Climate Policy Risk and Corporate Financial Decisions: Evidence from the NOx Budget Trading Program

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Climate Policy Risk and Corporate Financial Decisions:

Evidence from the NO_x Budget Trading Program[*]

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Abstract

We find that manufacturing firms adopt more conservative capital structures in response to the NO_x Budget Trading Program (NBP) of 2004, a regional cap-and-trade program aimed at mitigating the NO_x emissions of power plants in 11 midwestern and southeastern states in the United States. Our further analysis demonstrates that, because the NBP induces an electricity price shock, it affects manufacturers' financial decisions by raising their operating leverage and distress risk. We also find that firms respond to the NBP's adoption heterogeneously: they adjust their financial leverage more dramatically when facing greater electricity intensity, financial distress threats, or competitive pressure. In addition, firms adapt not only capital structure but also other financial policies in response to the regulation. Overall, our study shows that climate policy risk constitutes an essential consideration in firm financial decisions. It also highlights potential unintended consequences of policy responses to climate change for the corporate sector.

JEL Classifications: G32; Q53; L60

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1. Introduction

Over recent decades, a strong scientific consensus on climate change has emerged, attracting unprecedented attention from academia and industry, as well as policymakers and the public (Stern 2008, Allen et al. 2009, Heal 2009, Howe et al. 2013). To mitigate climate change consequences, emission control regulations have been introduced around the world, taking center stage in the fight against climate change (Stavins 1998, Fowlie 2010). Notable examples of this approach include the U.S. Acid Rain Program of 1995 and the European Union Emissions Trading Scheme (EU ETS) of 2005.[1] However, emission control regulations pose considerable policy risk to the corporate sector (Krueger et al. 2020). For instance, such regulations could impose substantial costs on firms and/or leave their carbon-intensive assets stranded. A paramount question facing many companies is how to adapt to not only the potential physical risk of climate change, but also the policy risk associated with increasingly stringent climate regulations. In this paper, we focus on this type of transition risk and ask how and to what extent manufacturing companies adapt their capital structure and other financial decisions in response to tightening emission mitigation policies.

Our in-depth analysis of corporate capital structure is motivated by a substantial literature showing that capital structure choice constitutes an essential part of management decisions.[2] It is well established that a firm's choice of financing instruments matters for its shareholders and various stakeholders (e.g., customers, suppliers, and employees) and plays an important role in shaping other corporate strategies, including real corporate behavior such as investment. For instance, prior research shows that financial leverage has significant real effects through its interactions with other firm decisions on investment, exit, diversification, and mergers and acquisitions (Kovenock and Phillips 1997, Childs et al. 2005, Ahn et al. 2006, Uysal 2011). There is also ample evidence that capital structure affects firms' competitive position (Bolton and Scharfstein 1990), their strategies to compete with rivals (Chevalier and Scharfstein 1996), and their product market behavior, including product quality and pricing (Phillips and Sertsios 2013). Financial leverage can also be adopted strategically by

[1] Broadly, emission control programs can be classified into command-and-control programs and cap-and-trade programs. The NO_X Budget Trading Program (NBP) we study in this paper belongs to the latter category.
[2] See Parsons and Titman (2009) for a literature review and Graham and Harvey (2001) for survey evidence.

management to reduce the bargaining power and/or induce optimal actions by the firm's stakeholders, such as suppliers, customers (Banerjee et al. 2008), and employees (Bronars and Deere 1991, Matsa 2010, Myers and Saretto 2016). Empirical evidence further suggests that capital structure has significant implications for firm sales and market share (Opler and Titman 1994, Zingales 1998, Campello 2003, 2006), as well as firm asset value and stock returns (Fama and French 1998, George and Hwang 2010, Korteweg 2010, van Binsbergen et al. 2010), rendering it one of the most pertinent issues in corporate financial management. Although our research focuses on capital structure, in additional analysis, we examine other important firm financial decisions and outcomes, such as cash holdings, payout, and investment, to gain a richer understanding of how climate policy risk affects the corporate sector.

We set our study in the context of the adoption of the NBP following the DC Court of Appeals ruling in the case of *Michigan et al. v. Environmental Protection Agency* (EPA), which affected 11 midwestern and southeastern states in the United States from 2004 to 2008.[3] The NBP regulated nitrogen oxide (NO_x) emissions through a regional cap-and-trade program that typically set a cap on the overall annual NO_x pollutant emissions in compliant states. The emission quota was then divided into annual emission permits and allocated to regulated power generating units.[4] Affected sources were required to submit a certain number of permits that matched the volume of emissions at the end of the compliance period. If the number of permits was lower than the actual volume of emissions, those sources had to buy permits in designated markets to cover the shortfall or face a penalty.

While the implementation of the NBP significantly reduced NO_x emissions, ambient ozone concentrations, pharmaceutical expenditures, and the mortality rate (Deschênes et al. 2017), the compliance costs proved to be substantial for the affected power generating utilities. Many emission sources purchased emission permits, made substantial initial and ongoing capital investments on emission control and monitoring equipment, and/or adjusted their production processes (Fowlie 2010). The capitalized emission abatement costs of the NBP are estimated to be between $1.1 billion and $2.15 billion per annum (Palmer et al. 2001, Deschênes et al. 2017). Importantly, the EPA (1998) and

[3] Included in the 2004 implementation of the NBP are 11 midwestern and southeastern states, namely, Alabama, Illinois, Indiana, Kentucky, Michigan, North Carolina, Ohio, South Carolina, Tennessee, Virginia, and West Virginia. We provide background details on the litigation and court ruling surrounding the NBP in Section 2.1.
[4] Each permit allows a power generating unit to emit a ton of NO_x. Most of these units are owned and operated by electrical utilities.

Platts Research and Consulting (2003) both projected that the NBP's implementation would increase electricity prices, ultimately affecting manufacturing firms in NBP-compliant states. Subsequent evidence shows that power generating utilities indeed managed to pass a significant proportion of the NBP compliance costs on to their customers, including business users (Veith et al. 2009, Bushnell et al. 2013, Marin et al. 2018). Curtis (2018) estimates that, at the state level, electricity prices increased after the NBP's adoption by between 3.5% and 5.9%.[5]

The implementation of the NBP serves as an appropriate quasi-natural experiment to establish the causal effect of emission mitigation policies on manufacturing firms' financing decisions for at least two reasons. First, unlike other cap-and-trade programs, such as the Acid Rain Program or the EU ETS covering the entire nation(s), the NBP regulated regional NO_x emissions. This important and unique feature of the NBP, that is, its regional rather than national coverage, allows us to identify the counterfactuals of the treated firms by selecting similar firms operating in unregulated states. Second, compared with other energy industry regulations that may also affect energy production, pricing, and consumption, including renewable portfolio standard (RPS) laws, electricity market restructuring (EMR), energy efficiency resource standards (EERS), net metering (NM), green power purchasing (GPP) programs, and public benefit funds (PBFs) (Greenstone and Nath 2020), the NBP provides arguably the most relevant policy shock for the purpose of our study, because it is a prominent climate regulation that resulted in a significant reduction in emissions, exerted a clear impact on the wider corporate sector, and was implemented following an (unexpected) court ruling.

Since the NBP had major implications for electricity prices, impacting manufacturing firms headquartered in compliant states, in our analysis, we focus on those manufacturers rather than the affected sources of emissions.[6] We do not examine the power generating units directly affected by the NBP, because, as mentioned above, they had to take various measures to comply with the program. In particular, these emission sources may have directly experienced policy effects on their operations, finance, and performance, which are difficult to disentangle. Meanwhile, we do not consider non-

[5] Using utility-level data, we provide corroborating evidence that the adoption of the NBP raised electricity prices by between 9.1% and 9.8% (see Section OA5.1 and Table OA1 of the Online Appendix).

[6] Most manufacturing firms purchase electricity from power generating units rather than generating it in-house. Only a few manufacturing firms (27 in total, or less than 2% of the total number of manufacturers in the sample) independently operate energy sources that were regulated by the NBP. Our results hold with and without those firms (see Section 4.4.3).

manufacturing firms in the industrial, commercial, and transportation sectors since they are not major electricity consumers like manufacturing firms. With a high degree of electricity intensity, manufacturers are most exposed to the NBP shock and, hence, the most relevant subject for our study. Overall, by focusing on manufacturing firms, we can estimate the isolated effect of climate change policy on firms' financial and better capture the related transition risk for the corporate sector.

Our previous discussion suggests that the NBP led to a substantial exogenous increase in manufacturing companies' energy costs in NBP-compliant states. We hypothesize that such a shock to electricity prices, in turn, significantly impacts the capital structure decisions of the manufacturers headquartered in those states. A priori, the impact can be twofold. First, the NBP's implementation may reduce manufacturing firms' financial leverage via its impact on firms' distress risk either directly or indirectly through their operating leverage. We argue that the NBP increases the risk of financial distress for the treatment firms in NBP-compliant states because, to the extent that the program imposes additional costs on electricity consumers via increased electricity prices, it makes it more difficult for firms to service their debt obligations. Regarding operating leverage, many manufacturers face a large amount of fixed electricity costs when operating industrial motors and machinery, heating and cooling facilities, and ventilation equipment. An exogenous increase in such costs may raise their operating leverage (i.e., the proportion of fixed costs to total costs), thus increasing the likelihood and expected costs of financial distress (Mandelker and Rhee 1984, Mauer and Triantis 1994, Serfling 2016, Kahl et al. 2019). In the spirit of the trade-off theory of capital structure (Kraus and Litzenberger 1973, Bradley et al. 1984), all else being equal, firms facing greater operating leverage and thus a higher likelihood of financial distress will have an incentive to lower their financial leverage. In short, to the extent that the increased electricity input costs induced by the NBP are more fixed than variable for an average manufacturing firm in NBP-compliant regions, we expect such a firm to adopt a more conservative capital structure in response to elevated operating leverage and distress risk.

However, a competing hypothesis is possible too, based on two different mechanisms. First, the NBP-induced electricity price shock may reduce the internal cash flows of the manufacturers headquartered in NBP-compliant states. Since pecking order theory predicts that debt is the preferred source of external financing (Myers 1984, Myers and Majluf 1984), those firms are likely to cover the

NBP-induced financing deficit by levering up, leading to an increase in financial leverage. Second, and in contrast to the argument above, it is plausible that, in some manufacturing industries, electricity usage may increase with a firm's production output and thus contribute considerably to the firm's variable costs. Accordingly, the exogenous increase in electricity prices caused by the NBP's implementation may reduce the firm's operating leverage, thus lowering its likelihood of financial distress. It follows from the trade-off framework that such a firm will increase, rather than decrease, its debt ratio in response to the NBP. Overall, these arguments suggest a positive association between the NBP's implementation and corporate capital structure.

To test the above competing hypotheses, we use difference-in-differences (DID) regression analysis. As briefly mentioned above, our treated firms are those manufacturers headquartered in the 11 NBP-compliant states in 2004, while the control firms are those headquartered in several other states that did not implement the NBP. Consistent with our first hypothesis, we find that firms' book (market) financial leverage decreased by 3.3 percentage points in NBP-compliant areas, which is economically important, since it represents a decrease of 14.3% (16.5%) of the sample mean. Further results show that, after the NBP's implementation, manufacturing firms headquartered in the affected areas faced higher distress risk, which is then priced in the capital markets via an increase in firms' credit default swaps (CDS) spread and a decrease in their distance to default (DTD) and credit ratings. We also find that operating leverage increased for the treated firms, indicating that the increased electricity costs due to NBP compliance are indeed more fixed in nature, resulting in less operating flexibility for those firms. Collectively, our findings suggest that manufacturing firms' capital structure response to the NBP was guided by trade-off theory, that is, the increased operating leverage and financial distress risk caused by the NBP-induced electricity price shock lowered companies' leverage ratio, ceteris paribus.

To address the potential endogeneity relating to pre-NBP trends and reverse causality, we investigate the timing of leverage changes before and after the NBP's implementation. We find that the decrease in the treated firms' financial leverage relative to that of the control firms appears only after the NBP's adoption. To alleviate the concern that systematic differences between the treated and control firms may exist and potentially drive our results, we further use propensity score matching (PSM) and entropy balancing (EB) to define the control group. Our treated and control samples are well balanced on multiple firm and industry characteristics, and the baseline results persist when using those matched samples. In addition, we find that the NBP's effect on firm financial leverage is long-term, remaining significant and negative beyond two years post-NBP. Our main results are also robust to several additional tests, such as those controlling for potentially confounding energy industry regulations, placebo tests using pseudo-samples of treated and control firms, as well as analyses using data on plant locations rather than headquarters locations and alternative measures of financial leverage and controlling for a host of time-varying firm and state characteristics and different combinations of firm, state, industry, and year fixed effects.

Next, we explore the heterogeneity in the treatment effect and provide additional evidence of the economic mechanisms driving our finding, that is, the distress risk and operating leverage channels. First, we study firms with various degrees of electricity intensity. Since the NBP influences corporate financial decisions via an increase in electricity prices, the treatment effect should be stronger for firms operating in electricity-intensive industries and, thus, being more exposed to this program. Our results support this conjecture. In another test, we find that those firms closer to financial distress experienced a more pronounced NBP effect on their financial leverage. Firms facing more immediate threats of financial distress seemed to have stronger incentives to use debt more conservatively in response to the NBP. Similarly, we find the treatment effect to be more pronounced for manufacturers facing greater competitive pressure and having weaker market power. Firms with lower market power typically found it more difficult to pass the increased electricity costs on to their customers, which, in turn, prevented them from increasing gross income by adjusting prices.

