)	(0.029)	(0.013)	(0.015)
Observations	3,509	2,920	3,673	1,801	2,636
Loan controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.634	0.671	0.636	0.597	0.588
Number of banks	81	72	81	26	61
Number of firms	367	312	380	356	404
Clustering	Bank	Bank	Bank	Bank	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. In Column (1), public sector firms are dropped from the regression. In Column (2), energy firms are dropped from the regression. In Column (3), we use the facility amount instead of the loan amount as a control variable. In Column (4), we drop loans that have multiple lead arrangers. In Column (5), we use an alternative definition of lead arranger(s). Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Confounding factors

	(1) Tax policy	(2) Trade policy	(3) Governance	(4) FC Index
HTR Loan × Trump	-0.114***	-0.093***	-0.094***	-0.109***
	(0.021)	(0.031)	(0.030)	(0.028)
Firm Tax Rate	0.002			
	(0.002)			
Trump × Firm Tax Rate	0.001			
	(0.002)			
Tradeable × Trump		-0.017		
		(0.059)		
Firm G Score			-0.020	
			(0.128)	
Trump × Firm G Score			-0.137	
			(0.142)	
Observations	2,759	3,673	3,661	3,673
Loan controls	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes
Loan type FE	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes
Adjusted R^2	0.648	0.637	0.639	0.640
Number of banks	78	81	81	81
Number of firms	334	380	377	380
Clustering	Bank	Bank	Bank	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. The estimations incorporate an additional interaction between *Trump* and firms' tax rates in Column (1), an indicator for the tradeable sector in Column (2), and firms' governance scores in Column (3). Column (4) additionally includes a proxy for firms' financial constraints as a control. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Alternative specifications

	(1) Alt. dependent	(2) FE	(3) Collapsed	(4) Target	(5) No Target
HTR Loan × Trump	-0.076***	-0.102***	-0.057**	-0.197***	-0.113***
	(0.019)	(0.031)	(0.021)	(0.015)	(0.031)
Observations	3,673	3,664	854	591	2,980
Loan controls	Yes	Yes	Yes	Yes	Yes
Firm controls	Yes	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes
Bank-time FE	Yes	Yes	Yes	Yes	Yes
Loan type FE	No	Yes	Yes	Yes	Yes
Loan purpose FE	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.422	0.668	0.578	0.476	0.667
Number of banks	81	81	21	35	75
Number of firms	380	379	318	79	299
Clustering	Bank	Bank	Bank	Bank	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3). *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. In Column (1), we use an alternative dependent variable and define loans likely to be securitized by whether it is a Term B loan. In Column (2), we additionally add industry-time fixed effects. In Column (3), we collapse the sample into a single pre- and post-shock period. In Columns (4) and (5), we estimate the equation separately for US states that had emission targets in place before the 2016 election and those that did not. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Falsification test

	(1) Securitization-Inclined
HTR Loan × Trump	-0.173
	(0.178)
Observations	2,244
Loan controls	Yes
Firm controls	Yes
Bank-firm FE	Yes
Bank-time FE	Yes
Loan type FE	Yes
Loan purpose FE	Yes
Adjusted R^2	0.855
Number of banks	93
Number of firms	63
Clustering	Bank

Note: This table explores the effect of firms' transition risks on banks' securitization intentions after Trump's election as specified in Equation (3) but on the basis of a sample that comprises only European firms. *Securitization-Inclined* is the dependent variable and identifies whether the syndicate at loan origination includes a CLO. *Trump* indicates the period after Trump's election. *HTR Loan* takes on a value of one if a firm increased its emissions to total assets ratio over the pre-shock period and zero otherwise. Firm controls are included as their first lags and encompass size, ROA, equity, and capital expenditures. Loan controls comprise spread, maturity, and logged loan volume. Standard errors are clustered at bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Paper 3:

Trade Shocks and Lending Specialization

Trade Shocks and Lending Specialization*

Isabella Mueller†

This Draft: September 21, 2022‡

Abstract

Banks adjust lending in response to trade liberalization as they are affected indirectly via their lending portfolios. Banks cut credit supply the higher their ex-ante lending to industries hit by increased import competition due to lower trade barriers. Importantly, I uncover large heterogeneity in banks' reactions depending on sectoral specialization. Banks shield the industries in which they specialize and simultaneously reduce the availability of credit to industries in which they do not specialize. While banks' adjustments have adverse real effects, sectoral specialization dampens the negative impact on firm outcomes.

JEL classification: G21, F14, F65

Keywords: Trade liberalization, credit supply, sector specialization, real effects

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1 Introduction

A large body of literature analyzing the effects of trade liberalization on welfare and economic activity has emerged.[1] Although the classical theories of international trade maintain that free trade creates winners and losers, the gains to winners are believed to offset any losses incurred by those adversely affected by changes in trade flows. However, financial frictions such as banks' funding constraints may alter prevailing considerations about the gains from trade. This paper enriches the evidence on the role of financial frictions in the allocation of credit in the aftermath of a trade shock by identifying how banks adjust lending in response to their corporate clients being hit by increased import competition due to lower trade barriers. I identify large heterogeneity in banks' responses depending on their sectoral specialization. These findings contribute to the existing literature on lending specialization by analyzing its role in the allocation of credit in a new context and country setting - in the case of a trade shock to US borrowers and then feeding through the system to large, globally active banks.

I employ the rise of China to economic power and the induced changes in global trade patterns as a trade shock - an approach well-established in the literature (see, among others, Autor et al. (2013), Autor et al. (2014), Acemoglu et al. (2016), Autor et al. (2016), Bloom et al. (2016), Hombert and Matray (2018), and Helm (2020)). On this basis, I first analyze the effect of banks' exposure to the China shock on credit supply to US firms. Given the sectoral specialization of banks' loan portfolios, banks differ in their indirect exposure to trade shocks (Giometti and Pietrosanti, 2022; Federico et al., 2020; Paravisini et al., 2020). Following the estimation strategy by Federico et al. (2020), I identify how much US industries are affected by the increase in Chinese imports and construct banks' exposure by considering banks' share of loans to each industry weighted by the sector's change in imports from China. Given their financial constraints, banks might find it necessary to cut credit in order to compensate losses or liquidity shortfalls as a result of the negative consequences of increased import competition for US firms (Paravisini, 2008; Chava and Purnanandam, 2011; De Haas and Van Horen, 2012; Adrian et al., 2013).

Second, I examine banks' differential responses to the shock depending on whether the borrower is part of an industry that the bank specializes in. Banks might adjust

[1] Examples from this literature are Ben-David (1993), Pavcnik (2002), Amiti and Konings (2007), Chiquiar (2008), Edmonds et al. (2010), Topalova (2010), McCaig (2011), Autor et al. (2013), Acemoglu et al. (2016), Bloom et al. (2016), and Hombert and Matray (2018).

credit heterogeneously, depending on how important the borrower's industry is for the bank. Banks may shield or even extend credit to sectors they are specialized in when they have to curtail lending. This rests on the argument that the more banks lend to an industry, the more they can acquire industry-specific information (Jahn et al., 2013; Giometti and Pietrosanti, 2022; De Jonghe et al., 2020). Hence, they can develop an information advantage through specialization that they are incentivized to protect in the case of a shock to their funding. This contrasts the traditional view on this matter, which rests on the benefits associated with bank diversification (Diamond, 1984; Boyd and Prescott, 1986; Rossi et al., 2009; Tabak et al., 2011; Shim, 2019). Classical banking theory as well as recent empirical evidence suggests that the more diversified a bank's portfolio, the more likely it is to grant loans (Jimenez et al., 2012; Doerr and Schaz, 2021). Therefore, I follow Paravisini et al. (2020) and construct an indicator for whether banks' share of lending to a specific industry is relatively high.

I rely on data on syndicated loans combined with lender characteristics and information on bilateral trade flows. For identification, I use a difference-in-differences set-up that allows illustrating the effect of banks' exposure to China's entry into the WTO on credit supply to US borrowers. One key challenge for identification poses the possibility that US product demand shocks may be related to changes in imports from China, used to construct banks' exposure, and bank lending. I, therefore, enrich the identification strategy by an instrumental variable approach which exploits that other high-income economies are similarly affected by the rise in import competition from China (Autor et al., 2013; Autor et al., 2014; Acemoglu et al., 2016). A second challenge is to isolate credit supply from credit demand because the China shock is likely to simultaneously affect firms' demand for credit. I overcome this by employing firm-time fixed effects, a standard procedure in the bank lending literature (Khwaja and Mian, 2008). The sample period ranges from 1991 to 2007 with Q4 2001 being the point in time when China entered the WTO.

The results confirm larger declines in outstanding credit for banks with higher exposure to the trade shock. A bank with exposure at the 75th percentile reduces lending by 26 percentage points more than a bank with exposure at the 25th percentile. Banks lend less both to the sub-sample of exposed and non-exposed firms hinting toward the existence of financial spillovers. However, banks adjust their lending differentially depending on their sectoral specialization. Banks shield borrowers in industries they are specialized in, while they reduce credit supply with increasing exposure in industries in which they are not specialized. This is not confined to non-exposed firms but also applies to firms whose prospects worsened due to increased import competition from China. Last, banks' adjustments in credit supply in response to the trade shock transmit to the real economy. Firms that borrow more from banks with larger exposures experience larger reductions in their growth in sales and fixed assets. Receiving credit from specialized banks helps dampen this negative effect on firm outcomes.

This paper contributes to several strands of the literature. First, it adds to the many papers that investigate how the economy responds to trade shocks. Many studies allow for labour market frictions and provide evidence on the short- and medium-term adjustment costs for workers and firms arising in response to large shifts in trade patterns (see, among others, Topalova (2010), Kovak (2013), Autor et al. (2013), Autor et al. (2014), Dix-Carneiro (2014), Autor et al. (2016), Acemoglu et al. (2016), and Dix-Carneiro and Kovak (2017)). Among the most prominent is the paper by Autor et al. (2013) who analyze the effects of rising import competition from China on local US labor markets. They find that rising exposure to China decreases employment and wages. Studies that consider financial frictions in this context are rather limited. Notable exceptions are the papers by Antràs and Caballero (2009), who illustrate the adjustment of cross-border capital flows in response to trade liberalization, Antràs and Caballero (2010), who outline the effects of trade liberalization on welfare in financially underdeveloped countries, or Lanteri et al. (2020), who show the reallocation of machines in Peruvian manufacturing industries in response to the rise of China. Closest to this project is the paper by Federico et al. (2020) who analyze how banks adjust their credit supply to Italian firms in response to the China shock. They find that banks exposed to trade shock decrease lending compared to non-exposed banks. I show that their findings apply also to US borrowers while digging deeper into the role that lending specialization plays in this context.

This work is also related to studies that consider the effect of shocks on banks' lending decisions (Khwaja and Mian, 2008; Ivashina and Scharfstein, 2010; Cetorelli and Goldberg, 2011; De Haas and Van Horen, 2012). With the advent of the global financial crisis, several studies have identified that banks transmit shocks heterogeneously to their borrowers, depending on bank or firm characteristics. Recently, the role of sectoral specialization in the reallocation of credit in the aftermath of shocks has received increased attention. For instance, Paravisini et al. (2020) assess this on the basis of a measure capturing lenders' specialization in export markets and find that exports to markets, in which banks specialize, are disproportionally affected by credit supply shocks. De Jonghe et al. (2020) address banks' specialization from a different angle.

They highlight the role of specialization in banks' credit allocation in the context of the interbank market freeze after the failure of Lehmann Brothers. Their study shows that banks reallocate credit to industries they specialize in. More generally, Blickle et al. (2021) as well as Giometti and Pietrosanti (2022) show that the US C&I lending market and US syndicated loan market are characterized by specialized lending toward specific industrial sectors and thereby provide an important basis for this work.

2 Empirical strategy

This study identifies how banks adjust their credit supply in response to a trade shock. I follow an identification strategy similar to Federico et al. (2020), who rely on a difference-in-differences design. The respective setting compares the availability of credit provided by banks after a trade shock depending on their ex-ante exposure via their lending portfolio. For each bank-firm-quarter observation, I estimate the following equation:

$$\begin{aligned} \ln(\text{Credit})_{b,f,t} = {} & \beta_1 \text{Exposure}_b^{US} \times \text{Post}_t \\ & + \gamma \text{X'}_{b,t} + \zeta_{b,f} + \zeta_{f,t} + \varepsilon_{b,f,t}. \end{aligned} \tag{1}$$

The dependent variable is the log of outstanding credit by bank b to firm f in quarter t. $Exposure_b^{US}$ measures the pre-shock exposure of bank b to the trade shock. Post_t divides the sample period into a pre- and post-shock period. The cut-off point is the last quarter in 2001 when China joined the WTO. Hence, the pre-shock period dates from Q1 1991 to Q3 2001. The vector $\text{X'}_{b,t}$ contains bank-specific, time-varying control variables such as size (log of total assets), return on assets that captures banks' performance, a measure of banks' funding structure (deposits to total assets), and a leverage ratio (short-term debt to total assets).

$\zeta_{b,f}$ are bank-firm fixed effects, which capture firm and bank heterogeneity as well as all time-invariant factors that influence loan-level outcomes for each bank-firm pair, e.g. relationship or distance. One key challenge for identification is how to isolate credit supply from credit demand. To overcome this challenge, I use firm-time ($\zeta_{f,t}$) fixed effects in order to capture credit demand as this is the standard approach in the banking literature (Khwaja and Mian, 2008).[2] Macroeconomic developments affecting

[2] To make sure that relying on multi-relationship firms only as well as assuming demand does not vary across banks do not bias my results, I show that they are qualitatively the same when using industry-location-size-time fixed effects in the fashion of Degryse et al. (2019) in a robustness check in Column (2) in Table A6.

all banks in the sample are implicitly absorbed by $\zeta_{f,t}$.

$\varepsilon_{b,f,t}$ is the idiosyncratic error term. Standard errors are clustered at the bank and 4-digit industry levels, i.e. the level of treatment as suggested by Abadie et al. (2017). Hence, the interaction term $Exposure_b^{US} \times Post_t$ identifies the effect of the shock on credit supply. A negative and statistically significant β_1 would indicate the more banks are exposed to the trade shock, the more they reduce their credit supply.

To alleviate potential endogeneity concerns that result from $Exposure_b^{US}$ being constructed on the basis of trade flows between the United States and China, I enrich the difference-in-differences set-up with an instrumental variable approach in the fashion of Autor et al. (2013). In the construction of $Exposure_b^{US}$, trade flows between the United States and China are used as weights for the degree of exposure to the China shock of each industry a bank is lending to. This implies that product demand shocks originating within the United States could correlate both with $Exposure_b^{US}$ via the trade flows and simultaneously with bank lending.[3] Because $Exposure_b^{US}$ enters Equation (1) interacted with $Post_t$, the system of structural equations is non-linear in its endogenous variable. Therefore, I proceed by retrieving the fitted values of the instrument and interacting them with $Post_t$. The product is then used as the instrument in the 2SLS procedure (Wooldridge, 2010). First-stage results illustrate the relevance of the instrument. I obtain a positive and statistically significant coefficient of 1.165 in Table A2. The F statistic is 10.18 indicating that the instrument is a good predictor for bank exposure (Staiger and Stock, 1997).

In a second step, I investigate whether banks adjust their credit supply differentially after the trade shock depending on their sectoral specialization. In addition to sectoral specialization being a decisive feature that introduces heterogeneity across banks in terms of exposure to the trade shock, it may also be of great importance which industry the particular borrower is part of. In fact, De Jonghe et al. (2020) have shown that banks' sectoral specialization matters for how banks adjust their credit supply in response to a financial shock. Similarly, albeit analyzing the topic from a different angle, Paravisini et al. (2020) find evidence of lender specialization in export markets and outline that exports to markets that the bank specializes in are disproportionally affected by credit supply shocks.

