

**Navigating Climate Changes:**  
**How Do Adaptation Initiatives Affect Corporate Debt Choice?**

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#### **Abstract**

Climate change adaptation initiatives are designed to navigate climate change. However, it is unclear how firms and creditors perceive regulatory risks and adaptation strategies associated with such initiatives. Employing 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. Our findings shed light on how climate issues determine the perceptions of various types of creditors and affect corporate decisions.

**Keywords:** Climate Change; Debt Choice; Adaptation Initiative; Regulatory Risk

**JEL Code :** G32; G33; Q54

## 1. Introduction

Climate-related issues attract much attention around the world, which is especially pertinent for creditors that are naturally concerned about downside risks. A growing number of investors, academics, policymakers, and regulators are questioning whether credit ratings—the ubiquitous scores that underpin much of the financial system—are accounting for the impact that extreme weather events and policy changes related to global warming will have on borrowers.<sup>1</sup> In a proposal that would heighten risk management expectations for financial institutions, New York’s influential state banking regulator said that big and small banks should pore over how climate change could impact their business.<sup>2</sup> Although it is well documented that climate issues can exert a pronounced impact on capital structure (e.g., Correa et al., 2020; Javadi and Masum, 2021; Nguyen et al., 2022), the heterogeneity in the sources of debt financing is largely ignored in this line of research. Our work aims at filling this gap by focusing on how the climate issue affects the corporate choice between bank debt and bond debt and, therefore, simultaneously considers various types of creditors.

In particular, we focus on how the staggered finalization of state-level climate change adaptation plans (CLAPs) affects corporate debt choice. Finalizing such plans encourages actions to reduce the negative impacts of climate change and take advantage of emerging growth opportunities, which tends to enhance the capacity to adapt to climate change in treated states. The finalization of CLAPs demonstrates the climate risks and opportunities that firms are expected to face and propose several policies to be finalized in the future.

We hypothesize that finalizing CLAPs affects corporate debt choice through various channels that yield different predictions. One hypothesis is that firms may shift more weights

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<sup>1</sup> Source: <https://www.bloomberg.com/news/articles/2021-09-23/climate-change-risk-looms-for-government-debt?leadSource=verify%20wall>.

<sup>2</sup> Source : <https://www.wsj.com/articles/new-york-flags-climate-risks-for-banks-11671650722>.

of debt financing from public debt to private bank loans. From a supply side, since the CLAPs depict the big picture of the climate risks of a firm's state of headquarter, investors perceived increased risks so that they become reluctant to provide capital to the firm. Since banks have advantages in information gathering, their information set about firms' climate risk environment is less sensitive to the CLAP. Therefore, the above effect is more pronounced for public bonds. Also, the anticipation of future adjustments to policies is expected to induce a concern about default risk; however, bondholders do not have the ability to renegotiate the debt, so they are more inclined to choose the reduction of the capital provided.

From the demand side perspective, firms tend to finance their investments from markets with lower transaction costs (Blackwell and Kidwell, 1988). As it is rather difficult to illustrate the climate risk and the potential effects of adaptation policies to the public, firms may tend to shift from bonds to bank loans. Also, in anticipation of the foreseeable future adjustments to climate change measures, there is a higher debt renegotiation demand in the future. As a result, in equilibrium, firms are expected to shift to bank loans after finalizing CLAPs (Chemmanur and Fulghieri, 1994).

An alternative hypothesis is that the finalization of CLAPs yields a lower level of reliance on bank loans. As all the CLAPs across different states share the common goal of protecting the environment and putting strategies in place to better handle climate-related adverse events (Kovacs et al., 2021) and help resolve uncertainty about future climate regulation, firms may be more able to obtain external financing from public creditors due to reduced perceived risk for outsiders. Ultimately, whether and to what extent finalizing the climate change adaptation plan affects debt structure is an empirical question.

We adopt a difference-in-differences regression design around the staggered state-level finalization of the CLAP to estimate its impact on debt structure. 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 year, and zero otherwise. Following prior studies on debt choice, we use the ratio of the total private bank loans to the total debt as the main measure of debt choice (Lin et al., 2013). Our sample period runs from 2001 to 2018 due to the data availability of debt structure. 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. This result suggests that when faced with a clearer climate risk environment and possible adjustment risks to the plan, firms increase the use of bank debt due to the potential benefits of renegotiation in bankruptcy and lower financing costs.

