Navigating Climate Change:

How Do Adaptation Initiatives Affect Bank versus Bond Financing?

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Abstract

Climate change adaptation initiatives are designed to increase climate resilience, which requires scaling up climate finance. It is therefore essential to consider the financial resources that underpin the process of navigating climate change. Employing the staggered introduction of U.S. state-level climate change adaptation plans (CLAPs) as quasi-exogenous shocks, we examine how climate change adaptation polices affect corporate debt choice, i.e., the mix of firm-level bank versus bond financing. We find that initiating such plans significantly increases the reliance of affected firms on bank debt relative to public debt. This effect is more pronounced among firms prone to suffer from climate risks, with more fragile fundamentals and higher information asymmetries. Furthermore, we find that the spreads of newly issued bank loans rather than corporate bonds decrease significantly after the finalization of CLAPs. Our overall findings indicate that relative to public debtholders who are lacking of capacities to acquire firm-specific information, banks are able to distinguish climate-resilient borrowers, which shed light on how debt instruments should be designed to better adapt to climate change.

1. Introduction

Climate change increasingly puts society at risk. Climate actions at the international, national, and local levels have been driven by efforts of climate change mitigation, i.e., reducing greenhouse gas (GHG) emissions, and adaptation, increasing climate resilience (Kane and Shogren, 2000; Duarte et al., 2013; Fankhauser, 2017; Howarth and Robinson, 2024), both of which require trillions of dollars annually and need scaling up climate finance (Bolton et al., 2023; De Haas, 2023). It is therefore essential to consider the financial resources that underpin the process of navigating climate change. Our work aims to fill a gap in the literature by focusing on how climate change adaptation polices affect corporate debt structure, i.e., the mix of firm-level bank versus bond financing.[1] Examining this issue can have important implications for how debt instruments could be designed to better support climate change adaptation and therefore tackle climate change challenges.

In particular, we focus on how U.S. state-level climate change adaptation plans (CLAPs) affect corporate debt structure. Finalizing such plans encourages actions to mitigate the negative impacts of climate change, enhances adaptation capacity in affected states, demonstrates the climate risks and opportunities firms face, and proposes future policies for implementation. We hypothesize that finalizing CLAPs affects corporate debt choice through various channels that yield different predictions.

On the one hand, following CLAP implementation, creditors become more aware of climate issues, and firms are increasingly willing to invest in solutions to address them. Banks' superior information and monitoring capabilities allow them to distinguish climate-resilient borrowers and offer flexible, tailor-made loans that public bondholders cannot

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[1] In this paper, referring to the mix of firm-level bank versus bond financing, which is determined by both the supply side (the creditors) and the demand side (the firm), we use debt structure and debt choice interchangeably.

provide, pushing firms toward private bank debt.[2] On the other hand, public debt may become more attractive because it dilutes banks' informational rents, supports long-term green investments that might be regarded too costly by banks, and benefits from the reduced regulatory uncertainty due to the implementation of CLAPs, leading firms to tilt toward public bonds.

Ultimately, whether and to what extent finalizing the CLAPs affects debt structure remains an empirical question. We employ a difference-in-differences (DID) regression design, centered on the staggered state-level finalization of the CLAP, to address this empirical question. Our main variable of interest, *CLAP*, is an indicator variable that equals one if the state of a firm's headquarter has finalized a CLAP by the end of the year, and zero otherwise. Building on prior studies on debt choice, we employ the ratio of total private bank loans to total debt as the primary measure of debt choice (Lin et al., 2013). Estimation results from difference-in-differences regressions suggest that the finalization of the CLAP leads to a significant increase in the weight of bank debt in the debt structure. The results are economically meaningful. The average firm in our sample experiences an increase in bank debt ratio corresponding to around 8.0% of the sample mean. To help validate the DID setting, we then conduct a dynamic timing test, the estimation results of which indicate that the increase in the bank debt ratio becomes statistically and economically significant only after the plans are finalized, but not before, suggesting that the parallel trend assumption is likely satisfied.

To address typical weighting and bias issues of DID estimators obtained through two-way fixed effects (TWFE) estimation, concerned by recent econometric studies (e.g., Baker, Larcker, and Wang, 2022), we employ a stack cohort approach with clean controls within

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[2] Climate-resilient borrowers refer to either (1) firms that are able to proactively cope with physical risk and transition risk ex-ante, or (2) firms that are vulnerable to climate change ex-ante, but whose capacity to cope with it improves following the implementation of government-led adaptation initiatives. Ex-ante, both types may not be well identified by creditors.

each event window, which allows firm and year fixed effects to vary across cohorts and is more conservative than including simple fixed effects (Gormley and Matsa, 2011, 2016). We also alleviate concerns regarding omitted variables associated with the nonlinear form of control variables by constructing a propensity score-matched sample based on observable firm characteristics from the years preceding policy changes. Our main findings remain valid for both the firm-year-cohort sample and the matched sample.

We further exploit three sources of cross-sectional variation in firm characteristics and estimate triple-difference regression models in the stack-cohort approach. First, firms with heightened regulatory risks also exhibit climate-related opportunities (Sautner et al., 2023a). firms with a safer ex-ante climate risk environment should exhibit weaker responses to finalizing CLAP because risks and opportunities due to climate regulations matter less for them. Second, because of the bank's unique ability to write tailor-made contracts and renegotiate with firms (Aghion and Bolton, 1992; Diamond, 1984), for firms potentially faced with more required adjustments to the CLAPs, having a bank loan is more beneficial than a bond. Third, banks' information advantages in identifying climate-resilient borrowers relative to public bondholders tend to be larger when a firm is suffer from higher information asymmetry when firm-specific information is more unobservable to outsiders. Indeed, our main results are weaker among firms with a safer climate risk profile and higher among those with more fragile fundamentals and those exhibiting higher levels of information asymmetry.

Furthermore, to shed light on potential channels, we conduct several additional analyses. On average, CLAPs can either increase climate-related regulatory risk and increase creditors' perceived risk (the regulation channel) or provide new opportunities and mitigate climate risk uncertainties for firms and reduce creditors' perceived risk (the opportunity channel). We find that CLAP initiation is associated with lower carbon emissions and higher capital expenditures, suggesting that CLAPs help mitigate climate risk uncertainties and helps firms

proactively adapt to climate change. Additionally, we observe that firms that are able to obtain new bank loans after the implementation of CLAPs have lower bank loan spreads, suggesting that the opportunity channel dominates the regulation channel in our setting. In addition, CLAP initiation does not significantly affect public loan spread or public loan maturity.

Our overall findings imply that CLAP implementation indeed facilitates (at least some) firms to better adapt to climate changes and potentially ease creditors' risk concerns. However, compared with public debtholders who are lacking of capacities to acquire firm-specific information, banks are able to gather information on firms' actual ability and efforts to tackle climate challenges and potential opportunities during green transition. In equilibrium, climate-resilient borrowers are distinguished from the pool of average borrowers and obtain more favorable credit terms from banks.[3] By comparison, those results are inconsistent with an alternative explanation arguing that following CLAP implementation, public creditors become more hesitate to provide external financing, making firms merely *passively* rely more on bank debt. Collectively, banks play an important role as promoters in the green transition by *actively* identifying and providing credit to climate-resilient borrowers in the context of government-initiated climate adaptation actions.

Our paper mainly contributes to two strands of literature. First, our paper extends the literature on how climate issues affect corporate debt. Although several prior studies examine how physical and transitional climate risks affect the cost of debt and capital structure (e.g., Huang et al., 2018; Nguyen and Phan, 2020; Javadi and Masum, 2021; Ehlers, Packer, and De Greiff, 2022; Nguyen et al., 2022; Seltzer et al., 2022), evidence on how climate issues

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[3] It is probable that CLAP implementation makes firms worse off on average, resulting in shrinkage in bank loans. Bank debt ratio can increase when the amount of public debt shrinks even more. If this is the case, we may observe lower loan spreads for existing borrowers due to "survivorship bias", as borrowers that are not climate-resilient are filtered out from the bank loan market (but can still operate). Estimation results in Table B6 of the Appendix B show that the ratio of bank loans to total asset increases on average, which is inconsistent with this view.

affect the sources of debt financing, i.e., the mix of market versus nonmarket debt, is relatively rare. Kovacs et al. (2025) show that the implementation of CLAPs increases firms' financial leverage. As debt has different priorities in terms of cash flow claims, information sensitivity, and managers' incentives, assessing firms' debt elements and overall leverage ratios is of paramount importance (Rauh and Sufi, 2010; Colla et al., 2013; Asimakopoulos, Asimakopoulos, and Li, 2023). Therefore, we fill an important gap in the literature by demonstrating that climate regulation matters not only for the level of debt but also for the composition of private and public debt within the total debt.

Notably, state-led adaptation plans increase the likelihood of new climate-related regulations while provide new opportunities for firms. Sautner et al. (2023b) find that CLAPs make the prospects of high-exposure stocks riskier, suggesting that in terms of equity investors, the regulation channel dominates the opportunity channel. However, our result suggests that with regard to creditors' perceived risk, the opportunity channel can dominate the regulation channel, suggesting that as banks are specialized in acquiring private information, they are able to distinguish climate-resilient firms and manage climate change challenges better than equity investors. Such findings highlight the importance of assessing climate regulations in view of creditors.

