Carbon-information Opacity and Loan Price: Evidence from the Greenhouse Gas Reporting Program

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Abstract:

Using the U.S. Environmental Protection Agency's mandatory Greenhouse Gas Reporting Program (GHGRP) as an exogenous shock, we find that the exogenous reduction in carbon information opacity lowers firms' loan price significantly. Such an effect is more pronounced for firms with greater opaqueness or for firms facing more material carbon risk. Additionally, this effect cannot be explained by emission reduction and does not disappear for firms with prior voluntary carbon disclosure. Overall, our findings show that lenders price in carbon information opacity when offering loans to emitters.

Keywords: carbon disclosure; carbon emission; bank loan; climate change; Greenhouse Gas Reporting Program (GHGRP).

JEL Classification: G14; G21; K32; M41; M48; Q54; Q56; Q58

1. Introduction

Mitigating carbon and greenhouse gases (GHG) emissions is an essential task to combat climate change and global warming.[1] Countries have been instituting laws and regulations (e.g., environment laws, carbon taxes, cap-and-trade schemes) to internalize companies' emission costs to the society, which expedite the transition to a greener economy. The corporate sector, on the other hand, has to face the transition risk of fast-changing regulatory environment and pay the costs associated with stranded assets, compliance, outdated technology, litigations, etc. Carbon risk, i.e., a financially material transition risk companies face due to their GHG emissions, has a profound impact on company behavior and outcomes (Balachandran & Nguyen, 2018; Bolton & Kacperczyk, 2021; Bose et al., 2021; Ilhan et al., 2021; Nguyen & Phan, 2020; Phan et al., 2021).

To appropriately assess carbon risk and perform efficient asset allocation and strategic planning, the information about firms' GHG emissions (carbon information henceforth) becomes crucial. Carbon information is so important that, in 2015, the G20 Finance Ministers and Central Bank Governors ask the Financial Stability Board (FSB) to establish the Task Force on Climate-related Financial Disclosures (TCFD) to help identify the information needed by shareholders, lenders, and insurance underwriters to evaluate and price climate-related risks and opportunities. The importance of carbon information is widely recognized by the national regulatory bodies. To name a few, the United State (U.S.) Federal Deposit Insurance Corporation (FDIC) states that "Sound climate risk management depends on the availability of relevant, accurate, and timely data" (Federal Deposit Insurance Corporation, 2022). The financial stability board (FSB) also maintains that "A key part of (carbon) risk management is clearly having the right data to understand the risks" (Financial Stability Board, 2021). The European Central Bank (ECB) states that "A lack of available data is often given as a reason for insufficient progress by institutions in incorporating C&E (climate-related and environmental) risks" (European Central Bank, 2021). Similarly, the

[1] The primary greenhouse gases in Earth's atmosphere include water vapor (H_2O), carbon dioxide (CO_2), nitrous oxide (N_2O), and ozone (O_3). The latter three are emissions from human activities and often form the targets of climate policies.

Bank of England surveyed British banks and find that "Many firms (banks) identified access to data as a significant challenge in addressing the financial risks from climate change" (Bank of England, 2018). Recently in the United States (U.S.), the Security and Exchange Commission (SEC) is consulting about a proposal for enforcing mandatory carbon disclosure among all exchange-listed companies. In summary, without accessible, accurate, comprehensive, and comparable carbon information, shareholders, lenders, and companies would face substantial carbon-information opacity, which leads to misunderstanding of risk assessment, misallocation of assets, and false strategic planning. Despite the importance of carbon information, the extant literature on carbon risk takes available carbon information as given and provides little empirical evidence on the effect of "*carbon-information opacity*" on the corporate sector. In this paper, we fill this gap by examining the relevance of carbon-information opacity through the lens of firms' bank loan pricing.

Borrowing from banks and financial institutions constitutes a primary source of finance for companies (Denis & Mihov, 2003; Sufi, 2007). These lenders are also arguably primary users of carbon information because they play an essential role in supporting an orderly transition to a low-carbon economy, demonstrated by the 2003 Equator Principles and the 2019 United Nations Principles of Responsible Banking. Climate-consistent credit allocation is vital in promulgating carbon-efficient activities across the corporate sector. Central banks and governments around the world have mandated banks and financial institutions to assume a strategic and board-level approach to manage their carbon-risk exposures (Bank of England, 2018; European Central Bank, 2020; Federal Deposit Insurance Corporation, 2022; Office of the Comptroller of the Currency, 2021; The China Banking Regulatory Commission, 2012). These mandates highlight the authorities' concern of both carbon-related systematic financial risk and risks specific to the corporate sector associated with credit, liquidity, operation, litigation, and reputation. These mandates also provide lenders with additional motives to demand high-quality disclosure of carbon information by the corporate borrowers to identify, measure, monitor, and manage their carbon-risk exposure.

Carbon-information opacity exists due to the lack of accessible, accurate, comprehensive, comparable, and timely carbon information. In our primary hypothesis we postulate that borrowers face lower loan price when their carbon-information becomes less opaque.

Previous literature shows that when investors have less information than managers do, they require higher returns for their investments as a compensation for bearing information risk (Barry & Brown, 1984, 1985; Merton, 1987).[2] Merton (1987) also shows that significant costs exist during information gathering, processing, and transmission. Easley and O'Hara (2004) maintain that the lack of high-quality information hinders investors' ability to allocate assets efficiently into portfolios and disadvantages uninformed investors relative to informed ones, leading to higher cost of capital. Poor information quality also discourages users from analyzing such information, reducing trading activities and lowering liquidity, which, in turn, makes capital more costly for firms (Amihud, 2002; Amihud & Mendelson, 1986; Brennan; & Subrahmanyam, 1996; Copeland & Galai, 1983; Glosten & Milgrom, 1985; Liu, 2006; Pástor & Stambaugh, 2003).

Moreover, a strand of literature suggests that lenders take information opacity into consideration when designing loan terms (e.g., Costello & Wittenberg-Moerman, 2011; Dennis & Mullineaux, 2000; Ertugrul et al., 2017; Sufi, 2007). Therefore, we expect lenders to demand lower interest when carbon-information opacity is lower. The null hypothesis is that carbon-information opacity is inconsequential for lenders and does not impact loan price.

To evaluate the impact of information opacity on bank loan price, we need a setting where an exogenous shock unexpectedly reduces a borrower's carbon-information opacity. Such an exogenous shock allows us to establish causality which cannot be established using a simple regression of loan price on measures of carbon-information opacity. Therefore, we build our empirical strategy in the context of the U.S. Environmental Protection Agency's (EPA) Greenhouse Gas Reporting Program (GHGRP). We adopt a difference-in-differences (DiD)

[2] Notably, we focus on the risk associated with information opacity instead of carbon risk. A company with greater carbon risk could face lower carbon-information opacity if it is forthcoming in disclosing its carbon information.

specification for our baseline analysis. The GHGRP is a nationwide policy which mandates the disclosure of GHG data and related information by large emission sources, fuel and industrial gas suppliers, and CO_2 injection sites in the U.S. The GHGRP emissions data was first released to the public in 2012 and was reported regularly thereafter in October of each year. After receiving the emissions reports, the EPA conducts a multi-step verification process to ensure the accuracy, completeness, consistency, and timeliness of the reported data.

Our analysis rests on the premise that the GHGRP considerably reduces lenders' carbon-information opacity. Before the GHGRP, lenders, at the best, rely on emissions data from firms' voluntary disclosure through public (e.g., the Carbon Disclosure Project (CDP)[3], annual reports, sustainability reports, or company websites) or private channels (e.g., private communications between the lender and borrower). However, voluntary disclosures are arguably and notably more limited in scope and less reliable in quality due to managerial agency costs (Bamber & Cheon, 1998) or firms' incentives to maintain liquidity or preempt competition (Bhattacharya & Ritter, 1980; Diamond & E., 1991; Hayes & Lundholm, 1996).

Surveys of voluntary reporting practice show that firms' voluntary carbon disclosures vary widely in quality and content (Hrasky, 2012; Rankin et al., 2011). Armour et al. (2021) argue that voluntary disclosure initiatives fail to ensure sufficiency and consistency of carbon information and are subject to opportunistic and selective reporting. They petition that the rationales to lower cost of capital and enhance social welfare call for mandatory carbon disclosure. Herbohn et al. (2022) also argue that mandatory carbon disclosures improve the carbon-information environment because they provide accessible and credible emissions data that is comparable across entities based on standardized measurement and reporting methodologies. Further, Bolton et al. (2021) point out that mandatory carbon disclosures facilitate effective enforcement that is difficult to achieve under voluntary disclosures.

[3] CDP is a leading source of publicly available carbon information based on firms' voluntary participations in CDP questionnaires (Griffin et al., 2017; Ilhan et al., 2021; Matsumura et al., 2014).

In our DiD analysis, we compare the changes in loan interest spreads around the GHGRP's adoption between the loans borrowed by the treated and control firms. The treated firms are those that report their carbon emissions under the mandate of GHGRP. We identify the control firms as those firms that are similar to the treated firms in observable firm characteristics that determine GHGRP compliance but actually not covered by the GHGRP, using a propensity score matching (PSM) procedure.

Our baseline analysis shows a significant reduction in loan interest spreads for the treated firms relative to the control firms from before the GHGRP's proposal to after the GHGRP's effective adoption. The reduction ranges from 28.1 basis points (bps) to 40.4 bps according to various specifications (i.e., 17.6%–25.1% of the average loan spreads of the treated firms), which translates into savings between \$5.1 million and \$7.3 million in interest payments for a loan with an average size and maturity. It is evident that the treated firms enjoy a significant reduction in their loan interest spreads after the GHGRP's adoption, consistent with our primary hypothesis that a reduction in a borrower's carbon-information opacity significantly reduces its loan price. Further examining the dynamic effect of GHGRP's adoption on loan interest spreads, we find that the difference in interest changes between the loans of treated and control firms is insignificant for all the years before the GHGRP, which is reassuring because our baseline effect is not due to the diverging trends in the interest spreads charged on the treated and control firms. The treatment effect is most pronounced in the first three years after the GHGRP's adoption and becomes insignificant thereafter, indicating that the GHGRP has a protracted but transitory effect, instead of a permanent one. Such a transitory nature could be due to alternative carbon-information channels that emerged after the GHGRP which improve the information environment for emissions of all firms.