To investigate if and how banks' response varies with sectoral specialization in the context of a trade shock, I extend Equation (1) by interacting $Exposure_b^{US} \times Post_t$ with

[3] I defer further discussion on how the exposure measure is constructed and the logic of the instrument to Section 3.

the binary variable $Specialized_{b,j}$ which indicates whether the bank is specialized in the industry of the respective borrower. Hence, β_3 in Equation (2) identifies the differential effect of bank exposure on credit supply depending on sectoral specialization:

$$\begin{aligned}\ln(\text{Credit})_{b,f,t} = {} & \beta_1 \text{Exposure}_b^{US} \times \text{Post}_t \\ & + \ \beta_2 \text{Post}_t \times \text{Specialized}_{b,j} \\ & + \ \beta_3 \text{Exposure}_b^{US} \times \text{Post}_t \times \text{Specialized}_{b,j} \\ & + \ \gamma \text{X'}_{b,t} + \zeta_{b,f} + \zeta_{f,t} + \varepsilon_{b,f,t}.\end{aligned} \tag{2}$$

The single terms $Exposure_b^{US}$ and $Post_t$ in Equation (1) as well as $Specialized_{b,j}$ and the interaction between $Exposure_b^{US}$ and $Specialized_{b,j}$ in Equation (2) are absorbed by the fixed effects.

3 Data

3.1 Data and data generation process

Loan-level data The primary data source for the main analysis is Thomson Reuters LPC's DealScan database encompassing detailed loan-level information covering the syndicated loan market. I begin with all term loans and credit lines granted to US borrowers in the period from 1991 until 2007. Aggregating at ultimate parent level this comprises 4,190 lenders and 22,649 borrowers. I exclude loans given to borrowers with missing industry codes and to the financial, real estate, or public sector. Lenders can be of any origin but I keep only loans syndicated in the United States. Similar to various previous studies, I sample only lenders that are lead arrangers (Chodorow-Reich, 2014; Acharya et al., 2018; Schwert, 2018). They are the focus of this work given that it is predominantly the lead bank that conducts the active management of the loan (e.g. origination and monitoring) and thereby possesses private information about the respective borrowers (Schwert, 2018). I identify lead arrangers in accordance with Bharath et al. (2011). This reduces the sample to 1,053 lenders and 16,423 borrowers.

Facility volumes are converted into US$ million if necessary, using the spot exchange rate provided by DealScan at loan origination. Loan proportions are allocated to lead arrangers according to the breakdown provided by DealScan if available or equally allocated among all participants in the syndicate (De Haas and Van Horen, 2013). Next, I use the loan proportions to construct a stock variable that captures the availability of credit at each point in time between each bank-firm combination. Each loan enters banks' loan books until it matures. The resulting structure resembles data from a credit registry and allows the dynamic representation of a bank's loan portfolio (Lin and Paravisini, 2013; Chakraborty et al., 2018; Doerr and Schaz, 2021; Giometti and Pietrosanti, 2022).[4] I aggregate all outstanding loan shares for each bank-firm pair in quarter t. Hence, the level of observation in this study is bank-firm-quarter.

Bank-level data DealScan is complemented by lender information from Compustat. Because DealScan and Compustat do not share a common identifier, I use the linking table made available by Michael Schwert (2018). I exclude all observations with negative or zero values in total assets or total debt from banks' balance sheets. Requiring bank-level information for the subsequent estimation, the sample is reduced to 50 lenders and 2,980 borrowers.

Trade data To construct firms' and subsequentially also banks' exposure to the China shock, I retrieve trade data from the UN Comtrade database that provides bilateral trade flows at the 6-digit HS product level. I concord these trade flows to 4-digit SIC industries using a crosswalk provided by the World Bank. Standard industry classifications allow merging trade flows with loan-level information. Following Autor et al. (2013), the sample period covers the years 1991 until 2007. Just as in their work, the availability of data not only for US-China trade but also for trade between China and other developed countries determines the start of the sample period. The sample ends in 2007 to avoid the inclusion of the global financial crisis.

Firm-level data For descriptive statistics as well as the subsequent analysis at the firm level, I use firm characteristics from Compustat. To merge firm-level data with loan-level information, I rely on the matching table provided by Michael Roberts, which builds on the work by Chava and Roberts (2008). Combining DealScan and Compustat reduces observations due to the limited availability of borrower information in Compustat. Therefore, the firm-level analysis can only be conducted for a subsample. I drop firm-quarters with negative or zero values in total assets or total debt. Moreover, firm-quarters in which annual growth in assets or sales is more than 100% are excluded to account for mergers and acquisitions. The sample, for which I can construct firms' exposure as well as have firm characteristics available, encompasses

[4] This approach is based on the assumption that loans are not repaid early, restructured, or renegotiated.

1,945 firms.

3.2 Variable construction

Banks' exposure To implement the outlined empirical approach, I first need to construct a measure that captures US firms' exposure to the trade shock. I exploit cross-industry variation in US firms' exposure to China and create a measure per industry on the basis of bilateral trade data between the United States and China. In its construction, I follow Bloom et al. (2016) as well as Acemoglu et al. (2016) and consider the change in imports from China to the United States normalized by total US imports at the 4-digit industry level:

$$\Delta\text{Import exposure}_j^{US} = \frac{\Delta\text{US Imports from China}_{j,1991-2000}}{\text{Total US imports}_{j,1991}}. \tag{3}$$

As Federico et al. (2020), I use only the pre-shock period for the calculation of this measure. On this basis, I construct a continuous measure of banks' indirect exposure to the China shock as the average share of loans bank b grants to industry j in the pre-shock period to its total loans weighted by the industry's import exposure:[5]

$$\text{Exposure}_b^{US} = \frac{\sum_{j=1}^{N}\left(\frac{\text{Loans}_{b,j}}{\text{Loans}_b} \times \Delta\text{Import exposure}_j^{US}\right)}{N}. \tag{4}$$

N is the total number of industries bank b lends to.

A key concern for the subsequent estimation is that trade flows may be related to unobserved US product demand shocks which, in turn, could correlate with bank lending and, thus, affect the causality of the underlying estimation. The instrumental variable approach applied to alleviate such concern isolates the supply component in the rise of import competition from China. I instrument $\mathit{Exposure}_b^{US}$ by $\mathit{Exposure}_b^{EO}$ in which the share of loans is weighted by an import exposure calculated on the basis of changes in the trade flows between eight other developed countries (Australia, Denmark, Finland, Germany, Japan, New Zealand, Spain, and Switzerland) and China, $\Delta\mathit{Import\ exposure}_j^{EO}$, standardized by total US imports in 1989. The underlying rationale behind this is that high-income economies are assumed to be similarly affected by the rise in import competition from China. Figure 1 illustrates that the increase in import competition in the United States is indeed concentrated in the same set of

[5] This abstracts from banks possibly being exposed to the trade shock via their invested capital or other types of lending.

industries as in the eight other developed countries. It plots the relationship between the change in US imports from China and the change in imports of eight other countries from China per 4-digit SIC industry. Moreover, I use total US imports from 1989 because, to the degree that contemporaneous imports are affected by anticipated China trade, this will mitigate a potential simultaneity issue.

[Figure 1]

The exclusion restriction implied by this instrumental variable approach is that the instrument does not have a direct influence on bank lending, other than its impact through industry exposure to Chinese import competition. One potential threat to this could be that product demand shocks are correlated across developed countries. While it cannot be systematically shown that product demand shocks are not the common driver behind the rise of imports in all developed countries, evidence suggests that the surge in imports from China is driven by internal developments within China (Autor et al., 2013; Autor et al., 2016; Hombert and Matray, 2018). Moreover, I exclude sectors that can be considered subject to another economic shock that potentially correlates with China trade in a robustness check displayed in Columns (2), (3), and (4) in Table A7. Alternatively, negative productivity shocks within the United States could drive imports from China in the United States as well as in the other developed countries if Chinese imports replace local production and US imports abroad. However, this scenario seems unlikely given the increase in imports in a variety of industries as well as evidence that it is rather China's own productivity driving the import growth from China (Autor et al., 2013; Feigenbaum and Hall, 2015).

Sectoral specialization To measure banks' sectoral specialization, I rely on the approach by Paravisini et al. (2020) and consider bank b to be specialized in industry j if its average share of loans over the pre-shock period is a right-tail outlier relative to the other banks' portfolio shares in industry j. More specifically, bank b is specialized in industry j if lending to industry j relative to its total lending is larger than the sum of the 75th percentile and the 1.5 interquartile range of the distribution of banks' portfolio shares in industry j:

$$\text{Specialized}_{b,j} = \begin{cases} 1, & \text{if } \frac{\text{Credit}_{b,j}}{\text{Credit}_b} >= \text{75th pctl.} + \text{1.5 IQR of } \frac{\text{Credit}^*_j}{\text{Credit}^*} \\ 0, & \text{otherwise.} \end{cases} \tag{5}$$

To define sectoral specialization on the basis of a pre-shock average has the advantage that less weight is put on banks sporadically specializing in certain industries that could simply be caused by a single large loan or low lending activity in a specific sector at one particular point in time (Giometti and Pietrosanti, 2022).[6] Moreover, this approach is corroborated by the fact that sectoral specialization is shown to be highly persistent across time (Blickle et al., 2021; Giometti and Pietrosanti, 2022).

3.3 Descriptive statistics and parallel trends

Table 1 contains the definitions of all variables used in this analysis, and Table A1 in the Appendix comprises corresponding summary statistics.

[Table 1]

Banks' exposure Figure 2 shows the distribution of banks' *ex-ante* exposure. Around 10% of the banks under study have zero exposure. The remaining share of banks exhibits at least some degree of exposure with a bunching between slightly larger than zero and 0.1. Banks' median exposure is 0.2. The highest exposure is 3.3. Hence, banks' lending portfolios result in some credit institutions with zero exposure, many banks with low or moderate exposure, and a few lenders with high exposure.

[Figure 2]

Sectoral specialization When defining specialization at the 2-digit SIC level, more than 80% of the industries exhibit at least one bank that specializes in them. Table 2 displays an overview of the 62 2-digit industries in the sample and the number of banks specialized in it. On average, there are roughly two banks specialized in each industry. This is broadly in line with the evidence by Giometti and Pietrosanti (2022), who show at a more aggregated level that the US syndicated loan market is characterized by sectoral specialization.

[Table 2]

[6] I show that results hold when specialization is determined on the basis of banks' lending activities two years before the trade shock in Column (2) in Table A7.

Parallel trends Central for the validity of any difference-in-differences design is that treatment and control group would follow similar trends in absence of treatment. However, given the continuous nature of treatment, it is challenging to display this graphically. In order to still assess whether this is the case, I report pre-shock average percentage changes of relevant variables for exposed and non-exposed banks respectively. Banks are exposed (non-exposed) if their exposure to the trade shock is above (below) the median bank exposure. Bank and firm characteristics are winsorized at the 99th percentile or the first and 99th percentile. Following Imbens and Wooldridge (2009), I report normalized differences by treatment status (exposed and non-exposed banks) in Table 3. A normalized difference between $\pm$ 0.25 indicates that groups are not systematically different and linear regression methods are adequate.

[Table 3]

Panel A in Table 3 presents differences by treatment at bank level. Exposed and non-exposed banks exhibit largely similar trends before the trade shock. The only characteristic where the difference is slightly outside the $\pm$ 0.25 interval is total assets. Exposed banks grow slightly more than non-exposed banks. Reassuringly, I control for banks' size in the regression equations. Panel B illustrates differences in loan and firm characteristics by treatment at the bank-firm level. Importantly, credit made available by exposed and non-exposed banks develops sufficiently equal in the pre-shock period. This applies both to loan as well as to facility volumes. Moreover, this applies also to percentage changes in the average spread charged as well as in the average maturity agreed upon. To ensure that exposed and non-exposed banks do not lend differentially to firms with divergent development, I also consider differences in firm characteristics. However, exposed and non-exposed banks lend to firms that follow sufficiently similar trends before the shock.

The evidence of this exercise is corroborated by running placebo regressions in which the shock is simulated to hit at different points in time over the pre-shock period. Figure 3 plots estimates for $Exposure_b^{US} \times Post_t$ and 95% confidence intervals for regressions in which I define 12 placebo events between Q1 1995 and Q4 1997. I find insignificant effects in each placebo regression. Overall, I do not find evidence that suggests that the two groups would not follow parallel trends in the absence of treatment.

[Figure 3]

4 Results

Table 4 presents results from estimating Equation (1). Estimation is conducted via OLS in Columns (1) as well as (2) and via 2SLS in Columns (3) and (4). I cluster standard errors at the bank and 4-digit industry levels. Columns (1) and (3) report results without bank controls included in the estimation while Columns (2) and (4) show results with controls. In all four columns, the coefficient of interest β_1 is negative and statistically significant. This identifies that after the shock banks react to an increase in exposure with a larger reduction in outstanding credit. After the shock, a bank with exposure at the 75th percentile reduces credit between 11 and 26 percentage points more, depending on the specification, compared to a bank with exposure at the 25th percentile.

[Table 4]

This confirms the results by Federico et al. (2020) and provides further evidence on the role of financial frictions in the adjustment processes in response to a trade shock. Banks are indirectly exposed to trade shocks via their composition of loans. This results in larger declines in credit supply for banks with higher exposure. This can impede factor reallocation in the economy and, thereby, lead to larger unrealized gains from trade. The results seem to be economically meaningful. Nevertheless, the research design as well as the particularities of the syndicated loan market and its participants give reasons to believe that the effect may be even stronger in other settings. First, the effect measures the response of the lead arranger only while I abstract from potential adjustments made by the other syndicate participants. Second, banks active in this market are very large which generally have a lower sensitivity of lending to financial constraints (Paravisini, 2008). Therefore, their reactions are expected to be weaker than the ones by smaller banks. Third, while the volume of syndicated lending is substantial, it does not cover all of the commercial lending in the United States, nor does DealScan cover the entire syndicated loan market. Hence, I may underestimate the overall reduction in lending.

Furthermore, the results in Columns (5) and (6) confirm another aspect of the findings by Federico et al. (2020). In Columns (5) and (6), Equation (1) is estimated on the sub-sample of non-exposed and exposed firms respectively. Borrowers are considered to be non-exposed (exposed) if they have an exposure smaller (larger) than the median. The results illustrate that banks react to an increase in exposure with a larger reduction in lending to both types of firms.[7] Unaffected firms are not expected to experience less credit supply because they are the firms that should rather expand economic activity and absorb more resources as they are not subject to competition from China. However, the trade shock seems to transmit from exposed firms to connected banks and then feed back to non-exposed firms as well. Hence, this can be considered as evidence hinting toward negative financial spillovers onto non-exposed firms.

While painting a rather gloomy picture so far, the results hide large heterogeneity in terms of sectoral specialization. Column (1) in Table 5 displays the results from estimating Equation (2) via 2SLS and with control variables. It uncovers how important the role of sectoral specialization really is in the context of trade liberalization - when considering not only the fact that it is the underlying reason why banks are exposed to different degrees but also whether the borrowers under consideration are part of industries banks are specialized in. The higher banks' exposure, the more they reduce credit when lending to borrowers that are part of an industry that banks are not specialized in. More specifically, a bank lending to an industry in which it is not specialized with exposure at the 75th percentile reduces credit supply by 77 percentage points more after the shock than a bank with exposure at the 25th percentile. In contrast, banks with different exposures do not adjust credit supply differentially after the shock when the borrower is part of an industry that the bank is specialized in. Hence, bank specialization does not only play an important role in determining banks' exposure to a trade shock but also in the allocation of credit after the shock. This confirms that the results by De Jonghe et al. (2020) apply also in the context of a trade shock hitting US borrowers. Moreover, it provides further evidence that banks protect industries in which they have built-up an information advantage and have invested resources in to do so (Jahn et al., 2013; Giometti and Pietrosanti, 2022; De Jonghe et al., 2020).