Second, our paper contributes to the line of research on the determinants of debt choice, which has explored various firm-level, industry-level, and macro-level factors (e.g., Denis and Mihov, 2003; Bharath, Sunder, and Sunder, 2008; Dhaliwal, Khurana, and Pereira, 2011; Lin, 2016; Boubaker, Saffar, and Sassi, 2018; Boubakri and Saffar, 2019; Li, Lin, and Zhan, 2019). In particular, our paper contributes to the literature on how regulatory changes affect debt choice (e.g., Ben-Nasr, Boubaker, and Sassi, 2021; Li, Ng, and Saffar, 2024). To the best of our knowledge, we are the first to explore how climate-related regulatory changes affects bank versus bond financing, which have important implications on how creditors could play a

role in tackling climate change challenges.

Several recent papers also consider corporate debt structure in the context of climate change and low-carbon transition, but with different angles. Beyene et al. (2024) document that fossil fuel firms with more stranded asset risk rely less on bond finance and more on bank credit. Ivanov, Kruttli, and Watugala (2024) show that high emission firms affected by climate regulation face shorter loan maturities and lower access to permanent forms of bank financing. Unlike their work, we examine how state-level climate regulation impacts the debt structure of the universe of U.S.-listed firms and identify channels that are not limited to firms faced with high transition risk. Goodell et al. (2025) examine the effect of firm-level climate change exposure on debt maturity structure. By comparison, we consider state-level government plans aiming to improve adaptation capacity to climate change, rather than firm-level climate change exposure, and focus on bank versus bond financing.

The remainder of the paper is organized as follows. Section 2 provides the institutional background and outlines the hypotheses. Section 3 describes data and empirical design. Section 4 reports the main empirical results. Section 5 conducts further analysis. Section 6 concludes.

2. Institutional Background and Hypothesis Development

2.1 Institutional background

Climate change and uncertainty about its potential consequences have become a central concern for economists, investors, and policymakers alike (Barnett, 2023). The challenges in managing climate change-related risks are largely due to the uncertainty and endogeneity of future policy shocks that ultimately shape the transition to a low-carbon economy. Many market participants find climate risks difficult to price and hedge, possibly because of their

systematic nature, a lack of disclosure by portfolio firms, and challenges in finding suitable hedging instruments (Krueger, Sautner, and Starks, 2020).

Adaptation, the process of adjustment to the effects of actual or expected climate change to mitigate harm or capitalize on beneficial opportunities, plays a key role in reducing climate-related risks (IPCC, 2022). While GHG mitigation is widely recognized for addressing the root causes of climate change and providing a global public good, adaptation is intended to tackle local risks of climate change, which are often geographically dependent and yield local benefits (such as avoided damage).

CLAPs enable states to proactively adapt to and prepare for the impacts of climate change. These state-led initiatives include actions aiming at mitigating the adverse effects of climate change, as well as capitalizing on emerging growth opportunities. The actions detailed in CLAPs offer quantifiable benefits to all residents of the states, with benefits extending to both the near and distant future. These actions involve building resilient infrastructure and implementing protective measures against adverse climate events, such as increasingly severe floods and hurricanes (Kovacs et al., 2025). Despite differences in scope, goals, and strategies, all these plans ultimately aim to protect the environment and more effectively manage climate-related adverse events. For example, one of the adaptation strategies in California involves identifying significant and sustainable funding sources to invest in measures that mitigate climate risks, reduce human loss, and minimize disaster-related expenditures. The timetable for finalizing state-led climate change adaptation plans is provided in Table 1.

[Insert Table 1 about here.]

2.2 Hypothesis development

State-led adaption plans increase the likelihood of new climate regulations and

standards, which can lead to a higher cost of compliance and greater risk of litigation (Kovacs et al., 2025), making the prospects of high-exposure firms riskier. However, due to the rising demand for "green" products and services, they also provide opportunities for some firms to develop firm-specific advantages and enhance competitiveness (Sautner et al., 2023a, b; Lu et al., 2024). That is, CLAPs may amplify climate regulatory risk and heighten creditors' perceived risk (the regulation channel), or they may help mitigate climate risk uncertainties, open up green opportunities and lower creditors' perceived risk (the opportunity channel). We hypothesize that CLAP implementation can enhance creditors' awareness towards climate issues, and the effect of CLAPs on firm debt structure is ex-ante ambiguous.

On the one hand, we argue that firms may shift more weight of debt financing from public debt to private bank loans. First, banks know more about a company's prospects than other investors do (James, 1987). As inside debt, bank loans provide creditors with access to private information that is not otherwise publicly available. By comparison, as outside debt, public bonds are held by a diffuse ownership that relies primarily on publicly available information after issuance (Fama, 1985; Ma, Stice, and Williams, 2019). By investing resources in screening and monitoring, banks gain an informational advantage compared to other capital providers, such as public bondholders (Diamond, 1984, 1991), and often have extended relationships with borrowers, which can confer an advantage in acquiring soft information (Petersen and Rajan, 1994; Degryse and Ongena, 2005).

The implementation of CLAPs clearly demonstrates that the state government is committed to taking legislative and regulatory measures to reduce emissions and combat climate change, which can not only raise public awareness but also enhance the capacity to adapt to climate change in the long run. Following CLAP implementation, firms are spurred to invest proactively in mitigation and adaptation strategies. As indicated above, this commitment could be bolstered by banks, which have better capabilities than the public for

gathering information on firms' actual ability and efforts to tackle climate challenges, ensuring that corporate actions align more closely with green transition objectives (Houston and Shan, 2022). Based on soft information that other investors do not possess, banks may be able to identify and provide credit to potential borrowers who can still operate stably despite the impact of climate policies (Luneva and Sarkisyan, 2024). Specifically, they might be capable of distinguishing between two types of borrowers: those for whom climate regulations imply financial risk, and those for whom climate regulations do not imply financial risk, or even create growth opportunities, and tilt credit resources towards the latter. The need for tailored financing solutions for diverse and evolving climate projects makes private bank loans more attractive than the standardized terms of public debt.

Second, adaptation plans can raise concerns regarding future policy changes that may transit from soft initiatives to stricter regulatory enforcement (Degryse et al., 2023). Banks are long-term players in the debt market and therefore have a desire to develop a reputation for financial flexibility (Chemmanur and Fulghieri, 1994), allowing firms to secure financing that aligns closely with their unique climate initiatives and timelines. For firms faced with increased regulatory risk, bank loans could be a more internal and safer financing option than bond issuance that be negotiated more quickly (James, 1987; Asimakopoulos, Asimakopoulos, and Li, 2023), which is beneficial for timely and dynamic climate action required under CLAPs, in line with firms' tendency to finance their investments from markets with lower transaction costs (Blackwell and Kidwell, 1988).

Based on the arguments above, we propose the following hypothesis:

H1a: The implementation of CLAPs induces firms to shift from public debt to private debt.

On the other hand, firms may shift more weight of debt financing from private bank loans to public debt for several reasons. To begin with, while bank monitoring and control

can improve investment decisions, a single bank lender may obtain an information monopoly that can facilitate rent extraction and distort managerial incentives. As the prospect of low-carbon transmission requires long-term investments, borrowing from public markets limits bank bargaining power and can improve investment efficiency (Rajan, 1992; Houston and James, 1996).

Additionally, banks may be conservative and hesitant to fund newer and cleaner technologies. If technological innovation is a crucial mechanism for containing environmental pollution, this implies that market-based financing may be relatively effective compared to bank-based financing in facilitating green transitions (De Haas and Popov, 2023; Beyene et al., 2024). Therefore, in the presence of CLAPs, managers may seek to avoid banks' constraints by relying more on public debt.

What's more, as all the CLAPs across different states share the common goal of protecting the environment and implementing strategies to better handle climate-related adverse events (Kovacs et al., 2025), they may help resolve uncertainty about future climate regulation. This, in turn, may enable firms to obtain external financing from public creditors by reducing the perceived risk for outsiders.

Based on the arguments above, we propose the following alternative hypothesis.

H1b: The implementation of CLAPs induces firms to shift from private debt to public debt.

3. Data and Empirical Design

3.1. Data

We collect data on debt structure from the Capital IQ database, which categorizes debt into commercial papers, revolving credits, term loans, senior bonds and notes, subordinated

bonds and notes, capital leases, and other types of debt. Our sample starts in 2001, when data from Capital IQ first became available and ends in 2018. Our financial data come from Compustat. We exclude financial firms (SIC codes 6000–6999) and regulated utility firms (SIC codes 4900–4999) because government regulations may have a greater influence on their capital structures than information asymmetry and debt renegotiation needs. Our final sample comprises 4,270 firms from 2001 to 2018, resulting in 28,596 firm-year observations.[4]

3.2. Empirical design

We estimate the following difference-in-differences regression model to estimate the impact of the state-level rejections of the IDD on debt choice:

$$BANK\_DEBT_{i,s,t} = \beta_0 + \beta_1 CLAP_{s,t} + \gamma Control_{i,t} + f_i + \tau_t + \varepsilon_{i,s,t} \tag{1}$$

where i indexes the firm, s indexes the state of the headquarter, and t indexes the year. The main dependent variable, *BANK_DEBT*, is the ratio of private bank debt to total debt by the end of the fiscal year. Bank debt includes term loans and revolving credit. $CLAP_{s,t}$ is an indicator variable that equals one if a firm is headquartered in a state that has finalized climate change adaptation plans by the end of year t, and zero otherwise.