Our baseline result is robust to a battery of additional robustness tests. First, we find the baseline result is not present when estimating the DiD regression using the same treated and control firms but a pseudo-event time, showing that the result is unlikely due to the diverging trends for the treated and control firms or due to unmeasured macro factors. Second, using entropy balancing, we also ensure our baseline result persists when treated and control firms are also balanced on higher moments of their distributions. Third, our baseline result is robust to several sample modifications: specifically, 1) we require the treated firms to continuously report their emissions over the entire five-year period post-GHGRP to exclude firms that are included in the GHGRP by chance; 2) we set the sample period as three-year instead of five-year before and after the GHGRP to check whether our baseline is driven by our choice of sample period; 3) we drop those control firms suspicious of manipulating their emissions downward to avoid GHGRP reporting so that our sample is purged of self-selection bias; and 4) we drop heavily regulated utility firms to minimize the influence of other regulations. Fourth, we also further include state × year fixed effects and industry × year fixed effects to control for unmeasured intertemporal shocks to industries or states. Fifth, in our baseline analysis, we excluded the period between GHGRP's initial proposal and the first carbon-information release under the policy because such a transition period is possibly contaminated by rumors, speculation, and information leakage. However, when we include this transition period in our pre-treatment period, our baseline result remains. Sixth, we randomly assign some non-GHGRP firms to the pseudo-treated group, re-perform PSM, and re-run the baseline regression using the pseudo-treated and pseudo-control groups but the same event date. We repeat this procedure 1,000 times. The mean value of the coefficient on *Treated* × *Post* from these placebo tests is zero and the coefficient from our baseline analysis lies outside of the 95th percentiles of the distribution of the placebo-test coefficients, eliminating the concern of concurrent shocks.

Our baseline analysis could be confounded by several alternative effects. First, lenders could reduce their estimations of the treated firms' carbon emissions after the GHGRP's adoption. That's because lenders could be so overly prudent before the GHGRP that they intentionally overestimate the borrowers' carbon emissions. Such intentional conservative adjustment (Leftwich, 1983) is attributable to the lenders' concern about the borrowers' downside risk and their suspicion of borrowers' underreporting risks (Watts, 2003a, 2003b). Previous research (Beatty et al., 2019; Beatty et al., 2008; Bens et al., 2020; Li, 2010) shows that lenders make conservative adjustments to accounting figures used for covenant compliance. Given the possibility of such excessive prudence, the GHGRP's adoption should correct the lenders' bias, decrease the level of estimated carbon emissions, and reduce the interest rate charged. It is empirically difficult to distinguish whether the GHGRP's effect on loan price is due to a reduction in the uncertainty of carbon emissions or a correction of the lenders' overly prudent estimation of carbon emissions but both effects stem from an improvement in carbon-information transparency.

The second confounding effect is that, due to carbon-information deficiencies before the GHGRP, lenders could be misled by borrowers' impression management (Brennan & Merkl-Davies, 2013; Merkl-Davies & Brennan, 2007, 2011). To gain access to more favorable loan terms, borrowers could overstate (understate) positive (negative) carbon information (Herbohn et al., 2022). Indeed, cases studied by Talbot and Boiral (2015) show that industrial and energy-sector emitters engage in various strategies to manage investors' impressions. To the extent that GHGRP reduces the scope of impression management by making more intrinsic carbon information available, disclosed carbon emissions should increase after this policy's adoption. However, this prediction is inconsistent with our baseline finding. The third confounding effect is that the GHGRP could raise the lenders' awareness of the importance of carbon risk and remind the lenders to begin pricing such risk in their lending. However, this confounding effect also predicts the opposite of our baseline finding. Moreover, since voluntary carbon disclosure exists even before the GHGRP, it is unlikely that the GHGRP shocks the lenders' carbon-risk awareness.

Finally, previous literature finds that the GHGRP's adoption decreases firms' actual carbon emissions (Tomar, 2023), which should in turn reduce firms' carbon-risk exposure and loan price. To examine the validity of this alternative explanation, we follow the previous literature (Tomar, 2022) and measure the treated firms' carbon emission change before and after the GHGRP. We find our baseline result is not driven by those firms with greater carbon emission reduction.

We also perform two sets of cross-sectional analyses to investigate the heterogeneity of our baseline result across various treated firms. First, we find that our baseline effect is more pronounced if the treated firms are not rated by credit rating agencies or they are subject to greater analyst forecast errors, or if the lenders of the loan are foreign lenders or do not have prior lending relationships with the firm. Carbon-information opacity is likely to be more severe under these circumstances and the GHGRP's adoption represents a more substantial improvement in these treated firms' carbon information environment, thus the more pronounced results. Second, we condition our test on the materiality of carbon risk and the importance of carbon information. We find a greater reduction for treated firms with more severe financial constraints, higher carbon emissions, headquarters in the states with more effective environmental enforcement, and loans issued by green banks. This evidence lends further support to our baseline result which is driven by carbon-information opacity rather than other factors.

In addition, we examine the impact of the pre-treatment carbon disclosure status. Specifically, we find that the baseline result still persists among the treated firms that have already voluntarily disclosed their carbon information through public channels (e.g., CDP, annual reports, sustainability reports, or company websites) before the GHGRP, confirming that mandatory disclosure provides carbon information beyond what voluntary disclosure does. This piece of evidence is consistent with the concerns raised about the reliability of voluntarily disclosed information (Coffee Jr, 1984; Healy & Palepu, 2001) and the support for mandatory disclosure because of its comparability, strong enforcement, and verifiability (Armour et al., 2021; Herbohn et al., 2022; Hrasky, 2012; Rankin et al., 2011). Furthermore, we isolate those treated firms located in states which have already enforced mandatory carbon disclosure before the GHGRP (namely, California and Massachusetts) and do not find any significant effect of the GHGRP's adoption among these firms. This finding also reinforces that our baseline result is driven by a reduction in carbon-information opacity rather than other factors.

Our study contributes to the growing literature on carbon disclosure. This literature examines the design of carbon disclosure policies (Armour et al., 2021; Bolton et al., 2021; Herbohn et al., 2022; Troeger & Steuer, 2021), determinants of firm voluntary carbon disclosure decision (Dawkins & Fraas, 2011; Guenther et al., 2016; Liesen et al., 2015), the quality and quality assurance of voluntarily disclosed carbon information (Depoers et al., 2016; Tauringana & Chithambo, 2015) (Fan et al., 2021; Green & Li, 2012; Simnett et al., 2009), the effect of carbon disclosure on carbon emission (Downar et al., 2021; Tomar, 2021; Yang et al., 2021), and the effect of carbon disclosure on the cost of equity capital and shareholders' value (Bolton and Kacperczyk, 2021b, Albarrak et al. (2019), Lemma et al. (2019), and Gerged et al. (2021); Ziegler et al. (2011), Liesen et al. (2017)).

Unlike these previous studies, we study the importance of carbon information in corporate finance by examining how mandatory carbon disclosure impacts companies' cost of loan financing. A concurrent work by Kleimeier and Viehs (2018) also documents a negative association between carbon disclosure and cost of debt. However, our study differs from theirs along several important dimensions. First, they rely on the voluntary carbon disclosure through CDP. Since voluntary disclosure is a company decision, their analysis is inevitably subject to a self-selection issue which makes it difficult to establish causality. In contrast, we rely on GHGRP as a mandatory shock to firms' carbon information opacity and mitigates the concern of voluntary disclosure. Also, the GHGRP as a natural experiment allows us to use the DiD specification which is different from the method used in Kleimeier and Viehs (2018). Second, Kleimeier and Viehs (2018) do not distinguish between carbon risk and carbon-information risk as we do. In their analysis, they emphasize both the firm's response to the CDP survey and the carbon emission levels disclosed through CDP. In contrast, we focus on the reduction of carbon-information opacity after the GHGRP.

Our paper also adds to the research on the use of non-financial information in debt contracting. Thompson and Cowton (2004) show that the UK banks rely on environmental information in their lending decisions and consider annual report as an important source of information on corporate environmental impact. Their survey also shows that banks recommend expanding the coverage of corporate environmental disclosure. Attig et al. (2021) find that, whereas greenwashing reduces interest rates, lenders charge higher fees and apply stricter non-price contract terms on borrowing firms engaging in greenwashing. Tan et al. (2020) find that borrowing firms with better CSR disclosure have better access to public debt than other firms and enjoy more favorable terms. Our study sheds new light on this line of research by emphasizing the importance of carbon information in loan contracting.

Our study also offers several practical implications. First, our findings contribute to the debates around imposing mandatory carbon disclosure by governments and regulatory bodies.[4] For example, The SEC in 2022 began its consultation on the proposal of requiring listed companies to disclose climate-related information in their regular filings. Controversies revolve around how material carbon information is for companies and whether climate change falls in the jurisdiction of the SEC.[5] We contribute to the debate by showing that carbon information is material for listed companies, lenders price in the carbon-information opacity when offering loans, and mandatory carbon disclosure reduces firms' borrowing costs and generate positive consequences for the corporate sector. Second, regulators and supervisors are urging banks and financial institutions to carefully assess the financial implication of climate-change risk on their loan portfolios and bear their social responsibility in combating climate change (Basel Committee on Banking Supervision, 2020; Network for Greening the Financial System, 2019, 2020). Our findings show that mandatory carbon disclosure provides lenders with useful information to achieve regulators' requirements. Such information helps to achieve the transparency essential for

[4] Such a move towards regulation began as early as in 2015 when the G20 and the Financial Stability Board established the Task Force on Climate-related Financial Disclosures (TCFD) in response to the Paris Agreement to improve the provision of climate-related financial information. In PriceWaterhousCooper's (PwC) 2020 Climate Risk and Banks Survey, 94% of respondents agreed that TCFD-aligned disclosures should become mandatory (see https://www.pwc.co.uk/financial-services/assets/pdf/rising-to-challenge-climate-risk-in-uk-banking-sector.pdf).
[5] See a post on the Harvard Law School Forum on Corporate Governance, https://corpgov.law.harvard.edu/2022/03/12/the-ongoing-debate-at-the-sec-on-climate-disclosure-rules/.

lenders' carbon risk management. Third, our evidence of reduced financing costs after enhanced carbon information disclosure could motivate more firms to improve the disclosure of their carbon emissions.

The remainder of this paper is structured as follows. Section two introduces the background of the GHGRP. Section three reviews the relevant literature. Section four presents the hypothesis. Section five describes the models, variables, and our sample. Section six presents the empirical results. Finally, section seven concludes the study.