[Table 5]

A possible concern in this context may be that specialized bank-industry pairs generally develop differently than non-specialized bank-industry pairs. Therefore, I replicate Table 3 but now compare bank, loan, and firm characteristics depending on whether they belong to specialized or non-specialized bank-industry pairs. Table 6 shows that banks, lending, and firms connected to these two groups follow the same

[7] While coefficients are slightly different in magnitude, they are not statistically different from each other.

trends prior to the trade shock. Moreover, it is conceivable that banks do not adjust lending to the industries they are specialized in but at the expense of extracting higher rents. This would challenge the view that banks shield important industries but rather exploit them. However, I do not find any evidence that this is the case (Table A3).

[Table 6]

As for whether a firm is exposed or not, it is conceivable that banks only shield industries that are unaffected by the shock and hence more likely to expand but possibly not those affected as their prospects are inherently uncertain due to the increased import competition from China. Therefore, I proceed in a similar vein as before and estimate Equation (2) for the subs-sample of non-exposed and exposed firms separately. Columns (2) and (3) in Table 5 report the results when lending to non-exposed and exposed firms is considered respectively. In both samples, β_3 is positive and statistically significant providing evidence for a differential role of sectoral specialization. However, coefficients are not statistically different from each other. Hence, banks protect borrowers that are part of an industry in which they are specialized, independent of the firms' exposure. This underpins how important these industries are to the banks and, thus, how important the differentiation in terms of sectoral specialization is in this analysis. Banks protect industries in which they have acquired an information advantage via specialization irrespective of firms' worsened prospects due to higher import competition.

5 Robustness checks

For robustness, I re-estimate Equation (1) with modifications along several dimensions.

Alternative exposure measures I employ alternative definitions of banks' exposure to show that results stay qualitatively unchanged when exposure is differently constructed. Table A4 in the Appendix displays the results: In Column (1), I weight banks' share of loans per industry by the change in imports over the full sample period instead of using only the pre-shock period; in Column (2), I use the average level of imports over the pre-shock period as a weight for banks' share of loans (Bernard et al., 2006; Bloom et al., 2016; Hombert and Matray, 2018); in Column (3), I weight the loan share by the change in net imports instead of only by imports (Autor et al., 2013); in Column (4), I adapt Federico et al. (2020)'s way of proceeding and use the change between pre- and post-shock averages as a weight.

Alternative model specifications Next, I illustrate that the results do not depend on the particular specification used. First, I show in Table A5 that results are unchanged when clustering standard errors at the bank level (Column (1)), 4-digit industry level (Column (2)), bank-firm level (Column (3)), and bank-time level (Column (4)).

Second, Table A6 demonstrates that results are qualitatively unchanged when including loan controls (Column (1)); using industry-location-size-time fixed effects to absorb loan demand (Column (2)); adding banks' country of incorporation-time fixed effects to control for any time-varying shocks in banks' home countries (Column (3)); excluding the two years before the shock from the construction of the exposure measure to make sure that results are not driven by anticipation (Column (4)).

Third, Table A7 shows that results are robust when collapsing the time dimension to check for the presence of serial correlation (Column (1)); dropping steel, glass, and cement industries (Column (2)), as well as computer industries (Column (3)) as in these areas, demand shocks could be correlated across developed countries and therefore invalidate our instrumental variable strategy; dropping footwear, apparel, and textile industries to illustrate that results are not driven by industries in which China plays a dominant role (Column (4)).

Securitization Loan securitization poses two distinct challenges for my estimation set-up. First, if banks sell their share after origination, $Exposure_b^{US}$ is not an adequate measure of a bank's exposure to its borrowers over the duration of these loans.[8] Second, if banks exposed to China engage in loan securitization to varying degrees, results may be biased. To address these two concerns, I identify loans that are especially likely to be securitized and exclude them from the sample Previous literature has shown that it is, in particular, Term B loans as well as loans by syndicates in which at least one Collateralized Loan Obligation participates that have a higher likelihood to be sold (Benmelech et al., 2012; Nadauld and Weisbach, 2012). Column (1) in Table A8 excludes these loans from the sample but results remain robust.

DealScan particularities Table A8 indicates that the results are additionally robust to alternative approaches how loan proportions are allocated to the syndicate members, to the type of loan considered, as well as to the lead arranger definition employed. So far, I split each facility's volume according to the breakdown provided by DealScan or, if not available, follow the procedure in De Haas and Van Horen (2013) and assume

[8] Blickle et al. (2020) show that if banks sell their share, they do so shortly after loan origination.

that each syndicate member contributed the same amount to the facility. Now, I use the 'alternative rule' by De Haas and Van Horen (2013) in Column (2) if no breakdown is available and allocate half of each facility's volume to the lead arrangers and half to the other participants. Among each group, the facility volume is then distributed equally. Moreover, I also use the allocation procedure by Schwert (2018) in which the lead arranger(s) retain the entire loan on its (their) balance sheet in Column (3). To check whether the results vary with the type of loan, I keep only term loans in Column (4) and only credit lines in Column (5). Last, I confirm in Column (6) that results do not depend on the lead arranger definition used. Here, I employ the definition by Ivashina (2009).

Alternative definitions of sectoral specialization I test the robustness of the results from Equation (2) to securitization and alternative ways of how sectoral specialization is defined. First, if banks that engage in specialization securitize loans differently than banks that do not engage in specialization, then results from Equation (2) might not capture the effect of the trade shock. Therefore, Column (1) in Table A9 excludes loans that are especially likely to be securitized. Second, the specialization dummy in Column (2) is constructed on the basis of banks' average lending activities in the two years prior to the trade shock instead of on the average lending activities in the pre-shock period. Third, I modify the binary indicator for sectoral specialization according to Paravisini et al. (2020) and define it alternatively by a share of credit larger than the 90th percentile of all banks' distribution. Column (3) reports the results of this modification. Fourth, I exchange the binary indicator with a continuous one, which is the average pre-shock lending by bank b to industry j to bank b's total loan volume. This essentially corresponds to the measure used by De Jonghe et al. (2020) and Blickle et al. (2021). Column (3) shows that the results remain unchanged.

Alternative allocation channels I illustrate that the allocation effects according to banks' sectoral specialization do not pick up other types of banks' portfolio choices. Therefore, I conduct horse races between other possible portfolio choices and sectoral specialization by including an additional interaction between $Exposure_b^{US} \times Post_t$ and these other possible allocation channels. First, I test whether the results are driven by firms' exposure to trade shock by including an additional interaction with $Exposed_f$. Column (1) in Table A10 demonstrates that this is not the case.

Second, previous studies point out that banks that specialize in an industry might also have a relatively high share of credit in that industry, that is providing a relatively larger share of credit compared to the total loan volume granted to that industry (Giometti and Pietrosanti, 2022; De Jonghe et al., 2020; Giannetti and Saidi, 2019). Banks' market share is the average pre-shock share of loans to industry j relative to the average loan volume extended to industry j by all banks. Column (2) shows that the results on sectoral specialization are independent of banks' market share.

Third, sectoral specialization could correlate with firm characteristics (De Jonghe et al., 2020). Testing the role of firm characteristics jointly with the role of sectoral specialization allows me to rule out that the results are driven by specific types of firms in the industry a bank is specialized in. Irrespective of whether $Exposure_b^{US} \times Post_t$ is additionally interacted firms' average pre-shock leverage or external dependence, the results hold (Columns (3) and (4)).

6 Real effects: Firm sales and investment

This paper has shown that banks adjust credit supply when hit by a trade shock via their lending relationships. The higher a bank's exposure to the shock, the more it reduces its credit supply. Moreover, I illustrate that banks adjust their credit supply differentially depending on their sectoral specialization. In this section, I investigate how this translates to the real economy. To identify how banks' adjustments affect firm-level outcomes, I first need to construct a measure that captures firms' exposure to the bank lending channel of the trade shock (Federico et al., 2020). Therefore, I weight the average share of firm f's credit from bank b over the pre-shock period by bank b's exposure to the trade shock. To arrive at a firm-specific measure to be used in the estimations, I average the product across banks:

$$\text{Firm exposure}_f^{US} = \frac{\sum_{b=1}^{N}(\frac{\text{Loans}_{f,b}}{\text{Loans}_f} \times \text{Exposure}_b^{US})}{N}. \tag{6}$$

N is the number of banks firm b borrows from. An exposure of zero indicates that firm f borrows only from non-exposed banks. I also assign an exposure of zero to firms that were not active on the syndicated loan market before the shock, since these firms are, by construction, not exposed to the bank lending channel of the trade shock (see Gropp et al. (2019) for a similar proceeding).

I employ this measure in the following regression equation to estimate how exposure to the bank lending channel of the trade shock affects real outcomes:

$$
\begin{aligned}
\mathrm{Y}_{f,t} = \eta & \text{Firm exposure}_f^{US} \times \text{Post}_t \\
& + \ \theta \mathrm{X'}_{f,t} + \iota_f + \iota_{j,s,t} + \varepsilon_{f,t}.
\end{aligned} \tag{7}
$$

As the dependent variable $Y_{f,t}$, I employ two measures: growth in sales and fixed assets. The vector $X'_{f,t}$ contains time-varying firm controls such as size (log of total assets), leverage (the ratio of total debt to total assets), and a proxy for the amount of trade credit a firm receives (the ratio of accounts payable to the cost of goods sold as in Raddatz (2010)).[9] As in the previous set-up, all variables are winsorized at the 99th or the first and 99th percentile. The estimation set-up includes firm fixed effects(ι_f) as well as industry-state-time fixed effects ($\iota_{j,s,t}$), which absorb the single terms $\textit{Firm exposure}_f^{US}$ and $\textit{Post}_t$. To isolate the causal effect of firms' exposure to the bank lending channel, I proceed by instrumenting $\textit{Firm exposure}_f^{US}$ with a measure in which $\textit{Exposure}_b^{EO}$ is used as a weight for a firm's share of loans from a specific bank. Hence, η captures the extent to which the shock to banks is transmitted to the firm level via adjustments in bank lending. First-stage results are reported in Table A11.

Table 7 reports the results from estimating Equation (7) on the reduced sample for which firm-level information is available. Standard errors are clustered at the industry level.[10] It illustrates that firms with larger exposure to the bank lending channel experience a larger reduction in the growth of sales (Column (1)) as well as fixed assets (Column (2)). Thus, the higher the exposure to the bank lending channel of the trade shock, the more firm outcomes worsen after the shock.

[Table 7]

To investigate whether the differential response according to sectoral specialization translates into heterogeneous developments at the firm level, I construct a firm-specific measure that captures the number of banks a firm is connected to that are specialized in it its industry. I transform the continuous indicator into a binary variable, $\textit{High}_f$, to simplify interpretation.[11] It assumes a value of one if a firm is connected to at least one bank that is specialized in its industry. I re-run Equation (7) with an interaction between $\textit{High}_f$ and $\textit{Firm exposure}_f^{US} \times \textit{Post}_t$.

[9] In unreported results, I show that the results stay qualitatively the same when firm controls are not included in the regression.

[10] Results are robust to clustering at the firm-industry level.

[11] The results still hold when the continuous indicator is used as well as when the share of specialized banks to total banks is employed.

Columns (3) and (4) in Table 7 display the results for the growth in sales and fixed assets respectively. The estimation results provide evidence that the allocation of credit according to banks' sectoral specialization has real effects. Growth in sales and fixed assets are affected differently depending on whether a firm is connected to specialized banks. More specifically, firms' outcomes are less negatively affected after the shock when they are connected to specialized banks compared to firms that are not connected to specialized banks. This contrasts with the findings by De Jonghe et al. (2020), who do not find any allocation effects in terms of sectoral specialization at the firm level when considering a financial shock.

7 Conclusion

I analyze how banks adjust credit supply when hit indirectly by a trade shock via their loan portfolios. I rely on detailed loan-level data combined with bank characteristics and information on industry-level trade flows. This allows constructing a bank-specific proxy for how much banks are exposed to the shock based on their lending portfolio and industry weights.

Focusing on the accession of China to the WTO as the trade shock under study, I identify that banks with higher ex-ante exposures to the trade shock respond with large reductions in credit to US borrowers after the shock. This provides further evidence on the role of financial frictions when analyzing the effects of trade liberalization on economic activity. Facing credit constraints themselves, banks reduce the availability of credit to their borrowers in response to a trade shock. Moreover, this reduction in credit is not limited to the group of exposed firms but also concerns non-exposed firms. This confirms previous evidence that financial spillovers take place in this context (Federico et al., 2020).

Moreover, I uncover important heterogeneity in banks' reactions when considering the role of sectoral specialization in the allocation of credit. Banks shield borrowers that are part of an industry in which they are specialized. In contrast, when lending to a borrower that is not part of such industries, banks increasingly reduce credit with higher exposures. This behavior is not confined to the group of non-exposed firms, which could be expected, but it also applies to firms negatively affected by increased import competition from China.

Banks' adjustments have important implications for firm outcomes. Firms that borrow more from banks with larger exposures experience larger reductions in their growth in sales and fixed assets. Receiving credit from specialized banks helps dampen this negative effect on firm outcomes. Hence, I uncover evidence that banks' heterogeneous responses in terms of sectoral specialization transmit to the real economy.

These findings provide valuable inputs for accounting the gains from trade liberalization and therefore allow for a more informed design of such policies. Considering financial frictions unveils banks' adverse reactions to trade liberalization, restraining the reallocation of factors across firms. The findings also contribute to the debate on portfolio specialization versus diversification by shedding more light on the complex implications of portfolio specialization. On the one hand, the applied approach contemplates that the more specialized a bank is in industries directly exposed to the trade shock, the higher their exposures to the shock. On the other hand, banks try to protect the industries in which they specialize.

There are a few limitations that should be kept in mind when interpreting the results. External validity may be limited given that the results cannot speak to effects in other lending markets. Nevertheless, the syndicated loan market represents an important part of banks' total lending (Doerr and Schaz, 2021). Moreover, I do not investigate why some banks choose to lend more to some industries than to others, i.e. specialize in the first place. These lending patterns may result from differences in business models, history, or preferences. Understanding what incentivizes banks to specialize and how policy can impact this, could, however, be informative for further plans to liberalize trade.