We also include a vector of control variables commonly used in the empirical debt choice literature (e.g., Boubaker, Saffar, and Sassi, 2018). These variables include firm size (*lnAssets*), leverage (*Leverage*), asset tangibility (*Tangibility*), cash flow volatility (*CFOVOL*), Tobin's Q (*TobinQ*), and return on assets (*ROA*). We provide a detailed description of all the main variables in Appendix A. In addition, we include firm (f_i) and year (τ_t) fixed effects to control for time-invariant characteristics across firms and time-variant

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[4] Our findings remain robust if extending the sample to 2023. However, due to the change in accounting rules for operating leases in 2019, unadjusted debt ratios for data after 2018 will conflate debt and lease activity for those years (Hanlon and Heitzman, 2022). Therefore, to insure the comparability of debt structure data, we end our main sample to 2018.

characteristics across years. Robust standard errors are clustered at the state-of-headquarter level to correct for potential covariance among firm outcomes within the same state of headquarter (Bertrand et al., 2004).

We report the summary statistics of the variables in Table 2. All continuous variables are winsorized at the 1st and 99th percentiles to mitigate the effect of outliers. The sample mean and median of *BANK_DEBT* are 0.472 and 0.432, respectively. This statistic is similar to those documented in prior studies. Throughout our sample period, 18.8% of firms were affected by the finalization of the CLAP.

[Insert Table 2 *about here.]*

4. Main Empirical Results

4.1. Baseline results

Table 3 presents the estimation results from the difference-in-differences regressions assessing the impact of the state-level finalization of the CLAPs on debt choice. In Column (1), we deliberately omit all the control variables other than the firm- and year-fixed effects to establish a baseline effect. Throughout the paper, we always use firm-fixed effects to control for time-invariant unobserved heterogeneity within firms and year-fixed effects to control for time-varying heterogeneity across time. The coefficient on *CLAP* is positive and statistically significant at the 1% level of significance. With the inclusion of firm-fixed effects, we interpret the results as treatment firms, compared to control firms, increased their reliance on private bank debt in the overall debt structure after the state finalizes a CLAP.

[Insert Table 3 *about here.]*

Column (2) includes a set of firm characteristics commonly used in the debt structure literature. We continue to find that the coefficient estimates on *CLAP* are positive and

statistically significant at the 1% threshold level. This effect is also economically meaningful. For instance, relative to the sample mean of bank debt ratio of 47.6%, the coefficient estimate on *CLAP* in Column (2) translates into an 8.0% (=0.038/0.476) increase in private bank debt ratio for an average sample firm. We use the model in Column (2) with the full set of control variables as our main specification throughout the rest of the paper.

4.2. Dynamic timing effects

This section conducts a dynamic timing test to i) study the timing of bank debt ratio changes relative to the timing of the finalization of the CLAP and ii) test the parallel trends assumption to further alleviate potential endogeneity concerns related to reverse causality and provide the support that our identification strategy satisfies the parallel trends assumption. If reverse causality is an issue or pre-treatment trends exist, then there would be a trend of increasing bank debt ratio among treatment firms before the finalization of the CLAP.

Empirically, we follow Bertrand and Mullainathan (2003) and replace the main explanatory variable *CLAP* with five indicator variables: *CLAP(–2)*, *CLAP(–1)*, *CLAP(0)*, *CLAP(+1)*, and *CLAP(2+)*. These variables indicate the years relative to the finalization of the CLAP. Specifically, *CLAP(–2), CLAP(–1), and CLAP(0)* are indicator variables that equal one if the state finalizes the CLAP in two years, one year, and at the end of the current year, respectively. *CLAP(+1)* is an indicator variable that equals one if the state finalizes the CLAP the year before, and zero otherwise. *CLAP(2+)* is an indicator variable that equals one if it has been two or more years since the state finalized the CLAP, and zero otherwise.

[Insert Table 4 *about here.]*

Table 4 replicates the tests in Table 3, except that the main explanatory variable is changed from *CLAP* to the five indicator variables indicated above. The coefficient estimates

on *CLAP(–2)* and *CLAP(–1)* are insignificantly different from zero in both columns, implying that there was no trend of increasing private bank loans in the debt structure before the finalization of CLAP. In comparison, the coefficient estimates on *CLAP(0)*, *CLAP(+1),* and *CLAP(2+)* are positive and statistically significant at the 5% threshold level. In addition, the coefficient estimates gradually increase, suggesting that firms take time to adjust their debt structure in response to CLAP; such effects persist in the long run. Overall, these results indicate that finalizing CLAP results in a significant increase in the use of bank debt within the debt structure. This relationship does not appear to suffer from endogeneity, and there are no pre-treatment trends; thus, the parallel trends assumption is satisfied.

4.3. Determinants of the finalization of the CLAP

Our identification strategy relies on staggered finalization of the CLAP to provide plausibly exogenous shocks to creditors' perceived climate risks and future operating risks. For this identification strategy to be valid, one must first show that the state-level finalization of the CLAP is i) a decision that is exogenous to the outcome variable (debt choice); ii) not related to state-level macroeconomic conditions, which could indirectly affect the outcome variable (debt choice). To verify this assumption, we follow Acharya, Baghai, and Subramanian (2014) and estimate a Cox proportional hazard model to predict the state-level finalization of CLAP. In this test, a "failure event" is the finalization of the CLAP in a state. Once a state finalizes the CLAP, it drops from the sample. State-level variables are measured in year $t–1$ to predict the finalization of CLAP in year t.

In Table B1 of Appendix B, in Column (1), the coefficient estimates on the state-level average *BANK_DEBT* are insignificantly different from zero, suggesting that the state-level debt structure does not predict the finalization of CLAP. Next, we include a vector of state-level macroeconomic and political factors to examine whether these variables predict the

finalization of CLAP. These factors include the state GDP growth rate, the natural logarithm of state GDP, the state-level unemployment rate, the natural logarithm of the total population in that state, the state union membership rate, and the political balance in that state (the fraction of a state's congress members in the U.S. House of Representatives that belong to the Democratic Party in a given year). The estimation results from Column (2) suggest that none of these macroeconomic or political variables predict the finalization at the conventional statistical level, except for the unemployment rate, which shows marginal significance, and political balance. The coefficient on the lagged Demo_Share is significantly positive at the 1% level, consistent with the notion that Democrats are more likely to support environment-related policies, such as the CLAP. Overall, the evidence suggests that the finalization of CLAP is generally exogenous to (i) the state-level value of the outcome variable (debt structure) and (ii) local macroeconomic factors, indicating that these finalizations are unanticipated and create meaningful exogenous variation in the corporate climate risk environment.

4.4. Stack-cohort approach

As we discussed in the previous subsection, recent econometric studies point out that the TWFE estimator from staggered DiD is not easily interpretable because it is a weighted average of all possible two-group/two-period DiD estimators (Goodman-Bacon, 2021), which may be biased or have a different sign due to the use of already-treated units as controls (Baker, Larcker, and Wang, 2022). To mitigate this bias, several studies propose event-based staggered DiD that uses alternative groups to serve as the controls. In particular, the stack-cohort approach can help circumvent this issue. The idea is to create event-specific "clean 2 × 2" datasets, including the outcome variable and controls for the treated cohort and all other observations that are "clean" controls within the treatment window (Baker, Larcker, and

Wang, 2022).

We follow Gormley and Matsa (2011) and create a cohort for each legislation event. Specifically, we treat each CLAP finalization event as a cohort and pool the data of treatment and control firms across cohorts. For each cohort, we retain observations within a window of ±5 years and require the control firms not to be treated in the following 5 years. In other words, we turn the original firm-year level sample into a firm-year-cohort level sample. As this sample has three dimensions (firm-year-cohort), this approach also allows the firm- and year-fixed effects to vary by cohort, which is more conservative than including the simple fixed effects (Gormley and Matsa, 2011).

[Insert Table 5 *about here.]*

The estimation results are presented in Table 5. We continue to find a positive and significant effect of CLAP on bank loan reliance. Column (1) replicates the baseline regression. Consistent with our main results, the coefficient on *CLAP* remains positive and significant. The economic magnitude (0.031) is also similar to those documented in the baseline model (0.038). Column (2) presents the estimation from dynamic timing tests. We observe that the coefficients on *CLAP(–2) and CLAP(–1)* are insignificant, and the coefficients on *CLAP(0)*, *CLAP(1)*, and *CLAP(2)* are positive and statistically significant.

4.5. Bacon decomposition

Goodman-Bacon (2021) argues that a causal interpretation of two-way fixed effects DiD estimates requires both a parallel trends assumption and treatment. When the possibility of time-varying treatment effect cannot be ruled out, the DiD estimator can be significantly biased by the problematic comparison between later-treated groups and earlier-treated groups. Following Goodman-Bacon (2021), we first construct a balanced panel by dropping

all firms with missing observations in any period between 2001 and 2018. Next, we derive the DiD estimator by regressing *Bank_Debt* on *CLAP* and firm- and year-fixed effects. Then we decompose the estimator into all possible two-group/two-period DiD estimators in the data. In Table B2 of Appendix B, the treatment effects are estimated to be 0.066 using the full sample, with most of these estimates derived from the comparison between treated firms and never-treated firms (85.8% of the full sample). The remaining cases only account for 14.2% of the full sample, and the estimates derived from them are both positive. These results suggest that the problem of time-varying treatment effects does not significantly bias our staggered DiD estimator.