We conduct the following tests to make sure that our experiment satisfies the parallel trend assumption and that our results are not driven by reverse causality or omitted variables. First, we use a Cox proportional hazard model to show that the timing of the finalization of CLAP is not driven by the level of bank debt ratio as well as underlying local economic factors, suggesting that the state court decision is largely unanticipated and exogenous to characteristics that may affect the debt choice of local firms. Second, we conduct a dynamic timing test to replace the *CLAP* indicator with five indicator variables representing years relative to the year the state finalizes a CLAP. The estimation results suggest no significant changes in debt structure between treatment and control firms in the years before the plan finalization. Depending on the specification, the effect only becomes statistically and economically significant the year after the finalization of the plans. This evidence is consistent with the argument that capital structure and debt structure adjustment take time to complete. Overall, the evidence from these two tests suggests that the staggered state-level finalization of CLAP are indeed exogenous to the focal firms headquartered in that state and

create exogenous and meaningful variation in the corporate climate risk environment, which causes firms to shift to a debt structure with a higher weight of bank debt.

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 et al., 2022)), we employ a stack cohort approach, 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 in the prior years of policy changes. Our main findings still hold on 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 by adopting a stack-cohort approach. First, firms with a safer ex-ante climate risk environment should respond weaker to finalizing CLAP because creditors' perceived risks may not change heavily, and firms are less likely to suffer from passive adjustment to climate policies which would generate a downside risk. Second, because of the bank's unique ability to write tailor-made contracts and renegotiate with firms in trouble (Aghion and Bolton, 1992; Diamond, 1984), for firms that are prone to get into trouble, having a bank loan is more beneficial than a bond when faced with potentially required adjustment to the CLAP. Third, the costs of disclosing climate-related information to issue public bonds are more prone to be higher as investors are more concerned about climate risks the firm would suffer from. Therefore, the main effects are expected to be stronger for firms with higher levels of information asymmetry. Indeed, our main results are weaker among firms with a safer climate risks profile and higher with more fragile fundamentals and those exhibiting higher levels of information asymmetry. These cross-sectional variations in the impact of the rejections of the IDD on debt choice not only shed light on the underlying

channels, the ease of debt renegotiation, and information asymmetry, it further alleviates concerns regarding omitted variables because such variables would have to be uncorrelated with all the control variables while being able to explain the cross-sectional variation.

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 climate issues affect the cost of debt and capital structure (e.g., Nguyen and Phan, 2020; Javadi and Masum, 2021; Choi et al., 2022; Seltzer et al., 2022), evidence on how climate issues affect debt choice is nonexistent.

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 et al., 2008; Dhaliwal et al., 2011; Boubaker et al., 2018; Li et al., 2019; Ben-Nasr et al., 2021). In particular, our paper contributes to the literature on how regulatory change affects debt choice, given climate-related regulatory change.

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

## **2. Data and Empirical Design**

### **2.1 Data**

We collect data on debt structure from the Capital IQ database, which classifies debt into commercial papers, revolving credits, term loans, senior bonds and notes, subordinated bonds and notes, capital leases, and other debts. Our sample starts in 2001, when data from Capital IQ first became available and ends in 2018. Our financial data are from Compustat. We exclude financial firms (SIC codes from 6000 to 6999) and regulated utility firms (SIC codes

from 4900 to 4999) because these firms' capital structure may be more affected by government regulations rather than information asymmetry and debt renegotiation needs. Our final sample comprises 4270 firms over 2001–2018, making 28596 firm-year observations.

## 2.2 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:

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

where  $i$  indexes firm,  $s$  indexes state of headquarter, and  $t$  indexes 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. The timetable of the finalization of state-led climate change adaptation plans is provided in Table 1.

*[Insert Table 1 about here]*

We also include a vector of control variables commonly used in the empirical debt choice literature (e.g., Boubaker et al. (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 ( $\tau_t$ ) fixed effects to control for time-invariant characteristics across firms and time-variant 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 headquarter state (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* is 0.472 and 0.432, respectively. This statistic is similar to those documented in prior studies. Throughout our sample period, 18.8% of firms are affected by the finalization of the CLAP.