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Tables and figures

Table 1: Variable definitions

Variable name	Description
Loan characteristics	
Credit	Outstanding loan volume in US$ million between bank b and firm f in quarter t
Facility	Outstanding facility volume between bank b and firm f in quarter t
Spread	Average spread of loans in basis points over Libor between bank b and firm f in quarter t
Maturity	Average maturity of loans in months between bank b and firm f in quarter t
Bank characteristics	
Exposure^{US}	Banks' exposure based on pre-shock lending shares weighted by US import exposure to China
Exposure^{EO}	Banks' exposure based on pre-shock lending shares weighted by import exposure by eight other developed countries to China
Specialized	A dummy variable indicating whether a bank is specialized in the industry of the respective borrower (*Specialized*=1) or not (*Specialized*=0)
Total assets	Total assets in US$ billion
Size	Log of total assets
Deposits	Deposits to total assets
ROA	Net income to total assets
Leverage	Short-term debt divided by total assets
Equity	Common equity divided by total assets
NPA	Non-performing assets to total assets
Market	Banks' average pre-shock share of loans to industry j to the total loan volume granted to industry j by all banks
Firm characteristics	
ΔImport exposure^{US}	Change in US imports from China over pre-shock period divided by beginning of period total US imports
ΔImport exposure^{EO}	Change in imports from China by eight other developed countries over pre-shock period divided by total US imports in 1989
Exposed	A dummy variable indicating whether a firm is part of an industry that has an ΔImport exposure^{US} above (*Exposed* = 1) or below (*Exposed* = 0) the median
Pre-Lev	Pre-shock average of *Firm leverage*
Pre-Ext. dep.	Pre-shock average of firms' external dependence defined as capital expenditures minus cash flow from operations divided by capital expenditures
Firm exposure^{US}	Firms' exposure to the bank lending channel of the trade shock based on pre-shock borrowing shares weighted by banks' exposure, which is constructed on US import exposure to China
Firm exposure^{EO}	Firms' exposure to the bank lending channel of the trade shock based on pre-shock borrowing shares weighted by banks' exposure, which is constructed on import exposure by eight other developed countries to China
Firm total assets	Firms' total assets in US$ million
Firm size	Log of total assets
Firm ROA	Income before extraordinary items divided by total assets
Firm Tobin's Q	Total assets minus book equity plus market capitalization divided by total assets
Firm sales	Sales divided by total assets
Firm fixed	Fixed assets to total assets
Firm leverage	Total debt divided by total assets
Firm trade credit	Account payable divided by cost of goods sold
Growth in fixed	Annual growth of fixed assets
Sales growth	Annual growth of sales
High	A dummy variable indicating whether a firm is borrowing from a share of specialized above (*High*=1) or below (*High*=0) the median

Table 2: Number of specialized banks per 2-digit SIC industry

2-digit SIC	*No. of banks*	*2-digit SIC*	*No. of banks*
1	1	40	1
2	0	41	0
7	0	42	0
8	0	44	2
10	1	45	1
12	1	46	1
13	3	47	2
14	1	48	0
15	2	49	2
16	0	50	3
17	2	51	1
20	3	52	2
21	2	53	3
22	2	54	1
23	1	55	3
24	3	56	1
25	2	57	1
26	2	58	4
27	3	59	2
28	1	70	1
29	2	72	3
30	1	73	2
31	1	75	1
32	1	76	0
33	3	78	1
34	2	79	1
35	5	80	5
36	1	82	0
37	2	83	0
38	3	86	4
39	3	87	2

Note: This table displays the number of banks specialized in each of the 62 2-digit SIC industries in the regression sample. A bank is considered to be specialized (*Specialized*=1) in a borrower's industry if its share of loans is a right-tail outlier relative to other banks' share in the industry, as defined in Equation (5).

Table 3: Parallel trends

	Exposed banks		*Non-exposed banks*		*Exposed - Non-exposed*
	Mean	SD	Mean	SD	Normalized diff.
Panel A: Bank level					
ΔTotal assets	21.885	11.683	17.220	11.527	0.28
ΔDeposits	-1.435	1.218	-1.164	2.477	-0.10
ΔROA	2.787	53.988	-0.267	49.467	0.04
ΔLeverage	7.686	18.426	5.124	9.513	0.12
ΔEquity	-2.552	51.937	-7.712	50.912	0.07
ΔNPA	-4.943	16.619	-4.273	12.451	-0.03
Panel B: Bank-firm level					
Loan characteristics					
ΔCredit	5.338	44.219	23.562	89.833	-0.18
ΔFacility	8.116	50.912	29.063	228.173	-0.09
ΔSpread	4.689	62.987	3.273	25.244	0.02
ΔMaturity	3.773	13.241	4.508	28.603	-0.02
Firm characteristics					
ΔFirm total assets	12.442	15.288	12.657	14.263	-0.01
ΔFirm ROA	-38.836	210.370	-33.650	190.395	-0.02
ΔFirm Tobin's Q	3.143	19.812	2.470	13.364	0.03
ΔFirm sales	2.548	17.884	1.208	12.369	0.06
ΔFirm fixed	5.924	17.604	2.910	20.372	0.11
ΔFirm leverage	31.370	112.172	22.786	87.690	0.06

Note: This table reports statistics for relevant variables as their annual percentage changes (in %) averaged over the pre-shock period (Q1 1991 until Q3 2001) dividing the sample between exposed and non-exposed banks. Exposed (non-exposed) banks have a share of loans to industries subject to competition from China above (below) the median. Panel A reports banks' characteristics at the bank level. Panel B displays loan and firm characteristics at the bank-firm level. Firm characteristics are based on a reduced sample due to limited data availability. For detailed variable definitions, see Table 1.

Table 4: The effect of bank exposure on lending

	(1) OLS	(2) OLS	(3) IV	(4) IV	(5) Non-exposed (IV)	(6) Exposed (IV)
Exposure^{US} $\times$ Post	-0.163**	-0.158**	-0.196***	-0.389**	-0.407**	-0.351*
	(0.078)	(0.078)	(0.069)	(0.153)	(0.192)	(0.179)
Exposure^{US} $\times$ Post$\big\|_{25\to75}$	-0.108**	-0.105**	-0.148***	-0.258**	-0.269**	-0.232*
	(0.052)	(0.052)	(0.043)	(0.101)	(0.127)	(0.119)
Observations	116,898	116,898	116,898	116,898	76,901	39,997
Bank controls	No	Yes	No	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of banks	50	50	50	50	49	42
Number of firms	2,980	2,980	2,980	2,980	1,975	1,005
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The estimation is conducted on the sub-sample of non-exposed firms in Column (5) and on the sub-sample of exposed firms in Column (6). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO} from Column (3) onward. *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: The role of specialization (2SLS)

	(1) All	(2) Non-exposed	(3) Exposed
Exposure^{US} × Post	-1.165**	-0.849*	-1.772**
	(0.454)	(0.467)	(0.799)
Exposure^{US} × Post × Specialized	1.058**	0.803*	1.624*
	(0.461)	(0.474)	(0.807)
Post × Specialized	-0.014	-0.081	0.080
	(0.084)	(0.078)	(0.155)
Exposure^{US} × Post$\vert_{25\rightarrow 75}$ if Specialized=0	-0.771**	-0.562*	-1.173**
	(0.301)	(0.309)	(0.529)
Exposure^{US} × Post$\vert_{25\rightarrow 75}$ if Specialized=1	-0.071	-0.031	-0.098*
	(0.044)	(0.056)	(0.051)
Observations	116,898	76,901	39,997
Bank controls	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes
Number of banks	50	49	42
Number of firms	2,980	1,975	1,005
Clustering	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (2). Columns (2) and (3) are estimated on the sub-sample of non-exposed and exposed firms respectively. The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO} in all three columns. *Post* indicates the time period after China's entry into the WTO. The binary variable *Specialized* illustrates whether a firm operates in a sector in which the bank is specialized. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Normalized differences: Sectoral specialization

	Specialized pairs		*Non-specialized pairs*		*Specialized - Non-specialized*
	Mean	SD	Mean	SD	Normalized diff.
Bank characteristics					
ΔTotal assets	21.672	9.721	19.509	10.342	0.15
ΔDeposits	-1.822	1.356	-2.227	2.363	0.15
ΔROA	-1.772	39.574	2.620	48.037	-0.07
ΔLeverage	6.462	15.006	5.978	9.006	0.03
ΔEquity	19.530	73.149	23.457	68.872	-0.04
ΔNPA	-5.473	11.869	-9.357	17.944	0.18
Loan characteristics					
ΔCredit	29.918	221.659	22.380	78.875	0.03
ΔFacility	20.060	64.792	28.323	226.592	-0.04
ΔSpread	3.367	31.318	3.345	28.300	0.00
ΔMaturity	1.692	10.708	4.583	28.499	-0.09
Firm characteristics					
ΔFirm total assets	13.101	14.178	12.625	14.327	0.02
ΔFirm ROA	-59.622	189.451	-32.818	191.531	-0.10
ΔFirm Tobin's Q	0.112	10.608	2.616	13.932	-0.14
ΔFirm sales	0.099	13.897	1.334	12.683	-0.07
ΔFirm fixed	3.525	13.750	3.057	20.477	0.02
ΔFirm leverage	25.485	80.987	23.065	88.801	0.02

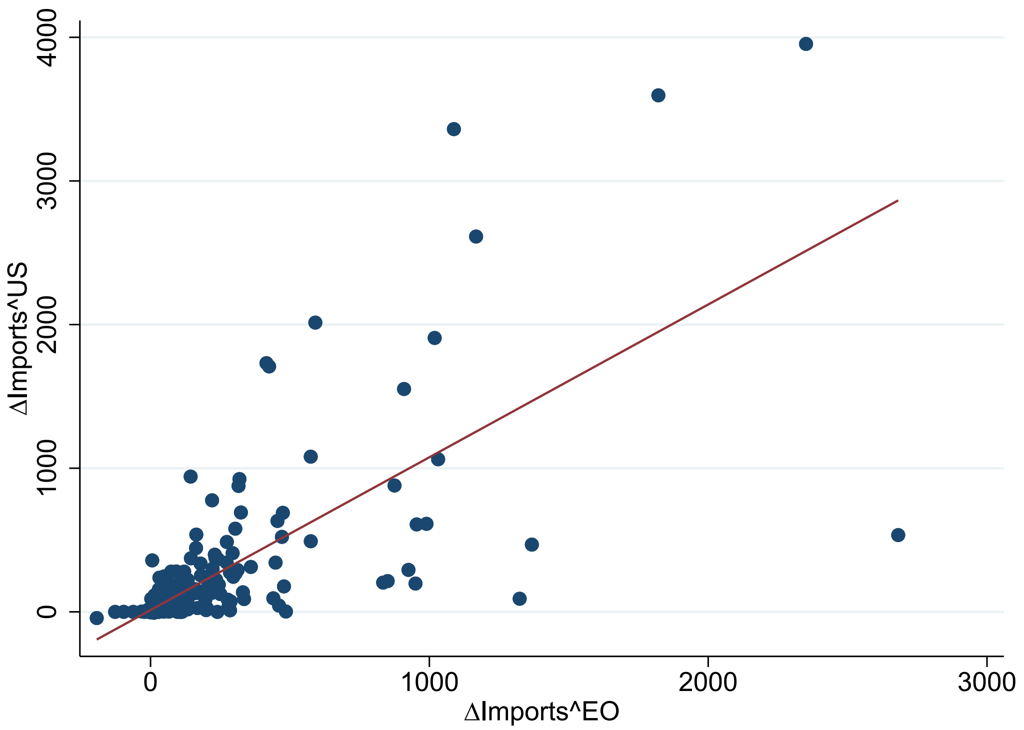
Note: This table reports statistics for relevant variables as their annual percentage changes (in %) averaged over the pre-shock period (Q1 1991 until Q3 2001) at the bank-firm level dividing the sample between specialized and non-specialized bank-industry pairs. A bank is considered to be specialized in a borrower's industry if its share of loans is a right-tail outlier relative to other banks' share in the industry, as defined in Equation (5). Firm characteristics are based on a reduced sample due to limited data availability. For detailed variable definitions, see Table 1.

Table 7: The effect of firms' indirect exposure on sales and fixed assets growth (2SLS)

	(1) Sales growth	(2) Growth in fixed	(3) Sales growth	(4) Growth in fixed
Firm exposure^{US} × Post	-0.217**	-0.358**	-0.788***	-1.543***
	(0.099)	(0.147)	(0.288)	(0.490)
Firm exposure^{US} × Post × High			0.595*	1.286**
			(0.319)	(0.513)
Post × High			0.008	-0.009
			(0.018)	(0.027)
Firm exposure^{US} × Post$\vert_{25\rightarrow75}$ if High=0	-0.002**	-0.004**	-0.009***	-0.018***
	(0.001)	(0.002)	(0.003)	(0.006)
Firm exposure^{US} × Post$\vert_{25\rightarrow75}$ if High=1			-0.002*	-0.003*
			(0.001)	(0.002)
Observations	71,567	68,472	71,567	68,472
Firm controls	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Industry-state-time FE	Yes	Yes	Yes	Yes
Number of firms	1,945	1,923	1,945	1,923
Clustering	Industry	Industry	Industry	Industry

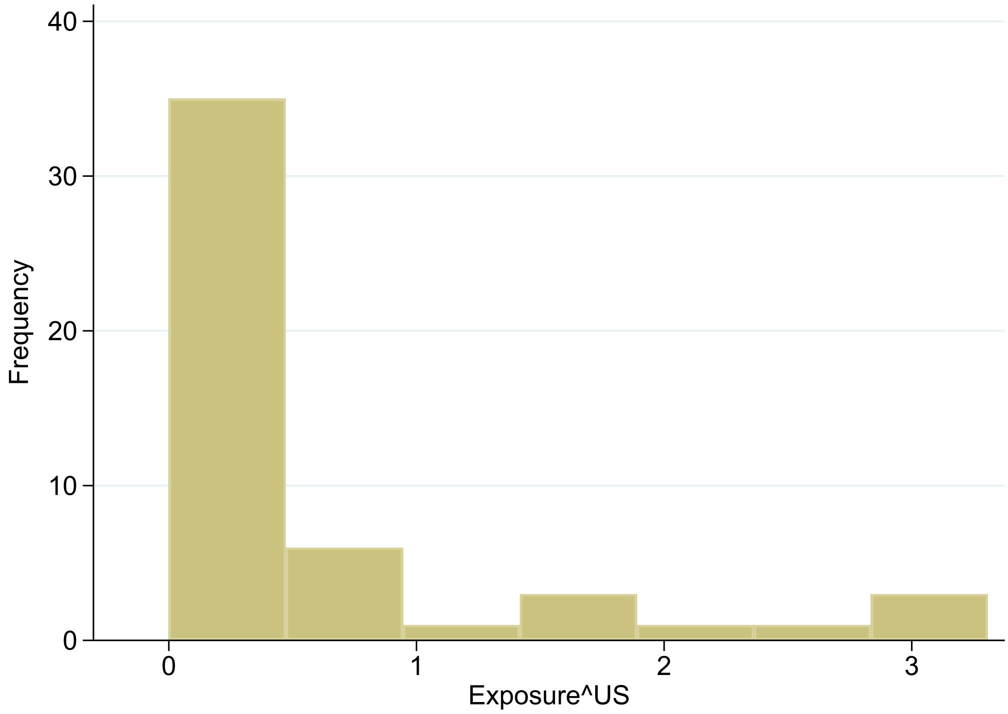
Note: This table investigates the effect of the bank lending channel of the trade shock and the differential role of sectoral specialization on firm-level outcomes as specified in Equation 7. In Columns (1) and (3), the annual growth of sales is the dependent variable. In Columns (2) and (4), the annual growth in fixed assets is used as the dependent variable. *Firm exposure*^{US} captures firms' indirect exposure to the trade shock as specified in Equation 6. It is instrumented with *Firm exposure*^{EO}. *High* assumes a value of one if firms borrow from a high share of specialized banks and zero otherwise. Firm controls include size, leverage, and trade credit. Each specification includes firm as well as 1-digit-industry-state-time fixed effects. Standard errors are clustered at the industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Imports from China to the United States and eight other developed economies



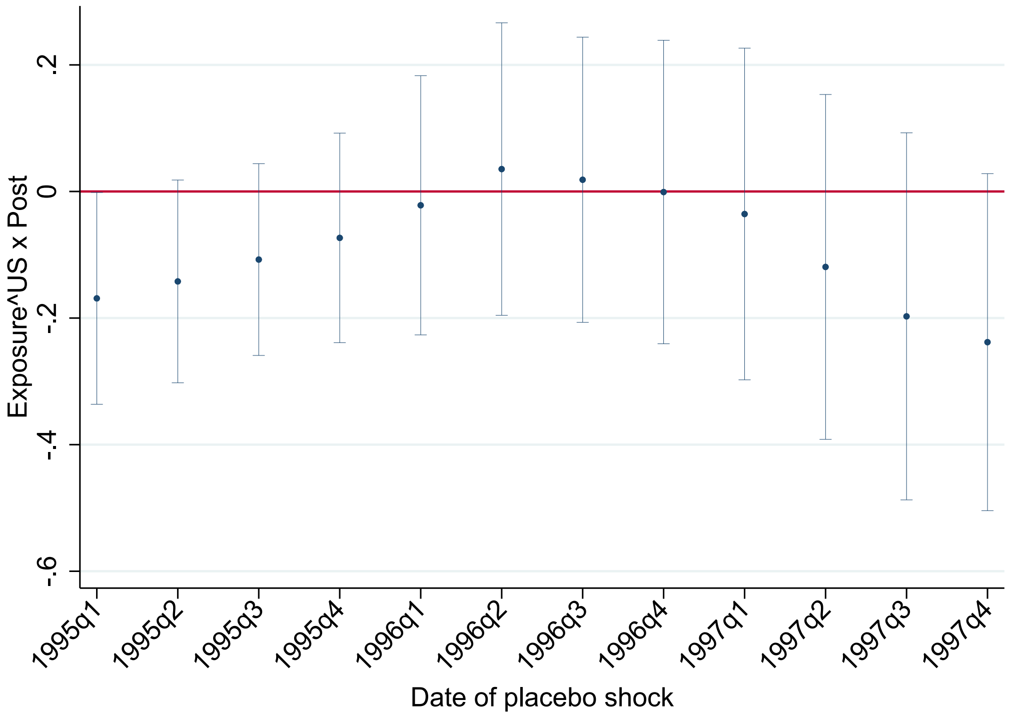
Note: This figure plots the change in imports of the United States and eight other developed countries from China by 4-digit SIC industry (N=407). Values are in US$ million.