5. Additional Analyses

5.1. Cross-sectional analysis

In this section, we exploit cross-sectional variation in the impact of the finalization of CLAP on debt choice to i) shed light on the underlying channels and ii) further alleviate concerns regarding omitted variables.[5] Ideally, we aim to explore the cross-sectional variations based on firm characteristics in the year preceding the finalization of CLAP (denoted as year t–1). However, in the staggered difference-in-differences setting, as there are multiple dates on which CLAP is finalized, no unique t–1 period exists for each firm in the panel. Following Gormley and Matsa (2011, 2016), we adopt the stack-cohort approach and estimate triple difference regressions to study the heterogeneity in firms' responses to the finalization of CLAP. We first identify each CLAP finalization event as a unique cohort and keep observations of treatment and control firms for ten years around each cohort. Then, we

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[5] For any omitted variables to explain our results, they have to be uncorrelated with the control variables and be able to explain the cross-sectional pattern.

assign the level of cross-sectional variables in period t–1 to the ten-year observations.[6] Next,

we stack the data from different cohorts to build the triple difference sample. This approach

enables us to use every untreated observation at a particular point in time as a control for

treated observations in that period. We estimate the average treatment effect across the

staggered finalization in this sample. The model is specified as follows:

$$BANK\_DEBT_{i,c,s,t} = \beta_0 + \beta_1 CLAP_{s,t} + \gamma_1 CLAP_{s,t} \times X_{i,c}$$

$$+\gamma_2 X_{i,c} + \omega_c \times v_i + \omega_c \times \tau_t + \sigma_s \times \tau_t + \varepsilon_{i,c,s,t} \qquad (2)$$

where c indexes cohort, and X_{ic} is firm characteristics used to identify cross-sectional

differences for each firm in each cohort. We use firm characteristics from the year prior to the

CLAP finalization (denoted as year t–1). We include cohort-times-firm fixed effects ($\omega_c \times$

v_i), cohort-times-year fixed effects ($\omega_c \times \tau_t$), and state-of-headquarter-times-year-fixed

effects ($\sigma_s \times \tau_t$) in the model to control for all confounding factors that vary at different

levels. In this stacked cohort-triple difference setting, $CLAP_{s,t}$ and $X_{i,c}$ in the above equation

are absorbed by the cohort-times-firm fixed effects ($\omega_c \times v_i$).[7]

We first examine how a firm's ex-ante climate risk exposure affects the impact of CLAP

finalization on debt choice. Sautner et al. (2023a) indicate that firms with heightened

regulatory risks also exhibit climate-related opportunities. We therefore predict that the

finalization of CLAP has a weaker impact on the debt choice of firms with a safer climate

risk environment, of which the marginal effect of climate regulations matter less. We employ

two commonly used proxies for the climate risk environment. The first proxy is a text-based

climate risk exposure index, *CREXPO*, developed by Sautner et al. (2023a), which positively

correlates with climate risk exposures. The second proxy is the number of strengths regarding

the environment, *ENVSTR*, derived from CSR scores in the KLD database. Intuitively, a

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[6] Firms with a missing t–1 observation are dropped in this process.
[7] We are using ex-ante firm characteristics to reduce the bias in the estimation. These cross-sectional variables are, therefore, the same within each cohort. Thus, they are absorbed by the cohort fixed effects.

better environment-related CSR profile can protect the firm from downside climate risks; thus, this measure negatively correlates with climate risk exposures.

Table 6 presents the estimation results from the triple differences tests. In column (1), the interaction term between *CLAP* and *CREXPO* is significantly positive at the 5% level, indicating that firms with higher ex-ante climate risk exposures allocate a greater percentage of their debt financing to private bank loans. The estimation results in column (2) show that the coefficient estimates for *CLAP* and *ENVSTR* are negative and significant at the 1% threshold level, indicating that firms with a better environmental profile are less likely to turn to private bank loans.

[Insert Table 6 *about here.]*

Our second source of cross-sectional variation exploits the ex-ante likelihood of default. CLAPs may help firms proactively adapt to climate change and seize opportunities, and the marginal effect tends to be stronger for firms with weaker fundamentals. Alternatively, the finalization of CLAP can lead to a risk of future adjustments to policy requirements. We conjecture that this induces higher debt renegotiation needs for firms with higher ex-ante default risks. That is, no matter whether the regulation channel or the opportunity channel dominates, we hypothesize that distress firms are expected to shift a larger percentage of debt to private bank loans in the debt structure after the finalization of CLAP. We use two proxies for the ex-ante likelihood of default. The first one is the expected probability to default (*EDF*) estimated from the option pricing model developed by Merton (1974). The second proxy is firm size (*SIZE*), as it negatively correlates with financial constraints and default risk.

Columns (3) and (4) show that the interaction term between *CLAP* and *EDF* is positive and statistically significant at the 5% level and the interaction term between *CLAP* and *SIZE* is significantly negative at the 1% level, indicating that firms with larger expected default risk

and higher financial constraints tend to react more to the finalization of *CLAP* through relying more on bank loans.

The last source of cross-sectional variation comes from firm-level information asymmetry. One underlying channel is that the public debtholder becomes more demanding of climate risk-related information after accessing public information, as indicated by the CLAP. Therefore, firms face higher information disclosure costs when issuing public debts. Consequently, firms may shift to bank loans for lower information disclosure costs. Private lenders are better equipped to monitor firms and resolve information asymmetry than public bondholders. Therefore, the ability and need to process private information in a time-sensitive manner are expected to determine the use of information-sensitive instruments, such as bank debts (Li, Lin, and Zhan, 2019). We hypothesize that, *ceteris paribus*, firms with higher ex-ante information asymmetry are more likely to shift towards private bank debts because public debts become more expensive in equilibrium.

We focus on the presence of information intermediaries, such as financial analysts (*COVERAGE*) and institutional investors (*INSTOWN*), because firms with more analyst coverage and institutional investors are expected to have less information asymmetry, and thus the need for private debt monitoring is reduced. *COVERAGE* is measured as the natural logarithm of one plus the number of analysts who issued earnings forecasts for the firm in the fiscal year. *INSTOWN* is the fraction of shares owned by institutional investors during the fiscal year. Columns (5) and (6) present estimation results of triple difference regressions with regard to information asymmetry measures. The coefficient estimates on the interaction term between *CLAP* and *COVERAGE* and between *CLAP* and *INSTOWN* are both negative and statistically significant, suggesting that the documented change in debt structure is less pronounced for firms covered by more financial analysts or with higher institutional ownership. This result is consistent with our prediction that finalizing CLAP requires higher

information disclosure costs, which makes firms reluctant to issue bonds since public debts become more expensive.

5.2. The real effects of CLAPs

Firms with heightened regulatory risks also exhibit climate-related opportunities (Sautner et al., 2023a). On average, CLAPs can either increase climate-related regulatory risk and increase creditors' perceived risk (the regulation channel) or provide new opportunities for firms and reduce creditors' perceived risk (the opportunity channel). To shed light on which channel dominates in our setting, we further explore the real effects of CLAPs from various aspects, which are illustrated as below.

5.2.1. The effects on carbon emissions

We begin by examining whether CLAPs lead to real firm-level actions on mitigating the adverse effects of climate change. Specifically, we investigate the real effects of CLAPs on carbon emissions. The data on carbon emissions is obtained from the Trucost database. In Table 7, we find that both the GHG scope 1 carbon emission amount and intensity decrease, confirming the actual effects on carbon emission reduction.[8] Such evidence is in support of the argument that following CLAP implementation, firms become more aware of climate scrutiny and involve more proactively in mitigation and adaptation strategies.

[Insert Table 7 *about here.]*

5.2.2. The effects on corporate investments

We then investigate the real effects of CLAP on corporate investments. Climate opportunities are risky, with plenty of uncertainty surrounding investments in green

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[8] Such finding is consistent with cross-country evidence on national climate legislation reduce greenhouse gas emissions (Eskander and Fankhauser, 2020).

technologies. CLAPs can either increase uncertainties regarding future changes in government policies can deter investments or enable firms to better cope with climate changes and more eagerly engage in investments. To see which effect dominates, we focus on the capital expenditure normalized by lagged asset (*CAPX*) and capital expenditure plus R&D expenditure normalized by lagged asset (*CAPX_RD*). In Table 8, the results indicate that CLAP significantly increases corporate investments. This supports our conjecture that adaptation initiatives can help mitigate uncertainty and enable firms to capitalize on beneficial opportunities. Such evidence complements to Li et al. (2024) who find that firms that proactively respond to climate risk tend to increase their investment subsequently.[9]

[Insert Table 8 *about here.]*

5.2.3. The effects on the supply side of the credit market

After confirming that CLAPs tend to have positive effects on helping firms mitigate and better adapt to climate changes, we then move to investigate their effects on bank loan spreads and corporate bond spreads, respectively. It is ex ante unknown how bank loan spreads and corporate bond spreads will be affected by CLAP. On one hand, the spreads can increase due to increased perceived risk by creditors. On the other hand, the spreads can decrease because carbon risks will be diminished in the long term and uncertainty related to climate risks will be resolved to some extent. The results in Table 9 show that the bank loan spreads (All in drawn spreads from Dealscan) significantly decrease after the finalization of CLAP. On the contrary, as shown in Table 10, newly issued corporate bond spreads remain largely unaffected. Such findings indicate that due to the superior ability to acquire firm-specific information regarding climate resilience, banks can actively allocate creditor resources to firms that are able to capitalize growth opportunities following CLAP

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[9] Such evidence is also consistent with Xu et al. (2025), who find that urban climate adaptation in China has significantly enhanced corporate investments by mitigating the uncertainty associated with future climate regulations.

implementation.