2. Background of the GHGRP

The EPA uses the GHGRP to mandate the disclosure of GHG data and related information by large GHG emission sources, fuel and industrial gas suppliers, and CO_2 injection sites in the U.S., as directed by Congress and under the Clean Air Act authority. A total of 41 categories of reporters are covered by the GHGRP. Reporting is at the facility level except for certain suppliers of fossil fuels and industrial GHGs. Facilities determine whether they are required to report based on the types of industrial operations located at the facility, their emission levels, or other factors. Briefly, facilities meeting any of the following criteria are required to report: (1) GHG emissions from covered sources exceed 25,000 metric tons of carbon dioxide equivalent (CO_2e) per year;[6] (2) Supply of certain products would result in over 25,000 metric tons of CO_2e of GHG emissions if those products were released, combusted, or oxidized; and (3) The facility receives 25,000 metric tons or more of CO_2 for underground injection. Emissions from agriculture and land use are exempted from the rule. The GHGRP covers 85–90% of all GHG emissions from over 8,000 facilities in the U.S. (Jones, 2021).[7]

[6] The CO_2e of a bundle of GHG refers to the amount of emissions in the form of CO_2 that generate the same potential global warming effect of the bundle of GHG. GHGs covered by the GHGRP include CO_2, Methane (CH_4), Nitrous oxide (N_2O), Hydrofluorocarbons (HFCs), sulfur hexafluoride (SF_6), perfluorinated compounds (PFCs), and other fluorinated gases.
[7] Also see https://www.mercatus.org/research/public-interest-comments/adopting-secs-proposed-climate-change-disclosure-rules-would-be.

EPA proposed the GHGRP reporting rule on April 10, 2009 and finalised it on October 30, 2009 (EPA, 2009). Compliant facilities started data collection at the beginning of 2010. The GHGRP first released the reported 2010 emission data to the public in January 2012. There were 29 industrial categories covered in the 2010 emission data. The 2011 emission data were released in September 2012 and included an additional 12 industrial categories, bringing the total coverage to 41 source categories (Kauffmann et al., 2012; Tomar, 2021).[8] From 2013 onwards, the GHGRP reports are submitted annually to EPA before March 31 for emissions in the previous calendar year. EPA uses a multi-step process to verify the accuracy, completeness, and consistency of submitted data. Any violation of the GHGRP requirements is a violation of the Clean Air Act and the offender is subject to heavy penalties[9]. After verification, EPA publishes the annual GHGRP reports to the public in October of the year.

A reporting facility can cease reporting if its annual GHG emissions are either (1) less than 25,000 metric tons of CO_2e for five consecutive years or (2) less than 15,000 metric tons of CO_2e for three consecutive years. In addition, if the facility's annual GHG emissions subsequently increase to the 25,000 metric tons threshold in any calendar year, the facility must resume reporting.[10] For 2020, 7,634 direct emitters reported a total emission of 2.6 billion metric tons of CO_2e. In the same year, 975 suppliers, 93 CO_2 injection facilities, and 6 facilities which inject CO_2 solely for geological sequestration reported data to EPA.[11]

3. Literature review

[8] See "GHGRP 2010: Reported Data," available at https://19january2017snapshot.epa.gov/ghgreporting/ghgrp-2010-reported-data_.html, and "GHGRP 2011: Reported Data," available at https://19january2017snapshot.epa.gov/ghgreporting/ghgrp-2011-reported-data_.html.

[9] $45,268 per day for reporting and recordkeeping violations. See https://www.epa.gov/enforcement/clean-air-act-vehicle-and-engine-enforcement-case-resolutions.

[10] See https://www.ecfr.gov/current/title-40/chapter-I/subchapter-C/part-98/subpart-A/section-98.2.

[11] Detailed information on the GHGRP can be found in the EPA websites: https://www.epa.gov/ghgreporting, https://www.epa.gov/sites/default/files/2014-09/documents/ghgrp-overview-factsheet.pdf, and https://www.epa.gov/ghgreporting/learn-about-greenhouse-gas-reporting-program-ghgrp.

In this section, we survey the previous and concurrent works most closely related to our study, to put our findings into the context of literature. Several studies have provided evidence that lenders price carbon risk in debt contracting. Using an international sample, Ehlers et al. (2021) show that more carbon-intensive firms incur higher interest spreads after the Paris Agreement's enactment and such a pricing effect is insensitive to the borrowing firms' disclosure under CDP. Jung et al. (2018) document a positive relation between carbon emissions and the cost of debt for Australian firms and show that such a positive relation disappears for firms that are aware of carbon risk as is demonstrated through their response to the CDP survey. Using international samples, Kleimeier and Viehs (2018) and Palea and Drogo (2020) find similar evidence.

Delis et al. (2019) find that, after the Paris Agreement, lenders charge higher interest spreads to fossil fuel firms compared with non-fossil fuel firms, due to the substantial policy risk of stranded fossil reserves. Laeven and Popov (2021) report that, after the introduction of carbon tax in a country, domestic banks reduce their fossil lending at home and increase their fossil lending abroad where environmental regulation is less restrictive. Ivanov et al. (2021) focus on two major cap-and-trade legislations in the U.S.: the federal Waxman-Markey cap-and-trade bill and the California cap-and-trade bill. They find that affected firms face shorter loan maturities, more restricted access to permanent forms of bank financing (e.g., term loans), higher interest rates, and more shadow-bank lenders in their lending syndicates. Safiullah et al. (2021) show that carbon emissions impact credit ratings negatively through increasing firms' cash flow uncertainty. Herbohn et al. (2019) find a bank-loan announcement has a positive certification effect on share prices. Their finding suggests that equity investors believe that banks factor borrower firms' carbon risk into loan contracting, and a loan announcement conveys valuable insider information about the firms' carbon-risk exposure.

Overall, this fast-growing literature demonstrates the materiality of carbon risk for loan issuance/grant and loan contracting. However, the literature has done little to understand the consequence of carbon information on borrowing/lending decisions and outcomes. In the current study, we fill in this gap by examining how carbon-information opacity affects loan price.

4. Research design and sample

4.1 Sample selection

Our sample selection starts with all firms that disclose their carbon emission data under the GHGRP in the first year of the GHGRP's public data release, i.e., 2012.[12] Since the GHGRP data are at the establishment level, we aggregate the establishment data to the firm level using their parent company names. There are 2,059 firms in this initial stage. We then merge the GHGRP firms with Compustat firms using parent company names. There are 764 GHGRP firms having financial data in Compustat. In the next step, we collect the loan information from the Thomson Reuters' DealScan database for the above GHGRP firms. As mentioned, since the EPA proposed the GHG reporting rule in April 2009 and the GHGRP reports were first released to public in January 2012, we define the period from April 2009 to December 2011 as the transition period. Our sample period includes five years before the transition period (April 2004 – March 2009) and five years after the transition period (January 2012 – December 2016). There are 4,733 loans issued to 581 GHGRP firms initially retrieved from DealScan. We further exclude loans issued to firms headquartered in Massachusetts or California[13], because these two states have already adopted mandatory carbon disclosure rules similar to the GHGRP before 2012.[14] 4,469 loans issued to 540 GHGRP firms remain in the sample in this step. We also remove loans issued to financial or governmental firms, require each firm to have at least one loan in the pre-GHGRP period and one loan in the post-GHGRP period, and exclude loans with missing data on variables used in the baseline analysis. 2,248 loans issued to 254 GHGRP firms stay in the sample and they form the

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[12] We do not include those firms that subsequently join the GHGRP to avoid some firms self-selecting into the GHGRP coverage, after observing the benefit of reduced loan cost, for example.

[13] Firm headquarter information is collected from the "comphist" table of corporate moves in Compustat.

[14] See https://www.mass.gov/guides/massdep-greenhouse-gas-emissions-reporting-program and https://ww2.arb.ca.gov/our-work/programs/mandatory-greenhouse-gas-emissions-reporting/about.

basis for PSM. Similarly, we filter the loans issued to non-GHGRP firms using the same criteria as above, resulting in 4,090 loans issued to 592 non-GHGRP firms before PSM. The PSM procedures are discussed in detail in Section 5.3. The PSM-matched sample includes 1,959 loans issued to 240 GHGRP and non-GHGRP firms. The sample selection procedure is presented in Table 1, Panel A.

We present the sample distribution by year in Panel B. The distribution is similar between GHGRP and the PSM-matched non-GHGRP firms. For both groups, there are around 100 loans issued every year, except in the year before the GHGRP (April 2008-March 2009). Only 48 loans were issued to GHGRP firms and 56 to non-GHGRP firms during April 2008-March 2009, most probably because of the financial crisis (Ivashina & Scharfstein, 2010). The number of loans in the fourth and fifth year after the GHGRP (January 2015-December 2016) is also slightly lower than in other years. Panel C reports the sample distribution by industry based on the Global Industry Classification Standard (GICS). For GHGRP firms, the industrials sector has the highest number of loans (304) as well as the energy sector (188). Information technology and utilities have the lowest numbers (21 and 23, respectively). For non-GHGRP firms, consumer staples and industrials have the highest numbers of loans (199 and 181, respectively). Similar to the loans of treated firms, information technology and utilities have the lowest numbers (35 and 29, respectively). Also, the industry distribution is similar between the treated and control group.

[Insert Table 1]

4.2 Research design

To assess the impact of the GHGRP adoption on loan pricing, we use a DiD framework, where we compare the difference in the interest rates between the loans granted to the GHGRP firms (treated) and those made to the non-GHGRP (control) firms, before and after the GHGRP (the treatment). Specifically, we use the regression model below:

$$Int.Spread_{i,j,t,k,p,s} = \beta_0 + \beta_1 Treated_j + \beta_2 Treated_j * Post_t + \beta_3 X_{j,t-1} + \beta_4 Z_{i,j,t} + \upsilon_t + \phi_k + \omega_p + \lambda_s + \varepsilon_{i,j,t,k,p,s} \quad (1)$$

Our dependent variable $Int.Spread_{i,j,t}$ is the interest spreads charged on loan *i* issued to firm *j* at time *t*, measured with the DealScan variable All in Spread Drawn (AISD) (i.e., the annual spread paid over LIBOR for each dollar drawn down from the loan). $Treated_j$ is an indicator variable equals one if the borrowing firm *j* discloses under the GHGRP and zero otherwise. $Post_t$ is an indicator variable equals one if the time *t* at which the loan is issued is after January 2012 and zero otherwise. Since we control for year fixed effects, $Post_t$ is omitted from the model. The variable of interest is $Treated_j * Post_t$ whose coefficient β_2 captures the net impact of GHGRP's adoption on the loan price charged to the treated firms relative to that charged to the control firms. $X_{j,t-1}$ is a vector of firm characteristics and $Z_{i,j,t}$ is a vector of loan characteristics. υ_t, ϕ_k, ω_p, λ_s are year, industry, loan purpose, and state fixed effects, respectively, as is suggested in the loan-contracting literature (Bharath et al., 2008; Francis et al., 2016; Ivashina, 2009). We discuss the justification for the inclusion of these control variables in Appendix II, based on the previous literature.