Figure 2: The distribution of banks' exposure



Note: This figure shows the distribution of banks' ex-ante exposure to the trade shock, $Exposure^{US}$, at the bank level (N=50). It is constructed as in Equation (3).

Figure 3: Placebo tests (2SLS)



Note: This figure plots estimates of $Exposure^{US} \times Post$ and 95% confidence intervals from 12 placebo tests in which the shock is simulated to take effect in the pre-shock period, specifically between Q1 1995 and Q4 1997.

Appendix

Table A1: Summary statistics

	N	Mean	SD	p25	p50	p75
Credit	116,898	104.601	364.760	16.691	36.495	92.841
Post	116,898	0.507	0.500	0.000	1.000	1.000
Exposure^{US}	116,898	0.051	0.192	0.010	0.017	0.029
Exposure^{EO}	116,898	0.153	0.885	0.021	0.075	0.079
Total assets	116,898	171.470	85.057	82.423	214.102	246.361
ROA	116,898	0.003	0.002	0.002	0.003	0.004
Leverage	116,898	0.140	0.079	0.077	0.139	0.191
Deposits	116,898	0.574	0.108	0.500	0.579	0.650
Specialized	116,898	0.033	0.180	0.000	0.000	0.000
ΔImport exposure^{US}	116,898	0.078	0.238	0.000	0.000	0.033
ΔImport exposure^{EO}	116,898	0.149	1.710	0.000	0.000	0.023
Exposed	116,898	0.342	0.474	0.000	0.000	1.000
Equity	111,620	0.002	0.004	0.000	0.000	0.003
NPA	104,098	0.005	0.005	0.003	0.004	0.006
Spread	100,625	170.661	113.848	75.000	150.000	250.000
Maturity	96,213	60.381	27.581	42.000	60.000	72.000
Facility	116,898	720.986	2040.580	57.928	187.293	602.789
Market	116,898	0.192	0.177	0.031	0.137	0.313
Pre-Lev	88,118	0.386	0.222	0.241	0.352	0.471
Pre-Ext.dep.	86,050	2.920	128.995	-4.054	-1.740	0.020
Firm exposure^{US}	71,567	0.015	0.055	0.000	0.007	0.011
Firm exposure^{EO}	71,567	0.044	0.261	0.000	0.020	0.035
Sales growth	71,567	0.112	0.228	0.007	0.098	0.209
Gr. in fix. a.	68,712	0.107	0.281	-0.014	0.064	0.178
High	71,567	0.099	0.298	0.000	0.000	0.000
Firm total assets	71,567	2700.463	7839.572	157.916	552.932	1943.968
Firm ROA	70,009	0.007	0.035	0.002	0.011	0.021
Firm Tobin's Q	63,977	1.710	1.040	1.125	1.407	1.927
Firm sales	71,567	0.317	0.216	0.172	0.275	0.398
Firm fixed	71,477	0.355	0.238	0.159	0.298	0.528
Firm leverage	71,567	0.318	0.235	0.161	0.294	0.417
Firm trade credit	71,567	0.588	0.800	0.286	0.422	0.619

Note: This table reports descriptive statistics for the variables used in the main empirical analysis. Table 1 provides detailed variable definitions.

Table A2: Instrument for banks' exposure: First-stage regression

	Exposure^{US} $\times$ Post
Exposure^{EO} $\times$ Post	1.165***
	(0.365)
F statistic	10.18
Observations	116,898
Bank controls	Yes
Bank-firm FE	Yes
Firm-time FE	Yes
Number of banks	50
Number of firms	2,980
Clustering	Bank-Ind

Note: This table shows the results from the first-stage regression of the instrumental variable approach applied in Column 4 in Table 4. The dependent variable is *Exposure*^{US} $\times$ *Post*. It is regressed on *Exposure*^{EO} $\times$ *Post*. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. The specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: The role of specialization - Loan spreads (2SLS)

	(1) All	(2) Non-exposed	(3) Exposed
Exposure^{US} × Post	-5.872	-2.902	-1.316
	(23.296)	(25.063)	(51.209)
Exposure^{US} × Post × Specialized	-2.560	-19.878	22.744
	(27.440)	(28.123)	(55.432)
Post × Specialized	4.228	16.004***	-23.904*
	(8.005)	(5.120)	(11.846)
Observations	92,102	61,375	30,727
Controls	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes
Number of banks	49	49	40
Number of firms	2,603	1,738	865
Clustering	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust interest rates following a trade shock, as specified in Equation (2). The dependent variable is the average spread of outstanding loans at the bank-firm-quarter level. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO} in all three columns. *Post* indicates the time period after China's entry into the WTO. The binary variable *Specialized* illustrates whether a firm operated in a sector in which the bank is specialized. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Alternative exposure measures (2SLS)

	(1) Full	(2) Level	(3) Net	(4) Federico et al.
Exposure^{US} × Post	-0.049**	-0.903**	-0.488***	-0.077***
	(0.020)	(0.351)	(0.171)	(0.028)
Observations	116,898	116,898	116,898	116,898
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Number of banks	50	50	50	50
Number of firms	2,980	2,980	2,980	2,980
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. The exposure measure is calculated on the basis of changes in imports over the full sample period in Column (1), imports in levels in Column (2), changes in net imports in Column (3), and changes in imports between the pre- and post-shock period in Column (4). *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Alternative clustering (2SLS)

	(1) Cluster 1	(2) Cluster 2	(3) Cluster 3	(4) Cluster 4
Exposure^{US} × Post	-0.389**	-0.389**	-0.389**	-0.389***
	(0.149)	(0.152)	(0.151)	(0.131)
Observations	116,898	116,898	116,898	116,898
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Number of banks	50	50	50	50
Number of firms	2,980	2,980	2,980	2,980
Clustering	Bank	Industry	Bank-firm	Bank-time

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered as indicated and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Alternative specifications I (2SLS)

	(1) Loan controls	(2) ILST	(3) Banks' countries	(4) Anticipation
Exposure^{US} × Post	-0.629**	-0.217*	-0.414**	-0.447**
	(0.255)	(0.120)	(0.166)	(0.188)
Spread	-0.001*			
	(0.001)			
Maturity	-0.013***			
	(0.001)			
Observations	79,679	185,811	116,757	116,898
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	No	Yes	Yes
Number of banks	49	50	50	50
Number of firms	2,419	5,401	2,980	2,980
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. In Column (4), it is calculated over a shortened pre-shock period ranging from Q1 1991 until Q4 1999. *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. In Column (1), average spread and maturity are incorporated as further controls. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects, except for Columns (2) and (3). Column (2) uses industry-location-size-time instead of firm-time fixed effects. Column (3) additionally includes banks' country of origin-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Alternative specifications II (2SLS)

	(1) Collapsed	(2) Steel/glass/cement	(3) Computer	(4) Footwear/apparel/textile
Exposure^{US} × Post	-0.454**	-0.370**	-0.339**	-0.402**
	(0.176)	(0.145)	(0.150)	(0.162)
Observations	4,618	115,709	112,940	112,110
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	No	Yes	Yes	Yes
Number of banks	38	50	50	50
Number of firms	1,046	2,945	2,837	2,856
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects, except for Column (1) where the time dimension is collapsed. Columns (2), (3), and (4) are estimated on a sub-sample excluding steel/glass/cement, computer, and footwear/apparel/textile industries, respectively. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: DealScan particularities (2SLS)

	(1) Securitization	(2) Allocation 2	(3) Allocation 3	(4) Term loans	(5) Credit lines	(6) Alternative lead
Exposure^{US} × Post	-0.377**	-0.303**	-0.421**	-0.083*	-0.466**	-0.285**
	(0.150)	(0.143)	(0.173)	(0.044)	(0.188)	(0.118)
Observations	115,612	116,898	116,898	36,640	101,919	96,903
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Bank-Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Number of banks	50	50	50	43	49	50
Number of firms	2,966	2,980	2,980	1,028	2,717	2,522
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (1). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. *Post* indicates the time period after China's entry into the WTO. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Column (1) excludes loans that are likely to be securitized. In Columns (2) and (3), loan shares are allocated according to alternative rules. Columns (4) and (5) exclude credit lines and term loans, respectively. Column (6) rests on an alternative lead arranger definition. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Alternative definitions of specialization (2SLS)

	(1) Securitization	(2) Alternative period	(3) P90	(4) Continuous var.
Exposure^{US} × Post	-1.195**	-1.631***	-1.107**	-0.723**
	(0.446)	(0.533)	(0.422)	(0.309)
Exposure^{US} × Post × Specialized	1.109**	1.774***	1.074**	0.570*
	(0.453)	(0.549)	(0.420)	(0.298)
Post × Specialized	0.001	-0.281*	-0.123*	0.313
	(0.079)	(0.159)	(0.074)	(0.290)
Observations	115,612	116,898	116,898	116,898
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Number of banks	50	50	50	50
Number of firms	2,966	2,980	2,980	2,980
Clustering	Bank-ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (2), except for Column (1), which is estimated on a sub-sample excluding loans that are likely to be securitized. The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. *Exposure*^{US} captures banks' exposure to the trade shock. It is instrumented by *Exposure*^{EO}. *Post* indicates the time period after China's entry into the WTO. The binary variable *Specialized* illustrates whether a firm operates in a sector in which the bank is specialized, defined on the basis of the last two years before the shock in Column (2) and the 90th percentile in Column (3). In Column (4), banks' pre-shock average industry portfolio shares are used to proxy bank-industry specialization. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A10: Alternative allocation channels (2SLS)

	(1) Exposed	(2) Market	(3) Pre-Lev.	(4) Pre-Ext. Dep.
$\text{Exposure}^{US} \times \text{Post}$	-1.130**	-0.686**	-0.849*	-1.246**
	(0.460)	(0.310)	(0.500)	(0.494)
$\text{Exposure}^{US} \times \text{Post} \times \text{Specialized}$	1.086**	0.630**	1.099**	1.101**
	(0.467)	(0.298)	(0.487)	(0.494)
Post $\times$ Specialized	-0.019	-0.086	0.013	0.019
	(0.091)	(0.278)	(0.096)	(0.098)
$\text{Exposure}^{US} \times \text{Post} \times \text{Other}$	-0.094	37.724	-1.089	0.023
	(0.225)	(87.036)	(0.994)	(0.017)
Post $\times$ Other		-0.118	-0.981**	-0.023**
		(0.810)	(0.429)	(0.011)
Other			-0.267	-0.003
			(0.637)	(0.020)
Observations	116,898	116,898	87,647	85,509
Bank controls	Yes	Yes	Yes	Yes
Bank-firm FE	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes
Number of banks	50	50	49	49
Number of firms	2,980	2,980	2,125	2,063
Clustering	Bank-Ind	Bank-ind	Bank-ind	Bank-ind

Note: This table explores how banks adjust their credit supply following a trade shock, as specified in Equation (2). The dependent variable is the log of outstanding credit at the bank-firm-quarter level, *ln(Credit)*. $\textit{Exposure}^{US}$ captures banks' exposure to the trade shock. It is instrumented by $\textit{Exposure}^{EO}$. *Post* indicates the time period after China's entry into the WTO. The binary variable *Specialized* illustrates whether a firm operates in a sector in which the bank is specialized. *Other* is either *Exposed*, *Market*, *Pre-Lev*, or *Pre-Ext. dep.*. Bank controls include size, profitability, leverage, and funding structure. For detailed variable definitions, see Table 1. Each specification includes bank-firm as well as firm-time fixed effects. Standard errors are clustered at the bank-industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A11: Instrument for firms' exposure: First-stage regression

	Firm exposure^{US} $\times$ Post
Firm exposure^{EO} $\times$ Post	0.989***
	(0.181)
F statistic	30.01
Observations	71,567
Firm controls	Yes
Firm FE	Yes
Industry-state-time FE	Yes
Number of firms	1,945
Clustering	Industry

Note: This table shows the results from the first-stage regression of the instrumental variable approach applied in Table 7. The dependent variable is *Firm exposure*^{US} $\times$ *Post*. It is regressed on *Firm exposure*^{EO} $\times$ *Post*. Firm controls include size, leverage, and trade credit. For detailed variable definitions, see Table 1. The specification includes firm as well as industry-state-time fixed effects. Standard errors are clustered at the industry level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Paper 4:

A Note on the Use of Syndicated Loan Data

A Note on the Use of Syndicated Loan Data*

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This Draft: September 21, 2022¶

Abstract

Syndicated loan data provided by DealScan is an essential input in banking research. This data is rich enough to answer urging questions on bank lending, e.g., in the presence of financial shocks or climate change. However, many data options raise the question of how to choose the estimation sample. We employ a standard regression framework analyzing bank lending during the financial crisis to study how conventional but varying usages of DealScan affect the estimates. The key finding is that the direction of coefficients remains relatively robust. However, statistical significance seems to depend on the data and sampling choice.

JEL classification: C50, G15, G21

Keywords: Syndicated lending, DealScan, scrutiny, meta-analysis

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1 Introduction

The financial crisis starting in 2007/08 has shown the necessity to understand the transmission of shocks to the real sector via (international) banks (Ivashina and Scharfstein, 2010a; Chodorow-Reich, 2013; Cerutti et al., 2015; Kapan and Minoiu, 2018; Doerr and Schaz, 2021). The lack of data on banks' (international) lending activities has significantly increased the interest in syndicated lending data provided by Thomson Reuters LPC's DealScan. A key feature of the database is the multitude of options to define sample and lending outcomes. For example, a common decision authors have to make is which syndicate members to retain in the sample or which loan types to consider.

Our study employs a well-established laboratory to analyze how banks adjust lending during the financial crisis depending on balance sheet characteristics like the tier 1 capital and deposit ratio. We contribute to the literature by highlighting how different sample selections using DealScan data affect the estimation results and we provide upper and lower bounds of coefficient estimates across various specifications. We specifically construct three samples as the basis for our analyses, varying in terms of which syndicate members are considered and how lead arrangers are defined (Ivashina, 2009; Chakraborty et al., 2018; Doerr and Schaz, 2021). For these three samples, we conduct various tests, which we identified to be the most commonly used in the literature. While each paper uses one option or the other, no study shows a structured scrutiny analysis across all possible choices.