[Insert Table 9 *about here.]*

[Insert Table 10 *about here.]*

5.3. Robustness checks

This section conducts several tests to gauge the robustness of our results, including alternative measures of debt structure, alternative model specifications, and subsamples.

5.3.1. Propensity score matching

In this section, we estimate the effect of the finalization of CLAP on debt structure using the propensity score matching technique to control for the concern that treated firms are fundamentally different from control firms across observable characteristics. In other words, this approach addresses the issue that our linear control variables may fail to control for differences between treated and control firms. It also controls for the bias due to omitted variables associated with the non-linear form of our control variables that could explain our results. Empirically, we create two matched samples by matching treatment to control firms using a propensity score methodology. We begin by retaining all observations for the treatment and control firms in the year preceding the finalization of CLAP and treat each finalization as a separate cohort. We then estimate a logistic regression to calculate the probability of being a treatment firm. The dependent variable is, therefore, an indicator variable that equals one if the firm is in the treatment group, and zero otherwise. In this model, we include the same set of control variables as in the baseline model, as well as SIC 2-digit industry and cohort fixed effects to control for time-invariant differences across industries and time-variant differences across cohorts. We match each treated firm in year $t-1$ to the control firm (without replacement) with the closest propensity score within a 0.1% caliper. Our final propensity score-matched samples have 753 unique matched pairs. Using

these matched pairs within each cohort, we construct a stack-cohort sample, as described in Section 3.5.

We verify the matching procedure in Table B3 of Appendix B. First, in Panel A, we present the estimation results from the logistic regression used to estimate the probability of being a treatment firm in both the pre-match and post-match samples. Some of the coefficient estimates in Column (1) are significant at a conventional level, suggesting that certain firm characteristics do differ between treatment and control firms in the main sample. When we estimate the same regression in the post-match sample in Column (2), none of the coefficient estimates is significant at a conventional level and the pseudo-R-square shrinks from 0.114 to 0.013, suggesting that the differences across these variables are no longer significant between the treatment and control groups and the matching procedure is successful.

Panel B reports the sample means of the control variables for the treatment and control firms in the pre- and post-matched sample. In the pre-matched sample, the *T*-test results suggest that there are significant differences in the mean of most covariates between the treatment and control groups. However, the differences disappear for the post-matched samples, providing further assurance that the matching procedure is successful.

In Table B4 of Appendix B, after confirming the validity of the matching procedure, we re-estimate the baseline and the dynamic difference-in-differences regressions in the matched stack-cohort sample. Overall, in the propensity score-matched samples, we continue to find that finalizing CLAP is associated with a higher proportion of private bank loans in the debt structure. The effect emerges only after the law change occurs, again consistent with the findings in sections 3.2 and 3.3. In summary, the estimation results from the propensity score-matched samples suggest that our identified results are not driven by omitted variables associated with the non-linear form of our control variables.

5.3.2. The effects of the number of goals

It is reasonable to predict that what matters for firm decisions is not simply the timing of CLAPs adoption, but also the scope, depth, and coverage of those plans (and, consequently, resources allocated to implement them). For example, plans and strategies adopted in California may differ significantly from those adopted in Alaska. In Table B5 of Appendix B, we consider the above arguments by replacing the CLAP dummy with continuous variables that indicate the number of goals specified in the adaptation plans. Specifically, we use the natural logarithm form of four sets of goals: all kinds of goals, goals related to planning, goals related to laws, and goals related to monitoring. We find that all the coefficients are statistically positive, indicating that more specific goals are associated with more reliance on bank debts.

5.3.3. The switch from public debt to private debt

In Table B6 of Appendix B, we examine the impact of finalizing CLAP on the use of public versus private debt in the capital structure. We employ the ratio of public debt to total assets (*PUB_AT*) and the ratio of bank debt to total assets (*BANK_AT*) as dependent variables. Consistent with the baseline findings, the finalization of the CLAP leads to a decrease in the public debt ratio and an increase in the bank debt ratio, confirming that firms shift a portion of public debt to private bank loans when faced with the CLAP. In particular, the decrease in the public debt ratio (–0.014) is nearly equal to the decrease in the bank debt ratio (0.014), indicating a one-for-one substitution between these two debt sources.

5.3.4. Alternative measures of debt choice

We employ two alternative measures of debt choice to verify the robustness of our results to different measures of debt choice. The estimation results are presented in Table B7 of Appendix B. In Column (1), we calculate bank debt (the sum of term loans and total revolving credit) by taking the sum of bank debt and public debt (the sum of subordinated bonds and notes, senior bonds and notes, and commercial paper) as the deflator. In Column

(2), we calculate the bank debt ratio (*BANK_DEBT3*) by using the sum of term loans, total revolving credit, subordinated bonds and notes, senior bonds and notes, commercial papers, capital leases, and other debts as the deflator. The coefficient estimates on *CLAP* remain positive and statistically significant in all the columns at the conventional levels, suggesting that our results are robust to alternative definitions of debt choice.

5.3.5. Alternative model specifications and subsamples

In Table B8 of Appendix B, our next set of robustness tests checks alternative model specifications and subsamples. We first verify whether our results are robust to alternative clustering levels, which correct for heteroscedasticity at different levels. In the main analyses, we cluster standard errors at the headquarter state level to correct for potential covariance among firm outcomes within the same headquarter state (Bertrand et al., 2004). We now consider two-way clustering at the state-of-headquarter and year level to correct for serial dependence in the error terms from multiple observations within each unique state and year. We next consider clustering standard errors at the firm level to mitigate the effect of time-series dependence in the residuals within each firm (Petersen, 2009). In columns (1) and (2), we report estimation results of our main difference-in-differences regression with these two alternative clustering levels. Our results are robust to these two alternative clustering methods, suggesting that time-series dependence in the residuals does not explain our findings.

We next consider the possibility that firms may alter debt choice for reasons associated with financial distress during the 2008 financial crisis, which could potentially bias our estimation. To alleviate such concerns, we exclude observations in 2008 and 2009 and estimate our main regression in this subsample. We continue to find qualitatively similar results.

In Column (4), we treat years of the law changes as transition years and exclude

observations in those years to better capture changes in debt choice before and after CLAP finalization events. Column (5) excludes observations prior to 2003, as the quality of debt structure data dramatically improved after 2003 (Colla et al., 2013, 2020).

Overall, our results are robust to each of these alternative specifications, further buttressing our main arguments.

5.3.6 Placebo tests

This section conducts a placebo test to rule out the possibility of a spurious causal relation between finalizing the CLAP and debt choice. We randomize the assignment of treatment states and the enactment years to verify whether our main results in column (2) of Table 4 persist in the randomized samples. We estimate the effect of pseudo-events on pseudo-treated states using the full set of control variables from the baseline regression and store the coefficients and standard error estimates for each placebo estimation. We repeat the exercise 2,000 times and present the distribution of placebo coefficient estimates in Figure 1.

[Insert Figure 1 *about here.]*

The vertical line represents the actual coefficient estimates on *CLAP* in Column (2) of Table 3. Two patterns emerge. First, the distribution of the coefficient estimates among the 2,000 placebo samples is centered around zero, suggesting that the impact of CLAP finalization on debt structure adjustment disappears when states and enactment years are artificially selected. Second, the mean value of the placebo coefficient estimates is 0.0009, which is significantly smaller than the actual estimate of 0.038 in 95.70% (=1,914/2,000) of the placebo samples, implying that the probability of randomly observing the actual estimate when the null effect of the finalization of CLAP is true is less than 5%. Overall, the results from the placebo samples suggest that our baseline findings are unlikely to be driven by chance.

6. Conclusions

Climate change adaptation initiatives are designed to mitigate the impacts of climate change. However, it is unclear how firms and creditors perceive regulatory risks and adaptation strategies associated with such initiatives. This paper employs the staggered introduction of state-level climate change adaptation plans as quasi-exogenous shocks. We find that initiating such plans significantly increases the reliance on private debt financing for affected firms. This effect is more pronounced among firms prone to suffer from climate risks, with more fragile fundamentals and higher information asymmetries. Furthermore, the bank loan spreads significantly decrease after the finalization of CLAP. On the contrary, newly issued corporate bond spreads remain largely unaffected. Our overall findings indicate that relative to public debtholders who are lacking of capacities to acquire firm-specific information, banks are able to distinguish climate-resilient borrowers, which shed light on how debt instruments should be designed to better adapt to climate change.