*[Insert Table 2 about here]*

### **3. Main Estimation Results**

#### **3.1 Duration model for timing of finalization of 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 has first to show that the state-level finalization of the CLAP are i) a decision that is exogenous to the outcome variable (debt choice); ii) not related to state-level macro-economic conditions, which could indirectly affect the outcome variable (debt choice). To verify this assumption, we follow Acharya et al. (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  $t$ .

*[Insert*

Table 3 *about here*

Table 3 presents the estimation results. 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 with marginal significance and political balance. The coefficient on lagged *Demo\_Share* is significantly positive at the 1% level, consistent with the notion that Democrats are more likely to support environment-related policies like the CLAP. Overall, the evidence from

Table 3 indicates that the finalization of CLAP are generally exogenous to i) the state-level value of the outcome variable (debt structure) and ii) the local macro-economic factors, suggesting that these finalization are unanticipated and create meaningful exogenous variation in corporate climate risk environment.

### **3.2 Baseline Results**

Table 4 presents the estimation results from the difference-in-differences regressions assessing the impact of the state-level finalization of the CLAP 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. 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 4 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% ( $=0.038/0.474$ ) 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.

### **3.3 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 5 about here]*

Table 5 replicates the tests in Table 4 except for changing the main explanatory variable 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 show that finalizing CLAP leads to a significant increase in the use of bank debt within the debt structure. This relation does not seem to suffer from endogeneity and there are no pre-

treatment trends, thus, the parallel trends assumption is satisfied.

### **3.4 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), firstly, we construct a balanced panel by dropping all firms with missing observations in any period during 2001-2018. 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. The results are presented in Table 6. The treatment effects are estimated to be 0.066 using the full sample, most of them 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 our staggered DiD estimator is not significantly biased by the problem of time-varying treatment effects.

*[Insert Table 6 about here]*

### **3.5 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 et al., 2022). To mitigate this bias, several studies propose event-based staggered DiD that uses alternative groups to serve as the controls (Fauver et al., 2021). In particular, the stack-cohort approach can help circumvent this issue. The idea is to create event-specific “clean  $2 \times 2$ ” datasets, including the outcome variable and controls for the treated cohort and all other observations that are “clean” controls within the treatment window (Baker et al., 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  $-5$  and  $+5$  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 7 about here]*

The estimation results are presented in Table 7. 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* continues to load positively and significantly. 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 coefficients on *CLAP*(0), *CLAP*(1), and *CLAP*(2) are positive and statistically significant.

### **3.6 Propensity Score Matching**

In this section, we estimate the effect of the finalization of CLAP on debt structure using the propensity score matched technique to control for the concern that the treated firms are fundamentally different from the 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 the observations for the treatment and control firms in the year before 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 (Table 4, Column 2) 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 in each cohort, we construct a stack-cohort sample like in Section 3.5.

We check the matching procedure in the following ways. First, in Panel A of Appendix B, we report the estimation results from the logistic regression used to estimate the probability of being a treatment firm in the pre-match and the post-match sample. 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 of Appendix 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, *T*-test results suggest that there are significant differences in the mean of most covariates between treatment and control groups. However, the differences disappear for the post-matched samples, providing further assurance that the matching procedure is successful.

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. Table 8 presents the estimation results. Overall, in the propensity score-matched samples, we continue to find that finalizing CLAP is associated with an increased weight 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 sum, 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.

*[Insert Table 8 about here]*

## **4. Further Analysis**

### **4.1 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.<sup>3</sup> Ideally, we want to explore the cross-sectional variations based on firm characteristics in the year prior to 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 in Section 3.5 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 assign the level of cross-sectional variables in period  $t-1$  to the ten-year observations.<sup>4</sup> Next, we stack the data from different cohorts to build the triple difference sample. This approach allows us to use every untreated observation at a particular point in time as a control for treated observations in that time period. We estimate the average treatment effect across the staggered finalization in this sample. The model is specified as follows:

$$BANK\_DEBT_{i,c,s,t} = \beta_0 + \beta_1 Clap_{s,t} + \gamma_1 Clap_{s,t} \times X_{i,c} + \gamma_2 X_{i,c} + \omega_c \times v_i + \omega_c \times \tau_t + \sigma_s \times \tau_t + \varepsilon_{i,c,s,t} \quad (2)$$

where  $c$  indexes cohort, and  $X_{i,c}$  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

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<sup>3</sup> 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.