Finally, to gain a better understanding of the NBP's effects on other corporate decisions and outcomes, we extend our analysis by examining how manufacturing firms adapted various financing policies and how the financing ecosystem responded to this regulation. First, we find that lenders charged higher interest spreads on the treated firms' borrowings, with the effect being more pronounced for public bonds. After the NBP's implementation, manufacturing firms faced higher debt costs, particularly larger bond yield spreads, and they altered their debt structure by relying more on private debt (e.g., bank loans) than public debt (e.g., bonds). Our findings are consistent with the argument that lenders price the risk of default into their lending to those borrowers with increased operating leverage and distress risk (e.g., van Binsbergen et al. 2010). Our evidence also reveals that those firms found it easier to access private debt such as bank loans, because private lenders are in a better position to lend to riskier borrowers due to their superior information acquisition and monitoring capabilities (Diamond 1984, Gilson et al. 1990, Rajan 1992, Denis and Mihov 2003). Second, we document that manufacturing firms reduced their total payouts (DeAngelo and DeAngelo 1990) and cut back on their overall investments (Asquith et al. 1994, Duchin et al. 2010) to maintain liquidity when facing more costly and limited access to external finance post-NBP. There is also evidence that firms increased their cash holdings in response to the NBP, even though the statistical significance of the result varies according to the measure of cash used. Overall, these findings are broadly consistent with the view that firms adopted conservative financial policies to manage the downside risk following the NBP's adoption. Our extended analysis shows that this regulation affected several corporate behavior and outcomes, and firms adopted a multifaceted response.

Our paper contributes to the literature in three ways. First, we find that manufacturing firms respond to the policy risk of climate change regulations by adopting more conservative financial policies. The finance literature has mainly focused on how the physical risk of climate change, such as droughts and hurricanes, influences firm financing decisions. For example, using the Federal Emergency Management Agency's natural disaster declarations summary, Elnahas et al. (2018) find that firms residing in more disaster-prone counties adopt more conservative financing policies. Dessaint and Matray (2017) show that perceived hurricane risk increases firm cash holdings in the short term, whereas Massa and Zhang (2021) find no effect of Hurricane Katrina on firms' financial structure. Ginglinger and Moreau (2019) use a unique forward-looking measure of physical climate risk and show that a firm's leverage decreases with its exposure to this physical risk.[7] Our research is different from these studies, in that we focus on the transition risk of climate change. As Krueger et al. (2020) highlight in their survey report, regulatory risk constitutes a major consideration when institutional investors

[7] A broader literature studies to what extent physical climate risk is priced in financial assets, for example, Bernstein et al. (2019), Hong et al. (2019), Kumar et al. (2019), Lin et al. (2019), Choi et al. (2020), Dong et al. (2020), Huang et al. (2020), Murfin and Spiegel (2020), Painter (2020), and Zhang and Zhu (2020). Another strand of research investigates firm values related to the disclosure of climate change risks, for example, Fernando et al. (2017), Amel-Zadeh (2019), Grewal et al. (2019), Boone and Uysal (2020), and Flammer et al. (2020).

select and engage with portfolio companies. However, to the best of our knowledge, only a few studies have examined the policy risk of climate change. Delis et al. (2020) show that banks have begun to price the policy risk of stranded fossil fuel reserves when lending to firms since the Paris Agreement of 2015. Seltzer et al. (2020) show that firms with problematic environmental profiles face lower credit ratings and higher bond yield spreads following the same agreement. Ivanov et al. (2020) document that banks adjust loan contracts to mitigate climate transition risk, with the impact concentrated among private firms, while Bartram et al. (2021) find that climate policy affects financially constrained firms' operations. In the current study, we demonstrate that climate policy risk is an essential consideration in firms' capital structure choices, as well as other financial decisions. Our research adds to the above studies by showing that policy responses to climate change have a significant impact on various corporate financing behaviors and outcomes. Our finding that manufacturing firms adapted several corporate strategies in response to the increased operating leverage and distress risk post-NBP suggests that environmental regulations could have important unintended consequences for the corporate sector.

Second, we add to a related strand of the economics and management literature focusing on the micro effects of climate regulations. Environmental economists have examined how emission abatement policies impact firm productivity, employment, accounting, and stock market performance (Anger and Oberndorfer 2008, Veith et al. 2009, Commins et al. 2011, Bushnell et al. 2013, Oestreich and Tsiakas 2015, Dardati and Riutort 2016, Kang and Letourneau 2016, Marin et al. 2018), political strategy (Patnaik 2020), as well as expenditures on environmental innovation (Porter 1991, Porter and Van der Linde 1995, Borghesi et al. 2015, Calel and Dechezlepretre 2016, Cainelli et al. 2020). Only two recent studies, by Ivanov et al. (2020) and Bartram et al. (2021), examine the effects of cap-and-trade policy on bank lending and firm resource relocation, respectively. We are aware of no study that asks how firms adjust their financing package in response to the transition risk associated with climate change measures. The current study fills this gap.

Third, we provide novel evidence on the link between an input cost shock and corporate capital structure. The literature has largely focused on the impact of input costs on other firm policies, such as vertical integration, risk management, and risk taking (Fan 2000, Rampini et al. 2014, Gupta and Krishnamurti 2018). Our study sheds new light on how a shock to electricity prices affects a firm's debt policy via the operating leverage and distress risk channels. We show that manufacturing firms, on average, adopt more conservative financial policies to mitigate the elevated operating leverage and financial distress costs resulting from higher electricity prices. Our study also reveals that the impact of energy input costs on corporate financial decisions is conditional on the nature of such costs, that is, whether they are more fixed than variable and how they contribute to firm operating flexibility. Taken together, our findings suggest that research on how input costs affect firm policies should consider the nature of such costs.

We organize the remainder of this paper as follows. Section 2 provides the background of the NBP. Section 3 describes the sample, data, and methodology. Section 4 reports our main empirical results on capital structure and the economic mechanisms, before summarizing the results of various robustness tests (we present more details in our Online Appendix). Section 5 provides an additional analysis of other firm responses to the NBP. Section 6 concludes the paper.

2. The NBP

2.1. Institutional Background

Since the 1960s, the United States has enacted several legislations to abate greenhouse gas emissions and mitigate public health issues related to ground-level ozone. These legislations include the Clean Air Act (CAA) of 1963, the Clean Air Act Extension of 1970, and the Clean Air Act Amendments of 1977 and 1990, all of which formed the basis for national ambient air quality standards (NAAQS) that regulate NO_x emissions, among other harmful air pollutants.[8] The CAA established a federal–state partnership to tackle air pollution and authorized the EPA to formulate the NAAQS. States implemented the NAAQS by developing state implementation plans (SIPs) subject to the EPA's approval. If the EPA found a state's SIP inadequate, it would require the state to revise the plan to address the inadequacy.

Since the winds on the North American continent blow primarily from the southwest to the northeast, the interstate transmission of air pollutants became a critical issue for many eastern states,

[8] Ground-level ozone is the result of a complex chemical reaction among NO_x, other compounds, sunlight, and heat. Unlike the ozone in the upper atmosphere, ground-level ozone, a major component of so-called smog, presents a health hazard by irritating airways, causing breathing difficulties, and damaging lungs. Sources of NO_x include stationary units (e.g., power plants and boilers) and mobile units (e.g., trucks and planes).

which often found it difficult to attain the ozone NAAQS. In 1990, the U.S. Congress set up the Ozone Transport Commission (OTC), which was responsible for assessing the extent of ozone transport across state borders and recommending measures to the EPA to tackle this problem. Consequently, Congress amended two provisions of the CAA to mitigate interstate air pollutant transport. Section 110(a)(2)(D) established the "good neighbor" provision, requesting a state's SIP to restrict emissions and preventing it from contributing to the nonattainment of its neighboring states. Section 126 allowed "downwind" states to petition the EPA to impose restrictions on the emission sources in "upwind" states.

In 1997, the EPA began to tighten the ozone NAAQS. Against this backdrop, an initial version of the NBP was introduced in 1999 under the name OTC NO_x Budget Program (OTC NBP), covering the District of Columbia (DC) and, initially, 11 northeastern states, namely, Connecticut, Delaware, Maine, Maryland, Massachusetts, New Hampshire, New Jersey, New York, Pennsylvania, Rhode Island, and Vermont. This tightening of regulation, however, instigated a series of major legal battles between the EPA and some of the affected states, ultimately leading to an expanded and more stringent version of the NBP. In what follows, we provide a detailed timeline of the events that gave rise to the introduction and implementation of this program.

In August 1997, several of the northeastern states mentioned above petitioned the EPA under Section 126 of the CAA that their nonattainment was due to the interstate transport of emissions from upwind states in the Midwest and Southeast. Accordingly, in October 1998, the EPA instituted the NO_x SIP Call, requiring 22 states, namely, eight of the 11 northeastern states listed above (except for Maine, New Hampshire, and Vermont) and 14 new upwind states, plus DC, to revise their SIPs to address the ozone transport issue. The 14 upwind states affected by this call were Alabama, Georgia, Illinois, Indiana, Kentucky, Michigan, Missouri, North Carolina, Ohio, South Carolina, Tennessee, Virginia, Wisconsin, and West Virginia. In light of this more stringent regulation, the newly affected states challenged the EPA in the DC Circuit in May 1999, known as the *Michigan et al. v. EPA* lawsuit. In a ruling on March 3, 2000, the Court of Appeals for the DC Circuit favored the EPA. As the result of this court ruling, 11 upwind states, that is, the 14 states listed above except Georgia, Missouri, and Wisconsin, had to abide by the NO_x SIP Call from 2004.[9,10]

The NBP sought to reduce regional NO_x emissions and ozone transportation by operating a conventional cap-and-trade program. States included in the program were charged with establishing regulations, identifying sources of emissions, and ensuring compliance, while the EPA was responsible for approving the state-level regulations, as well as tracking allowances and emissions. The EPA set a region-wide cap on NO_x emissions from May 1 to September 30 each year (i.e., the ozone season). Emission permits (i.e., NO_x allowances) were allocated to each state subject to the cap. NBP-compliant states had the discretion to distribute their permits to emission sources for free, and these permits could then be traded on open allowance markets or banked for future use. At the end of each compliance period, regulated resources had to submit sufficient emission allowances to match their actual NO_x emissions. Sources that failed to comply would receive a penalty in the form of a deduction in emission allowances in the next compliance period. The NBP operated until 2008, when it was replaced by the Clean Air Interstate Rule (CAIR) NO_x ozone season program. Figure 1 depicts the NBP-affected regions.

[Insert Figure 1 about here]

2.2. Effect of the NBP on Manufacturing Firms

The NBP reduced NO_x emissions dramatically in the regulated areas, affecting more than 2,500 sources, including electric generating units and large industrial boilers and turbines (EPA 2008). The affected emission sources could choose from a variety of options to comply with the regulation, including 1) directly purchasing emission permits from the allowance markets, 2) reducing production during the compliance period, 3) modifying the combustion process to prevent NO_x formation, 4) optimizing

[9] Major legal battles between some of the 11 states and the EPA continued until 2004. Eventually, those states began compliance on May 31, 2004, a month later than anticipated owing to the ongoing contentious litigations. There were a few exceptions, however. Certain areas of Missouri only began compliance from May 1, 2007, while Georgia was removed from the NO_x SIP Call on June 8, 2007, due to a petition.

[10] Eight northeastern states, namely, Connecticut, Delaware, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island, and DC were also covered by an earlier implementation of the NBP in 2003. This was introduced to replace the original OTC NBP, implemented from 1999 to 2002, which was much less stringent than the NBP and had a milder policy impact (Deschênes et al. 2017). Hence, we do not consider those states in our analysis and focus instead on the 11 states that were included in the 2004 implementation of the NBP for the first time following the court ruling.

boiler operation to minimize NO_x generation, and/or 5) using add-on control technologies (Fowlie 2010).

Compliance with the NBP was costly. For example, it cost a coal generator approximately $40 million to install selective catalytic reduction technology to reduce up to 90% of emissions (Linn 2010). In 2007, units with retrofits (i.e., those that optimize their operations, adopt new technologies, or both) accounted for 70% of the total number of emission sources (EPA 2008), indicating that many utilities had made costly capital investments for compliance. Palmer et al. (2001) projected the additional annual costs of the NBP to be $2.15 billion for the regulated units. Using allowance prices, Deschênes et al. (2017) estimate the aggregate compliance costs for NBP-regulated emission sources to be about $1.1 billion per year, making their total bill as high as $4.8 billion.

Both ex ante and ex post evidence suggests that the regulated units affected by the NBP were able to pass a substantial proportion of the compliance costs on to their customers (including manufacturing firms, the focus of our study). An early study by the EPA (1998) estimated that the regulation could lead to a 1.6% increase in electricity prices, while Platts Research and Consulting (2003) predicted that electricity prices could rise by between 2.47% and 7.41% (i.e., an increase between $1 and $3 per megawatt-hour). Recently, Curtis (2018) has found that electricity prices in NBP-compliant states rose by between 3.5% and 5.9% following the program's implementation.[11] Using utility-level data, we document in Table OA1 in our Online Appendix that the NBP's adoption raised electricity prices by between 9.1% and 9.8% in the 11 upwind states under examination. In summary, the NBP's implementation appears to have dealt a significant shock to manufacturers' input cost structure.

2.3. Other Electricity Policies

In addition to the NBP, several other state-level regulations may have affected electricity production, pricing, and consumption, namely, RPS laws, EMR, EERS, NM, GPP programs, and PBFs (e.g., Greenstone and Nath 2020). First, RPS laws, adopted by around 30 U.S. states in the 1990s and

[11] Several studies also show that power generating utilities see their performance and stock market value increase after the implementation of cap-and-trade programs, suggesting these power utilities not only can pass their substantial compliance costs on to customers, but also can benefit from overcompensation for these costs (Veith et al. 2009, Bushnell et al. 2013, Marin et al. 2018).