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Appendix A: Variable definitions

Variable	Description (variable definitions in parentheses refer to Compustat designations where appropriate)
Panel A: Main variables	
BANK_DEBT	The ratio of bank debt (the sum of term loans and total revolving credit) to total debt provided by Capital IQ.
CLAP	An indicator variable that equals one if a firm's headquarter state has finalized a CLAP by the end of year *t*, and zero otherwise.
CLAP(–2)	An indicator variable that equals one if the state will finalize a CLAP in two years, and zero otherwise.
CLAP(–1)	An indicator variable that equals one if the state will finalize a CLAP in one year, and zero otherwise.
CLAP(0)	An indicator variable that equals one if the state will finalize a CLAP by the end of year *t*, and zero otherwise.
CLAP(+1)	An indicator variable that equals one if the state finalized a CLAP the year before, and zero otherwise.
CLAP(2+)	An indicator variable that equals one if it has been two or more years since the finalization of CLAP, and zero otherwise.
Size	Firm size, the natural logarithm of total assets (*at*).
Leverage	Firm leverage, the ratio of the sum of short-term debt (*dlc*) and long-term debt (*dltt*) to total assets (*at*).
Tangibility	Asset tangibility, the ratio of net property, plant, and equipment (*ppent*) to total assets (*AT*).
CFOVOL	Cash Flow Volatility, the standard deviation of the ratio of income before extraordinary items plus depreciation and amortization to book assets over the past four years.
TOBINQ	Tobin's Q, the ratio of [total assets (*at*) plus the market value of equity (*prcc*csho*) minus book value of equity (*ceq*) minus deferred taxes (*txdb*)] to lagged total assets (*at*).
ROA	Return on assets, the ratio of net income (*ni*) to total assets (*at*).
Panel B: Other variables	
PUB_AT	The ratio of public debt (the sum of subordinated bonds and notes, senior bonds and notes, and commercial paper) to total assets (*at*).
BANK_AT	The ratio of bank debt (the sum of term loans and total revolving credit) to the total assets (*at*).
BANK_DEBT2	The ratio of bank debt (the sum of term loans and total revolving credit) to the sum of bank debt and public debt (the sum of subordinated bonds and notes, senior bonds and notes, and commercial paper)
BANK_DEBT3	The ratio of bank debt to the sum of term loans, total revolving credit, subordinated bonds and notes, senior bonds and notes, commercial paper, capital leases, and other debt.
CREXPO	A text-based climate risk exposure index from Sautner et al. (2023a).
ENVSTR	The number of strengths regarding the environment derived from CSR scores in the KLD database.
EDF	The expected probability of default estimated from the option pricing model developed by Merton (1974).
COVERAGE	Analyst coverage, the natural logarithm of one plus the number of analysts who issued earnings forecasts for a firm in the fiscal year.
INST	Institutional ownership, the fraction of shares owned by institutional investors.

Appendix B: Table B1 Determinants of the Finalization of the CLAP

This table reports the results from a Cox proportional hazard model analyzing the hazard of a state finalizing the CLAP. The sample period is from 2001 to 2018. A "failure event" is the finalization of CLAP in a state. Once a state finalizes the CLAP, it drops from the sample. Appendix A provides definitions of all variables. Dollar values are expressed in 2000 dollars. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	$CLAP_t$	$CLAP_t$
	(1)	(2)
State average $BANK\_DEBT_{t-1}$	0.921	0.929
	(0.70)	(0.41)
State GDP Growth $_{t-1}$		−12.232
		(−1.47)
State log(GDP) $_{t-1}$		1.853
		(1.08)
State log(population) $_{t-1}$		−0.737
		(−0.41)
State unemployment rate $_{t-1}$		−0.444*
		(−1.79)
State union membership rate $_{t-1}$		0.002
		(0.04)
Demo_Share $_{t-1}$		4.619***
		(2.83)
N	481	481
pseudo R^2	0.003	0.172

Appendix B: Table B2 Bacon Decomposition

This table reports the results from the Bacon decomposition of the staggered DiD estimator using a balanced sample. All pairs involve the full sample. Early Treated Treatment vs. Late Treated Control compares the differences between earlier-treated treatment groups and later-treated control groups. Late Treated Treatment vs. Early Treated Control compares the differences between later-treated treatment groups and earlier-treated control groups. Treated vs. Never Treated compares the differences between treated treatment groups and never-treated control groups.

DiD Comparison	Weight	Average DiD Estimate
All pairs	1.000	0.066
Early Treated Treatment vs. Late Treated Control	0.094	0.041
Late Treated Treatment vs. Early Treated Control	0.048	0.042
Treated Treatment vs. Never Treated Control	0.858	0.070

Appendix B: Table B3 Statistics of Propensity Score Matching (PSM)

This table presents statistics of post-match differences in propensity score matching. Panel A presents parameter estimates from a logistic model used to estimate propensity scores for firms in the treatment and control groups. In Panel B, Column (1) presents the sample average of firm characteristics in the treated group; Column (2) presents the sample average of firm characteristics in the control group; Column (3) presents the sample-mean difference test between columns (1) and (2); Column (4) presents the *t*-value of the sample-mean difference test between columns (1) and (2). Appendix A provides definitions of all variables. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

Panel A: Pre-match regression and post-match diagnostic regression

	Pre-match (1)	Post-match (2)
Size	−0.035	0.016
	(−1.50)	(0.51)
Leverage	−0.419*	−0.248
	(−1.80)	(−0.81)
Tangibility	−0.738***	0.160
	(−2.62)	(0.41)
CFOVOL	0.162	0.490
	(0.34)	(0.71)
TobinQ	−0.010	−0.027
	(−0.25)	(−0.49)
ROA	−0.230	−0.064
	(−1.12)	(−0.23)
Constant	−1.607**	−0.545
	(−2.08)	(−0.57)
Cohort FE	Yes	Yes
Industry FE	Yes	Yes
N	13142	1503
pseudo R^2	0.114	0.013

Panel B: Pre- and post-match differences

	Pre-matched Sample			
	Treated (N=757)	Control (N=12,442)	Differences	T-value
	(1)	(2)	(3)	(4)
Size	6.163	6.657	−0.008	6.583***
Leverage	0.227	0.260	0.494	4.592***
Tangibility	0.211	0.265	0.033	6.251***
CFOVOL	0.084	0.074	0.054	−2.768***
TobinQ	1.703	1.754	−0.010	1.295
ROA	−0.054	−0.023	0.051	4.009***
	Post-matched Sample			
	Treated (N=753)	Control (N=753)	Differences	T-value
	(1)	(2)	(3)	(4)
Size	6.169	6.163	0.007	0.062
Leverage	0.227	0.231	−0.004	−0.408
Tangibility	0.212	0.200	0.011	1.174
CFOVOL	0.084	0.080	0.004	0.719
TobinQ	1.707	1.693	0.013	0.24
ROA	−0.054	−0.051	−0.003	−0.235

Appendix B: Table B4 Stack-cohort Approach Based on the PSM Sample

This table reports the results from difference-in-differences regressions that estimate the baseline dynamic effect of the climate change adaptation plan (CLAP) on corporate debt choice, using propensity score-matched stack-cohort samples from 2001 to 2018. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to the total debt. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *CLAP(–2), CLAP(–1), and CLAP(0)* are indicator variables that equal one if the state will finalize a CLAP in two years, one year, and at the end of the current year, respectively. *CLAP(+1)* is an indicator variable that equals one if the state finalized a CLAP the year before, and zero otherwise. *CLAP(2+)* is an indicator variable that equals one if it has been two or more years since the state finalized a CLAP and zero otherwise. Appendix A provides definitions of all variables. Appendix B reports the statistics and post-match diagnostics for the matched sample. All columns control for firm-times-cohort and year-times cohort-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT (1)	BANK_DEBT (2)
CLAP	**0.037****	
	(2.25)	
CLAP(–2)		0.010
		(0.56)
CLAP(–1)		0.012
		(0.73)
CLAP(0)		**0.040***
		(2.00)
CLAP(+1)		**0.032**
		(1.25)
CLAP(2+)		**0.047****
		(2.05)
Controls	Yes	Yes
Firm * Cohort FE	Yes	Yes
Year * Cohort FE	Yes	Yes
N	8995	8995
Adj_R^2	0.684	0.684

Appendix B: Table B5 The Effect of Climate Goals

This table reports the results from staggered difference-in-differences regressions that estimate the effect of the finalization of CLAP on corporate debt choice from 2001 to 2018. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to total debt. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. Appendix A provides definitions of all variables. All regressions control for firm-times-cohort- and year-times--cohort-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT (1)	*BANK_DEBT* (2)	*BANK_DEBT* (3)	*BANK_DEBT* (4)
lngoals	0.007***			
	(3.41)			
lnplanning		0.008***		
		(3.63)		
lnlaw			0.009***	
			(3.13)	
lnmonitoring				0.017***
				(3.00)
_cons	0.483***	0.483***	0.483***	0.483***
	(1070.01)	(1114.04)	(1022.52)	(1862.79)
*Firm*Cohort FE*	Yes	Yes	Yes	Yes
*Year*Cohort FE*	Yes	Yes	Yes	Yes
N	82138	82138	82138	82138
Adj. R²	0.707	0.707	0.707	0.707