<sup>4</sup> Firms with a missing  $t-1$  observation are dropped in this process.

are absorbed by the cohort-times-firm fixed effects ( $\omega_c \times v_i$ ).<sup>5</sup>

We first examine how a firm's ex-ante climate risk exposure affects the impact of CLAP finalization on debt choice. We predict that the finalization of CLAP has a weaker impact on the debt choice of firms with a safer climate risk environment. If the underlying channel that drives our documented results is public creditors' increased risk perception, *Ceteris Paribus*, the effect should be weaker when the firms are higher exposed to climate risks. We finalize two commonly used proxies for climate risk environment. The first proxy is a textual-based climate risk exposure index from Sautner et al. (2022)<sup>6</sup>, *CREXPO*, 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 better environment-related CSR profile can protect the firm from downside climate risks; thus, this measure negatively correlates with climate risk exposures.

Table 9 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, suggesting that firms with higher ex-ante climate risk exposures shift a greater percentage of debt financing to private bank loans. The estimation results in column (2) show that the coefficient estimates on *CLAP* and *ENVSTR* are negative and significantly significant at the 1% threshold level, indicating that firms with a better environmental profile turn less to private bank loans.

*[Insert Table 9 about here]*

Our second source of cross-sectional variation exploits the ex-ante likelihood of default. The finalization of CLAP leads to a risk of future adjustments to policy requirements. We

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<sup>5</sup> 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.

<sup>6</sup> We thank the authors for sharing the data on the website: <https://osf.io/fd6jq/>

conjecture that this induces higher debt renegotiation needs for firms with higher ex-ante default risks. Consequently, these firms are expected to shift a larger percentage of debt to private bank loans in 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 one is the firm size (*Size*) since size 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 climate risk related-information demanding after accessing public information, 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 at monitoring firms and resolving information asymmetry than public bondholders. Therefore, the ability and the need to process private information in a time-sensitive manner is expected to determine the use of information-sensitive instruments like bank debts (Li et al., 2019). We hypothesize that *Ceteris Paribus*, higher ex-ante information-asymmetry firms, shift more to 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 should have less information asymmetry and 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.

#### 4.2 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 check whether our main results in column (2) of Table 4 still exist in the randomized samples. We estimate the effect of pseudo-events on pseudo-treated states with the full set of control variables used in 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 4. 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 if the states and enactment years are artificially chosen. 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 indicate that our baseline findings are unlikely to be driven by chance.

### **4.3 The effect on capital structure**

In this section, we examine the impact of the finalization of CLAP on the use of public versus private debt in the capital structure. In Columns (1)-(2), Table 10, 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 the same as the bank debt ratio (0.014), indicating a one-for-one substitution between these two debt sources.

*[Insert Table 10 about here]*

### **4.4 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.

#### **4.4.1 Alternative Measures of Debt Choice**

We employ two alternative measures of debt choice to check whether our results are robust to using different measures of debt choice. Estimation results are shown in Table 11. 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.

*[Insert Table 11 about here]*

#### **4.4.2 Alternative Model Specifications and Subsamples**

Our next set of robustness tests checks alternative model specifications and subsamples. We first check whether our results are robust to alternative clustering levels to 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) of Table 12, 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 results.

*[Insert Table 12 about here]*

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 before 2003 since the quality of debt structure data became better after 2003 (Colla et al., 2013, 2020).

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

## **5. Conclusion**

Climate change adaptation initiatives are designed to navigate climate changes. 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. Our overall findings shed light on how climate issues determine the perceptions of various types of creditors and affect corporate decisions.



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## Appendix A: Variable definitions