2000s to reduce CO_2 emissions in the electricity sector, require retail electricity suppliers to meet a certain fraction of their electricity demand with renewable energy. The second program, EMR, was employed by 17 states between 1998 and 2002 to introduce competition to retail electricity markets by allowing consumers to purchase electricity from suppliers beyond local utilities. Third, adopted by more than 30 states between 1999 and 2018, EERS aim to reduce energy consumption and carbon emissions by requiring utilities to achieve specified customer energy savings targets through end-use efficiency programs. The fourth policy, NM, enacted by most states from 1982 to 2017, compensates customers for the excess electricity that they generate (e.g., via solar power) and add to the grid. Fifth, GPP programs, introduced by several states over the period 2001–2015, mandate government-affiliated customers to purchase a certain amount of electricity from renewable power sources. Finally, implemented by 21 states from 1996 to 2007, PBFs impose a small surcharge on retail electricity prices to fund various green programs, including energy efficiency and education and energy support for low-income households. Table OA2 of our Online Appendix shows the adoption timelines of these policies by U.S. states and their potential overlap with the NBP.[12]

Although these programs may have affected electricity prices and hence manufacturing firms as electricity consumers, when we compare them with the NBP, we find that the NBP provides arguably the most suitable quasi-natural experiment for our empirical analysis. As articulated in the introduction, to tackle the question of how climate policy risk affects corporate financing decisions, we require a (state-level) policy shock based on the adoption of an environmental regulation that has important policy implications for emission control and reduction, has a strong impact on the wider corporate sector beyond the directly regulated sources, and preferably is the consequence of an (unexpected) court ruling. The last feature is useful for our identification strategy. Our policy comparison shows that the NBP satisfies all these important criteria and therefore provides an opportune setting to answer our research question (see Online Appendix OA3 for more details and Table OA3 for a summary of the

[12] We retrieve details on RPS laws from Greenstone and Nath (2020). We collect data on EMR from Fabrizio et al. (2007) for the period up to 1999 and from Craig and Savage (2013) for the period after 1999. We retrieve details on EERS from the American Council for an Energy-Efficient Economy's State and Local Policy Database and information on NM, GPP programs, and PBFs from the North Carolina Clean Energy Technology Center's Database of State Incentives for Renewables & Efficiency.

discussion.[13] While we do not use these six electricity sector programs in our analysis, in Section 3.3 we address the concern that these policies may confound our main finding.

3. Data and Methods

3.1. Sample Selection

The NBP's adoption in 2004 led to a shock to the electricity costs of the manufacturing firms headquartered in the 11 upwind states in the Midwest and Southeast (detailed in Section 2.1). These firms constitute our treatment group. We exclude the eight northeastern states plus DC from our sample because, as mentioned in footnote 10, they were much less affected by the NBP's implementation, having already been included in the earlier and less stringent OTC NBP (effective during 1999–2002).

There are two reasons for our focus on manufacturing firms. First, empirical studies examining the micro impact of climate policies on energy costs typically focus on firms operating in manufacturing industries (e.g., Linn 2009, Martin et al. 2014, Aldy and Pizer 2015, Curtis 2018), because, compared to non-manufacturing companies, manufacturers are fundamentally different in terms of their labor–capital ratios, energy consumption, and pollution intensity (Kahn and Mansur 2013). Second, manufacturing industries typically exhibit higher levels of electricity intensity (Horowitz 2007, Lund 2007, Kim 2015) and, thus, greater exposure to electricity price shocks. In the bar chart of Figure 2, we rank manufacturing and non-manufacturing industries in the industrial sector and the commercial and transportation sectors by their electricity intensities. Our analysis shows that, with the only exception of metal-based durables, all the manufacturing industries are more electricity intensive than the non-manufacturing industries and the commercial and transportation sectors. To the extent that the NBP affects firms via a significant increase in electricity prices, manufacturers should be among those most affected by this regulation and, hence, the most relevant subject for our study.

To form our control group, we exclude three states covered by the independent system operator (ISO) of New England (ISO-NE),[14] namely, Vermont, New Hampshire, and Maine, and seven states

[13] The discussion in Online Appendix OA3 also reveals that, among these policies, RPS programs have certain merits that make them a potentially useful setting for future studies.
[14] An ISO operates the power system and sets electricity prices for a designated area covering one or more states. ISO-NE is a regional transmission organization that oversees the operation of the power system in the New England region.

covered by the Midcontinent ISO (MISO),[15] namely, Montana, North Dakota, South Dakota, Iowa, Missouri, Minnesota, and Wisconsin. The manufacturing companies in those states participate in day-ahead markets operated by either the MISO or the ISO-NE.[16] Since the MISO and ISO-NE are NBP compliant, manufacturers operating in those states may still have experienced higher electricity prices because of the NBP's implementation, even though they are not located in the 11 treated states.

In summary, we identify 11 treated states and 20 control states.[17] Online Appendix OA4 provides relevant details on the U.S. electricity markets and explains why an increase in electricity prices in one area may affect the corresponding ISO region. Since the NBP was replaced by the CAIR in 2009, our sample period ends in 2008, four years after the program's implementation in 2004. We therefore set our test window between 2000 and 2008, consistent with recent research on the NBP (Deschênes et al. 2017, Curtis 2018, Shapiro and Walker 2018).

To obtain firm-level data from 2000 to 2008, we use the Compustat and Center for Research in Security Prices (CRSP) databases subject to several criteria below. The baseline sample includes the (listed) manufacturing firms (i.e., firms with Standard Industrial Classification, or SIC, codes between 2000 and 3999) headquartered in the treated or control states. An important limitation of Compustat is that it does not report a firm's historical headquarters locations. Using Compustat data on the latest headquarters locations may bias our estimates by inaccurately identifying treatment and control firms and potentially ignoring headquarter relocation risks. To alleviate this issue, we follow Bai et al. (2020) and collect historical data on headquarters locations from the CRSP–Compustat merged database.[18] Finally, we ensure that those manufacturing firms included in our sample have no missing values for the main variables of interest. Our final sample consists of 12,454 firm–year observations for 1,974

[15] The MISO has operated the power system in the Midwest of the United States and Manitoba, Canada, since 1998 and covered additional southern U.S. regions since 2014.
[16] Day-ahead electricity markets enable market participants to trade electricity one day before the operating day at the wholesale level. These markets help reduce electricity price volatility.
[17] During the sample period, there was no firms headquartered in the state of Alaska.
[18] Since Compustat switched to Xpressfeed files in 2007, the CRSP–Compustat merged database has begun to report header historical data in the COMPHIST file rather than the CST_HIST file. We use CST_HIST and COMPHIST files to obtain historical data on headquarters locations for the 2000–2006 and 2007–2008 periods, respectively.

manufacturers, of which 524 firms are headquartered in the treated states and 1,589 firms in the control states.[19]

3.2. Empirical Methods

We employ the DID approach to investigate the impact of the NBP's implementation on the financial leverage of the manufacturing firms headquartered in the compliant states. Specifically, we estimate the following model:

$$Debt_{i,t} = \beta NBP_{i,t} \times Post_t + \delta X_{i,t} + \theta Z_{s,t} + \nu_i + \phi_s + \omega_t + \varepsilon_{i,t}, \quad (1)$$

where i, s, and t denote firms, states, and years, respectively; $Debt_{i,t}$ is the debt ratio of firm i in year t; $NBP_{i,t}$ is an indicator equal to one if a firm is headquartered in a treated state in year t, and zero otherwise; $Post_t$ is an indicator set to one for the year 2004 and later, and zero for the year 2003 and before; $X_{i,t}$ and $Z_{s,t}$ are a set of time-varying firm- and state-level control variables, respectively; and $\varepsilon_{i,t}$ is the error term. The regression model also includes firm fixed effects ν_i, state fixed effects ϕ_s, and year fixed effects ω_t. The firm fixed effects remove unobserved time-invariant firm-level factors that impact leverage ratios (Lemmon et al. 2008). The state fixed effects account for time-invariant state-level characteristics, while the year fixed effects control for nationwide economic conditions. Standard errors are clustered at the firm level.[20]

Regarding the dependent variable, we adopt two measures of the debt ratio, namely, book leverage and market leverage, which are commonly used in the capital structure literature (Agrawal and Matsa 2013, Serfling 2016). We also employ alternative measures of leverage in our robustness checks (see Table OA13 in the Online Appendix). In terms of firm-level control variables, we follow Frank and Goyal (2009) and Heider and Ljungqvist (2015) and include profitability (*ROA*), growth opportunities (*Market-to-Book*), firm size (*Log Total Assets*), and tangibility (*Fixed Assets*). We also include common dividends payment (*Dividend Payer*) as a dummy variable to control for the degree of

[19] A very small number of firms may appear in both the treated and control groups during the sample period, because they relocated their headquarters from treated to control regions or vice versa. We address relocation risks in a robustness check (see Section 4.4.3).

[20] As mentioned in Section 3.1, there are 11 treated and 20 control states in our final sample. Since Angrist and Pischke (2008) caution against using clustered standard errors with fewer than 42 clusters, we do not cluster standard errors at the state level, that is, the level at which the interaction term of interest, $NBP_{i,t} \times Post_t$, is defined. Instead, we estimate standard errors clustered at the firm level. In our additional analysis (Table OA4 of the Online Appendix), however, we find that the results hold when we cluster standard errors at the state, NBP region–industry, or state–industry level.

financial constraint (Serfling 2016, Klasa et al. 2018). To control for state-level political and economic conditions that may affect both capital structure and the adoption of the regulation, as Serfling (2016) does, we add to our model the state gross domestic product (GDP) growth rate (*State GDP Growth*) and political balance ratio (*Political Balance*). Furthermore, we include the default spread (*Default Spread*) to account for credit market conditions (Heider and Ljungqvist 2015). Online Appendix OA1 provides the definitions of all the variables and the details of their construction.

3.3. Endogeneity Concerns about the Implementation of the NBP

A key assumption of the DID approach we use is that the NBP's implementation is exogenous to manufacturing firms' capital structure decisions. In what follows, we discuss and address several concerns relating to the potential endogeneity of this program. First, the objective of the NBP was to alleviate interstate air pollutant emission transport, that is, reduce the NO_x emissions of the 11 upwind states, thus helping the downwind states address nonattainment issues. By design, the regulated power units targeted by the NBP were mostly electric utilities. We are not aware of any report suggesting that the NBP was designed and implemented to directly influence the operations of the manufacturing firms in compliant states, namely, the treatment group in our empirical setting.

The implementation of the NBP is unlikely to be affected by lobbying activities. As detailed in Section 2.1, the NBP's adoption was the consequence of a court ruling, that is, a judicial decision rather than a legislative or regulatory one, and such a judicial decision is largely based on the merits of the specific case rather than political pressure or lobbying (Serfling 2016, Klasa et al. 2018). It is important to further note that the ruling by the Court of Appeals for the DC Circuit in favor of the EPA was arguably unexpected, because, in a previous and separate court case (i.e., *State of Virginia v. EPA*), the court ruled against the EPA.

Still, to empirically address the concern about the endogeneity of the NBP, we investigate whether the likelihood of the NBP's adoption can be determined by certain state-level factors. In Table OA5 of the Online Appendix, we find no evidence that the NBP's implementation is driven by state-level economic or political factors or by firm financial leverage aggregated at the state level. These findings provide further assurances that the NBP's adoption is likely to be exogenous to the capital structure decisions of manufacturing firms, the subject of our study.

Due to the lag between the NBP's implementation in 2004 and the court ruling in 2000 (*Michigan et al. v. EPA*), the NBP-induced increase in electricity costs might have been anticipated by manufacturers, thus raising some concerns about our identification strategies. However, as discussed in Section 2.1, after the 2000 court decision, uncertainties regarding the NBP's implementation did not resolve entirely until 2004, due to several legal battles between some affected states and the EPA. In short, whether the NBP would be eventually adopted remained in question until May 2004. Meanwhile, the affected power utilities only had to comply with the program from 2004 onward, with certain compliance options available only in the implementation period (e.g., purchasing emission allowances). Therefore, the effects of the NBP, especially those regarding compliance and electricity prices, are likely to be nonsignificant prior to the implementation year of 2004. As we further show in our dynamic analysis in Section 4.4.2, the NBP's effect only manifests itself post-2004. Thus, although we cannot entirely rule out the possibility that a minority of the affected sources may have made initial responses in the run-up to 2004, this is unlikely to be significant enough to affect our statistical inferences.

Another endogeneity concern about our analysis is the presence of several energy industry regulations other than the NBP that may impact electricity consumption and pricing and hence confound our empirical findings. As discussed in Section 2.3, over our sample period, six potentially confounding electricity policies were implemented, namely, RPS laws, EMR, EERS, NM, GPP programs, and PBFs. To mitigate this concern, we follow Greenstone and Nath (2020) and control for RPS laws, EMR, EERS, NM, and PBFs. Since GPP programs were adopted by six northeastern (downwind) states from 2001 to 2015, which are excluded from our treated and control samples, these programs are unlikely to confound our analysis. The results reported in Online Appendix OA5.4 and Table OA6 show that our statistical inferences are not affected by these policies. Overall, the above discussions and analysis provide further support for the conclusion that our statistical inferences are unlikely to be affected by endogeneity concerns.