Appendix B: Table B6 The Switch from Public Debt to Private Debt

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on corporate capital structure from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *PUB_AT* is the ratio of public debt to the total assets. *BANK_AT* is the ratio of bank debt to total assets. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	PUB_AT	BANK_AT
	(1)	(2)
CLAP	**−0.014*****	**0.014*****
	(−3.16)	**(3.68)**
Size	0.001	−0.006
	(0.21)	(−1.46)
ROA	−0.066***	0.018
	(−2.89)	(1.04)
Tangibility	−0.005	−0.007
	(−0.35)	(−0.44)
CFOVOL	0.001	−0.002
	(0.55)	(−1.55)
TobinQ	−0.009	−0.006
	(−1.21)	(−1.10)
Leverage	0.527***	0.358***
	(27.99)	(18.19)
Constant	0.008	0.055**
	(0.27)	(2.10)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	28596	28596
Adj_R^2	0.762	0.683

Appendix B: Table B7 Alternative Measures of Debt Choice

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on alternative measures of corporate debt choice from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *BANK_DEBT2* is the ratio of bank debt (the sum of term loans and total revolving credit) to the sum of bank debt and public debt (the sum of subordinated bonds and notes, senior bonds and notes, and commercial paper). *BANK_DEBT3* is the ratio of bank debt to the sum of term loans, total revolving credit, subordinated bonds and notes, senior bonds and notes, commercial paper, capital leases, and other debt. Appendix A provides definitions of all variables. All columns control for firm and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

|	BANK_DEBT2	BANK_DEBT3
	(1)	(2)
CLAP	**0.035****	**0.038****
	(2.32)	**(2.27)**
Size	−0.033**	−0.032**
	(−2.46)	(−2.33)
Leverage	−0.175***	−0.197***
	(−2.93)	(−3.16)
Tangibility	0.107*	0.145**
	(1.89)	(2.28)
CFOVOL	−0.099**	−0.088*
	(−2.08)	(−1.80)
TobinQ	−0.004	−0.003
	(−0.94)	(−0.75)
ROA	0.012	0.011
	(0.80)	(0.66)
Constant	0.723***	0.729***
	(7.99)	(7.81)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	28596	28596
adj. R^2	0.630	0.640

Appendix B: Table B8 Additional Robustness Checks

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on corporate debt choice from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year t, and zero otherwise. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT	BANK_DEBT	BANK_DEBT Excluding observations of 2008–2009	BANK_DEBT Excluding observations of the policy year	BANK_DEBT Excluding observations before 2003
	Cluster by state and year	Cluster by firm			
	(1)	(2)	(3)	(4)	(5)
CLAP	0.038**	0.038***	0.037**	0.042**	0.032**
	(2.72)	(2.88)	(2.23)	(2.53)	(2.36)
Size	–0.031**	–0.031***	–0.031**	–0.032**	–0.032**
	(–2.25)	(–3.78)	(–2.17)	(–2.27)	(–2.31)
Leverage	–0.153**	–0.153***	–0.173***	–0.155***	–0.154***
	(–2.76)	(–5.29)	(–2.70)	(–2.70)	(–3.43)
Tangibility	0.095*	0.095*	0.100*	0.097*	0.069
	(1.81)	(1.91)	(1.72)	(1.74)	(1.38)
CFOVOL	–0.094*	–0.094**	–0.101**	–0.095*	–0.112**
	(–1.94)	(–2.24)	(–2.11)	(–1.96)	(–2.15)
TobinQ	–0.004	–0.004	–0.005	–0.005	–0.001
	(–0.85)	(–1.01)	(–0.92)	(–1.11)	(–0.23)
ROA	0.009	0.009	0.013	0.006	0.002
	(0.47)	(0.50)	(0.68)	(0.35)	(0.12)
Constant	0.694***	0.694***	0.699***	0.701***	0.717***
	(7.50)	(12.33)	(7.19)	(7.49)	(7.52)
Firm-fixed effects	Yes	Yes	Yes	Yes	Yes
Year-fixed effects	Yes	Yes	Yes	Yes	Yes
N	28596	28596	25345	27882	24870
Adj. R^2	0.627	0.627	0.626	0.626	0.648

Figure 1: Distribution of Placebo Coefficient Estimates

This figure presents the result from placebo tests that randomize the assignment of CLAP finalization years to each state (without replacement). In particular, we estimate the effect of pseudo-events on pseudo-treated states with the full set of control variables in the baseline regression and store the coefficients and standard error estimates for each placebo test. We repeat this procedure 2,000 times. The vertical line in red shows the actual coefficient from our baseline regression.
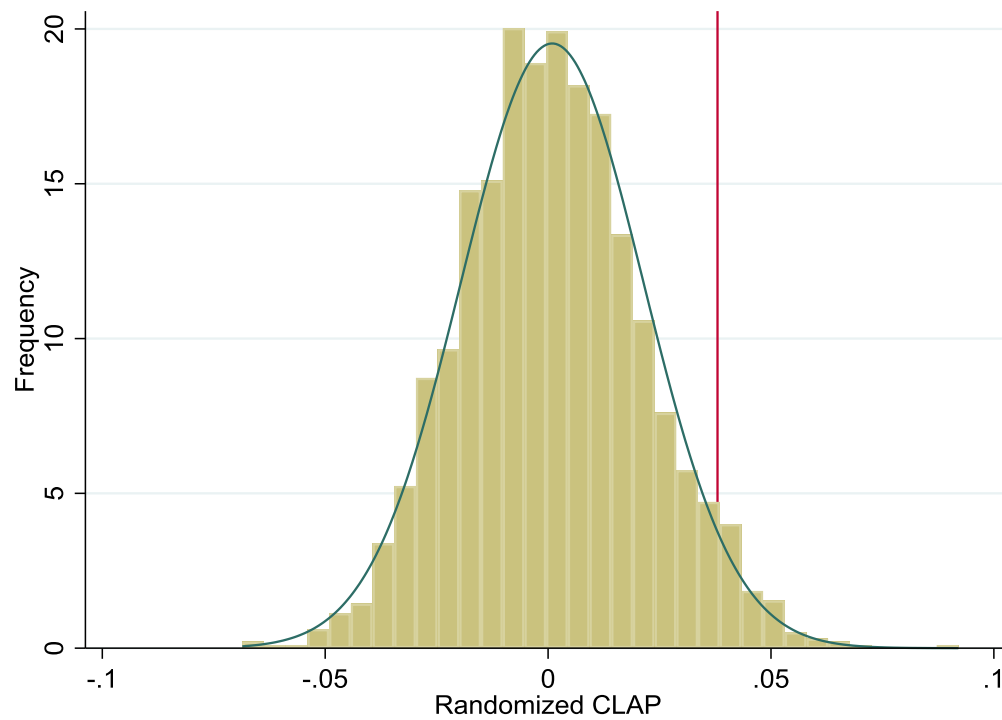
Table 1: Finalization date of state-led climate change adaptation plans

This table provides the finalization date of state-led climate change adaptation plans (As of Sept. 2023). Information is derived from https://www.georgetownclimate.org/adaptation/plans.html.

State	Adaptation plans finalization year
Alaska	2010
California	2009
Colorado	2018
Connecticut	2013
District of Columbia	2016
Delaware	2015
Florida	2008
Maine	2010
Maryland	2008
Massachusetts	2011
Montana	2020
New Hampshire	2009
New Jersey	2021
New York	2010
North Carolina	2020
Oregon	2010
Pennsylvania	2011
Rhode Island	2018
Virginia	2008
Washington	2012

Table 2: Summary Statistics of Main Variables

This table reports summary statistics for the main variables in the regression models. Appendix A provides definitions of all variables.

	# Obs. (1)	Mean (2)	Standard deviation (3)	1st percentile (4)	First quartile (5)	Median (6)	Third quartile (7)	99th quartile (8)
BANK_DEBT	28596	0.476	0.410	0.000	0.014	0.432	0.957	1.000
CLAP	28596	0.188	0.390	0.000	0.000	0.000	0.000	1.000
Size	28596	6.359	2.034	2.070	4.834	6.440	7.791	11.062
Leverage	28596	0.257	0.189	0.001	0.110	0.227	0.363	0.853
Tangibility	28596	0.262	0.226	0.008	0.089	0.191	0.367	0.908
CFOVOL	28596	0.080	0.109	0.003	0.020	0.042	0.093	0.705
TobinQ	28596	1.831	1.134	0.612	1.131	1.484	2.102	7.200
ROA	28596	−0.033	0.227	−1.248	−0.035	0.032	0.070	0.248

Table 3: Climate Change Adaptation Plan (CLAP) and Debt Choice

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on corporate debt choice from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to the total debt. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	BANK_DEBT (1)	BANK_DEBT (2)
CLAP	0.039***	0.038***
	(2.72)	(2.68)
Size		−0.031**
		(−2.20)
Leverage		−0.153***
		(−2.72)
Tangibility		0.095*
		(1.74)
CFOVOL		−0.094*
		(−1.96)
TobinQ		−0.004
		(−0.89)
ROA		0.009
		(0.54)
Constant	0.469***	0.694***
	(172.38)	(7.41)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	28596	28596
Adj_R²	0.624	0.627