Variable	Description (variable definitions in parentheses refer to Compustat designations where appropriate)
Panel A: Main variables	
<i>BANK_DEBT</i>	The ratio of bank debt (the sum of term loans and total revolving credit) to total debt provided by Capital IQ.
<i>CLAP</i>	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.
<i>CLAP(-2)</i>	An indicator variable that equals one if the state will finalize a CLAP in two years, and zero otherwise.
<i>CLAP(-1)</i>	An indicator variable that equals one if the state will finalize a CLAP in one year, and zero otherwise.
<i>CLAP(0)</i>	An indicator variable that equals one if the state will finalize a CLAP by the end of year $t$ , and zero otherwise.
<i>CLAP(+1)</i>	An indicator variable that equals one if the state finalized a CLAP the year before, and zero otherwise.
<i>CLAP(2+)</i>	An indicator variable that equals one if it has been two or more years since the the state finalized a CLAP, and zero otherwise.
<i>Size</i>	Firm size, the natural logarithm of total assets ( $at$ ).
<i>Leverage</i>	Firm leverage, the ratio of the sum of short-term debt ( $dlc$ ) and long-term debt ( $dltt$ ) to total assets ( $at$ ).
<i>Tangibility</i>	Asset tangibility, the ratio of net property, plant, and equipment ( $ppent$ ) to total assets ( $at$ ).
<i>CFOVOL</i>	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.
<i>TOBINQ</i>	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 ( $at$ ).
<i>ROA</i>	Return on assets, the ratio of net income ( $ni$ ) to total assets ( $at$ ).
Panel B: Other variables	
<i>PUB_AT</i>	The ratio of public debt (the sum of subordinated bonds and notes, senior bonds and notes, and commercial paper) to total asset ( $at$ ).
<i>BANK_AT</i>	The ratio of bank debt (the sum of term loans and total revolving credit) to the total asset ( $at$ ).
<i>BANK_DEBT2</i>	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)
<i>BANK_DEBT3</i>	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.
<i>CREXPO</i>	A textual-based climate risk exposure index from Sautner et al. (2022).
<i>ENVSTR</i>	The number of strengths regarding the environment derived from CSR scores in the KLD database.
<i>EDF</i>	The expected probability of defaulting estimated from the option pricing model developed by Merton (1974).
<i>COVERAGE</i>	Analyst coverage, the natural logarithm of one plus the number of analysts who issued earnings forecasts for a firm in the fiscal year.
<i>INST</i>	Institutional ownership, the fraction of shares owned by institutional investors.

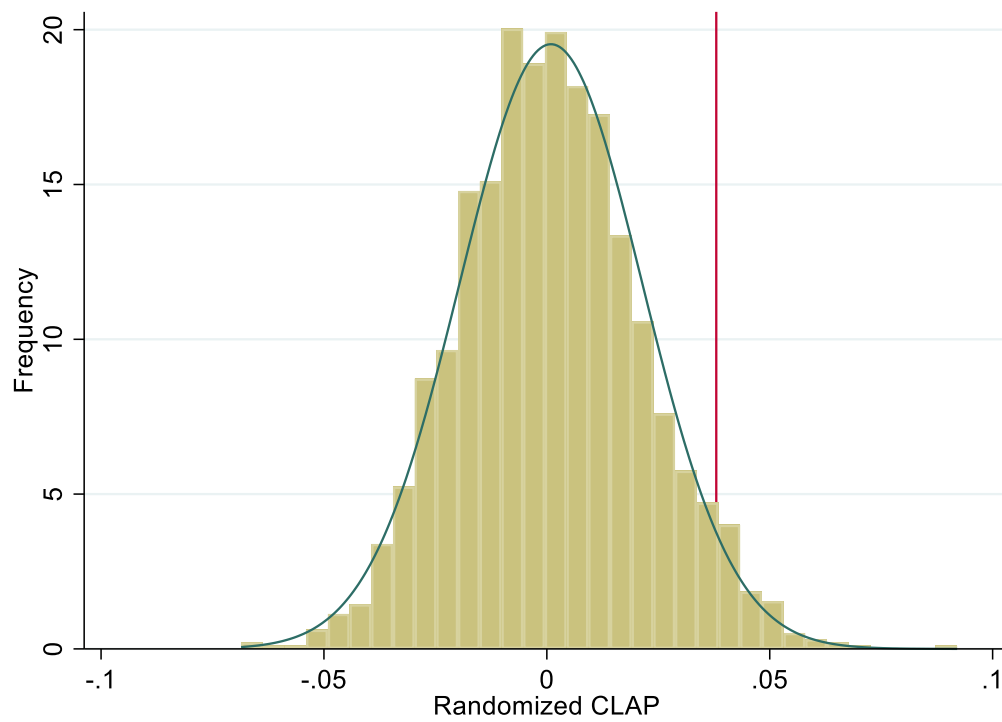
## Appendix B: Statistics of Propensity Score Matching

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.

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

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



**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 (As of Sept. 2023)**

This table provides the finalization date of state-led climate change adaptation plans. Information is derived from <https://www.georgetownclimate.org/adaptation/plans.html>.