4. Empirical Results

4.1. Summary Statistics

Table 1 presents the summary statistics of the variables used in our analysis, including the additional dependent variables used in robustness checks. All the variables, except for dummy and macroeconomic ones, are winsorized at the first and 99th percentiles to alleviate the potential impact of outliers. Our measures of financial leverage are censored at zero and one. We find that the mean values of book leverage and market leverage are 23.1% and 20.0%, respectively, consistent with prior studies.[21]

[Insert Table 1 about here]

4.2. Baseline Results

Table 2 presents the baseline regression estimates of the NBP's impact on manufacturing firms' financial leverage. Columns (1) and (4) and (5) and (6) report the results estimated based on the full sample for book leverage and market leverage, respectively. Columns (1) and (5) include only firm, state, and year fixed effects and no controls. In columns (2) and (6), we add four standard firm-level control variables used in the capital structure literature, namely, firm size (*Log Total Assets*), growth opportunities (*Market-to-Book*), profitability (*ROA*), and tangibility (*Fixed Assets*). Columns (3) and (7) further control for dividend-paying status (*Dividend Payer*) and the default spread ratio (*Default Spread*). Lastly, in columns (4) and (8), we add to our model the state GDP growth rate (*State GDP Growth*) and state political balance ratio (*Political Balance*). The results from all model specifications consistently show significant and negative coefficients on $NBP \times Post$, suggesting a negative impact of the NBP's implementation on firm financial leverage.[22]

Without any time-varying firm- and state-level controls, the results in column (1) of Table 2 show that, following the adoption of the NBP, book leverage declined by 3.4 percentage points.

[21] The mean values of the return on assets and the market-to-book ratio are −31.2% and 4.124, respectively, which, in absolute values, are greater than those reported in prior research. However, these statistics are consistent with the fact that, during 2000 and 2008, some manufacturing firms experienced substantial losses. We find that around 12% of the firm–year observations in our sample (i.e., 1,460 of 12,454) have negative equity, similar to the findings of Kahle and Stulz (2017) and Denis and McKeon (2018). In untabulated tests, we show that the regression results do not change qualitatively after dropping those firm–year observations.

[22] Besides multivariate analysis, we also perform univariate analysis to compare the means of financial leverage between the treatment and control groups pre- and post-NBP. Panels A and B of Table OA7 in the Online Appendix report the results for book leverage and market leverage, respectively. Consistent with our main findings in Table 2, we find that manufacturers reduced their financial leverage following the NBP's adoption.

Compared to the average book leverage ratio of 0.231, this change represents a decrease of 14.7% (= 0.034/0.231). After including three sets of control variables in columns (2) to (4), both the economic and statistical significance of the coefficients on *NBP* × *Post* persists. Specifically, in column (4), with the full set of controls, book leverage decreased significantly by 3.3 percentage points, or 14.3%, relative to its sample mean of 0.231. The results in columns (5) to (8) show a similar pattern. For instance, in column (8), the coefficient on *NBP* × *Post* suggests that market leverage declined by 3.3 percentage points, or 16.5%, relative to its sample mean of 0.200. The economic magnitudes of the NBP's impact on financial leverage are comparable to those documented in recent empirical studies on the effect of other policy shocks. For instance, Li et al. (2016) report that, following the adoption of anti-recharacterization laws, firms raised their debt ratios by 13%. Serfling (2016) documents a 4.5% decrease in book leverage following the (state-level) passage of employment protection laws. Klasa et al. (2018) show that book (market) leverage decreased by 19.1% (14.6%) in response to the adoption of the inevitable disclosure doctrine.

Overall, the negative effect of the NBP's implementation on capital structure is robust to conventional measures of financial leverage and different model specifications. This result is consistent with the hypothesis that the NBP's adoption induced manufacturers to reduce their debt ratios.

[Insert Table 2 about here]

4.3. Mechanisms of the NBP's Effect

We argue that the NBP affected firms' financing decisions by directly and indirectly increasing financial distress through raising operating leverage. To validate these mechanisms, we examine the impact of the NBP's implementation on firms' distress risk and operating leverage.

In Panel A of Table 3, we first study the NBP's effect on firms' distress risk. We hypothesize that the NBP increased the risk of financial distress for the treatment firms in NBP-compliant states. To the extent that the program imposed additional costs on electricity consumers via increased electricity prices, it made it more difficult for firms to service their debt obligations, thus increasing their distress risk. To test this prediction, in column (1) of Panel A, we examine the impact of the NBP on firms' DTD, one of the most widely used measures of financial distress in the literature (Bharath and Shumway

2008). Columns (2) and (3) present the results of robustness tests using two alternative measures of distress risk: credit ratings and the CDS spread, where the data are more limited.[23] We estimate DID models of the DTD, credit ratings, and the CDS spread with firm, state, and year fixed effects, as in our study of capital structure. The choice and measures of the control variables follow previous research on the determinants of the DTD (e.g., Goyal and Wang 2013, Brogaard et al. 2017), credit risk (e.g., Huang et al. 2016, Jiang et al. 2018), and the CDS spread (e.g., Pan et al. 2018). The results across all models in Panel A of Table 3 (columns (1) to (3)) are consistent with our prediction that the degree of financial distress increased post-NBP. The NBP's impact on distress risk was statistically significant and economically relevant. Specifically, post-NBP, firms' implied default probability and CDS spread increased by approximately 27.7% (= 0.018/0.065)[24] and 58.1% (= 111.173/191.492), respectively, relative to the sample means. Following the NBP's implementation, manufacturers' credit ratings dropped by 0.311, which is comparable to the economic impact of several other determinants of credit ratings analyzed in recent studies (Jiang et al. 2018, Badertscher et al. 2019).

Second, we examine how the NBP impacts firm operating leverage. Based on the recent literature (Kulchania and Thomas 2017, Ljungqvist et al. 2017, Kim et al. 2018, Albuquerque et al. 2019, Chen et al. 2019, Gamba and Saretto 2019, Carter et al. 2021, Dou et al. 2021), we identify the three most widely used measures of operating leverage, developed by 1) Mandelker and Rhee (1984), 2) Kahl et al. (2019), and 3) Novy-Marx (2011), which, for convenience, we label, respectively, as *MROL*, *KLNOL*, and *NMOL*. The measure *MROL* reflects the elasticity of a firm's operating income after depreciation to its sales, *KLNOL* captures the sensitivity of the innovations in the growth rate of operating costs to the innovations in the growth rate of sales, and *NMOL* is defined as the ratio of the cost of goods sold and selling, general, and administrative expenses to total assets. Online Appendix OA2 provides details on how we calculate these three measures. Next, we fit these measures to the DID framework as specified in Eq. (1). Panel B of Table 3 presents the regression estimates of the NBP's

[23] Data on credit ratings and CDS spreads are from the Compustat and Markit CDS databases, respectively. In the credit rating and CDS spread regressions, approximately 80% and 95% the firm–year observations were dropped, respectively, due to missing data after merging with our Compustat sample. By using the DTD measure, we retain more than half of the original sample.

[24] As in Vassalou and Xing (2004) and Bharath and Shumway (2008), this impact is calculated using the formula *Implied Default Probability* = *N*(–*DTD*), where *N* denotes the cumulative standard normal distribution function.

effect on operating leverage. Across all models, the results show a significant and positive treatment effect, which is consistent with our operating leverage channel. In terms of economic magnitude, after the adoption of the NBP, *MROL*, *KLNOL*, and *NMOL* increased by 27.0% (= 0.463/1.716), 9.9% (= 0.036/0.363), and 11.4% (= 0.155/1.365), respectively, relative to their sample means. Taken together, these results suggest that the impact of the NBP on operating leverage is statistically and economically significant.

[Insert Table 3 about here]

4.4. Robustness Tests

4.4.1. Covariate Balance between the Treated and Control Groups

To alleviate the concern that systematic differences in the characteristics of the control and treated firms may be driving their differences in financial leverage, we use PSM and EB to identify the control sample. We begin by retaining all firm–year observations in 2003 (the year before the NBP's implementation) with data available. Then, we use a logit model to estimate the propensity score of being a treated firm, that is, we regress the treatment indicator (*NBP*) on relevant firm-level covariates (i.e., *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, and *Dividend Payer*). To achieve industry balance between the treatment and control groups, in each manufacturing industry cluster we match a treated firm to a control firm using a caliper of 0.005 without replacement. The industry classification follows the U.S. Energy Information Administration (EIA) data,[25] as shown in Figure 2.[26] Our matching process yields a sample of 261 pairs of treated and control firms. In Panel A of Table OA8 in the Online Appendix, we compare the means of the propensity scores as well as the firm covariates between the two groups of firms in our propensity score matched sample. We find no significant difference in the characteristics between these firms, suggesting that the covariate balance condition has been satisfied, in addition to industry balance, as imposed by the design of our matching algorithm. In columns (1) and (2) of Table 4, we estimate our

[25] To match the EIA data with our Compustat sample, we use the North American Industry Classification System codes provided by the Manufacturing Energy Consumption Survey for the following industries: food (311), paper (322), refining (324), bulk chemical (325), cement (3273), glass (3272), iron and steel (3311 and 3312, respectively), aluminum (3313), metal-based durables (332, 333, 334, 335, and 336), and other manufacturing industries.

[26] Our results persist if we use the two-digit SIC industry classification (see Panel B of Table OA9 in the Online Appendix).

DID model for the propensity score matched sample. Consistent with the baseline results, we continue to observe the NBP had a significant and negative effect on firm leverage.

We then used EB in a multivariate matching approach, to achieve balance on the higher moments of the covariates and to allow for nonlinear relations (Hainmueller 2012, King and Nielsen 2019). As in the PSM analysis above, we begin with all firm–year observations with data available for 2003. We then use EB to match treated firms to control firms and assign weights to the latter group based on the first two moments (i.e., the mean and variance) of the firm-level covariates. In a robustness check, we also match on the first moment or on all of the first three moments (i.e., the mean, variance, and skewness) and find similar results (see Panel A, Table OA9, in the Online Appendix). To further ensure industry balance, we adopt the same algorithm as in the PSM analysis; that is, we perform EB for each manufacturing industry cluster.[27] In Panel B of Table OA8 in the Online Appendix, we compare the mean and variance of the firm characteristics across the treatment and control groups. The results suggest that entropy balance has been achieved. In columns (3) and (4) of Table 4, we report the DID regression results for the EB sample. We continue to find a significant and negative NBP effect, in line with our baseline results.

Overall, we conclude that our main finding continues to hold after controlling for both covariate balance and industry balance and is robust to two common matching methods, PSM and EB.

[Insert Table 4 about here]

4.4.2. Dynamic Effects of the NBP's Implementation on Capital Structure

To alleviate the concern about reverse causality and to validate the parallel trends assumption required for the DID approach, we follow Bertrand and Mullainathan (2003) and examine the dynamic effects of the NBP on financial leverage. If reverse causality affects our main results or if pre-treatment trends exist (i.e., a violation of the parallel trends assumption), we should observe a significant change in the leverage ratios even before the implementation of the NBP.

[27] Again, our results persist if we use the two-digit SIC industry classification (see Panel B of Table OA9 in the Online Appendix).

In Table 5, we replace the *Post* indicator with six dummy variables, namely, $Before^{-3}$, $Before^{-2}$, $Before^{-1}$, $After^{0}$, $After^{+1}$, and $After^{2+}$, to investigate the effects of the NBP before and after its implementation. These six indicators are set to one for the third year before the NBP's implementation (i.e., the year 2001), the second year before the NBP's implementation (i.e., 2002), the first year before the NBP's implementation (i.e., 2003), the NBP's implementation year of 2004, the first year after the NBP's implementation (i.e., 2005), and two years or more after the NBP's implementation (i.e., 2006 and later), respectively, and zero otherwise.

The results show that the coefficients on $NBP \times Before^{-3}$, $NBP \times Before^{-2}$, and $NBP \times Before^{-1}$ in both the book and market leverage regressions are statistically insignificant, verifying the validity of the parallel trends assumption and suggesting that reverse causality is unlikely to drive our results. The coefficients on $NBP \times After^{0}$, $NBP \times After^{+1}$, and $NBP \times After^{2+}$ are significant and negative, consistent with our baseline results. The effect of the NBP's implementation on financial leverage appears to be persistent, since the coefficient on $NBP \times After^{2+}$ remains economically and statistically significant.

[Insert Table 5 about here]

4.4.3. Additional Robustness Checks

In this section, we summarize a battery of additional robustness tests. For brevity, we report these results in Tables OA10 to OA16 of Online Appendix OA5. Overall, our baseline findings are robust to 1) a placebo test using a sample of firms in non-manufacturing industries, which have lower degrees of electricity intensity and, hence, are less exposed to the NBP (Table OA10), and a placebo test using pseudo-treated and pseudo-control groups (randomly assigned) to rule out the concern that the treatment effect is due to unobserved shocks (Table OA11); 2) analysis using plant locations (Table OA12); 3) models using alternative leverage measures (Table OA13); 4) tests controlling for alternative firm responses, namely, cash holdings, the payout ratio, total investment, and the number of employees, that is, firm investment in employment (see Table OA14); 5) an augmented model with state × industry and industry × year fixed effects to further control, respectively, for time-invariant characteristics across state–industries and time-variant industry-level factors that may drive our results (Table OA15); 6) analysis without the 2007–2008 period, to alleviate the impact of the global financial crisis (Table

OA16); 7) a test where manufacturing firms (27 in total) with in-house NO_x emission sources, which were directly regulated by the NBP, were dropped (Table OA16); 8) analysis considering firm relocating risks where firms that relocated their headquarters to another state during the sample period were dropped (Table OA16); 9) tests focusing on firm–years with limited international operations (Klasa et al. 2018), to ensure that manufacturers' operations were concentrated around their headquarters locations and hence more exposed to the NBP-induced electricity price shock (Table OA16); 10) analysis based on a smaller sample without firms headquartered in Michigan or Alabama, since these two states were only partially covered by the NBP in 2004 (Table OA16); 11) tests without firms headquartered in the states of California or Texas, which constitute almost half of our firm–year observations (5,680 of 12,454), to address the concern that these two non-NBP states alone might be driving our main finding (Table OA16); and 12) analysis without the event year 2004, given the fact that the NBP was implemented in the treated states from May 31, 2004, that is, in the middle of the event year, which may have confounded the treatment effect (Crane and Koch 2018) (Table OA16).