Table 4: CLAP and the Timing of Debt Choice Changes

This table reports the results from difference-in-differences regressions that estimate the dynamic effect of the climate change adaptation plan (CLAP) on corporate debt choice from 2001 to 2018. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to the total debt. *CLAP(–2), CLAP(–1), and CLAP(0)* are indicator variables that equal one if the state will finalize a CLAP in two years, one year, and at the end of the current year, respectively. *CLAP(+1)* is an indicator variable that equals one if the state finalized a CLAP the year before, and zero otherwise. *CLAP(2+)* is an indicator variable that equals one if it has been two or more years since the state finalized a CLAP and zero otherwise. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT	BANK_DEBT
	(1)	(2)
CLAP(–2)	0.001	–0.000
	(0.15)	(–0.04)
CLAP(–1)	0.008	0.006
	(0.59)	(0.49)
CLAP(0)	**0.035****	**0.032****
	(2.48)	**(2.33)**
CLAP(+1)	**0.040****	**0.037****
	(2.28)	**(2.13)**
CLAP(2+)	**0.045****	**0.044****
	(2.48)	**(2.44)**
Size		–0.031**
		(–2.20)
Leverage		–0.153***
		(–2.72)
Tangibility		0.094*
		(1.73)
CFOVOL		–0.094*
		(–1.96)
TobinQ		–0.004
		(–0.89)
ROA		0.008
		(0.53)
Constant	0.468***	0.693***
	(134.92)	(7.39)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	28596	28596
Adj_R^2	0.624	0.627

Table 5: Stack-cohort Approach

This table reports the results from difference-in-differences regressions that estimate the baseline dynamic effect of the climate change adaptation plan (CLAP) on corporate debt choice using the stack-cohort sample from 2001 to 2018. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to the total debt. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *CLAP(–2), CLAP(–1), and CLAP(0)* are indicator variables that equal one if the state will finalize a CLAP in two years, one year, and at the end of the current year, respectively. *CLAP(+1)* is an indicator variable that equals one if the state finalized a CLAP the year before, and zero otherwise. *CLAP(2+)* is an indicator variable that equals one if it has been two or more years since the state finalized a CLAP and zero otherwise. Appendix A provides definitions of all variables. Appendix B reports the statistics and post-match diagnostics for the matched sample. All columns control for firm-times-cohort- and year-times-cohort-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT (1)	BANK_DEBT (2)
CLAP	**0.031*****	
	(2.78)	
CLAP(–2)		–0.010
		(–0.99)
CLAP(–1)		–0.010
		(–0.71)
CLAP(0)		**0.021***
		(1.95)
CLAP(+1)		**0.024***
		(1.70)
CLAP(2+)		**0.030****
		(2.18)
Controls	Yes	Yes
Firm * Cohort FE	Yes	Yes
Year * Cohort FE	Yes	Yes
N	82138	82138
Adj_R^2	0.708	0.708

Table 6: The Effect of Cross-Sectional Variation in Firm Characteristics

This table reports the results from triple-difference regressions that estimate the heterogeneous effect of CLAP finalization on corporate debt choice from 2001 to 2018. *BANK_DEBT* is the ratio of bank debt (the sum of term loans and total revolving credit) to total debt. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *CREXPO* is a text-based climate risk exposure index from Sautner et al. (2023a). *ENVSTR* is the number of strengths regarding the environment derived from CSR scores in the KLD database. *EDF* is the expected probability of default estimated from the option pricing model developed by Merton (1974). *COVERAGE* Analyst coverage, the natural logarithm of one plus the number of analysts who issued earnings forecasts for a firm in the fiscal year. *INST* is the fraction of shares owned by institutional investors. Appendix A provides definitions of all variables. All regressions control for firm-times cohort- and year-times-cohort-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1%, respectively.

	BANK_DEBT (1)	BANK_DEBT (2)	BANK_DEBT (3)	BANK_DEBT (4)	BANK_DEBT (5)	BANK_DEBT (6)
CLAP	−0.001	0.037***	0.008	0.183***	0.100***	0.067***
	(−0.05)	(2.69)	(0.94)	(6.02)	(4.76)	(4.12)
CLAP×CREXPO	21.027**					
	(2.08)					
CLAP×ENVSTR		−0.030***				
		(−2.69)				
CLAP×EDF			0.167**			
			(2.26)			
CLAP×Size				−0.025***		
				(−5.47)		
CLAP×COVERAGE					−0.049***	
					(−5.18)	
CLAP×INSTOWN						−0.069**
						(−2.46)
Constant	0.420***	0.394***	0.460***	0.461***	0.461***	0.461***
	(843.41)	(737.47)	(1135.27)	(953.16)	(1060.32)	(1122.39)
Firm*Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Year*Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
N	45105	47675	60706	60950	60950	59907
Adj_R²	0.683	0.682	0.700	0.700	0.700	0.703

Table 7: The Effect on Carbon Emissions

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on carbon emissions from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year t, and zero otherwise. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	logscope1	*logscope1_intensity*
	(1)	(2)
CLAP	**–0.129***	**–0.118***
	(–1.74)	**(–1.82)**
Size	0.614***	–0.079**
	(14.64)	(–2.28)
Lev	–0.191	0.003
	(–1.26)	(0.03)
Tan	0.544**	0.334
	(2.10)	(1.46)
CFOVOL	–0.379	–0.018
	(–0.99)	(–0.10)
TobinQ	0.112***	0.008
	(6.64)	(0.56)
ROA	0.148	–0.003
	(1.26)	(–0.05)
_cons	6.023***	3.908***
	(15.56)	(13.02)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	9899	9899
Adj. R²	0.943	0.916

Table 8: The Effect on Corporate Investments

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on corporate investments from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year t, and zero otherwise. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	$CAPX_{t+1}$	$CAPX\_RD_{t+1}$
	(1)	(2)
CLAP	**0.005****	**0.006****
	(2.41)	**(2.07)**
Size	–0.015***	–0.029***
	(–4.27)	(–7.87)
Lev	–0.046***	–0.058***
	(–4.98)	(–5.88)
Tan	–0.021*	–0.016
	(–1.71)	(–0.97)
CFOVOL	–0.019**	–0.042***
	(–2.66)	(–3.28)
TobinQ	0.017***	0.024***
	(5.58)	(10.55)
ROA	0.039***	0.010
	(5.33)	(0.72)
_cons	0.151***	0.263***
	(6.31)	(9.48)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	76742	76742
Adj. R^2	0.718	0.767

Table 9: The Effect on Newly Borrowed Bank Loans

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on bank loans from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	AllInDrawn	*lnMaturity*
	(1)	(2)
CLAP	**−0.124****	**−0.009**
	(−2.60)	**(−0.54)**
Size	−0.270***	0.038***
	(−8.06)	(3.05)
Lev	0.801***	0.028
	(6.80)	(0.91)
Tan	−0.669***	0.037
	(−3.00)	(0.58)
Growth	0.210***	0.010
	(3.80)	(0.54)
CFOVOL	1.841***	0.002
	(5.45)	(0.01)
TobinQ	−0.074***	0.015*
	(−2.94)	(1.68)
ROA	0.900***	0.025
	(5.62)	(0.33)
lnListAge	−0.155**	0.047*
	(−2.28)	(1.81)
Zscore	−0.203***	0.014
	(−6.28)	(1.25)
_cons	4.921***	3.265***
	(15.80)	(25.67)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
Loan Type-fixed effects	Yes	Yes
Loan Purpose-fixed effects	Yes	Yes
N	15662	15383
Adj. R²	0.610	0.735

Table 10: The Effect on Newly Issued Bonds

This table reports the results from difference-in-differences regressions that estimate the effect of the Climate Change Adaptation Plan (CLAP) on new issued bond spreads and maturity from 2001 to 2018. *CLAP* is an indicator variable that equals one if the state has finalized a CLAP by the end of year *t*, and zero otherwise. *PUB_AT* is the ratio of public debt to the total assets. Appendix A provides definitions of all variables. All columns control for firm- and year-fixed effects. Robust standard errors are clustered at the state-of-headquarter level. T-values are reported in parentheses. Coefficients marked with *, **, and *** are significant at 10%, 5%, and 1% levels, respectively.

	Yield_spread	lnMaturity
	(1)	(2)
CLAP	**0.002**	**0.019**
	(0.01)	**(0.42)**
Size	−0.459***	0.035
	(−2.75)	(1.38)
Lev	2.061***	0.005
	(3.75)	(0.05)
Tan	−1.353	0.257
	(−1.38)	(1.45)
Growth	0.251	−0.069**
	(1.43)	(−2.26)
CFOVOL	0.783	0.028
	(0.89)	(0.12)
TobinQ	−0.365***	0.046**
	(−3.67)	(2.63)
ROA	0.252	0.096
	(0.30)	(0.94)
lnListAge	0.950***	−0.114*
	(3.14)	(−1.88)
Zscore	−0.064	0.032
	(−0.42)	(1.51)
_cons	4.777**	2.098***
	(2.50)	(6.70)
Firm-fixed effects	Yes	Yes
Year-fixed effects	Yes	Yes
N	4760	6539
Adj. R^2	0.645	0.139