4.3 Propensity score matching

As mentioned, we adopt the PSM approach to address the concern that our treated and control firms might be systematically different in the pre-GHGRP period, which could lead to a difference in their loan price independent of the GHGRP's adoption. We use PSM to identify control firms that do not adopt the GHGRP but otherwise comparable to the GHGRP firms in terms of observable firm characteristics. The PSM procedure starts with the estimation of a logit regression that models each firm's likelihood of reporting under the GHGRP on the mean values of a range of firm characteristics in the pre-GHGRP period. The sample for this regression consists of all borrowing firms (254 GHGRP firms and 592 non-GHGRP firms) that satisfy the sample selection criteria described in Panel A of Table 1. The matching covariates include all firm characteristics in the baseline model (Equation (1)) and industry fixed effects. The results of the logit regression are reported in Panel A of Table 2. Unsurprisingly, larger firms and firms with more tangible assets (measured by Property Plant and Equipment (PPE)) as a percentage of total assets are more likely to be GHGRP compliant. Firms with a higher current ratio are less likely to be compliant, however. Profitability, volatility, and information environment variables do not show a significant impact on the likelihood of compliant. We calculate the propensity score based on the fitted value of the logit regression for each firm. We then match GHGRP-compliant firms with non-GHGRP compliant firms using a one-to-one nearest neighbor (caliper = 0.1) matching based on the propensity scores, without replacement. The PSM procedure leads to 120 GHGRP firms which issue 1,007 loans and 120 matched non-GHGRP firms which issue 952 loans.

Panel B compares firm characteristics between the treated and control firms before the matching and after, for the pre-GHGRP period. It shows that, on average, the treatment and control firms have significant differences in many firm characteristics before the matching. However, none of the differences remains significant at the conventional level after the matching, showing that our PSM sample has achieved covariate balance between the treated and control firms.

[Insert Table 2]

4.4 Summary statistics

Table 3 reports the summary statistics for the PSM sample. Panel A presents the univariate analysis for *Int. Spread*. The average interest spreads for GHGRP firms are higher than that for non-GHGRP firms by 21.934bps pre-GHGRP. However, after GHGRP, this difference reduces to 18.438bps. Notably, the interest spreads increase from pre- to post-GHGRP period for both our treated and control firms, which could be the consequence of the 2008 financial crisis.

Panel B presents the comparisons of the sample means for firm variables and other loan variables between the pre- and post-GHGRP periods, for the treated and control firms respectively. In terms of firm variables, both the GHGRP and non-GHGRP firms increase in size and leverage, decline in earnings quality, and improve in CSR information transparency from pre- to post-GHGRP period. The GHGRP firms also have an increase in current ratio and a reduction in tangibility, whereas the non-GHGRP firms incur a reduction in ROA and credit rating availability. The changes in loan variables from the pre- to post-GHGRP period are generally consistent between the GHGRP and non-GHGRP firms. Specifically, the loan amount is larger; the covenants are less restrictive; the likelihoods of imposing performance pricing provision (PPPs) and credit lines are lower; and the likelihoods of having reputable and related lead arranges are higher. The only exception is maturity. The non-GHGRP firms experience a lift in maturity, but the GHGRP firms do not.

[Insert Table 3]

5. Empirical results

5.1 The baseline results

Table 4 presents the baseline results. In Column (1), *Int. Spread* is regressed on *Treated* and *Treated* * *Post*. The coefficient on the interaction term is negative and significant at the 1% level, indicating that the GHGRP-compliant firms experience a larger reduction in interest rate compared with the control firms after the GHGRP's adoption, consistent with our baseline hypothesis. Column (2) includes the firm-specific and loan-specific controls and the coefficient on *Treated* * *Post* remains negatively significant. Column (3) adds the year, industry, state, and loan purpose fixed effects and the coefficient on *Treated* * *Post* is still negative and significant at the 1% level (coef. = −28.117, t-stat. = -2.72). Economically, this translates to a 28bps reduction in interest spreads after the adoption of GHGRP for GHGRP-compliant firms relative to control firms, which is as much as 17.6% of the average spreads for GHGRP firms before the GHGRP's adoption. These findings provide evidence that lenders used carbon information reported through the GHGRP in pricing their loans.

However, a concern with our main test is that *Treated* might be capturing high carbon emission rather than GHGRP disclosure, since a firm's carbon emission level to a good extent determines whether it needs to report under the GHGRP. Nevertheless, this concern should bias against our findings. If *Treated* indeed captures emission effect rather than disclosure effect, the coefficient on *Treated* * *Post* should have been positive. Given the heightened attention to climate change issues in recent years (Bolton & Kacperczyk, 2021; Ehlers et al., 2021; Ilhan et al., 2021), carbon risk should increase the interest spreads to a larger extent in later years than in earlier years.

The findings on control variables are largely consistent with our expectations. Regarding firm-level controls, we document that interest rate is negatively and significantly correlated with firm size, current ratio, interest coverage ratio, and earnings quality, and positively and significantly correlated with leverage, ROA volatility, and the inverse measure of credit market condition. In terms of loan-level controls, we find that interest spreads are higher for loans with longer maturity, smaller amount, and issued by less reputable lead lenders and institutional lenders, while those with PPPs and revolving terms have lower interest spreads.

5.2 Robustness tests

We conduct a battery of robustness tests to bolster our baseline results. A common concern for the DiD approach is that the results may be driven by pre-existing trends in the interest spreads diverging between treated and control firms. To mitigate this concern, we test the dynamic effect of GHGRP to examine whether the treatment effect appears before or after the GHGRP adoption. Specifically, we replace *Post* in Equation (1) with $Post^m$ (m $\in$ {-4, -3, -2, -1, 1, 2, 3, 4, 5}). $Post^m$ equals one if the loan is issued m years from the GHGRP's adoption and zero otherwise. If the baseline results are driven by diverging trends, such effect should exist even prior to the GHGRP. That is to say, the coefficients on *Treated* * $Post^m$ should be statistically significant both for m<0 and m>0. In contrast, if the treated and control firms satisfy the pre-treatment parallel trend assumption, the coefficients on *Treated* * *Post*^{m} should be significant only for k>0. The results reported in Panel A of Table 5 show that the coefficients on *Treated* * *Post*^{m} are all insignificant for k<0, consistent with the parallel trend assumption and confirming that the baseline results are unlikely to be driven by pre-existing trend differences between the treated and control firms. The coefficients on *Treated* * *Post*^{m} are negative and significant at the 1%, 5%, and 10% levels for k=1, 2, and 3, respectively. The coefficient is insignificant for m>3. The loss in statistical significance of the treatment for years beyond the third year shows that the GHGRP has a prolonged transitory effect instead of a permanent effect, which could be due to the emergence of alternative channels for disclosing or obtaining carbon disclosure in more recent years.

To further establish the casual relation between GHGRP's adoption and interest rate changes, we conduct a placebo test. We choose January 2007 as the placebo event date and re-examine the change in loan pricing from three years before to three years after the placebo event using the same DID model and treated and control firms as in the main test. We chose the placebo event date and the three-year test window as such to maximize the placebo period's overlap with the pre-GHGRP period and, meanwhile, minimize its overlap with the post-GHGRP and the transition periods. If our baseline results are driven by unobserved factors that affect loan spreads differently for treated vs. control firms, we should expect the placebo event to have similar effects on loan spreads as does the GHGRP, assuming the same factors take effect at the actual treatment event and the placebo event. The results reported in Panel B, Table 5 show that the coefficient on *Treated* * *Post_pseudo* is insignificant, suggesting that the reduction in interest rate for the treated relative to the control firms does not occur due to unmeasured underlying factors.

[Insert Table 5]

To further rule out the possibility that our baseline result is caused by unobserved shocks, we conduct a placebo test using randomly assigned pseudo-treated group and pseudo-control group. Specifically, we randomly assign 120 firms as the pseudo treated group, in accordance with the actual number of treated firms used in our baseline analysis, and match the control group using the same PSM prediction equation from the baseline analysis as the pseudo-control group. Based on these pseudo-treated and control groups, we re-estimate model (1) and save the coefficients on $Treated * Post$. We repeat this procedure 1,000 times. The distribution reported in Figure 1 indicates that the randomly assigned treated and control firms do not present significant results, which further exclude the potential effect of any observed confounding shocks.

[Insert Figure 1]

In Table 6, we test the robustness of our baseline results to alternative matching methods. In the first column, we adopt within-industry matching instead of including industry fixed effects in the PSM. This is another approach to ensure that our treated and control firms are balanced across industries. Under this approach, it's more difficult to find matching pairs and the sample size reduced by 32.5% to 1,323 accordingly. However, the baseline results still hold with this alternative matching method. In Column (2), we control for industry fixed effects in the PSM based on the 4-digit instead of 2-digit GICS code. The number of matching pairs is also reduced in this test, but the coefficient on *Treated* * *Post* remains negatively significant.

In Column (3), we adopt the entropy-balanced matching technique of Hainmueller (2012). This technique re-weights each control observation so that post-weighting distributional properties of matching variables of treated and control observations are virtually identical, thereby ensuring covariate balance. The matching variables we use here are the same as those used in the PSM and we ensure that the first three moments (i.e., mean, standard deviation, and skewness) of the matching variables are balanced between the treated and control firms in the pre-GHGRP period. Several recent studies (Chapman et al., 2019; Di Maggio & Yao, 2020; Fei, 2022) have used this technique to mitigate the potential selection bias. Compared with PSM, entropy balancing retains the full sample rather than discards the unmatched observations, and therefore preserves statistical power and generalizability. It also obtains a higher degree of covariance balance by matching on the mean, variance, and skewness rather than just the mean. In addition, entropy balancing permits less researcher discretion on matching algorithms than PSM, overcoming the concern about PSM that "seemingly innocuous design choices greatly influence sample composition and estimates" raised by Shipman et al. (2017). Our baseline results remain qualitatively unchanged using this entropy-balanced sample.

[Insert Table 6]

Table 7 reports the sensitivity analyses related to sample adjustments for various reasons. In the baseline sample, as mentioned, we do not allow a firm to be in our treated sample if it joins the GHGRP after the first year of the GHGRP's adoption, but we allow our treated firms to drop out from the GHGRP during the post period. In a robustness test, we strengthen this requirement and only include as treated those firms that continuously report under the GHGRP during the whole post-GHGRP period.[15] 13 out of 120 treated firms and 240 loans are excluded with this additional filter.[16] Column (1) shows that our baseline results are qualitatively unaffected. Column (2) presents the result using three years before and after the GHGRP as our test period, instead of five years. We continue to document a significantly larger decrease in the interest spreads for the treated relative to the control firms. Columns (3) and (4) address the concern that some firms may manipulate their emissions downward before the GHGRP, e.g., through outsourcing or selling facilities, to avoid reporting. If the emission management is at the expense of increasing other financial risks, lenders may raise the interest rate on these firms and, therefore, including these firms in the control group adds noise to our analysis. We identify those firms suspicious of emission management based on their changes in carbon emissions[17] and PPE around 2010[18] in Columns (3) and (4), respectively. If a firm's reduction in carbon emissions/PPE is among the top

[15] To alleviate the concern on survival bias, we require control firms to have data in Compustat in the whole post-GHGRP period as well.