4.5. Cross-Sectional Analysis

In this section, we examine possible cross-sectional heterogeneity in the NBP's effects and provide further evidence on the economic channels based on distress risk and operating leverage. Our mediating variables include the degree of electricity intensity, as well as the severity of financial distress and product market competitive pressure. We estimate difference-in-difference-in-differences (DDD) models with a triple interaction term between *NBP* × *Post* and the mediating variable of interest. An important advantage of the DDD model specification over the previous DID approach is that we can include state × year fixed effects, thus further alleviating the concern that omitted state-level time-varying factors may be confounding our results.

4.5.1. Electricity Intensity and the NBP's Effect

Our findings suggest that the NBP-induced electricity price shock led firms to adopt more conservative debt policies, via the distress risk and operating leverage channels. A natural question to ask is whether more electricity-intensive firms adjusted their capital structure more dramatically. Because the NBP affected manufacturing firms as electricity consumers, we expect its effect to have been stronger for firms with a higher degree of electricity intensity and hence greater exposure to this program. To measure electricity intensity, we use data from the Annual Energy Outlook reports prepared by the U.S. EIA. For each manufacturing industry, as shown in Figure 2, we follow Abeberese (2017) and define our electricity intensity measure as the ratio of the quantity of purchased electricity (measured in trillions of British thermal units) to the value of total shipments (measured in billions of dollars). This electricity intensity measure captures the degree of exposure of firms in different manufacturing industries to the electricity price shock triggered by the NBP.

Following Curtis (2018), we define high–electricity intensity (low–electricity intensity) industries as those in the top (bottom) five manufacturing industries in terms of their electricity intensities (see Figure 2). We estimate the DDD model, where we interact the treatment effect (*NBP* × *Post*) with *High Electricity Intensity*, an indicator variable set to one for firms operating in the top five electricity-intensive industries, and zero otherwise. The results reported in Table 6 show that the coefficient on the triple interaction term is negative and significant. Consistent with our conjecture, the impact of the NBP on firm leverage was indeed more pronounced for firms in electricity-intensive industries.

[Insert Table 6 about here]

4.5.2. Financial Distress and the NBP's Effect

Some firms are closer to financial distress than others. Our hypothesis builds on the premise that higher electricity prices resulting from the NBP's implementation led to higher operating leverage and distress risk. According to the trade-off theory of capital structure, firms closer to financial distress have a stronger incentive to adopt more conservative debt policies after a positive shock to their operating leverage and an increase in their distress risk, ceteris paribus. Thus, we predict that the negative impact of the NBP's implementation on capital structure is more pronounced for firms more concerned about financial distress ex ante. As in Section 4.3, we use the DTD ratio (Bharath and Shumway 2008) to gauge the degree of financial distress. To alleviate the endogeneity concern that the NBP may have affected firms' distress risk when implemented, we set the conditioning variable, namely, *Low DTD*, as one if a firm had a below-median DTD value in 2003, and zero otherwise.

The results in columns (1) and (3) of Table 7 show that the NBP's implementation did not affect the debt ratios of less financially distressed firms. However, for more distressed firms (columns (1) to (4)), the NBP significantly and negatively impacted firm financial leverage. These results are consistent with our prediction that the effect of the NBP on capital structure was more pronounced for financially distressed firms.

4.5.3. Competitive Pressure and the NBP's Effect

Manufacturers may have passed higher electricity input costs on to their customers to mitigate the NBP's effect. However, the ability to pass on such costs relies on the market power of the firms in question to set product prices. Facing higher competitive pressure, firms have weaker market power. Firms operating in highly competitive product markets, therefore, have a greater incentive to adopt more conservative financial policies to mitigate increased operating leverage and distress risk. Accordingly, we expect the negative NBP's effect on financial leverage to have been more pronounced for firms facing higher competitive pressure.

To capture the degree of product market competition, we use two firm-level measures, namely, the text-based measure of product similarity (Hoberg and Phillips 2016) and product fluidity (Hoberg et al. 2014). The former measures the extent to which substitutes exist in the market for a firm's products and services, while the latter captures the frequency with which new products and services emerge in the market. We set the conditioning variables, *High Similarity* and *High Fluidity*, to one if a firm has an above-median value of product similarity and fluidity in the year 2002, respectively, and zero otherwise. We benchmark those variables in 2002 in case the NBP's implementation in the eight initial NBP states and DC in 2003 could have affected the competition landscape. Columns (5) to (12) of Table 7 show that firms with above-median competitive pressure reduced financial leverage significantly more than those with below-median pressure, consistent with our prediction above.

[Insert Table 7 about here]

5. Additional Analysis: The NBP's Effects on Debt Costs, Debt Structure, and Other Firm Financing Decisions

To gain a richer and more in-depth understanding of the responses of manufacturing firms and the financing ecosystem to the NBP, we extend our empirical analysis by examining how this regulation influenced other firm financial policies and outcomes.

First, having studied the NBP's effect on the choice between debt and equity, we now ask whether and how this regulation affected firms' access to debt financing, as well as their choice between different forms of debt. Specifically, we focus on the cost of debt and the choice between private debt (e.g., bank loans) and public debt (i.e., bonds) facing the manufacturers in NBP-compliant states. Lenders typically demand a higher rate of return when extending credit to those borrowers with a higher likelihood of bankruptcy and greater difficulties servicing their debt obligations (e.g., van Binsbergen et al. 2010). To the extent that, post-NBP, manufacturing firms experienced a significant increase in their operating leverage and distress risk, their cost of debt will have increased. Similarly, these firms may have found it easier to access private debt, such as bank loans. Banks and other private lenders have superior information acquisition and monitoring capabilities and more efficient liquidation and renegotiation processes than public lenders (Diamond 1984, Gilson et al. 1990, Rajan 1992, Denis and Mihov 2003), allowing them to offer credit to riskier borrowers. Overall, we expect the manufacturers in NBP-compliant states to have faced higher debt costs and to have relied less on public debt.

To test these predictions, we follow previous literature and estimate DID models of debt costs (e.g., Borisova et al. 2015) and debt structure (e.g., Bharath and Hertzel 2019, Li et al. 2019).[28] As in the capital structure analysis, we estimate these models with relevant fixed effects. The results in columns (1) to (3) of Table 8 support our predictions. Following the NBP's adoption, the costs of bank loans and public bonds increased by approximately 9.1% (= 18.677/204.133) and 28.1% (= 67.282/239.810), respectively, relative to their sample means. The economic magnitude of the NBP's impact on debt costs was greater for public bonds than for bank loans, suggesting that, while firms

[28] We retrieve data on bank loans and public bonds from the DealScan database and the Mergent Fixed Income Securities Database, respectively. Data on debt structure are from the S&P Capital IQ database; for the debt choice analysis, we follow previous literature (e.g., Colla et al. 2013, Li et al. 2019) and use a sample starting from 2002, when data coverage became more comprehensive.

generally faced more costly access to debt financing, it is their access to public debt that was most restricted. Consistent with this finding and our earlier conjecture, post-NBP, the proportion of public debt in firms' total debt structure decreased by 7.8% (= 0.038/0.488).

Next, we study the impact of the NBP on several other important corporate financial decisions, to further our understanding of firm responses to this regulation. Specifically, we investigate how the NBP affected manufacturers' cash holdings, payout policies, and investment decisions; the investment analysis sheds light on the real effects of the regulation. Since manufacturers faced higher operating leverage and distress risk following the NBP's adoption, we expect them to have employed more conservative financial policies (e.g., Kahl et al. 2019). Specifically, firms will build up their cash reserves to buffer expected downside risk (Almeida et al. 2004, Acharya et al. 2007), reduce their total payouts to preserve liquidity (DeAngelo and DeAngelo 1990), and cut back on their investments due to the costly supply of external finance (Asquith et al. 1994, Duchin et al. 2010).

In columns (4) to (6) of Table 8, we report the estimates of the DID models of cash holdings, payouts, and investment, respectively. We include control variables as in the previous literature on payout policy (e.g., Hoberg et al. 2014, Chu 2018), cash holdings (e.g., Bates et al. 2009), and investment (e.g., Bai et al. 2020). Consistent with our predictions, the NBP's impact on payouts and total investment was statistically and economically significant. Post-NBP, firms reduced their payouts and total investment by 27.8% (= 0.010/0.036) and 11.0% (= 0.027/0.245), respectively, relative to the sample means.[29] The impact of the program on cash holdings was positive and insignificant; however, when using an alternative definition of cash, namely, the logarithm of cash and short-term investments over net assets—that is, total assets minus cash and short-term investments (Foley et al. 2007, Bates et al. 2009)—rather than cash and short-term investments over total assets in the reported regression, we find that firms hoarded significantly more cash after the NBP's adoption.[30] In sum, our additional analysis suggests that the NBP affected several corporate outcomes and firms adapted not only their capital structure, but also other financial policies to counter the impact of the new regulation.

[29] In further analysis (untabulated), we also document a negative impact of the NBP on firms' investment in labor (i.e., employment), which is consistent with the results of Curtis (2018).
[30] The results are untabulated but are available upon request.

[Insert Table 8 about here]

6. Conclusion

The literature provides limited knowledge of how the transition risk of climate change influences corporate financial management. In this paper, we fill this gap by studying the adjustments in manufacturing firms' financial decisions around the 2004 implementation of the NBP, a regulation aimed at reducing NO_x emissions in 11 midwestern and southeastern states in the United States. Our evidence supports the view that higher electricity prices post-NBP instigated firms to adopt more conservative financial policies to mitigate the increased financial distress costs arising from elevated distress risk and operating leverage. This finding survives a myriad of robustness checks, including those controlling for potentially confounding electricity policies or using alternative measures of key outcome variables, using PSM and EB analysis, as well as placebo tests based on pseudo-samples of treated and control firms, among others. Our further analysis shows that manufacturing firms exhibited heterogeneous responses to the NBP. Manufacturers operating in electricity-intensive industries and those firms more vulnerable to distress risk or competitive pressure responded more dramatically. Besides financial leverage, firms also adapted their debt structure and other corporate policies (e.g., payout and investment) to the NBP, showing their multifaceted responses to the regulation.

Our study highlights the importance of understanding the implications of the transition risk associated with climate change for the corporate sector. In line with the recent evidence that institutional investors are concerned about climate policy risk (e.g., Krueger et al. 2020), we show that industrial companies factor this risk into their financial decisions. Our study suggests that policy responses to climate change may have unintended consequences for the corporate sector, and it is important for both policymakers and businesses to understand and manage such regulatory risk. A natural and fruitful avenue for future research would be to study whether and how companies respond by adapting other (nonfinancial) policies, such as for their product mix and offerings, as well as process innovation and strategic ventures, among others.

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Figure 1. Coverage of the NBP's Implementation

This map displays the regions affected by the NBP's implementation. The dark blue region (the 11 upwind states, including Alabama, Illinois, Indiana, Kentucky, Michigan, North Carolina, Ohio, South Carolina, Tennessee, Virginia, and West Virginia) participated in the NBP from 2004 to 2008. The medium blue regions (the eight downwind states, including Connecticut, Delaware, Maryland, Massachusetts, New Jersey, New York, Pennsylvania, and Rhode Island, plus Washington, DC) participated in the NBP from 2003 to 2008. The light blue region, consisting of several eastern counties of Missouri, participated in the NBP from 2007 to 2008. The gray region did not participate in the NBP.