State	Adaptation plans finalization date
Alabama	No state-led adaptation plan finalized.
Alaska	Jan-2010
Arizona	No state-led adaptation plan finalized.
Arkansas	No state-led adaptation plan finalized.
California	2009
Colorado	Jul-2018
Connecticut	2013
District of Columbia	15-Nov-2016
Delaware	2-Mar-2015
Florida	15-Oct-2008
Georgia	No state-led adaptation plan finalized.
Hawaii	No state-led adaptation plan finalized.
Idaho	No state-led adaptation plan finalized.
Illinois	No state-led adaptation plan finalized.
Indiana	No state-led adaptation plan finalized.
Iowa	No state-led adaptation plan finalized.
Kansas	No state-led adaptation plan finalized.
Kentucky	No state-led adaptation plan finalized.
Louisiana	No state-led adaptation plan finalized.
Maine	Feb-2010
Maryland	Jul-2008
Massachusetts	2011
Michigan	State adaptation planning underway.
Minnesota	State adaptation planning underway.
Mississippi	No state-led adaptation plan finalized.
Missouri	No state-led adaptation plan finalized.
Montana	Aug-2020
Nebraska	No state-led adaptation plan finalized.
Nevada	No state-led adaptation plan finalized.
New Hampshire	Mar-2009
New Jersey	12-Oct-2021
New Mexico	No state-led adaptation plan finalized.
New York	2010
North Carolina	2-Jun-2020
North Dakota	No state-led adaptation plan finalized.
Ohio	No state-led adaptation plan finalized.
Oklahoma	No state-led adaptation plan finalized.
Oregon	Dec-2010
Pennsylvania	2011
Rhode Island	2-Jul-2018
South Carolina	No state-led adaptation plan finalized.
South Dakota	No state-led adaptation plan finalized.
Tennessee	No state-led adaptation plan finalized.
Texas	No state-led adaptation plan finalized.
Utah	No state-led adaptation plan finalized.
Vermont	State adaptation planning underway.
Virginia	15-Dec-2008
Washington	Apr-2012
West Virginia	No state-led adaptation plan finalized.

Wisconsin  
Wyoming

State adaptation planning underway.  
No state-led adaptation plan finalized.

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

**Table 3 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.

	$CLAP_t$ (1)	$CLAP_t$ (2)
<i>State average BANK_DEBT</i> <sub>t-1</sub>	0.921 (0.70)	0.929 (0.41)
<i>State GDP Growth</i> <sub>t-1</sub>		-12.232 (-1.47)
<i>State log(GDP)</i> <sub>t-1</sub>		1.853 (1.08)
<i>State log(population)</i> <sub>t-1</sub>		-0.737 (-0.41)
<i>State unemployment rate</i> <sub>t-1</sub>		-0.444* (-1.79)
<i>State union membership rate</i> <sub>t-1</sub>		0.002 (0.04)
<i>Demo_Share</i> <sub>t-1</sub>		4.619*** (2.83)
<i>N</i>	481	481
<i>pseudo R<sup>2</sup></i>	0.003	0.172

**Table 4 The Finalization of 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.

	<i>BANK_DEBT</i>	<i>BANK_DEBT</i>
	(1)	(2)
<b><i>CLAP</i></b>	<b>0.039***</b>	<b>0.038***</b>
	<b>(2.72)</b>	<b>(2.68)</b>
<i>Size</i>		-0.031**
		(-2.20)
<i>Leverage</i>		-0.153***
		(-2.72)
<i>Tangibility</i>		0.095*
		(1.74)
<i>CFOVOL</i>		-0.094*
		(-1.96)
<i>TobinQ</i>		-0.004
		(-0.89)
<i>ROA</i>		0.009
		(0.54)
<i>Constant</i>	0.469***	0.694***
	(172.38)	(7.41)
<i>Firm FE</i>	Yes	Yes
<i>Year FE</i>	Yes	Yes
<i>N</i>	28596	28596
<i>Adj_R<sup>2</sup></i>	0.624	0.627

**Table 5 The Finalization of CLAP and the Timing of Debt Choice Changes**

This table reports the results from difference-in-differences regressions that estimate the dynamic effect of 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.