We derive three main results for our baseline sample. First, coefficient estimates are robustly comparable in terms of the sign. On average, 95% of estimates show the same sign when considering banks' lending response during the crisis conditional on their capital ratios. For the deposit ratio interaction, the sign of the coefficient coincides in 100% of cases. Second, the significance of coefficients can vary across specifications. This result holds either way: when looking at how the capital ratio matters for lending during the crisis, most coefficients show null results. Nevertheless, one can always find a case that yields significant estimates. Vice versa, we find mainly significant results for the deposit ratio interaction, whereas significance vanishes in a few circumstances. Third, if a coefficient significantly deviates from the others, there is often reasoning provided by the selected sample choice. For example, we observe that a sample containing lead and participant lenders might yield a consistently different result in terms of coefficient significance. The latter, however, applies to most variations for the sample of lead and participant lenders and thus represents a consistent result in itself.

In sum, we consider our results a somewhat positive outcome. Estimates are – across many definitions of the DealScan data – surprisingly robust. At the same time, we show in further tests that the treatment of loan observations is relevant for these conclusions. In this vein, our study provides insights to researchers on how specific usages of DealScan might affect coefficient estimates and offer structured guidance for possible scrutiny tests. Especially given the heavy use of the data to answer urging questions on, for example, banks' responses to the sovereign debt crisis (Acharya et al., 2018), the Brexit (Berg et al., 2021), the Covid-19 pandemic (Hasan et al., 2021) or their adjustments depending on climate risk exposures (Delis et al., 2021; Kacperczyk and Peydró, 2021), a more structured analysis, and understanding might be worthwhile.

The study is most related to the literature on banks' behavior during the financial crisis regarding lending responses. Seminal papers include the one by Ivashina and Scharfstein (2010a) who analyze the role of wholesale runs and credit line draw-downs on bank lending following the Lehman shock. Chodorow-Reich (2013) assesses based on DealScan data the role of credit market relationships for employment. Cerutti et al. (2015) find for the period from 1995 until 2012 that syndicated loans constituted up to one-third of cross-border loans and confirm the draw-down of credit lines. Kapan and Minoiu (2018) show that being exposed to liquidity shocks during the financial crisis, banks maintained loan supply when having higher levels of common equity. Finally, when it comes to cross-border lending spillovers, studies are frequently based on syndicated lending data (e.g., De Haas and Van Horen, 2012).

Furthermore, we contribute to banking and finance studies analyzing the robustness of results across various model specifications. For example, within the International Banking Research Network (IBRN), several studies used bank-level data from different central banks to study the same question on, e.g., the transmission of prudential or monetary shocks via banks' cross-border activities (Buch and Goldberg, 2017; Buch et al., 2019). A meta-study of all results revealed consistent heterogeneity across country-specific findings. A recent study by Menkveld et al. (2021) analyzes results from the research outcome of 164 teams working independently and analyzing the same question on market efficiency based on the same data. The study reveals evidence for significant standard errors across the teams' results. Regarding DealScan data, a study that assesses differences in results across regions is Berg et al. (2016). The authors find differences in loan pricing structures in Europe compared to the United States. At the same time, the total borrowing costs resemble each other.

2 Methodology and data

This section first describes how we set up the regression model to estimate how banks adjust syndicate lending during the financial crisis depending on balance sheet characteristics. Second, we describe the core theme of our study: the different sample specifications we use to estimate the coefficients of interest. Third, we explain the data that underlies our estimations before presenting the results in the following section.

Regression equation We use a straightforward research design to focus on the variation of results depending on the ingredients that enter into the estimations. We choose the fall of Lehman Brothers (e.g., Chodorow-Reich, 2013; De Haas and Van Horen, 2013) as an unexpected event to analyze how banks adjust their syndicated lending volumes during the financial crisis. Equation (1) looks as follows:

$$\begin{aligned} \mathrm{y}_{b,f,t} = {} & \beta_1 \mathrm{z}_{b,t-1} \times \mathrm{Crisis}_t + \beta_2 \mathrm{z}_{b,t-1} \\ & + \beta_3 \mathrm{X}_{b,t-1} + \zeta_{b,f} + \zeta_{f,t} + \varepsilon_{b,f,t}. \end{aligned} \tag{1}$$

The dependent variable is the log of outstanding credit between bank b and firm f in quarter t. $Crisis_t$ divides the sample into a pre-crisis and crisis period. The cut-off point at which the dummy variable turns one is the third quarter of 2007, which corresponds to the failure of Lehman Brothers. Following Cornett et al. (2011) or Kapan and Minoiu (2018), we interact the financial crisis dummy with different balance sheet characteristics, $z_{b,t-1}$, that are i) banks' risk-adjusted capital ratio or ii) their deposit ratio lagged by one quarter. We include a vector of control variables, $X_{b,t-1}$, that encompasses bank size, return on assets, as well as the respective other balance sheet characteristic, that is the deposit or capital ratio.

We saturate the equation with bank-firm fixed effects ($\zeta_{b,f}$) as well as firm-time fixed effects ($\zeta_{f,t}$). $\varepsilon_{b,f,t}$ is the idiosyncratic error term. The fixed effects absorb the single term $Crisis_t$. Standard errors are clustered at the bank level.[1]

DealScan variations First, we specify three baseline samples. The first sample is limited to contain only the lead arranger(s), which are determined following the definition

[1] In robustness tests, we also cluster standard errors at the bank-firm level.

by Chakraborty et al. (2018).[2] The second sample equally encompasses only lead arranger(s). However, we identify them following the definition by Ivashina (2009).[3] The third sample comprises all lenders in the syndicate, i.e. lead arrangers and participants (e.g., Doerr and Schaz, 2021).

Second, we conduct scrutiny tests across all of these three baseline samples. These tests are motivated by the related literature, and we consider the most commonly applied robustness checks. The main difference is that the relevant papers do not show the complete set of combinations of tests. While obviously, each study chooses the most appropriate tests for its purposes in isolation, we consider our paper complementary, providing a guideline on which options there are and how they might matter.

We provide a list of the tests that we will conduct in the following[4]:

1. Keep only facilities that have one lead arranger (if applicable) (Chakraborty et al., 2018; Schwert, 2018)
2. Keep only facilities that have more than one lender (Doerr and Schaz, 2021)
3. Keep only facilities that have less than 11 lead arrangers (if applicable) (Giometti and Pietrosanti, 2022)
4. Keep only loans for which the loan share is available in DealScan (Chu et al., 2019)
5. Keep only non-financial borrowers (Doerr and Schaz, 2021)
6. Keep only non-financial and private borrowers (Giannetti and Saidi, 2019; Wix, 2017)
7. Keep only common loan types (i.e., credit lines and term loans) (Wix, 2017)
8. Keep only credit lines (Berg et al., 2016; Doerr and Schaz, 2021)
9. Keep only term loans (Berg et al., 2016; Doerr and Schaz, 2021)
10. Keep only loans with a purpose that is either working capital or corporate purposes (Chodorow-Reich, 2013)
11. Keep only loans that can be considered general purpose loans (Giannetti and Saidi, 2019)
12. Keep only loans that do not have a purpose of a takeover or acquisition (Chakraborty et al., 2018)
13. Keep only commercial banks (Gatev and Strahan, 2009)

[2] Chakraborty et al. (2018) follow a ranking hierarchy and the lender in the syndicate with the highest rank is considered the lead agent: 1) lender is denoted as “Admin Agent”, 2) lender is denoted as “Lead bank”, 3) lender is denoted as “Lead arranger”, 4) lender is denoted as “Mandated lead arranger”, 5) lender is denoted as “Mandated arranger”, 6) lender is denoted as either “Arranger” or “Agent” and has a “yes” for the lead arranger credit, 7) lender is denoted as either “Arranger” or “Agent" and has a “no” for the lead arranger credit, 8) lender has a “yes” for the lead arranger credit but has a role other than those previously listed (“Participant” and “Secondary investor” are also excluded), 9) the lender has a “no” for the lead arranger credit but has a role other than those previously listed (“Participant” and “Secondary investor” are also excluded), and 10) lender is denoted as a “Participant” or “Secondary investor”.

[3] Ivashina (2009) defines lead arranger(s) as follows: If identified, the administrative agent is defined as the lead bank. If the syndicate does not have an administrative agent, then lenders that act as book runner, lead arranger, lead bank, (lead) manager, or agent are defined as the lead bank.

[4] It does not make logical sense to conduct some tests on the third sample that encompasses the full syndicate. These tests are indicated with “if applicable”.

Data and summary statistics We draw on two primary data sources. First, to obtain information on syndicated lending, we use data provided by DealScan. The sample spans the period from Q3 2005 until Q2 2009. The length of the global financial crisis is adopted from Cornett et al. (2011) such that the dummy variable takes on a value of one between Q3 2007 and Q2 2009 and zero otherwise. We select an equally long pre-crisis period.[5] The loan-level data is aggregated at the ultimate parent level for banks and firms. We focus on US banks being part of a syndicate that provides credit to US and non-US firms. This choice reduces potential confounders, for example, due to differences in financial sector regulation across countries.

We treat facilities as individual loans (see e.g., Ferreira and Matos, 2012). If applicable, we convert facility volumes to US$ million utilizing the spot exchange rate that DealScan provides at loan origination. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, we distribute the facility amount equally among all lenders in the syndicate (De Haas and Van Horen, 2013).

On this basis, we follow the most recent approach in the literature and use loan shares to create a stock variable that captures the outstanding loan volume of each bank-firm pair (Chakraborty et al., 2018; Doerr and Schaz, 2021). We follow this approach to remedy that DealScan captures loan information at loan origination. It implies that a loan enters a bank's book from origination until maturity. Outstanding loan volumes are then summed up each quarter per bank-firm pair to arrive at bank-firm-quarter as the observation level. In further analysis, we also investigate the implication of this sampling approach.

Second, we complement the dataset by adding bank-level information from Compustat. Given that there is no common identifier between DealScan and Compustat, we rely on the link file provided by Schwert (2018). Compustat provides measures for bank size, profitability, deposit share, and risk-adjusted capital ratio. Table 1 provides a more detailed overview of variable descriptions. We require total assets to be non-negative and non-zero. Bank-level variables are winsorized at the 1st and 99th percentile to adjust for extreme outliers (Chen and Chen, 2012; Kahle and Stulz, 2013).

[Table 1]

Table 2 shows summary statistics for the variables of interest for each of the three

[5] In a robustness check, we consider a more prolonged crisis definition (Q2 2007 until Q1 2010) and an equally extended pre-shock period.

different baseline samples. The average pre-crisis outstanding loan volume lies between US$ 75 million and US$ 135 million. The average loan volume in the sample encompassing the whole syndicates (Column (3)) is lower than in the two samples containing only lead arrangers (Columns (1) and (2)). The reason is that participants usually retain lower loan shares (Sufi, 2007). Irrespective of the underlying sample, banks are well-capitalized. Their average tier 1 capital ratio takes on values around 9%. Capital requirements at that time stipulate a ratio of 8%. Deposit funding constitutes, on average, between 64% and 67% of total assets. Banks with a higher deposit ratio might be shielded more from wholesale funding runs during the financial crisis.

[Table 2]

3 Results

We first show in Table 3 the regression results across the three baseline samples when interacting the crisis dummy with i) the capital ratio (Columns (1)-(3)) and ii) the deposit ratio (Columns (4)-(6)). Then, we repeat the estimations for these three samples and the two interacting variables for the 13 different specifications as outlined above. For better comparability, we plot the coefficient estimates surrounded by their 90% confidence bands across these iterations in Figure 1(a)-(b).[6]

[Table 3]

Results in Columns (1) to (3) in Table 3 reveal that the interaction term between the financial crisis indicator and the lagged tier 1 capital ratio is negative. Hence, while capitalization seems to enter with a positive (but insignificant) sign, better-capitalized banks tend to lend less in syndicated markets during the financial crisis. The latter result is significant in Columns (1) and (3). In principle, bank capitalization can relate to lending decisions differently. On the one hand, better-capitalized banks might have more buffer to expand lending. On the other hand, banks with low capital ratios have less equity at stake, which might increase risky lending activities. For example, Cerutti et al. (2015) find that syndicated lending declines with higher capital ratios suggesting that low-capitalized banks make use of syndicated lending by having a smaller share in the total loan, which might be feasible despite their capital constraints.

Similarly, we find in Columns (4) to (6) in Table 3 that a higher deposit ratio relates positively to lending. However, the effects are mitigated during crisis times.

[6] We provide the underlying regression tables upon request.

This result might indicate that banks with higher capital and deposit ratios behaved more prudently and retracted from (often international) syndicated loan markets during the financial crisis. Furthermore, these banks are less likely to have applied for TARP funding due to their more solid balance sheets (Duchin and Sosyura, 2012), without possible stimulating effects as concerns lending (Duchin and Sosyura, 2014; Berger et al., 2019).

Our key contribution is to test the estimates for the three baseline samples through our proposed alternative sample specifications as outlined in Section 2. Figures 1(a)-(b) present the effect size of the coefficient of the interaction term across different specifications. Figure 1(a) presents the ones when considering the interaction with the capital ratio and Figure 1(b) with the deposit ratio respectively. Results based on the lead arranger definition by Chakraborty et al. (2018) are depicted by a circle, results based on the definition by Ivashina (2009) are depicted by squares and those for the sample containing the full syndicate by diamonds. The different colors indicate the type of variation that we apply to re-estimate the model.[7]

[Figure 1]

Figure 1(a) starts by depicting the three coefficient estimates of the interaction term with the capital ratio in line with results in Table 3, Columns (1)-(3). In the second step, we keep only facilities in the sample that have one lead arranger ("Nr lead=1"). Results are shown in reddish color.[8] We proceed like this and show estimates of all alternative specifications previously described.

Comparing results across specifications, we derive the following main conclusions. First, the two figures reveal that the results regarding their signs are pretty robust. Only in two out of 40 cases, the sign turns positive for the capital ratio in Figure 1(a). In Figure 1(b), the coefficient of the interaction term with the deposit ratio is always negative.

Second, also in terms of significance, results seem quite robust. For example, they show a high fraction of null results for the capital ratio (27/40). A significant result appears across most specifications for the interaction with the deposit ratio, excluding 15 out of 40 cases.

[7] The legend provides more information on the selected specification and has to be read from left to right, while the ordering resembles the bullet points in Section 2.

[8] Note that this specification only applies to the two samples that depend on keeping the lead arranger(s).

Third, the deviation in the significance of the results is not random. For example, Figure 1(a) indicates that the interaction term with the capital ratio becomes significantly negative if we consider the sample containing the whole syndicate. This deviation is not a contradicting result. Participants play different roles as lead arrangers, and the sample size is more extensive, which might result in more variation. Also, there is evidence highlighting differences in lead banks and participants that might result in heterogeneous reactions during crisis times (Ivashina, 2009; Ivashina and Scharfstein, 2010b). Again, the result remains consistent for all iterations based on the participant sample, with few insignificant cases.

In Figure 1(b), the coefficient loses significance when considering the interaction with the deposit ratio in selected cases. However, these results might not impede the general message but fit the selected samples' information content. For example, adding restrictions that reduce sample size might result in lower significance (e.g., when focusing on syndicates with only one lead arranger ("Nr lead=1") or when keeping only loans for which the lead share is available in DealScan ("Avail share")).

In further tests, we change the clustering scheme and how loans between bank-firm pairs are treated. Regarding the latter point, we do not use the outstanding loan volumes but only look at the loan volumes at origination. The sample that results from this alternative approach is significantly smaller and consequently does not allow going through all of the 13 specifications. Thus, we compare the baseline results from Table 3 with the findings obtained when running the same regression but only considering loans at origination. In this estimation, we additionally vary the chosen clustering scheme and compare results when clustering at the bank level with those obtained when there is no clustering of standard errors. This might be of relevance since the number of clusters turns relatively small when only loans at origination are considered.