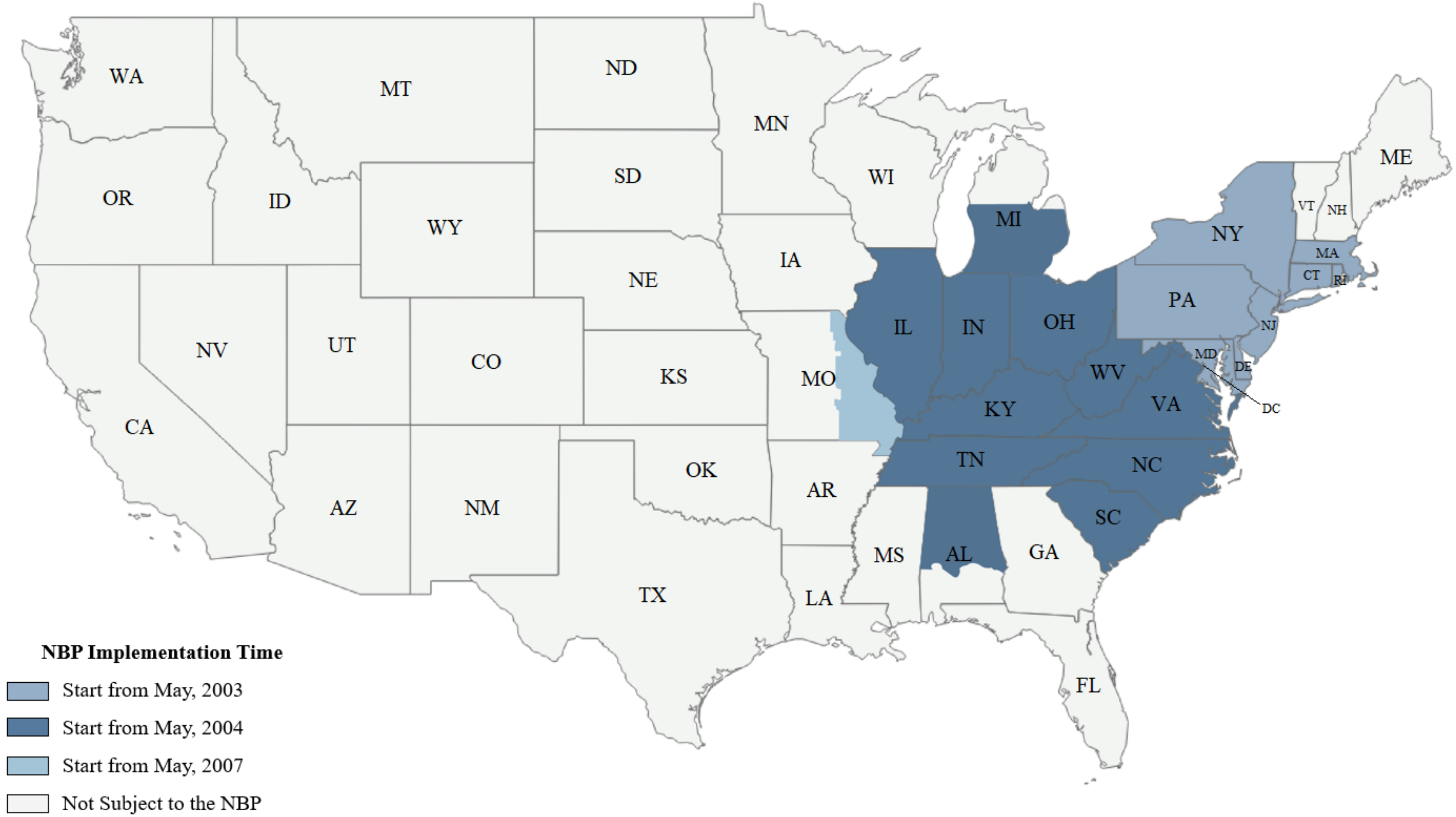

Figure 2. Electricity Intensity by Industry

This bar chart illustrates the electricity intensity of the U.S. industrial, commercial, and transportation sectors in 2003. The *x*-axis represents industry or sector electricity-intensity levels, measured as electricity consumption in trillions of British thermal units (Btu) scaled by the total value of shipments (GDP) in billions of 2000 dollars. The *y*-axis represents industry or sector names. The blue bars represent all the manufacturing industries; the metal-based durables industry includes fabricated metal products, machinery, computers, transportation equipment, and electrical equipment, while other manufacturing includes wood products, plastics and rubber products, and the balance of manufacturing. The red bars represent non-manufacturing industries and two other sectors, namely, the commercial and transportation sectors; non-manufacturing industries include mining, agriculture, and construction. For the commercial and transportation sectors, the GDP and electricity consumption data were obtained from the U.S. Bureau of Economic Analysis and Table 7.6 of the U.S. Energy Information Administration (EIA) Monthly Energy Review, respectively. The remainder of the data are from Supplemental Tables 24–32 of the EIA's Annual Energy Outlook 2006.

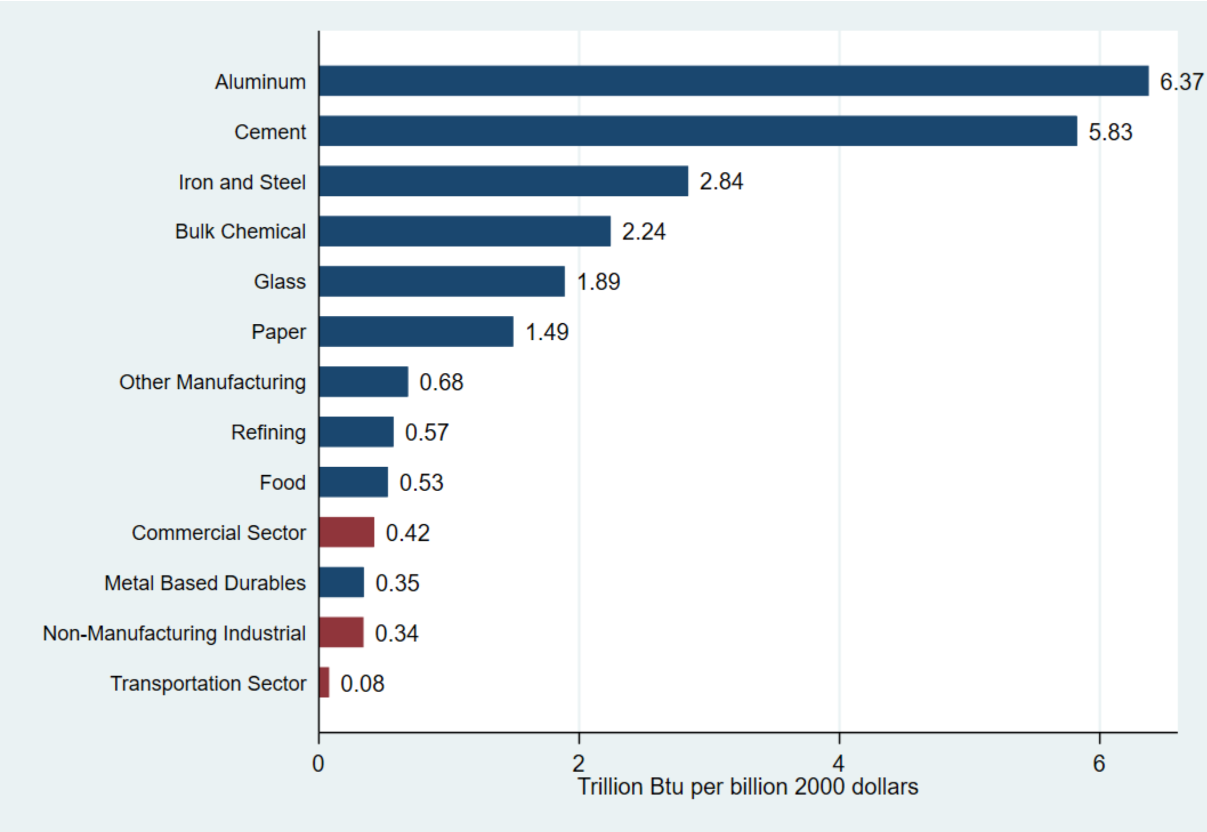

Table 1. Summary Statistics

This table reports the summary statistics of all the variables in our main empirical analysis. The sample consists of Compustat manufacturing firms from 2000 to 2008. The sample used for the baseline results includes 12,454 firm–year observations. For the definitions of all the variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars.

	N	Mean	P25	Median	P75	Std. Dev.
Dependent Variables						
Book Leverage	12454	0.231	0.004	0.146	0.350	0.268
Market Leverage	12454	0.200	0.002	0.096	0.310	0.248
Control Variables						
Total Assets ($m)	12454	1173.570	20.868	101.953	532.697	3863.111
Market-to-Book	12454	4.124	1.133	1.669	2.966	10.269
ROA	12454	-0.312	-0.166	0.067	0.141	1.473
Fixed Assets	12454	0.204	0.066	0.152	0.301	0.177
Dividend Payer	12454	0.219	0.000	0.000	0.000	0.413
Default Spread (in %)	12454	1.177	0.840	0.950	1.240	0.654
State GDP Growth	12454	0.051	0.033	0.050	0.069	0.028
Political Balance	12454	0.491	0.348	0.510	0.615	0.156
Additional Dependent Variables						
Distance to Default	6402	23.332	8.592	18.320	32.172	21.317
Credit Rating	2197	13.071	10.000	13.000	15.000	3.465
CDS Spread (in basis points (bps))	745	191.492	30.303	70.640	211.710	361.906
MROL	6945	1.716	0.457	1.193	2.279	2.301
KLNOL	7239	0.363	0.093	0.272	0.606	0.377
NMOL	9865	1.365	0.677	1.021	1.478	1.609
Cost of Bank Loans (in bps)	3396	204.133	87.500	187.500	300.000	139.348
Cost of Public Bonds (in bps)	453	239.810	109.588	183.000	333.198	171.536
Pct. Public Debt	6186	0.495	0.000	0.516	0.963	0.430
Cash Holdings	11051	0.252	0.035	0.150	0.398	0.264
Payout Ratio	10035	0.029	0.000	0.000	0.020	0.087
Total Investment	10515	0.215	0.052	0.112	0.226	0.367

Table 2. The NBP's Implementation and Capital Structure

This table reports the results from the ordinary least squares (OLS) regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise, and *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage				Market Leverage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NBP × *Post*	–0.034***	–0.032***	–0.033***	–0.033***	–0.033***	–0.030***	–0.032***	–0.033***
	(–3.22)	(–3.20)	(–3.29)	(–3.23)	(–3.10)	(–2.87)	(–3.04)	(–3.17)
Log Total Assets		–0.044***	–0.043***	–0.043***		–0.008	–0.007	–0.007
		(–6.35)	(–6.28)	(–6.28)		(–1.33)	(–1.19)	(–1.18)
Market-to-Book		–0.000	–0.000	–0.000		–0.003***	–0.003***	–0.002***
		(–0.18)	(–0.14)	(–0.14)		(–5.36)	(–5.20)	(–5.15)
ROA		–0.030***	–0.030***	–0.030***		–0.016***	–0.016***	–0.016***
		(–5.73)	(–5.71)	(–5.72)		(–4.33)	(–4.29)	(–4.26)
Fixed Assets		0.195***	0.195***	0.195***		0.165***	0.165***	0.163***
		(5.38)	(5.37)	(5.36)		(5.49)	(5.49)	(5.45)
Dividend Payer			–0.020**	–0.020**			–0.037***	–0.037***
			(–2.22)	(–2.21)			(–3.45)	(–3.44)
Default Spread			0.009	0.009			0.029***	0.030***
			(1.17)	(1.18)			(3.99)	(4.08)
State GDP Growth				0.025				–0.355***
				(0.22)				(–3.27)
Political Balance				0.037				0.092**
				(0.96)				(2.44)
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454
R-Squared	0.718	0.746	0.746	0.746	0.746	0.751	0.752	0.753

Table 3. The NBP's Implementation, Distress Risk, and Operating Leverage

Panels A and B report the results from the OLS regressions estimating the NBP's effects on financial distress risk and operating leverage, respectively, for Compustat manufacturing firms from 2000 to 2008. The dependent variables in columns (1) to (3) of Panel A are three distress risk measures, namely, *Distance to Default (DTD), Credit Rating*, and *CDS Spread*. The variable *DTD* is the distance-to-default ratio calculated following Bharath and Shumway (2008), *Credit Rating* is Standard & Poor's domestic long-term issuer rating score (AAA = 23, AA+ = 22, AA = 21, etc.), and *CDS Spread* is the five-year spread on CDS contracts. The dependent variables in columns (1) to (6) of Panel B are three operating leverage measures, namely, *MROL*, *KLNOL*, and *NMOL* (see Online Appendix OA2 for detailed calculations). The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. In Panel A, *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. In columns (1) to (4) of Panel B, *Post* is an indicator variable set to one for 2007 and later, and zero for 2003 and before, excluding the observations in 2004, 2005, and 2006. In columns (5) to (6) of Panel B, *Post* is an indicator variable set to one for 2005 and later, and zero for 2003 and before, excluding the observations in 2004. In the *DTD* regression (column (1) of Panel A), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Book Leverage*, *Cash Flow Volatility*, *Interest Coverage*, *Default Spread*, and *Term Structure.* In the *Credit Rating* regression (column (2) of Panel A), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Loss Dummy*, *Book Leverage*, *Interest Coverage*, *Subordinated Debt Dummy,* and *Stock Return Volatility*. In the *CDS Spread* regression (column (3) of Panel A), the control variables include *Recovery Rate* and the one-year-lagged *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Book Leverage*, *Cash Flow Volatility*, *Default Spread*, *Term Structure*, and *Share Prices*. In Panel B, the control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Financial Distress Measures

	DTD	Credit Rating	CDS Spread
	(1)	(3)	(3)
NBP × Post	−2.182**	−0.311*	111.173**
	(−1.98)	(−1.77)	(2.35)
Control Variables	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
Observations	6,402	2,197	745
R-Squared	0.673	0.927	0.708

Panel B: Operating Leverage

	MROL		KLNOL		NMOL	
	(1)	(2)	(3)	(4)	(5)	(6)
NBP × Post	0.477***	0.463**	0.037*	0.036*	0.159***	0.155***
	(2.59)	(2.51)	(1.82)	(1.76)	(2.99)	(2.88)
Book Leverage		−0.484**		−0.044		−0.175
		(−2.48)		(−1.63)		(−0.79)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	6,945	6,945	7,239	7,239	9,865	9,865
R-Squared	0.666	0.666	0.787	0.787	0.612	0.612