[16] Only few treated firms are filtered out with this sample requirement, because as discussed in Section 2, it's rare for firms to drop out from the GHGRP. Their GHG emissions must have dropped below certain thresholds for three/five consecutive years before they can cease reporting.

[17] Since GHGRP emission data is only available for treated firms but not for control firms. We use the S&P Trucost carbon emission data. The data coverage starts from 2002 and covers our whole sample period. However, only a subset of our sample firms has data in Trucost, reducing the sample size in this robustness test.

[18] Whether a facility must disclose or not was initially considers the facility's emission level in 2010.

5% in the full sample, we deem the firm as suspicious and remove it from the group of potential controls before PSM. As shown in Columns (3) and (4), our baseline result still holds after removing those suspicious firms. Debt contracting studies often exclude utility firms because these firms are highly regulated and their debt contract could be different from other firms' (Ben-Nasr et al., 2021; Chu, 2021; Ertugrul et al., 2017; Jiang et al., 2018). We don't remove utility firms in the main tests because carbon risk and carbon information are particularly relevant for this industry. In Column (5), we exclude the utility firms and the coefficient on *Treated* * *Post* remains significantly negative. In column (6), we include transition period into pre-GHGRP period, i.e., five years before 2012 as the pre-treatment period. We have excluded the transition period in our baseline analysis, to minimize the complication of noises due to possible rumors, speculations, and information leakage. Our result in Column (6) shows that our baseline effects remain significant after adding back this transition period.

[Insert Table 7]

In Table 8, we further include year × industry fixed effects and year × state fixed effects to control for any intertemporal shocks (e.g., policies, technology updates, etc.) occurring at the industry or state level and our result remains unchanged.

[Insert Table 8]

5.3 Cross-sectional tests

This section explores the cross-sectional heterogeneity of our baseline analysis. First, we examine whether the GHGRP's effect varies with the information asymmetry between borrowers and lenders. We add a triple interaction term between *Treated*, *Post*, and an indicator for greater information asymmetry to Equation (1). Following prior literature (Bharath et al., 2011; Haselmann & Wachtel, 2011; Maskara & Mullineaux, 2011; Sufi, 2007), we adopt four indicators for greater information asymmetry: (1) the borrower firm doesn't have a credit rating (*Unrated*); (2) the borrower firm's analyst forecast error is above the sample median (*Large Analyst-forecast Error*); (3)

the average percentage of the loans issued to the borrower firm in the previous five years arranged by the lead lenders of the current loan is below the sample median (*Weak Lending Relationship*); and (4) the percentage of foreign lead lenders is above the sample median (*High Foreign Lead%*). If the GHGRP's effect is driven by carbon-information opacity, it should be more pronounced with greater information asymmetry. Therefore, we expect a negative coefficient on the triple interaction term between *Treated*, *Post*, and the indicator for greater information asymmetry. The results reported in Table 8 are consistent with this prediction. The coefficients on the triple interaction term are consistently negative and significant at the conventional levels for all four indicators of information asymmetry, lending support to the information channel as the underlying mechanism through which the GHGRP's adoption affects the treated firm's loan pricing.

[Insert Table 9]

Next, we examine a competing explanation that GHGRP firms' measured carbon emissions decrease after the GHGRP's adoption. As discussed in the introduction, such a decrease could be for two reasons. Under the prudent investor theory, the GHGRP should reduce the lenders' concern and their intention to overestimate borrowers' carbon emission for prudence purposes. Several recent studies (Tomar, 2021) find that facilities reduce their carbon emissions after mandatory carbon disclosure. The reduction in carbon emissions mitigates firms' carbon-risk exposure, which in turn decreases borrowers' loan spreads. To examine the validity of this alternative explanation, we add a triple interaction term between *Treated*, *Post*, and an indicator for treated firms that substantially cut their carbon emissions (*Large Relative Emission Reduction*) to Equation (1). If the GHGRP effect is indeed driven by the reduction in carbon emissions, the coefficient on *Treated* * *Post* * *Large Relative Emission Reduction* should be significantly negative and the coefficient on *Treated* * *Post* should be less significant than in the baseline tests. In Table 10, following Tomar (2021), non-GHGRP firms benchmark the GHGRP firms and reduce their emissions. Therefore, we set *Large Relative Emission Reduction* to one if the treated firm's reduction in carbon emissions post-GHGRP (calculated as the mean carbon emissions in year 1-2 minus the mean carbon emissions in year 3-5) is above the sample median, and zero otherwise. Carbon emissions data are from the GHGRP's website[19, 20]. The result shows that the coefficient on *Treated* * *Post* * *Large Relative Emission Reduction* is insignificant and the coefficient on *Treated* * *Post* remains significantly negative with a magnitude even larger than that in Table 4.

However, this test is not perfect. As mentioned in Jiang (2023) and Yang et al. (2021), they do not find evidence that treated firms reduce their emissions after the GHGRP, and there is an emission shifting behavior across the facilities after GHGRP.

The results in Table 10 that the reduction in interest rate post-GHGRP does not rely on the reduction in carbon emissions, inconsistent with the emission reduction channel as an alternative mechanism of the GHGRP effect. At the same time, we find that the GHGRP effect is more pronounced for firms with greater opaqueness. Overall, we can say that our result is mainly driven by the reduction in carbon-information opacity rather than the reduction in carbon emissions.

[Insert Table 10]

After documenting evidence for the information channel and against the carbon-emissions-reduction channel, we continue to examine whether the GHGRP's effect varies with the importance of carbon information. Specifically, we identify four scenarios where carbon information is particularly relevant important. First, a borrower firm is financially constrained. It is usually costly to invest in carbon risk management and switch to carbon-efficient technologies. Financially constrained firms could be more exposed to carbon-risk, ceteris paribus, due to their lack of financial resources. Therefore, carbon information could be particularly important for these firms. We define a firm as financially constrained if its Hadlock-Pierce (HP) index is above the sample median (*High Financial Constraints*). Second, the borrower firm's carbon emission is high.

[19] https://www.epa.gov/ghgreporting/data-sets

[20] The emissions data are not available for a small number of GHGRP-compliant firms, since EPA classifies the data of these firms as "confidential business information" EPA. (2013). *Greenhouse Gases Reporting Program implementation: Fact sheet*. Retrieved 21 April from https://www.epa.gov/sites/default/files/2014-09/documents/ghgrp-overview-factsheet.pdf. These data are submitted to the GHGRP, but not released to the public. Therefore, there is a small (6 firms) reduction in sample size for the tests using the GHGRP emissions data (Table 9 and Table 10 Column (2)).

High-emission firms are especially sensitive to climate policy risks because these policies (e.g., cap-and-trade) are designed to target heavy emitters. High emission firms could be less willing to disclose their carbon information voluntarily with good quality. Therefore, mandated carbon disclosures are particularly important for these firms. We define a firm as having high emissions if its emissions are among the top quartile of the sample (*High Emission*). Third, the borrower firm is headquartered in a state with high environmental enforcement intensity. Intensive public enforcement increases emitters' litigation risk and potential penalties, and, therefore, raises the importance of carbon information. A firm is deemed as from a high enforcement intensity state if its headquarters are in a state where the number of environmental enforcement cases scaled by the total number of establishments is among the top 20 states (*High Enforcement Intensity*). Fourth, the lead lenders of the loan have high environmental awareness. Creditors of high awareness would attach greater importance to carbon information. We define a loan as issued by lenders with high environmental awareness if none of its lead lenders have environmental-related negative incidents covered in RepRisk during our sample period (*Green Bank*). We add a triple interaction term between *Treated*, *Post*, and one of the indicators above to Equation (1) and report the results in Table 11. The coefficients on the triple interaction terms are consistently negative and significant across Columns (1)–(4), confirming that the GHGRP's effect is stronger when carbon information is more important.

[Insert Table 11]

Next, we examine the GHGRP's impact on loan interest spreads for those firms that have either disclosed carbon information voluntarily or have done so under a mandatory disclosure regime (i.e., in the states of Massachusetts and California), before the GHGRP. The results in Table 12 Column (1) shows that the coefficient on *Treated* × *Post* remains significantly negative for those firms that voluntarily disclosed, suggesting that mandatory carbon disclosure through the GHGRP provides useful incremental information to lenders, relative to the voluntary disclosure. In Column (2), we use those firms that headquartered in Massachusetts or California and repeat our estimation. The coefficient on *Treated* × *Post* is insignificant (*coef.* = 21.576, t = 0.30), showing that the carbon information provided by GHGRP is not superior to other mandatory carbon disclosure rules.

[Insert Table 12]

6. Conclusion

This paper investigates to what extent carbon-information opacity affects the price of bank loans. Using the GHGRP as an exogenous shock to carbon-information opacity, we find that GHGRP-compliant firms experience a greater reduction in loan interest rate than control firms. This finding is robust to a series of robustness tests, showing the results are not driven by the selection of sample and sample period, matching methods, reduction in actual carbon emissions, or macro confounding events. Greater firm opaqueness and carbon-risk materiality strengthen the above finding, further confirming the GHGRP's informational effect. Also, the mandatory disclosure of GHGRP lowers loan price even for firms with previous voluntary carbon disclosure, but its effect vanishes if a borrower firm has already been disclosing under other mandatory carbon-disclosure policies.

Our study contributes to the growing literature on carbon disclosure (e.g., Downar et al., 2021; Tomar, 2021; Yang et al., 2021), by highlighting the importance of mandatory carbon disclosure in reducing information opacity and financing costs. We also add to the research on lenders' use of non-financial information in debt contracting (e.g., Thompson and Cowton, 2004; Attig, Rahaman, and Trabelsi, 2021; Tan, Tsang, Wang, and Zhang, 2020), by showing that mandatory carbon disclosure provides lenders with useful information. Practically, we contribute to the debate around to what extent the regulators should impose mandatory carbon disclosure (e.g., the SEC's 2022 consultation of imposing mandatory carbon disclosure on its registrants), by showing that carbon information is an essential consideration for lenders.

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Table 1: Sample Selection and Distribution

This table presents the sample selection criteria (Panel A) and distribution of the main sample by year (Panel B) and by industry (Panel C).