Figure A1 in the Appendix shows results for the tier 1 ratio in Panel A and the deposit ratio in Panel B. The left panels show results for the three baseline samples and outstanding loan volumes, the right panels show results when keeping only loans at origination. Considering the results for the baseline samples in the left panels, it turns out that the choice of clustering can result in confidence bands narrowing down once no clustering is applied. Comparing results in Panel A for the baseline (left side) and original structure (right side), it becomes visible that coefficient signs go in the same direction while there are relevant differences in terms of significance. When turning to the interaction with the deposit ratio in Panel B, differences in results become even more evident as previously significant coefficient estimates turn insignificant for the sample for which we retain loans only at origination (right side).

Additionally, we estimate the baseline models shown in Table 3 but include standard errors clustered at the bank-firm instead of the bank level, or we define the deposit ratio as in Cornett et al. (2011) by $Deposits_t/Assets_{t-1}$ (Figures A2-A3).[9] These tests do not affect our main conclusions in the case of the alternatively chosen clustering scheme. At the same time, there seems to be some level effect when changing the definition of the interacted deposit variable. Moreover, we employ an alternative rule to allocate loan shares: Again, we allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, while equally subdividing within these two groups (De Haas and Van Horen, 2013). Note that this approach results in differences in the loan amounts of participants depending on whether the lead arranger definition by Chakraborty et al. (2018) or Ivashina (2009) is used. Therefore, we show the results for the full syndicate for both definitions in Figures A4 and A5, whereas the results remain their key pattern. However, coefficient estimates show a slight tendency to gain significance in the case of the sample defined following Chakraborty et al. (2018). Lastly, we allow for an extended crisis period following Kapan and Minoiu (2018) with results remaining mostly robust regarding the key pattern. Yet this does not rule out that changes in significance can occur for some coefficients (Figure A6).

In sum, our checks show that we observe relatively robust results across all iterations and sample choices. The only restriction is the change to a sampling structure where we consider loans at origination instead of the stock of outstanding loan volume. The significantly reduced number of observations from 11% to 37% might be one explanation for this result, which itself might point toward the trade-off between gaining variation versus changing sample structure when varying between the two approaches.

4 Conclusions

We use syndicated lending data from DealScan to analyze banks' lending responses depending on balance sheet variables exploiting the occurrence of the financial crisis as an exogenous event. Based on this established setting in the literature, we scrutinize our results across many specifications derived from specifics of the DealScan data structure. The baseline estimations are based on a sample of US banks active in the syndicated

[9] Banks' capital ratio is pre-constructed in Compustat such that we cannot show this robustness test for banks' tier 1 ratio.

market and the period from Q3 2005 to Q2 2009. We conduct the estimations based on three sample definitions regarding lead arrangers and participants, which the literature uses when drawing on syndicated loan data from DealScan. For these three baseline samples, we repeat the estimations for different data adjustments commonly used in related work, such as the choice of loan types.

The broad dimension of results we obtain from our approach helps detect three key patterns. First, the signs of the coefficient estimates are quite robust across samples. Second, the same holds for significance. Third, if some coefficients are significant while others are not (or vice versa), this is not random but goes back to the specific information content of the considered specification. For example, we consistently find differences in significance when comparing results for lead arrangers only versus all lenders of a syndicate.

Consequently, our results provide further insights into the usefulness of syndicated loan data provided by DealScan and reveal potential data avenues that researchers might choose and that might lead to diverging findings such as the treatment and allocation of loans at origination. Nevertheless, depending on the chosen sampling method, our study supports the robustness of estimates obtained based on syndicated loan data irrespective of (the many) options DealScan data offers.

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Tables and figures

Table 1: Variable definitions

Variable	Description	Source	Data items
Loan volume	Outstanding loan volume in US$ million between bank b and firm f in quarter t	DealScan	
Crisis	A dummy variable that takes on a value of one between Q3 2007 and Q2 2009 and zero otherwise		
Bank characteristics			
Size	Log of total assets	Compustat	Ln(atq)
ROA	Net income divided by total assets	Compustat	niq/atq
Deposit	Total deposits divided by total assets	Compustat	dptcq/atq
Tier 1	Risk-adjusted capital ratio	Compustat	capr1q

Table 2: Summary statistics

Sample: *Variable*	(1) Chakraborty's lead	(2) Ivashina's lead	(3) Participants
Panel A: Loan characteristics			
Ln(loan volume) Mean	3.94	3.84	3.56
SD	1.27	1.37	1.11
Loan volume Mean	128.89	134.86	74.72
SD	301.11	343.39	170.48
N	3,914	2,411	24,748
Panel B: Bank characteristics			
Size Mean	11.12	11.15	10.13
SD	1.74	1.59	2.09
ROA Mean	0.33	0.33	0.30
SD	0.14	0.13	0.15
Tier 1 Mean	9.10	9.13	9.74
SD	1.40	1.25	1.96
Deposit Mean	64.54	64.07	67.11
SD	9.31	9.57	10.04
N	26	26	47

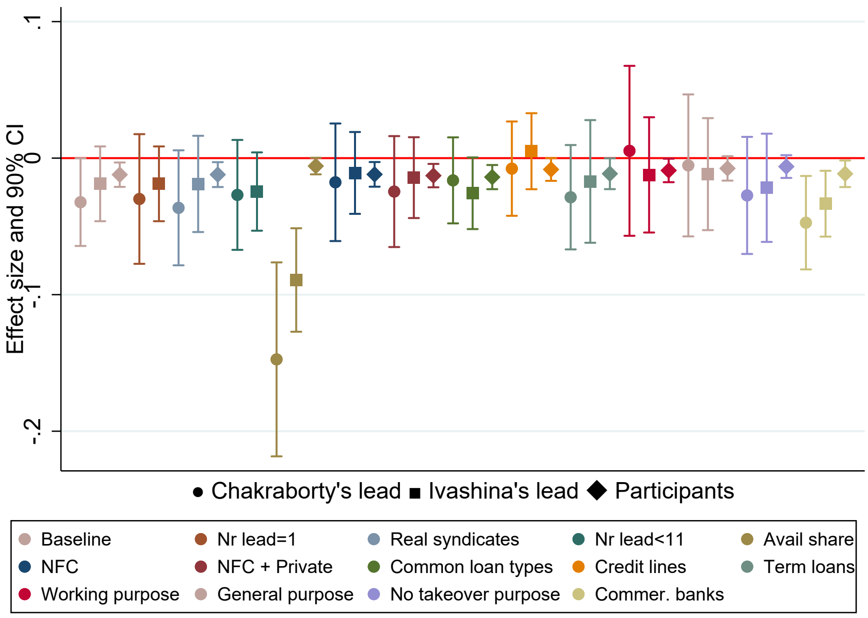
Note: This table shows summary statistics of the dependent variable defined at the bank-firm level in Panel A and of the control variables at the bank level in Panel B for each of the three baseline samples respectively. All variables are reported as averages over the pre-crises period that ranges from Q3 2005 to Q2 2007.

Table 3: Baseline

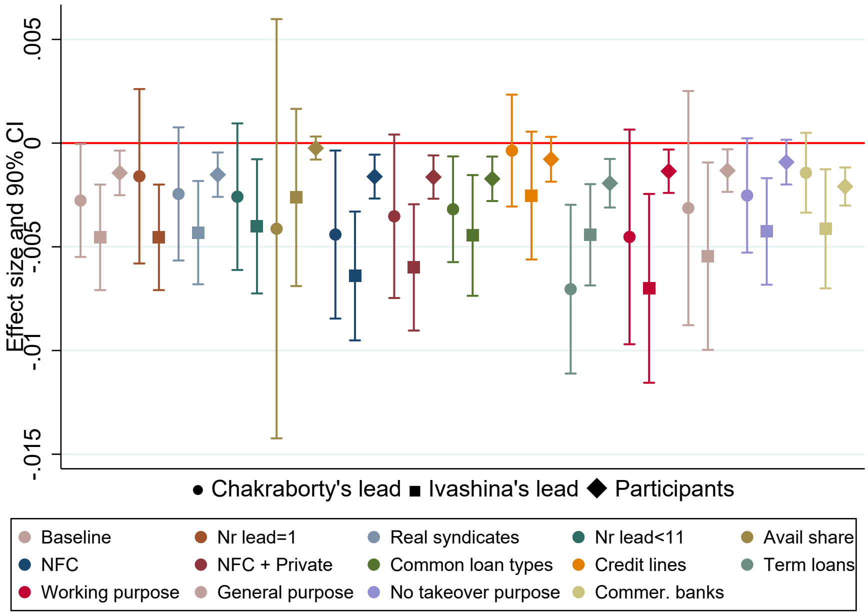
	(1)	(2)	(3)	(4)	(5)	(6)
Sample:	Chakraborty's	Ivashina's	Participants	Chakraborty's	Ivashina's	Participants
L.Tier 1 × Crisis	-0.032*	-0.019	-0.012**			
	(0.019)	(0.016)	(0.005)			
L.Deposit × Crisis				-0.003*	-0.005***	-0.001**
				(0.002)	(0.001)	(0.001)
L.Size	-0.065**	-0.049	-0.004	-0.058**	-0.045	-0.001
	(0.030)	(0.057)	(0.026)	(0.027)	(0.054)	(0.025)
L.ROA	0.007	0.037	0.014	0.004	0.027	0.010
	(0.031)	(0.037)	(0.011)	(0.025)	(0.031)	(0.010)
L.Tier 1	0.008	0.005	0.000	-0.021***	-0.012**	-0.010**
	(0.019)	(0.015)	(0.008)	(0.006)	(0.005)	(0.005)
L.Deposit	-0.000	0.001	-0.000	0.001	0.003	0.000
	(0.003)	(0.003)	(0.001)	(0.003)	(0.003)	(0.001)
Observations	18,951	23,794	324,480	18,951	23,794	324,480
Bank-firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Firm-time FE	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.936	0.942	0.942	0.936	0.942	0.942
Number of banks	26	27	50	26	27	50
Number of firms	983	1,185	7,473	983	1,185	7,473
Clustering	Bank	Bank	Bank	Bank	Bank	Bank

Note: This table explores how banks adjust their lending following the global financial crisis, as specified in Equation (1). The dependent variable is the log of outstanding loans at the bank-firm-quarter level. *Crisis* indicates the duration of the global financial crisis from Q3 2007 until Q2 2009. *Tier 1* is the risk-adjusted capital ratio lagged by one quarter. *Deposit* is the ratio of total deposits to total assets and it is lagged by one quarter. We include lagged bank size, return on assets, as well as the deposit ratio (tier 1 ratio) in Columns (1) to (3) (Columns (4) to (6)) as controls. Standard errors are clustered at the bank level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Coefficient estimates and confidence bands across sample specifications



(a) Tier 1 ratio

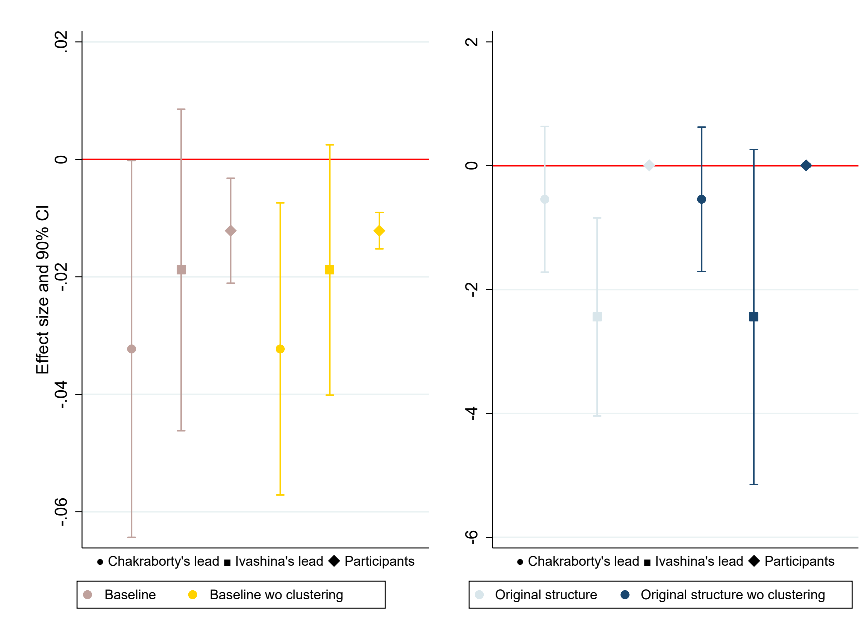


(b) Deposit ratio

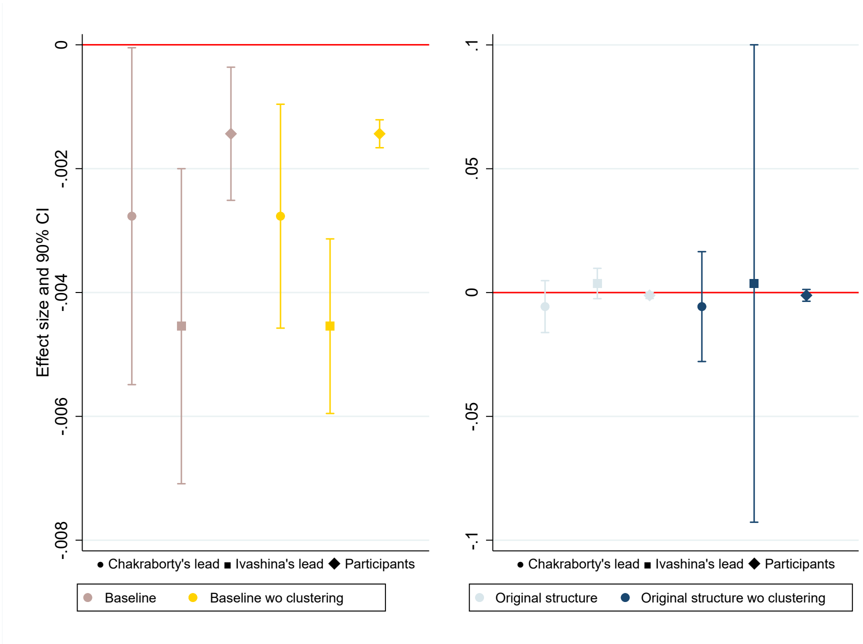
Note: This figure plots the coefficients from estimating Equation (1) for the two interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. The first three coefficients (Baseline) in each sub-figure correspond to the results presented in Table 3 for the baseline samples. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Appendix

Figure A1: Coefficient estimates and confidence bands across sample specifications: No clustering scheme and alternative sample structure



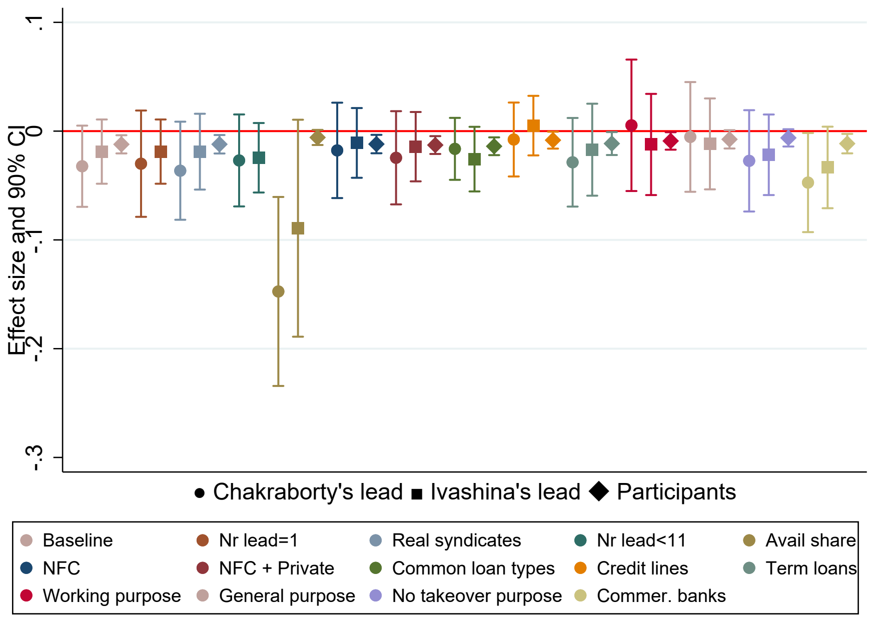
(a) Tier 1 ratio



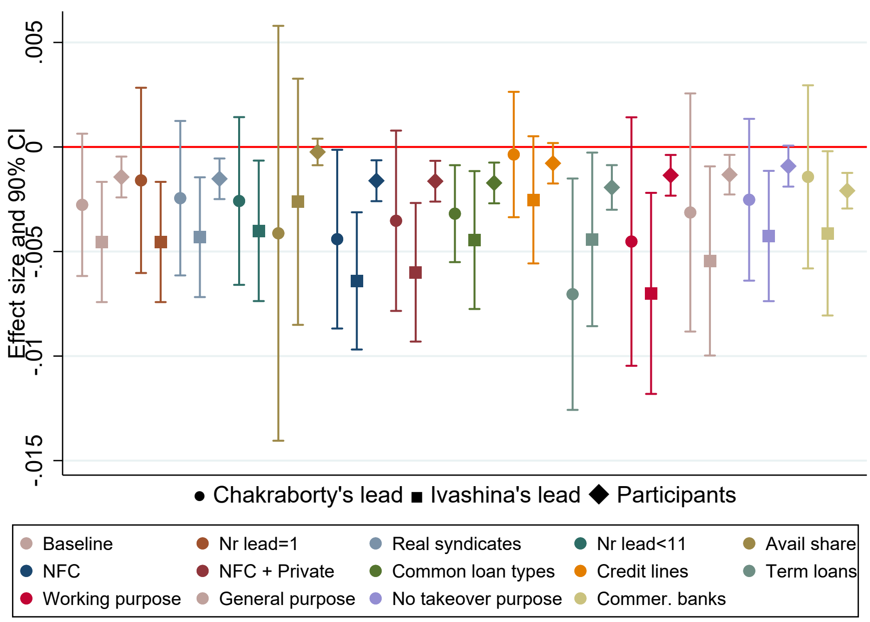
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol), respectively. We alter whether a clustering scheme is applied (or not) and how the DealScan data is constructed (outstanding loans (baseline) versus loans at origination (original structure)). We show the 90% confidence intervals for each estimate. If a clustering scheme is applied, standard errors are clustered at the bank level.