Table 4. Effect of the NBP, Estimated Using

Propensity Score Matched and Entropy Balanced Samples

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation, using propensity score matched and entropy balanced samples. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise, and *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. Columns (1) and (2) report the results using the propensity score matched sample. For the matching procedure, we estimate the propensity scores using the full set of firm-level control variables in our baseline analysis in Table 2 (i.e., *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, and *Dividend Payer*) and the logit model. In addition, we match each treated firm to a control firm in the same EIA manufacturing industry cluster (as depicted in Figure 2), without replacement. The matching is carried out for 2003 and is based on the closest propensity score (within a caliper of 0.005). Columns (3) and (4) report the results using the entropy balanced sample. We balance treated and control firms for 2003 using the first two moments (i.e., the mean and variance) of all the firm-level controls in each EIA manufacturing industry cluster (as depicted in Figure 2). The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Propensity Score Matching		Entropy Balancing	
	(1)	(2)	(3)	(4)
	Book Leverage	Market Leverage	Book Leverage	Market Leverage
NBP × *Post*	–0.030**	–0.043***	–0.032**	–0.030**
	(–2.14)	(–2.97)	(–2.30)	(–2.05)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	4,047	4,047	7,702	7,702
R-squared	0.762	0.773	0.756	0.774

Table 5. Dynamic Effects of the NBP's Implementation on Capital Structure

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise; $Before^{-3}$ is an indicator variable set to one for 2001, and zero otherwise; $Before^{-2}$ is an indicator variable set to one for 2002, and zero otherwise; $Before^{-1}$ is an indicator variable set to one for 2003, and zero otherwise; $After^{0}$ is an indicator variable set to one for 2004, and zero otherwise; $After^{+1}$ is an indicator variable set to one for 2005, and zero otherwise; and $After^{2+}$ is an indicator variable set to one for 2006 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage	Market Leverage
	(1)	(2)
$NBP \times Before^{-3}$	0.007	0.012
	(0.72)	(1.07)
$NBP \times Before^{-2}$	–0.007	–0.005
	(–0.62)	(–0.35)
$NBP \times Before^{-1}$	–0.016	–0.011
	(–1.28)	(–0.78)
$NBP \times After^{0}$	–0.028**	–0.038***
	(–2.04)	(–2.59)
$NBP \times After^{+1}$	–0.039***	–0.045***
	(–2.67)	(–2.86)
$NBP \times After^{2+}$	–0.041***	–0.029*
	(–2.79)	(–1.77)
Control Variables	Yes	Yes
Firm Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	12,454	12,454
R-Squared	0.746	0.753

Table 6. Impact of Electricity Intensity

This table presents the impact of the NBP on the financial leverage of firms with various degrees of electricity intensity. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise; *Post* is an indicator variable set to one for 2004 and later, and zero otherwise; and *High Electricity Intensity* is an indicator variable that is equal to one (zero) if a firm's industry is among the top (bottom) five manufacturing industries in Figure 2 in terms of electricity intensity. Each specification includes all double and triple interaction terms, though some interaction terms are not tabulated for brevity. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage		Market Leverage	
	(1)	(2)	(3)	(4)
NBP × *Post*	−0.014		−0.021*	
	(−1.32)		(−1.83)	
NBP × *Post* × *High Electricity Intensity*	−0.082***	−0.085***	−0.055**	−0.054**
	(−2.76)	(−2.77)	(−2.15)	(−2.05)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No
Year Fixed Effects	Yes	No	Yes	No
State × Year Fixed Effects	No	Yes	No	Yes
Observations	12,454	12,445	12,454	12,445
R-Squared	0.747	0.753	0.754	0.762

Table 7. Impact of Financial Distress and Product Market Competition

This table presents the impact of the NBP on the corporate capital structure of firms with various degrees of financial distress (columns (1) to (4)) and competitive pressure (columns (5) to (12)), respectively. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise; *Post* is an indicator variable set to one for 2004 and later, and zero otherwise; *Low DTD* is an indicator variable that is equal to one if a firm has a below-median distance-to-default ratio in 2003, and zero otherwise; *High Similarity* is an indicator variable that is equal to one if a firm has an above-median value for the text-based measure of product similarity in 2002, and zero otherwise; and *High Fluidity* is an indicator variable that is equal to one if a firm has an above-median value of the text-based measure of product market fluidity in 2002, and zero otherwise. Each specification includes all double and triple interaction terms, though some are not tabulated for brevity. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Financial Distress				Product Market Competition							
	Book Leverage		Market Leverage		Book Leverage		Market Leverage		Book Leverage		Market Leverage	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
NBP × Post	–0.008		–0.000		–0.002		0.003		–0.002		0.004	
	(–0.65)		(–0.02)		(–0.16)		(0.19)		(–0.19)		(0.32)	
NBP × Post × Low DTD	–0.046**	–0.046**	–0.050**	–0.044*								
	(–2.11)	(–1.97)	(–2.12)	(–1.84)								
NBP × Post × High Similarity					–0.055**	–0.059**	–0.050**	–0.048*				
					(–2.32)	(–2.32)	(–2.22)	(–1.96)				
NBP × Post × High Fluidity									–0.048**	–0.050**	–0.066***	–0.060**
									(–2.07)	(–2.05)	(–2.64)	(–2.27)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
Year Fixed Effects	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No
State × Year Fixed Effects	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	7,189	7,160	7,189	7,160	9,360	9,332	9,360	9,332	9,249	9,221	9,249	9,221
R-Squared	0.684	0.694	0.725	0.738	0.722	0.728	0.755	0.765	0.722	0.728	0.754	0.763

Table 8. The NBP's Implementation, Debt Costs, Debt Structure, and Other Financial Decisions

This table reports the results from the OLS regressions estimating the NBP's effects on the cost and choice of debt, as well as firm financial and investment policies other than capital structure, for Compustat manufacturing firms from 2000 to 2008. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise; *Post* is an indicator variable set to one for 2004 and later, and zero otherwise; *Cost of Bank Loans* is measured as the all in drawn spread in basis points; *Cost of Public Bonds* is measured as the difference between the corporate bond's yield to maturity and the benchmark Treasury bond yield in basis points; *Pct. Public Debt* is measured as the ratio of public bonds (i.e., the sum of senior bonds and notes, subordinated bonds and notes, and commercial papers) to total debt; *Cash Holdings* is measured as the ratio of cash and short-term investments to total assets; *Payout Ratio* is measured as the ratio of share repurchases and dividends to total assets; and *Total Investment* is measured as the ratio of capital, research and development, advertising, and acquisition expenditures to one-year-lagged total assets. In the *Cost of Bank Loan* and *Cost of Public Bond* regressions (columns (1) and (2), respectively), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Book Leverage*, *Term Structure*, *Default Spread*, *State GDP Growth*, and *Political Balance*. In the *Cost of Bank Loans* regression (column (1)), we further control for loan-level controls, namely, *Loan Maturity*, *Loan Size*, *Performance Pricing*, *Syndication*, *Financial Covenants*, *General Covenants*, and *Loan Type* and *Loan Purpose* fixed effects. In the *Cost of Public Bonds* regression (column (2)), in addition to firm-level controls, we include bond characteristics, namely, *Bond Maturity*, *Bond Size*, *Bond Rating*, *Bond Covenants*, *Callable Dummy*, *Subordinated Bond Dummy*, and *Issue Type* fixed effects. In the *Pct. Public Debt* regression (column (3)), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *Fixed Assets*, *Cash Flow*, *Rating Dummy*, *Investment Grade*, *State GDP Growth*, and *Political Balance*. In the *Cash Holdings* regression (column (4)), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *ROA*, *Dividend Payer*, *Book Leverage*, *Net Working Capital*, *Capital Expenditures*, *Acquisition Expenditures*, *R&D Expenses*, *R&D Dummy*, *Industry Cash Flow Volatility*, *State GDP Growth*, and *Political Balance*. The control variables in the *Payout Ratio* regression (column (5)) include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *Fixed Assets*, *Cash Flow*, *Cash Holdings*, *Book Leverage*, *Sales Growth*, *Firm Age*, *State GDP Growth*, and *Political Balance*. In the *Total Investment* regression analysis (column (6)), the control variables include the one-year-lagged *Log Total Assets*, *Market-to-Book*, *Cash Flow*, *Cash Holdings*, *Book Leverage*, *State GDP Per Capita*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (t-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Cost of Bank Loans	Cost of Public Bonds	Pct. Public Debt	Cash Holdings	Payout Ratio	Total Investment
	(1)	(2)	(3)	(4)	(5)	(6)
NBP × *Post*	18.677**	67.282**	–0.038*	0.009	–0.010**	–0.027**
	(2.02)	(2.03)	(–1.76)	(1.29)	(–2.36)	(–2.06)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	3,396	453	6,186	11,051	10,035	10,515
R-Squared	0.766	0.872	0.738	0.827	0.318	0.624

Online Appendix for

"Climate Policy Risk and Corporate Financial Decisions:

Evidence from the NO_x Budget Trading Program"

In this online appendix, we provide additional discussions and robustness tests that supplement the empirical analysis in the main body of the manuscript. Below is the list of contents.

Online Appendix OA1. Variable Definitions

Panel A: Variables Appearing in the Main Body	
Variable	**Description** (For data definitions in parentheses refer to Compustat designations)
Leverage Measures	
Book Leverage	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by the book value of total assets (at).
Market Leverage	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) divided by market value of debt and equity (long-term debt (dltt) plus debt in current liabilities (dlc) plus market value of equity (prcc_f * csho)).
Control, Conditioning, and Other Dependent Variables	
Acquisition Expenditures	The ratio of acquisitions (aqc) to total assets (at).
Bond Covenants	The number of covenants used in a bond issue obtained from the Mergent Fixed Income Securities Database (FISD).
Bond Maturity	The natural logarithm of bond maturity in months obtained from the Mergent Fixed Income Securities Database (FISD).
Bond Rating	The average of the S&P's and Moody's ratings of a bond (for S&P's rating, AAA=23, AA+=22, etc.; for Moody's rating, Aaa=23, Aa1=22, etc.). If only one agency exists, the existing agency's rating is used. The bond rating data is obtained from the Mergent Fixed Income Securities Database (FISD).
Bond Size	The natural logarithm of bond amount in millions of dollars obtained from the Mergent Fixed Income Securities Database (FISD).
Callable Dummy	An indicator variable that is equal to one if a bond issue is callable, and zero otherwise. The callable dummy data is obtained from the Mergent Fixed Income Securities Database (FISD).
Capital Expenditures	The ratio of capital expenditures (capx) to total assets (at).
Cash Flow	The ratio of operating income before depreciation less interest, dividends, and tax (oibdp – xint – txt – dvc) to total assets (at).
Cash Flow Volatility	The standard deviation of a firm's return on assets (oibdpq / atq) over the previous fifteen quarters. We require firms to have at least six quarters of non-missing data to enter the sample.
Cash Holdings	The ratio of cash and short-term investments (che) to total assets (at).
CDS Spread	The five-year spreads on CDS contracts. The CDS spread data is obtained from Markit CDS database.
Cost of Bank Loans	The all-in spread drawn (AISD) in basis points obtained from the DealScan database.
Cost of Public Bonds	The difference between a corporate bond's yield to maturity (YTM) and its benchmark Treasury bond yield of comparable maturity in basis points. The benchmark Treasury bond yields have 1-, 2-, 3-, 5-, 7-, 10-, 20-, and 30-year constant maturity. The corporate bond's YTM data and treasury bond yield data are obtained from the Mergent Fixed Income Securities Database (FISD) and the Federal Reserve Economic Data (FRED) at the Federal Reserve Bank of St. Louis, respectively.
Credit Rating	The S&P domestic long-term issuer rating (splticrm) score (AAA = 23, AA+ = 22, AA = 21, etc.).
Default Spread	The difference between the yield on Baa and Aaa rated corporate bonds, measured as of the firm's fiscal-year month end. The corporate bond yield data is obtained from the Federal Reserve Economic Data (FRED) at the Federal Reserve Bank of St. Louis.
Distance-to-Default	The distance-to-default measure is constructed following Bharath and Shumway (2008).
Dividend Payer	An indicator variable set to one if a firm pays a common dividend (dvc) and zero otherwise.
Financial Covenants	The number of financial covenants used in a loan contract obtained from the DealScan database.
Firm Age	The number of years for a firm appeares in Compustat database.

Fixed Assets	The ratio of property, plant, and equipment (ppent) to book value of total assets (at).
General Covenants	The number of general covenants used in a loan contract obtained from the DealScan database.
High Electricity Intensity	An indicator variable that is equal to one (zero) if a firm's industry is among the top (bottom) five manufacturing industries in Figure 2 in terms of electricity intensity. The electricity intensity measure is obtained from the Supplemental Tables 24–32 in the Annual Energy Outlook 2006 of the U.S. EIA.
High Fluidity	An indicator variable that is equal to one if a firm is with an above-median value of the text-based measure of product market fluidity in 2002, and zero otherwise. The product market fluidity measure is obtained from the Hoberg-Phillips Data Library.
High Similarity	An indicator variable that is equal to one if a firm is with an above-median value of the text-based measure of product similarity in 2002 and zero otherwise. The product similarity measure is obtained from the Hoberg-Phillips Data Library.
Industry Cash Flow Volatility	The average of firm cash flow volatility for each two-digit SIC industry cluster.
Interest Coverage	The ratio of earnings before interest, taxes, depreciation, and amortization (ebitda) to interest expenses (xint).
Investment Grade	An indicator variable set to one if a firm has an investment grade S&P domestic long-term issuer rating (splticrm) in a given year, and zero otherwise.
Issue Type	The type of a bond issue, including U.S. corporate debenture, foreign currency debenture, and preferred security.
KLNOL	A measure of the degree of operating leverage, which is the sensitivity of the innovations in the growth rate of operating costs (xoprq) to the innovations in the growth rate of sales (saleq), following Kahl et al. (2019). We require firms with at least six quarters of non-missing data to enter the sample. For the detailed construction procedure, see Online Appendix OA2.
Loan Maturity	The natural logarithm of loan maturity in months obtained from the DealScan database.
Loan Purpose	The purpose of a loan facility, including corporate purposes, debt repayment, working capital, commercial paper backup, acquisition, and other purposes.
Loan Size	The natural logarithm of total loan amount in millions obtained from the DealScan database.
Loan Type	The type of a loan facility, including term loan, 364-day facility, revolver greater than 1 year, and revolver less than 1 year,
Log Total Assets	The natural logarithm of the value of total book assets (at) in millions and is expressed in the 1999 dollars.
Loss Dummy	An indicator variable that is equal to one if a firm has negative net income before extraordinary items (ibc) in the current and the previous year, and zero otherwise.
Low DTD	An indicator variable that is equal to one if a firm is with a below-median value of the distance-to-default (DTD) ratio in 2003, and zero otherwise. The DTD measure is constructed following Bharath and Shumway (2008).
Market-to-Book	Book value of liabilities (dlc + dltt + pstkl) plus market value of equity (prcc_f * csho) divided by total assets (at).
MROL	A measure of the degree of operating leverage, which is the elasticity of a firm's operating income after depreciation (oiadpq) with respect to its sales (saleq) over the past fifteen quarters (Mandelker and Rhee 1984). We require firms with at least six quarters of non-missing data to enter the sample. For the detailed construction procedure, see Online Appendix OA2.
Net Working Capital	The ratio of net current assets minus cash (wcap – che) to book total assets minus cash (at – che).