Panel A: Sample selection criteria

	No. of GHGRP Firms	No. of Loans to GHGRP Firms	No. of non-GHGRP Firms	No. of Loans to non-GHGRP Firms
All firms that have reported under the GHGRP since 2012.	2,059			
Require firm financial data available from Compustat.	764			
Require loan data available from DealScan during April 2004–March 2009 and January 2012–December 2016.	581	4,733	3,512	17,722
Exclude loans issued to firms headquartered in Massachusetts or California.	540	4,469	2,927	15,211
Exclude loans issued to financial firms or governmental firms.	530	4,333	2,402	12,351
Require each firm to have at least a loan in the pre-GHGRP period (i.e., April 2004–March 2009) and a loan in the post-GHGRP period (January 2012–December 2016).	346	3,650	891	7,494
Exclude loans with missing data on variables used in the baseline analysis.	254	2,248	592	4,090
The PSM sample.	120	1,007	120	952

Panel B: Sample distribution by year (after PSM)

Year	No. of Loans to GHGRP firms	No. of Loans to non-GHGRP firms
Y^{-5} (April 2004–March 2005)	121	108
Y^{-4} (April 2005–March 2006)	130	112
Y^{-3} (April 2006–March 2007)	116	107
Y^{-2} (April 2007–March 2008)	119	95
Y^{-1} (April 2008–March 2009)	48	56
Y^{1} (January–December 2012)	103	99
Y^{2} (January–December 2013)	93	116
Y^{3} (January–December 2014)	110	106
Y^{4} (January–December 2015)	82	91
Y^{5} (January–December 2016)	85	62
Total	1,007	952

Panel C: Sample distribution by Global Industry Classification Standard (GICS) industry

Industry	No. of loans to GHGRP firms	No. of loans to non-GHGRP firms
Energy	188	153
Materials	149	155
Industrials	304	181
Consumer Discretionary	126	113
Consumer Staples	143	199
Health Care	53	87
Information Technology	21	35
Utilities	23	29
Total	1,007	952

Table 2: Propensity Score Matching Estimation and Diagnostics

This table reports the propensity score estimates and the covariate balance diagnostics. We match firms one to one using the nearest-neighbor (caliper = 0.1) propensity score matching (PSM) procedure, based on the mean values of a set of firm variables measured in the five years before GHGRP and the industry fixed effects. *Treated* is an indicator variable equals one if the borrowing firm are required to report their emissions under the GHGRP and zero otherwise. Panel A presents the estimates of the logit model used to estimate the propensity scores of the treatment and control groups. *z*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Panel B reports the covariate balance test results for the treatment and control firms, before and after matching. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

Panel A: Propensity score estimation

	P(*Treated*=1)
Ln(Total Assets)	0.705***
	(6.62)
Leverage	1.095
	(1.11)
Current Ratio	−0.309**
	(−2.12)
ROA	0.553
	(0.20)
ROA Volatility	3.778
	(1.07)
Ln(Int. Coverage)	0.155
	(0.84)
Market-to-Book	−0.344
	(−1.54)
Tangibility	2.499***
	(3.90)
Earnings Quality	−0.110
	(−0.78)
Unrated	−0.124
	(−0.41)
Good CSR Information	−0.009
	(−0.04)
Industry fixed effects	Yes
Pseudo R^2	0.439
N	846

Panel B: Covariate balance test - comparison of the firm means in the five years before the GHGRP

	Before matching (the full sample)			Propensity-score matched		
	Treated Firm (*N*=254) (A)	Control Firm (*N*=592) (B)	Difference *t*-test (A)−(B)	Treated Firm (*N*=120) (C)	Control Firm (*N*=120) (D)	Difference *t*-test (C)−(D)
Ln(Total Assets)	8.278	7.195	1.083***	7.845	7.979	−0.126
Leverage	0.268	0.221	0.048***	0.251	0.238	0.013
Current Ratio	1.461	1.934	−0.473***	1.716	1.730	−0.015
ROA	0.055	0.060	−0.005	0.059	0.066	−0.006
ROA Volatility	0.034	0.031	0.003	0.031	0.032	−0.001
Ln(Int. Coverage)	2.009	2.485	−0.476***	2.279	2.367	−0.088
Market-to-Book	1.576	1.882	−0.306***	1.751	1.765	−0.014
Tangibility	0.578	0.416	0.164***	0.507	0.527	−0.020
Earnings Quality	−1.163	−1.034	−0.129*	−1.002	−0.972	−0.030
Unrated	0.177	0.443	0.266***	0.275	0.233	0.042
Good CSR Information	0.623	0.637	−0.014	0.663	0.667	−0.033

Table 3: Summary Statistics

This table reports the differences in the *Int. Spread* (Panel A) and other variables (Panel B) between pre- and post-GHGRP periods for the treated and control firms. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

Panel A: Univariate analysis of the differences in *Int. Spread* - comparison of the sample means

	Treated firm (A)	Control firm (B)	Difference *t*-test (A)−(B)
Pre-GHGRP (C)	159.423	137.489	21.934***
Post-GHGRP (D)	197.885	216.323	−18.438**
Difference *t*-test (D)−(C)	38.462***	78.834***	−40.372***

Panel B: Other variables - comparison of the sample means

	Treated firm			Control firm		
	Pre-GHGRP (*N*=534) (A)	Post-GHGRP (*N*=473) (B)	Difference *t*-test (B)−(A)	Pre-GHGRP (*N*=478) (C)	Post-GHGRP (*N*=474) (D)	Difference *t*-test (D)−(C)
Firm variables						
Ln(Total Assets)	8.119	8.886	0.767***	8.090	8.676	0.586***
Leverage	0.267	0.310	0.044***	0.262	0.334	0.071***
Current Ratio	1.548	1.637	0.088*	1.698	1.666	−0.032
ROA	0.057	0.054	−0.003	0.061	0.047	−0.013***
ROA Volatility	0.027	0.031	0.003	0.030	0.031	0.001
Ln(Int. Coverage)	2.167	2.152	−0.015	2.197	2.106	−0.091
Market-to-Book	1.762	1.728	−0.034	1.732	1.713	−0.019
Tangibility	0.494	0.473	−0.021*	0.497	0.481	0.016
Earnings Quality	−0.984	−1.599	−0.615***	−0.961	−1.364	−0.403***
Unrated	0.193	0.233	−0.040	0.178	0.272	−0.094***
Good CSR Information	0.577	0.725	0.148***	0.711	0.816	0.105***
Credit-market Condition	−0.372	0.716	1.088***	−0.317	0.719	1.036***
Loan variables						
Ln(Maturity)	3.785	3.801	0.016	3.828	3.928	0.100***
Ln(Loan Amt.)	5.571	6.187	0.616***	5.663	5.963	0.300***
Ln(Covenant Count)	1.352	1.192	−0.160***	1.455	1.203	−0.252***
PPP	0.515	0.336	−0.179***	0.561	0.352	−0.208***
Inst. Loan	0.086	0.104	0.017	0.121	0.101	−0.020
Revolver	0.719	0.658	−0.061**	0.676	0.593	−0.083***
Reputable Lead	0.768	0.911	0.143***	0.818	0.943	0.125***
Related Lender	0.614	0.850	0.236***	0.611	0.861	0.250***

Table 4: Baseline Analysis: the GHGRP's Treatment Effect on Loan Interest Spreads

This table reports the results of the baseline analysis of the effect of GHGRP's adoption on the loan interest spreads using the difference-in-differences approach and the propensity-score matched sample. The dependent variable is Int. Spread, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. Treated is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. Post is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. t-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. N denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*		
	(1)	(2)	(3)
Treated	21.934***	22.150***	17.520**
	(2.74)	(3.63)	(2.20)
*Treated * Post*	−40.372***	−32.173***	−28.117***
	(−3.51)	(−3.69)	(−2.72)
Firm Characteristics			
Ln(Total Assets)		−3.600	−9.342**
		(−1.54)	(−2.41)
Leverage		42.940**	50.279*
		(2.25)	(1.78)
Current Ratio		−11.513***	−11.327***
		(−3.67)	(−2.73)
ROA		−95.227**	−76.147
		(−1.97)	(−1.15)
ROA Volatility		197.796***	162.550*
		(2.95)	(1.92)
Ln(Int. Coverage)		−10.039***	−12.72*
		(−2.68)	(−1.86)
Market-to-Book		−8.460**	−7.488
		(−1.98)	(−1.24)
Tangibility		16.607	23.718
		(1.28)	(1.17)
Earnings Quality		−9.385***	−8.514***
		(−4.77)	(−2.83)
Unrated		5.151	0.018
		(0.84)	(0.00)
Good CSR Information		−3.503	7.436
		(−0.67)	(1.00)
Credit-market Condition		21.028***	21.166***
		(6.70)	(3.04)
Loan Characteristics			
Ln(Maturity)		15.167***	10.196*
		(3.80)	(1.92)
Ln(Loan Amt.)		−20.270***	−19.025***
		(−9.98)	(−5.86)
Ln(Covenant Count)		15.675***	8.242
		(3.23)	(1.33)
PPP		−43.811***	−35.336***
		(−8.09)	(−6.10)
Inst. Loan		65.332***	47.678***
		(7.59)	(3.02)
Revolver		−55.501***	−44.022***
		(−10.28)	(−5.56)
Reputable Lead		−34.157***	−31.254***
		(−4.92)	(−3.43)
Related Lender		−27.448***	−3.177
		(−5.03)	(−0.45)

Table 4: Continued

	Dependent variable = *Int. Spread*		
	(1)	(2)	(3)
Year Fixed Effects	No	No	Yes
Industry Fixed Effects	No	No	Yes
Loan Purpose Fixed Effects	No	No	Yes
State Fixed Effects	No	No	Yes
Adjusted R^2	0.054	0.466	0.577
N	1,959	1,959	1,959

Table 5: Dynamic Effect Test and Placebo Test

This table reports the results of the parallel trend test. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. Panel A reports the dynamic effect of the GHGRP's adoption on the loan interest spreads. $Post^m$ (m ∈ {-4, -3, -2, -1, 1, 2, 3, 4, 5}) is an indicator variable equals one if a loan is issued k years from the GHGRP's adoption and zero otherwise. Panel B reports the placebo test results using a pseudo sample period (i.e., 2004–2006 vs. 2007–2009). *Post_pseudo* is an indicator variable equals one if the loan is issued after January 2007 and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

Panel A: Dynamic effect

	Dependent variable = *Int. Spread*
$Treated * Post^{-4}$	3.485
	(0.26)
$Treated * Post^{-3}$	−19.468
	(−1.18)
$Treated * Post^{-2}$	−7.994
	(−0.43)
$Treated * Post^{-1}$	−2.077
	(−0.09)
$Treated * Post^{1}$	−59.267***
	(−2.90)
$Treated * Post^{2}$	−45.360**
	(−2.16)
$Treated * Post^{3}$	−36.636**
	(−1.97)
$Treated * Post^{4}$	−15.596
	(−0.72)
$Treated * Post^{5}$	−4.389
	(−0.21)
Control variables and fixed effects	Yes
Adjusted R^2	0.574
N	1,959

Panel B: Placebo test

	Dependent variable = *Int. Spread*
$Treated *$ *Post_pseudo*	−7.975
	(−0.71)
Control variables and fixed effects	Yes
Adjusted R^2	0.643
N	1,107