Figure A2: Coefficient estimates and confidence bands across sample specifications: Alternative clustering



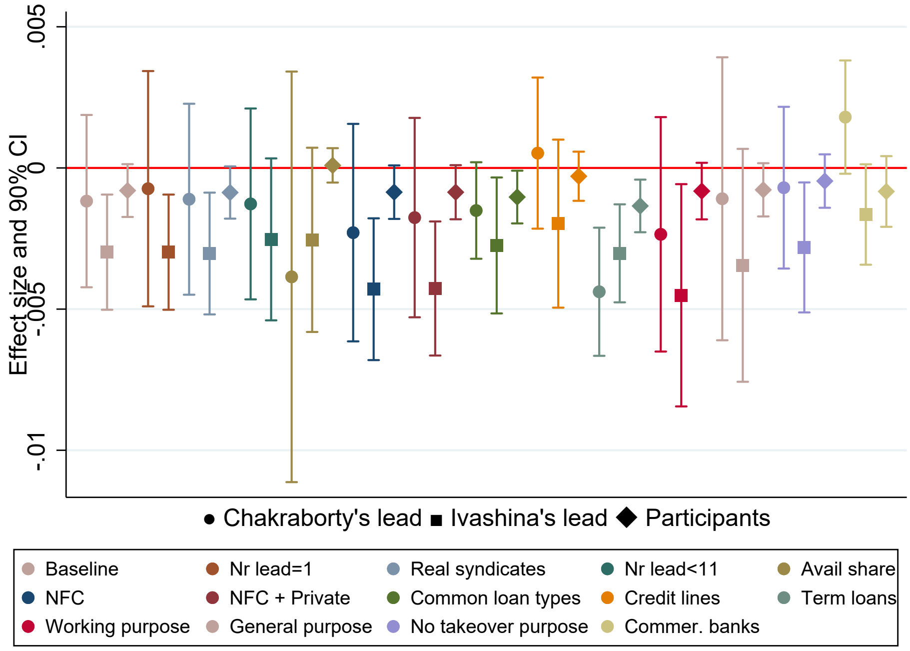
(a) Tier 1 ratio



(b) Deposit ratio

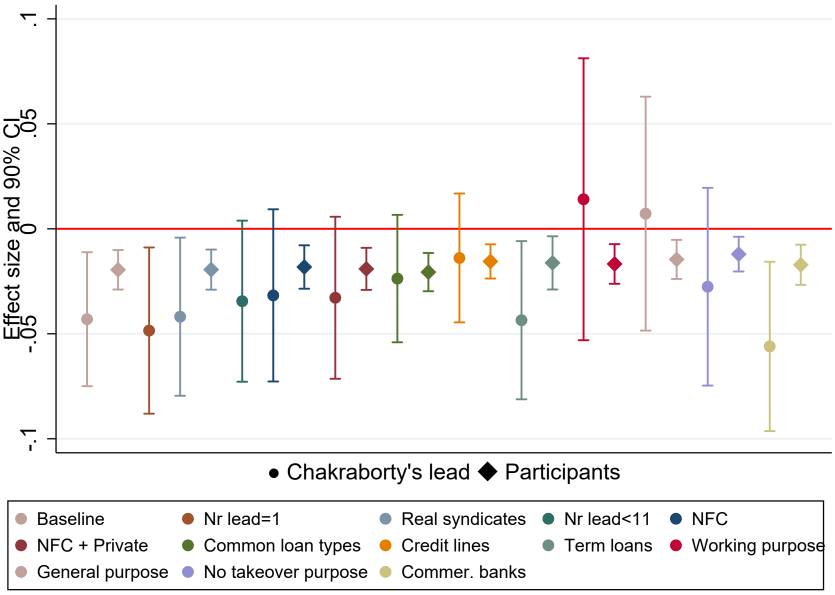
Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank-firm level.

Figure A3: Coefficient estimates and confidence bands across sample specifications: Change in definition of deposit ratio

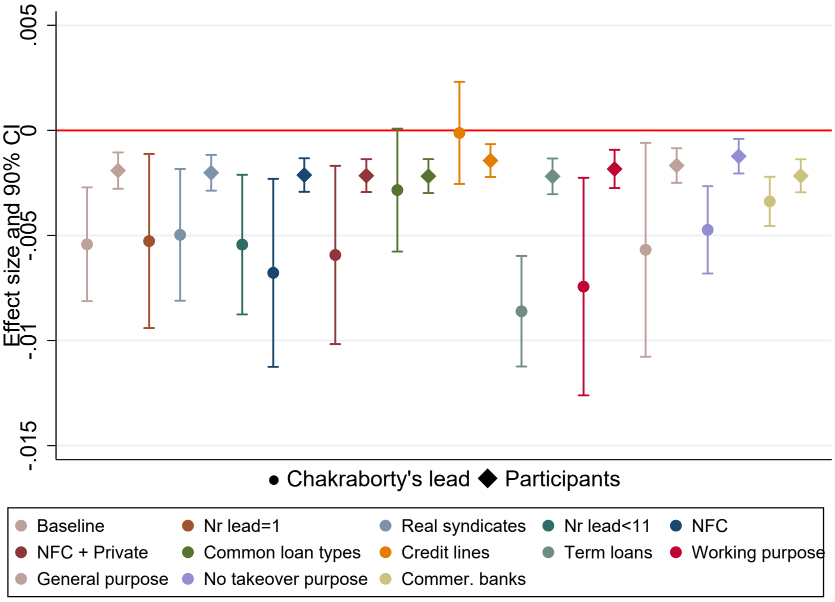


Note: This figure plots the coefficients from estimating Equation (1) for the interaction with banks' deposit ratio (defined as in Cornett et al. (2011)) as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A4: Coefficient estimates and confidence bands across sample specifications: Alternative allocation rule



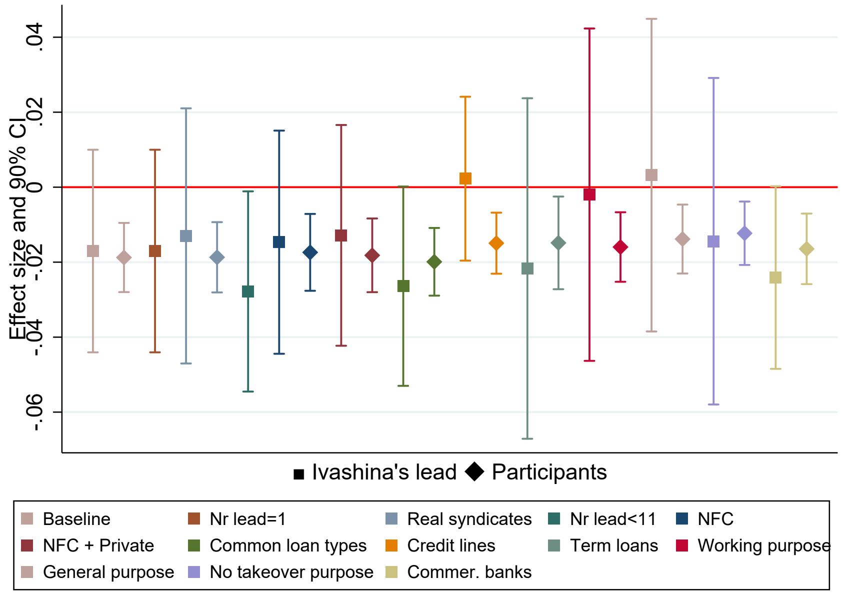
(a) Tier 1 ratio



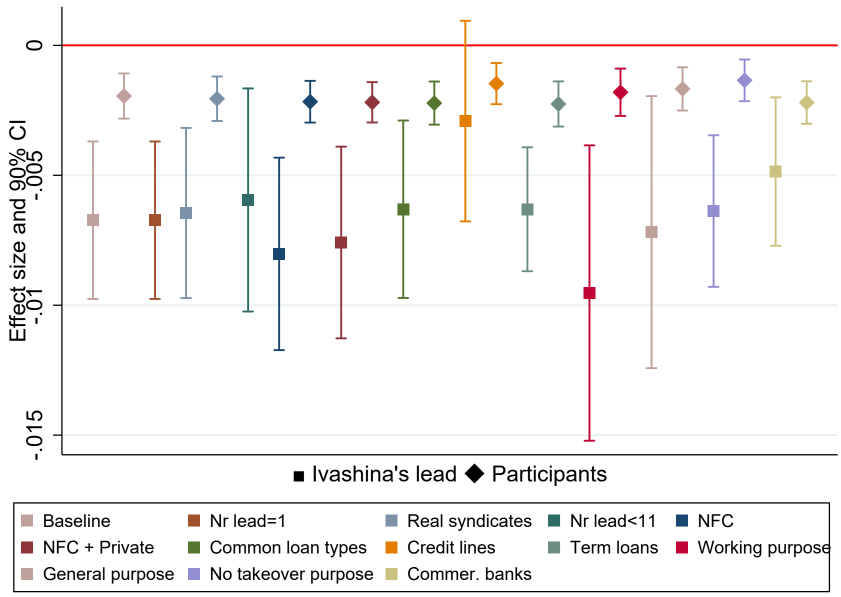
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, while equally subdividing within these two groups. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A5: Coefficient estimates and confidence bands across sample specifications: Alternative allocation rule



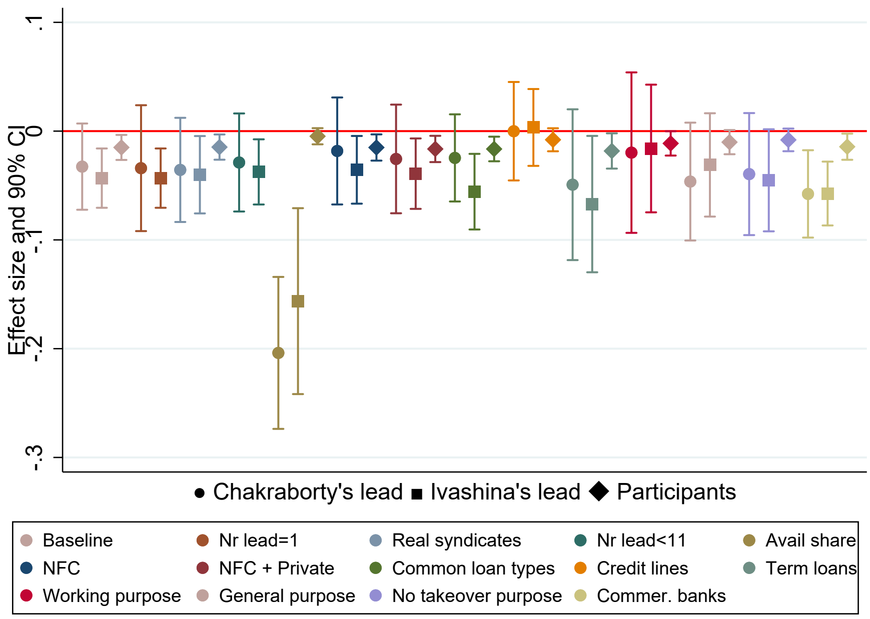
(a) Tier 1 ratio



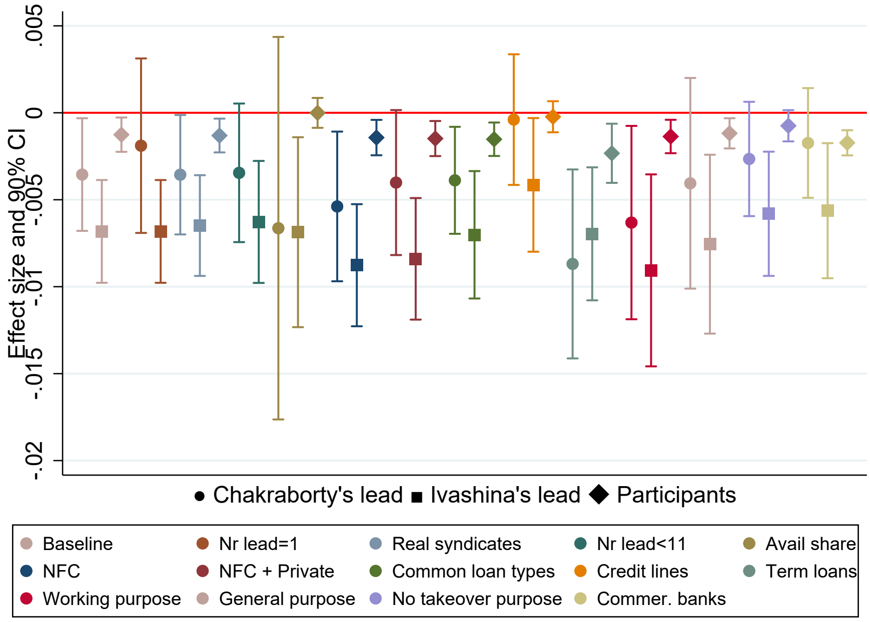
(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. We allocate loan shares according to the breakdown provided by DealScan, or if this information is missing, lead arranger(s) and participants receive 50% of the facility volume, respectively, while equally subdividing within these two groups. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.

Figure A6: Coefficient estimates and confidence bands across sample specifications: Alternative crisis length



(a) Tier 1 ratio



(b) Deposit ratio

Note: This figure plots the coefficients from estimating Equation (1) for the interactions with (a) banks' risk-adjusted capital ratio and (b) banks' deposit ratio as the independent variable for each sample (symbol) and each scrutiny test (color), respectively. The global financial crisis dates from Q3 2007 until Q1 2010. We show the 90% confidence intervals for each estimate. Standard errors are clustered at the bank level.