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

**Table 6 Bacon Decomposition**

This table reports the results from Bacon decomposition of the staggered DiD estimator using a balanced sample. All pairs involve the full sample. Earlier Treated Treatment vs. Later Treated Control compares the differences between earlier treated treatment groups and later treated control groups. Later Treated Treatment vs. Earlier 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
Earlier Treated Treatment vs. Later Treated Control	0.094	0.041
Later Treated Treatment vs. Earlier Treated Control	0.048	0.042
Treated Treatment vs. Never Treated Control	0.858	0.070

**Table 7 Stack-cohort Approach**

This table reports the results from difference-in-differences regressions that estimate the baseline dynamic effect of 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.

	<i>BANK_DEBT</i>	<i>BANK_DEBT</i>
	(1)	(2)
<b><i>CLAP</i></b>	<b>0.031***</b>	
	<b>(2.78)</b>	
<i>CLAP</i> (−2)		−0.010
		(−0.99)
<i>CLAP</i> (−1)		−0.010
		(−0.71)
<b><i>CLAP</i>(0)</b>		<b>0.021*</b>
		<b>(1.95)</b>
<b><i>CLAP</i>(+1)</b>		<b>0.024*</b>
		<b>(1.70)</b>
<b><i>CLAP</i>(2+)</b>		<b>0.030**</b>
		<b>(2.18)</b>
<i>Controls</i>	Yes	Yes
<i>Firm * Cohort FE</i>	Yes	Yes
<i>Year * Cohort FE</i>	Yes	Yes
<i>N</i>	82138	82138
<i>Adj_R</i> <sup>2</sup>	0.708	0.708

**Table 8 Using Propensity Score Matched Samples under Stack-cohort Approach**

This table reports the results from difference-in-differences regressions that estimate the baseline dynamic effect of 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 finalized 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.

	<i>BANK_DEBT</i> (1)	<i>BANK_DEBT</i> (2)
<b><i>CLAP</i></b>	<b>0.037**</b> <b>(2.25)</b>	
<i>CLAP</i> (-2)		0.010 (0.56)
<i>CLAP</i> (-1)		0.012 (0.73)
<b><i>CLAP</i>(0)</b>		<b>0.040*</b> <b>(2.00)</b>
<b><i>CLAP</i>(+1)</b>		<b>0.032</b> <b>(1.25)</b>
<b><i>CLAP</i>(2+)</b>		<b>0.047**</b> <b>(2.05)</b>
<i>Controls</i>	Yes	Yes
<i>Firm * Cohort FE</i>	Yes	Yes
<i>Year * Cohort FE</i>	Yes	Yes
<i>N</i>	8995	8995
<i>Adj_R</i> <sup>2</sup>	0.684	0.684

**Table 9 The Effect of Cross-Sectional Variation in Firm Characteristics**

This table reports the results from triple-difference regressions that estimate the heterogeneous effect of 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. *CREXPO* is a textual-based climate risk exposure index from Sautner et al. (2022). *ENVSTR* is the number of strengths regarding the environment derived from CSR scores in the KLD database. *EDF* is the expected probability to 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.

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



**Table 10 The Finalization of Climate Change Adaptation Plan (CLAP) and the Ratios of Public Debt and 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 asset. *BANK\_AT* is the ratio of bank debt total asset. 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.

	<i>PUB_AT</i> (1)	<i>BANK_AT</i> (2)
<b><i>CLAP</i></b>	<b>-0.014***</b> <b>(-3.16)</b>	<b>0.014***</b> <b>(3.68)</b>
<i>Size</i>	0.001 (0.21)	-0.006 (-1.46)
<i>ROA</i>	-0.066*** (-2.89)	0.018 (1.04)
<i>Tangibility</i>	-0.005 (-0.35)	-0.007 (-0.44)
<i>CFOVOL</i>	0.001 (0.55)	-0.002 (-1.55)
<i>TobinQ</i>	-0.009 (-1.21)	-0.006 (-1.10)
<i>Leverage</i>	0.527*** (27.99)	0.358*** (18.19)
<i>Constant</i>	0.008 (0.27)	0.055** (2.10)
<i>Firm FE</i>	Yes	Yes
<i>Year FE</i>	Yes	Yes
<i>N</i>	28596	28596
<i>Adj R<sup>2</sup></i>	0.762	0.683

**Table 11 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.

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

**Table 12 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.

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