Table 6: Robustness Tests: Alternative Matching Methods

This table reports the robustness test results using alternative matching methods. In Column (1), we adopt within-industry matching instead of including industry fixed effects in the PSM prediction equation. In Column (2), we control for industry fixed effects in the PSM prediction equation based on 4-digit instead of 2-digit GICS code. In Column (3), we adopt the entropy-balancing procedure. The dependent variable is *Int. Spread*, measured by the All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*		
	(1) Within industry matching	(2) 4-digit GICS code	(3) Entropy balancing
$Treated * Post$	−24.369**	−19.249*	−25.097**
	(−2.03)	(−1.66)	(−2.51)
Control variables and fixed effects	Yes	Yes	Yes
Adjusted R^2	0.556	0.569	0.566
N	1,323	1,820	6,338

Table 7: Robustness tests: Alternative Sample Selections

This table reports the robustness test results using alternative sample selections. In Column (1), we require the treated firms to report continuously under the GHGRP during the entire post-GHGRP period. In Column (2), we use the three years before and three years after the GHGRP as the test period. In Columns (3) and (4), we drop those firms suspicious of emission management based on their changes in carbon emissions and changes in PPE around 2010, respectively. If a firm's reduction in carbon emissions/PPE is among the top 5% of the full sample, we deem the firm as suspicious and remove it from the control group before conducting PSM. In Column (5), we exclude the utility firms. In Column (6), we add transition period to pre-treatment period. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm report its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*					
	(1) Require continuous reporting	(2) 3-year pre vs. 3-year post	(3) Drop suspicious firms (changes in emissions)	(4) Drop suspicious firms (changes in PPE)	(5) Drop utility firms	(6) Include transition period
$Treated * Post$	-24.941^{**}	-31.197^{**}	-23.442^{**}	-21.544^{**}	-26.394^{**}	-27.783^{***}
	(−2.14)	(−2.57)	(−2.07)	(−2.05)	(−2.32)	(−2.60)
Control variables and fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.587	0.619	0.624	0.566	0.562	0.556
N	1,719	922	1,345	1,713	1,699	1,692

Table 8: Robustness tests: Alternative Fixed Effects

This table reports the robustness test results using alternative fixed effects. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm report its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. The control variables are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*	
	(1)	(2)
$Treated * Post$	−26.263***	−30.816***
	(−2.69)	(−2.76)
Industry Fixed Effect	No	Yes
State Fixed Effect	Yes	No
Loan Purpose Fixed Effect	Yes	Yes
Year * Industry Fixed Effect	Yes	No
Year * State Fixed Effect	No	Yes
Control variables	Yes	Yes
Adjusted R^2	0.580	0.595
N	1,956	1,902

Table 9: The Moderating Effect of Information Asymmetry

This table reports the moderating effect of information asymmetry on the GHGRP's impact on loan interest spreads. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. *Unrated* is an indicator variable equals one if the borrowing firm is not rated by S&P for its long-term credit and zero otherwise. *Large Analyst-forecast Error* is an indicator variable equals one if the analyst forecast error is above the sample median and zero otherwise. Analyst forecast error is measured as the absolute value of the difference between actual earnings per share and the latest I/B/E/S median consensus analyst forecast (reported immediately before the earnings announcement date for the previous quarter) scaled by the stock price at the beginning of the announcement quarter. *Weak Lending Relationship* is an indicator variable equals one if the amount of loans borrowed by the borrowing firm from the lead bank scaled by the total amount of all loans borrowed by the borrowing firm in the past five years is below the sample median (Bharath et al., 2011) and zero otherwise. *High Foreign Lender %* is an indicator variable equals one if the percentage of foreign lead lenders among all lead lenders of a loan is above the sample median and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*			
	(1)	(2)	(3)	(4)
*Treated * Post*	−14.592	−4.619	−2.617	−23.098
	(−1.36)	(−0.31)	(−0.18)	(−1.57)
*Treated * Post * Unrated*	−61.770**			
	(−2.13)			
*Treated * Post * Large Analyst-forecast Error*		−38.820*		
		(−1.90)		
*Treated * Post * Weak Lending Relationship*			−43.690***	
			(−2.60)	
*Treated * Post * High Foreign Lender %*				−46.755**
				(−2.15)
Control variables and fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.582	0.578	0.579	0.584
N	1,959	1,959	1,959	1,959

Table 10: The Moderating Effect of Carbon-emission Reduction

This table reports the moderating effect of post-GHGRP carbon-emission reduction on GHGRP's impact on loan interest spreads. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if the loan is issued after January 2012 and zero otherwise. *Large Emission Reduction* is an indicator variable equals one if the treated firm's carbon-emission reduction after GHGRP is above the sample median and zero otherwise. In Column (1), carbon-emission reduction is measured by the difference between the mean annual carbon emissions in years 1–2 and the mean annual carbon emissions in years 3–5. In Column (2), reduction in carbon emissions is measured by the reduction rate which is the emission difference in Column (1) scaled by the mean annual carbon emissions in years 1–2. The intercept, controls, and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*	
	(1) Reduction amount	(2) Reduction rate
Treated * *Post*	−38.015**	−45.612***
	(−2.38)	(−2.83)
Treated * *Post* * *Large Emission Reduction*	13.435	26.894
	(0.63)	(1.25)
Intercept, control variables, and fixed effects	Yes	Yes
Adjusted R^2	0.561	0.562
N	1,907	1,907

Table 11: The Moderating Effect of the Importance of Carbon Information

This table reports the moderating effect of carbon information's importance to lenders or borrowers on the GHGRP's effect on loan interest rates. The dependent variable is *Int. Spread*, measured by All in Spread Drawn (AISD) retrieved from DealScan which is the annual spread paid over LIBOR for each dollar drawn down from the loan. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if a loan is issued after January 2012 and zero otherwise. *High Financial Constraints* is an indicator variable equals one if the borrowing firm's Hadlock-Pierce (HP) financial constraints index is above the sample median and zero otherwise. Following Hadlock and Pierce (2010), HP index is calculated as $-1.002 * Size + 0.043 * Size^2 - 0.040 * Age$, where *Size* is the log of Min(Total assets (#AT), $4.5 billion), and *Age* is Min(years on CRSP, 37 years). *High Emission* is an indicator variable equals one if the borrowing firm's carbon emission is in the top quartile of the sample and zero otherwise. *High Enforcement Intensity* is an indicator variable equals one if a borrowing firm's headquarter is in a state where the number of environmental enforcement cases scaled by the total number of establishments in the state is among the top 20 states and zero otherwise. *Green Bank* is an indicator variable equals one if none of the lead lenders of the loan has environmental-related negative incidents reported in RepRisk during the whole sample period and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

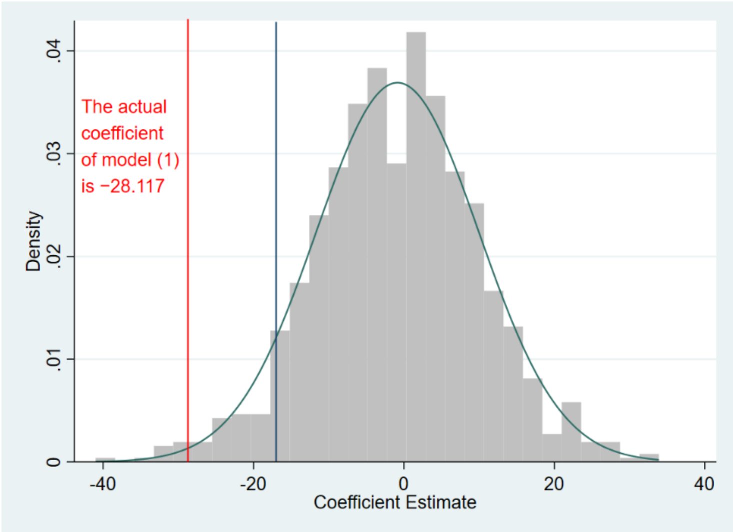
	Dependent variable = *Int. Spread*			
	(1)	(2)	(3)	(4)
*Treated * Post*	−5.256	10.083	−9.685	−10.155
	(−0.37)	(0.59)	(−0.70)	(−0.73)
*Treated * Post * High Financial Constraints*	−42.996**			
	(−2.02)			
*Treated * Post * High Emission*		−52.937**		
		(−2.42)		
*Treated * Post * High Enforcement Intensity*			−44.143**	
			(−2.02)	
*Treated * Post * Green Bank*				−70.033***
				(−3.11)
Control variables and fixed effects	Yes	Yes	Yes	Yes
Adjusted R^2	0.582	0.579	0.579	0.571
N	1,959	1,907	1,959	1,888

Table 12: The GHGRP's Impact on Loan Interest Spreads for Firms with Prior Voluntary or Mandatory Disclosure

This table reports the GHGRP's impact on firms' loan interest spreads for those firms that disclose their carbon information either voluntarily (Column 1) or under the (mandatory) regulations of Massachusetts (MA) and California (CA) (Column 2). The dependent variable is *Int. Spread*, measured by the All in Spread Drawn (AISD) retrieved from DealScan, which is the annual spread paid over LIBOR for each dollar drawn down from the loan. The PSM procedure is the same as in Table 2. *Treated* is an indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise. *Post* is an indicator variable equals one if a loan is issued after January 2012 and zero otherwise. The control variables and fixed effects are the same as those in Column (4) of Table 4 and are not reported here for brevity. *t*-statistics reported in parentheses are based on standard errors corrected for heteroskedasticity and clustered by firm. Continuous variables are winsorized at the 1st and 99th percentiles. Variable definitions are presented in the Appendix I. *N* denotes the number of observations. *, **, *** denote significance at the 10 percent, 5 percent, and 1 percent levels, respectively (two-tailed).

	Dependent variable = *Int. Spread*	
	(1) Firms under voluntary disclosure	(2) Firms under mandatory disclosure (CA & MA)
$Treated$ * *Post*	−35.816**	21.576
	(−2.58)	(0.30)
Control variables and fixed effects	Yes	Yes
Adjusted R^2	0.685	0.700
N	796	93

Figure 1: Placebo Treatment Effect



This figure shows the histogram of the coefficients on $Treated * Post$ from bootstrap simulations of model (1). We use a random sample of firms (the number is the same as the number of actual treated firms in our baseline analysis) as the "pseudo-treated group" and match the control group using the same PSM prediction equation from the baseline analysis as the "pseudo-control group". Based on these "pseudo" treated and control groups, we re-estimate model (1) and save the coefficients on $Treated * Post$. We repeat this procedure 1,000 times.

Appendix I: Variable Definitions and Data Sources

Variable name	Definition	Data source
Credit-market Condition	Credit market condition in the month of loan issuance, measured by the principal component of three macroeconomic factors: 1) the difference between the yields on Moody's BAA- and AAA-rated corporate bonds; 2) the difference between the yields on ten-year government securities and three-month Treasury Bill; and 3) the yield on the three-month Treasury Bill. A higher value indicates a worse credit market condition.	Federal Reserve Bank of St. Louis
Current Ratio	The ratio of current assets (#ACT) to current liabilities (#LCT).	Compustat
Earnings Quality	The ratio of the standard deviation of net income before extraordinary item (#IB) to the standard deviation of operating cashflow (#OANCF) using up to five years of data before the loan issuance.	Compustat
Good CSR Information	An indicator variable equals one if the borrowing firm is covered by the MSCI KLD STATS and zero otherwise. It measures the extent to which information is available on a firm's corporate social responsibility (CSR) conduct.	MSCI-KLD
Green Bank	An indicator variable equals one if none of the lead lenders of the loan has any environmental-related negative incidents reported by RepRisk during the whole sample period and zero otherwise.	RepRisk
High Emission	An indicator variable equals one if the borrowing firm's carbon emission is in the top quartile of the sample and zero otherwise.	https://www.epa.gov/ghgreporting/data-sets
High Enforcement Intensity	An indicator variable equals one if the borrowing firm's headquarter is in a state where the number of environmental enforcement cases before the GHGRP, scaled by the total number of establishments in the state, is among the top 20 states and zero otherwise.	Good Jobs First and EPA's Enforcement and Compliance History Online (ECHO) database
High Financial Constraints	An indicator variable equals one if the borrowing firm's Hadlock-Pierce (HP) financial constraints index is above the sample median and zero otherwise. Following Hadlock and Pierce (2010), HP index is calculated as $-1.002 * Size + 0.043 * Size^2 - 0.040 * Age$, where *Size* is the log of Min(Total assets (#AT), \$4.5 billion), and *Age* is Min(years on CRSP, 37 years).	CRSP and Compustat
High Foreign Lender %	An indicator variable equals one if the percentage of foreign lead lenders among all lead lenders of a loan package is above the sample median and zero otherwise.	DealScan
Inst. Loan	An indicator variable equals one for loans with a type of term loan B, C, D, E, F, G or H (institutional term loans) and zero otherwise.	DealScan
Int. Spread	Interest spread, measured by All in Spread Drawn (AISD) which is the annual spread paid over LIBOR for each dollar drawn down from the loan. The commitment fee, annual fee, upfront fee, etc. are all included in the calculation of AISD.	DealScan
Large Analyst-forecast Error	An indicator variable equals one if the analyst forecast error is above the sample median and zero otherwise. Analyst forecast error is measured as the absolute value of the difference between actual earnings per share and the latest I/B/E/S median consensus analyst forecast (reported immediately before the earnings announcement date of the previous quarter), scaled by the stock price at the beginning of the quarter.	I/B/E/S
Large Emission Reduction	An indicator variable equals one if the treated firm's reduction in carbon emissions after the GHGRP, calculated as the difference between the mean annual carbon emissions in years 1–2 and the mean annual carbon emissions in years 3–5, is above the sample median and zero otherwise.	https://www.epa.gov/ghgreporting/data-sets
Leverage	The long-term debt (#DLTT) divided by total assets (#AT).	Compustat
Ln(Covenant Count)	The natural logarithm of the number of covenants contained in a loan contract.	DealScan
Ln(Int. Coverage)	The natural logarithm of the ratio of operating income (#OIBDP - #DP) to interest expenses (#XINT).	Compustat

Ln(Loan Amt.)	The natural logarithm of the amount of the loan in millions of dollars.	DealScan
Ln(Maturity)	The natural logarithm of loan maturity in months.	DealScan
Ln(Total Assets)	The natural logarithm of total assets (#AT) in millions of dollars.	Compustat
Market-to-Book	The ratio of the market value of equity plus the book value of debt (#PRCC × #CSHO + #LT) to total assets (#AT).	Compustat
Post	An indicator variable equals one if a loan is issued after January 2012 and zero otherwise.	DealScan
PPP	An indicator variable equals one if the loan agreement contains performance pricing provisions and zero otherwise.	DealScan
Related Lender	An indicator variable equals one if at least one of the lead lenders has led the borrower's prior loan(s) over the five years before the present loan and zero otherwise.	DealScan
Reputable Lead	An indicator variable equals one if at least one of the lead lenders of the loan is among the top 25 lead lenders in the U.S. syndicated loan market in a year and zero otherwise. The ranking of lead lenders is based on their previous year market shares, in terms of the total amount of loans that they issued as lead lenders. In calculating the market share, the loan amount is split equally among all the leads if a loan involves multiple leads (Ball et al., 2008).	DealScan
Revolver	An indicator variable equals one for revolving loans and zero otherwise. A revolving loan is a loan with a type of any of the following: "Revolver/Line < 1 Yr.," "Revolver/ Line >= 1 Yr.," "Revolver/Term Loan," "364-Day Facility," "Demand Loan," or "Limited Line."	DealScan
ROA	Return on assets, calculated as earnings before extraordinary items (#IB) divided by total assets (#AT).	Compustat
ROA Volatility	The standard deviation of the ratio of earnings before extraordinary items (#IB) to total assets (#AT) estimated using up to five years of data before the loan issuance.	Compustat
Tangibility	The ratio of net PPE plus inventory (#PPENT + #INVT) to total assets (#AT).	Compustat
Treated	An indicator variable equals one if the borrowing firm reports its emissions under the GHGRP and zero otherwise.	https://www.epa.gov/ghgreporting/data-sets
Unrated	An indicator variable equals one if the borrowing firm is note rated by S&P for its long-term credit ratings and zero otherwise.	Compustat
Weak Lending Relationship	An indicator variable equals one if the amount of loans borrowed by the borrowing firm from the lead bank scaled by the total amount of all loans borrowed by the borrowing firm in the past five years is below the sample median (Bharath et al., 2011) and zero otherwise.	DealScan

Appendix II: Justification for the Control Variables in the Baseline Specification

We control for a series of firm characteristics for the borrowing firm *j* in time *t*-1 and loan characteristics for the loan *i* issued to firm *j* in time *t*, following the prior literature (Bharath et al., 2011; Deng et al., 2014; Ge et al., 2012; Graham et al., 2008; Hollander & Verriest, 2016; Valta, 2012). The firm characteristics first include the size of the borrowing firm, measured with the natural logarithm of total assets ($Ln(Total\ Assets)$). Smaller firms are more informationally opaque, less capable of accessing external financing, and more vulnerable to distress. We expect smaller firms to incur higher interest spreads. We also control for the borrowing firm's default risk using the leverage ratio ($Leverage$), current ratio ($Current\ Ratio$), return on assets (ROA), earnings volatility ($ROA\ Volatility$), and the natural logarithm of interest coverage ratio ($Ln(Int. Coverage)$). We expect firms with higher $Leverage$ and $ROA\ Volatility$, and lower $Current\ Ratio$, ROA, and $Ln(Int. Coverage)$ to have larger default risk, and therefore incur higher interest spreads. The market-to-book ratio (*Market-to-Book*) captures the additional value over book assets that debt holders can access in the event of default. Firms with higher market-to-book ratios should enjoy a lower interest charge. Tangible assets can be sold more easily than intangible assets to recover the loan in the event of default. We expect firms with greater tangibility (*Tangibility*) to have more favorable interest rates. Moreover, we add three variables on the borrowing firm's information environment: $Earnings\ Quality$ captures the smoothness of reported earnings; $Unrated$ is an indicator variable equals to one if the borrowing firm is unrated by S&P long-term credit ratings, and zero otherwise; and $Good\ CSR\ Information$ is an indicator variable equals to one if the borrowing firm is covered by the MSCI KLD STATS, and zero otherwise. Lenders are likely to charge higher interest rates on firms with worse information environment (Bharath et al., 2008; Sufi, 2007). Therefore, we predict $Int. Spread$ to be negatively affected by $Earnings\ Quality$ and $Good\ CSR\ Information$ and positively affected by $Unrated$. We also control for the credit market condition ($Credit - market\ Condition$), measured with a principal component analysis combined metric based on three different macroeconomic factors: (1) the difference between the yields on Moody's BAA- and AAA-rated corporate bonds; (2) the difference between the yields on ten-year government securities and three-month Treasury Bill; and (3) yields on the three-month Treasury Bill. Unlike other controls on firm characteristics which are measured at time t-1, $Credit - market\ Condition$ is measured at time t. A higher value indicates a worse market condition at the time the loan is issued. The cost of borrowing should grow with higher values of $Credit - market\ Condition$.

The controls on loan characteristics first include the natural logarithm of loan maturity ($Ln(Maturity$). We expect loans with longer maturities to have higher interest rates. We also control for the natural logarithm of loan amount ($Ln(Loan\ Amt.$). Loans with a larger amount are likely to receive lower interest rates due to the economies-of-scale effect in lending (Berger & Udell, 1990). $Ln(Covenant\ Count)$ is the natural logarithm of the number of covenants in the loan. Covenants reduce the agency cost of debt and therefore $Ln(Covenant\ Count)$ should have a negative impact on interest rates (Reisel, 2014). PPP is an indicator variable equals to one if the loan agreement contains performance pricing provisions (PPPs), and zero otherwise. Under PPPs, interest rates are directly tied to a prespecified measure of the borrower's credit quality. We expect the presence of PPPs to reduce interest rates since PPPs mitigate agency problems in lending (Asquith et al., 2005; Panyagometh et al., 2013) and play a signaling role (Manso et al., 2010). Moreover, we control for whether the loan is an institutional loan ($Inst. Loan$) and whether the loan is a revolving loan ($Revolver$). Institutional loans are typically extended to riskier borrowers. Thus, we expect them to have higher interest spreads than bank loans. Andre et al. (2001) provide evidence that banks bear a lower risk by issuing lines of credit than term loans. We, therefore, expect *Revolver* to be inversely related to interest spreads. We further address the effect of the lead arranger's reputation by including an indicator variable, $Reputable\ Lead$, to capture whether the loan is arranged by one of the top 25 lead arrangers in the U.S. syndicated loan market, based on market share. Prior literature suggests that the reputation of the lead bank plays a certification role in the bank's screening and monitoring abilities, which brings down the adverse selection and moral hazard problems within the syndicate and in turn lowers the interest charge required by the participant lenders (Bushman & Wittenberg-Moerman, 2012; Chaudhry & Kleimeier, 2015; Do & Vu, 2010; Godlewski et al., 2012; Ross, 2010). $Related\ Lender$ indicates whether the lead arranger of the loan has led the borrower's prior loans within the previous five-year period. Repeated lending, on the one hand, attenuates the information asymmetry between borrowers and lenders (Bharath et al., 2007). On the other hand, it exacerbates the hold-up problem (Rajan, 1992; Sharpe, 1990). It is therefore uncertain what the net effect of prior lending relationships on interest rates would be.