NMOL	A measure of the degree of operating leverage, which is the ratio of cost of goods sold (cogs) and selling, general, and administrative expenses (xsga) to book assets (at) at the end of the previous year (Novy-Marx 2011). For the detailed construction procedure, see Online Appendix OA2.
Payout Ratio	The ratio of the sum of repurchases (prstkc minus the change in pstkrv) and dividends (dvc) to total assets (at).
Pct. Public Debt	The ratio of public bonds (i.e., the sum of senior bonds and notes, subordinated bonds and notes, and commercial papers) to total debt. Data on debt structure is from the S&P Capital IQ database.
Performance Pricing	An indicator variable that is equal to one if a loan facility uses performance pricing, and zero otherwise. The performance pricing data is obtained from the DealScan database.
Political Balance	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.
Rating Dummy	An indicator variable set to one if a firm has the S&P domestic long-term issuer rating (splticrm) in a given year, and zero otherwise.
R&D Expenses	Research and development expenses (xrd) as a proportion of total assets (at).
R&D Dummy	An indicator variable set to one if a firm did not report research and development expenses (xrd), else zero.
Recovery Rate	The percentage of principal and interest on defaulted debt of a firm can be recovered. The recovery rate data is obtained from the Markit CDS database.
ROA	Operating income before depreciation (oibdp) divided by the book value of total assets (at).
Sales Growth	The growth rates of a firm's sale (sale).
Share Prices	The share price (prcc_f) for a firm at the end of a given year.
State GDP Growth	The annual GDP growth rate of a state over a fiscal year.
Stock Return Volatility	A firm's monthly stock return volatility in a given year. The stock return data is obtained from CRSP.
Subordinated Bond Dummy	An indicator variable that is equal to one if a bond issue is a subordinated bond, and zero otherwise. The subordinated bond data is obtained from the Mergent Fixed Income Securities Database (FISD).
Subordinated Debt Dummy	An indicator variable that is equal to one if a firm has subordinated debt (ds) in a given year, and zero otherwise.
Syndication	An indicator variable that is equal to one if a loan facility is a syndicated loan, and zero otherwise. The syndication data is obtained from the DealScan database.
Term Structure	The difference between the yield on 10-year government bonds and the yield on 1-year government bonds. The government bond yield data is obtained from the Federal Reserve Economic Data (FRED) at the Federal Reserve Bank of St. Louis.
Total Investment	The ratio of capital, research and development, advertising, and acquisition expenditures (capx + xrd + xad + aqc) to one-year-lagged total assets (at).

Panel B: Variables Appearing Only in the Online Appendix

Variable	**Description** (For data definitions in parentheses refer to Compustat designations)
Leverage Measures	
Book Leverage with Leases	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) plus the value of leases (xrent * 10) divided by the book value of total assets plus the value of leases (at + xrent * 10).
Log (Total Debt)	The natural logarithm of the book value of long-term debt (dltt) plus debt in current liabilities (dlc) and is expressed in millions of 1999 dollars.
Market Leverage with Leases	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) plus the value of leases (xrent * 10) divided by the market value of debt and equity plus the value of leases (dltt + dlc + prcc_f * csho + xrent * 10).

Net Book Leverage	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) less the book value of cash and short-term investments (che) divided by the book value of total assets (at).
Net Market Leverage	The book value of long-term debt (dltt) plus debt in current liabilities (dlc) less the book value of cash and short-term investments (che) divided by the market value of debt and equity (dltt + dlc + (prcc_f * csho)).
Control Variables	
Log Employees	The natural logarithm of the number of employees in thousands (emp).
Sales-Weighted State Book Leverage	The sales-based weighted average book leverage of firms headquartered in a state.
Sales-Weighted State Market Leverage	The sales-based weighted average market leverage of firms headquartered in a state.
State Carbon Emission Rate	The state carbon emission rate (lbs/mmBtu) obtained from the EPA's website https://ampd.epa.gov/ampd.
State Energy Intensity	The state energy consumption (measured by thousands of British Thermal Units per chained 2009 dollar of GDP) obtained from U.S. Energy Information Administration.
State GDP Per Capita	The natural logarithm of a state's GDP (in million dollars) divided by its total population.
State NO_x Emission Rate	The state NO_x emission rate (lbs/mmBtu) is obtained from the EPA's website https://ampd.epa.gov/ampd.
State SO_2 Emission Rate	The state SO_2 emission rate (lbs/mmBtu) is obtained from the EPA's website https://ampd.epa.gov/ampd.
State Unemployment Rate	The unemployment rate of a state in a fiscal year.

Online Appendix OA2. Definitions of Operating Leverage Measures

We adopt the three most popular measures of operating leverage, developed by 1) Mandelker and Rhee (1984), 2) Kahl et al. (2019), and 3) Novy-Marx (2011), which, for convenience, we label, respectively, as *MROL*, *KLNOL*, and *NMOL*. In the following, we describe in detail how we calculate the three measures.

MROL

The measure *MROL* reflects the elasticity of a firm's operating income after depreciation to its sales. *MROL* is considered a traditional measure of operating leverage (García-Feijóo and Jorgensen 2010, Chen et al. 2011, Ljungqvist et al. 2017, Kim et al. 2018, Carter et al. 2021, Dou et al. 2021). Since it is typically calculated using 15 quarters of time-series data, one immediate concern is that the data used to calculate *MROL* may not always be in the same pre- or post-treatment regime as the measure itself. To address this issue, we exclude the period 2004–2006 from our test window and calculate *MROL* only for the periods 2000–2003 (pre-NBP) and 2007–2008 (post-NBP). By excluding the period 2004–2006 from the test window, we ensure that post-treatment, our measure is calculated using firm financial data in the NBP's implementation period only (i.e., from the second quarter of 2004 to 2008) and thus not contaminated by pre-treatment information. For the pre-NBP period, between 2000 and 2003, *MROL* is calculated using the same method, based on the data from 1997.[31]

The detailed calculation of *MROL* consists of two main steps. First, as in previous research (e.g., García-Feijóo and Jorgensen 2010), for each firm–year in the test window, we detrend the natural logarithm of EBIT and sales over the past 15 quarters. To deal with negative earnings, we employ a common transformation method, that is to use $-\ln(1 - \text{EBIT})$. Second, we regress the detrended natural logarithm of EBIT on the detrended natural logarithm of sales in the same 15-quarter window, using the following model:

$$\mu_{i,q,Log\ EBIT} = MROL\mu_{i,q,Log\ Sales} + e_{i,t}, \tag{1}$$

[31] We note, however, that our results are qualitatively unchanged even if we include 2004–2006 in the test window.

where $\mu_{i,q,Log\ EBIT}$ and $\mu_{i,q,Log\ Sales}$ are the detrended natural logarithms of EBIT and sales, respectively, for firm *i* in quarter *q*.[32] The term *MROL* is the estimated coefficient on the latter variable.

KLNOL

The measure *KLNOL* captures the sensitivity of the innovations in the growth rate of operating costs to the innovations in the growth rate of sales (Lev 1974, Kahl et al. 2019). It reflects the relative importance of firms' (fixed) operating costs in their total cost structure (Kahl et al. 2019). Despite being a relatively new measure of operating leverage, *KLNOL* has become increasingly popular in recent research (e.g., Kulchania and Thomas 2017, Albuquerque et al. 2019, Gamba and Saretto 2019). Since *KLNOL* is also calculated using time-series data as *MROL*, for consistency, we process the required data in the same fashion as described above. Specifically, we compute *KLNOL* using data from the past 15 quarters.[33] We also discard the period 2004–2006 from the test window to ensure that the post-NBP calculation is not contaminated by the data in the pre-treatment period.

The calculation of *KLNOL* proceeds in three steps. First, we calculate the *ex-ante* expectations of operating costs and sales according to the geometric growth rates over the previous two quarters, as follows:

$$E\left[XOPR_{i,q}\right] = XOPR_{i,q-1}\left(\frac{XOPR_{i,q-1}}{XOPR_{i,q-3}}\right)^{1/2}, \tag{2}$$

$$E\left[Sales_{i,q}\right] = Sales_{i,q-1}\left(\frac{Sales_{i,q-1}}{Sales_{i,q-3}}\right)^{1/2}, \tag{3}$$

where $XOPR_{i,q}$ and $Sales_{i,q}$ are operating costs and sales, respectively, for firm *i* in quarter *q*. Second, to compute the innovations in the growth rate of operating costs (sales), we subtract the expected values of operating costs (sales) from their actual values, before scaling them by their lagged actual values, using the following equations:

[32] In these regressions, we include only firms with at least six quarters of non-missing data in the test window. This research design follows the recommendation of Kahl et al. (2019) to use seven data points to estimate *KLNOL*. This also ensures that we still have a large enough sample with operating leverage data to perform the DID analysis. We note, however, that our results are robust if we impose more stringent requirements, such as considering only firms with at least ten quarters of non-missing data in the test window.

[33] Kahl et al. (2019) use annual data and a rolling window of seven years to estimate *KLNOL*. Since we use quarterly data to calculate the operating leverage measures based on time-series data, for consistency, we also use a 15-quarter rolling window for *KLNOL*, as we do in the calculation of *MROL*. The results for *KLNOL* are, however, qualitatively similar if we use a seven-quarter rolling window and exclude the year 2004 rather than 2004–2006.

$$\mu_{i,q,XOPR} = \frac{(XOPR_{i,q} - E[XOPR_{i,q}])}{XOPR_{i,q-1}}, \tag{4}$$

$$\mu_{i,q,Sales} = \frac{(Sales_{i,q} - E[Sales_{i,q}])}{Sales_{i,q-1}}, \tag{5}$$

Third, we regress the innovations in the growth rate of operating costs, $\mu_{i,q,XOPR}$, on the innovations in the growth rate of sales, $\mu_{i,q,Sales}$, using the following model:

$$\mu_{i,q,XOPR} = (1 - KLNOL)\mu_{i,q,Sales} + e_{i,q}. \tag{6}$$

The coefficient on the innovations in the growth rate of sales, $\mu_{i,q,Sales}$, captures the sensitivity of operating costs to sales. Since firms with a higher portion of fixed costs (i.e., higher operating leverage) tend to have a lower sensitivity of operating costs to sales, we define *KLNOL* as one minus the coefficient on the innovations in the growth rate of sales. Just like for *MROL,* a higher value of *KLNOL* indicates a higher degree of operating inflexibility.

NMOL

Although *MROL* and *KLNOL* are conceptually appealing, they require relatively long time-series data and thus can be difficult to estimate for short samples. Hence, we also consider a static measure of operating leverage developed by Novy-Marx (2011). The measure *NMOL* is defined as the ratio of the cost of goods sold (COGS) and selling, general, and administrative expenses (XGSA) to total assets at the end of the previous year. Despite being less popular than the first two measures, *NMOL* and proxies based on similar concepts have been used in several studies (e.g., Reinartz and Schmid 2016, D'Acunto et al. 2018, Kim et al. 2018, Chen et al. 2019). One advantage of this type of measure is that it can be calculated for short windows, without the need for long time-series data. Nevertheless, since *NMOL* is scaled by the lagged values of total assets, we exclude the NBP implementation year (2004) from the test window and include only 2005–2008 in the post-NBP period.

Having constructed the three operating leverage measures following the procedures detailed above, we finally winsorize them at the first and 99th percentiles of their distributions to reduce the potential impact of outliers; this is the same winsorization approach that we apply to the other continuous variables used in our empirical study. In Table 1 of the manuscript, we report the summary statistics for these operating leverage measures. We find that they are broadly in line with those reported in the literature (e.g., Novy-Marx 2011, Kahl et al. 2019), though the mean for *MROL* is slightly lower than what is documented in previous research (e.g., Ljungqvist et al. 2017).

Online Appendix OA3. Comparison of the NBP and Other Electricity Policies

To tackle our research question, we require a (state-level) policy shock that 1) is based on the adoption of a climate regulation, 2) has important policy implications for emission control and reduction, 3) has a strong impact on the wider corporate sector beyond directly regulated sources of emissions, and 4) is preferably the consequence of an (unexpected) court ruling. In what follows, we use these four criteria to compare the NBP with RPS laws, EMR, EERS, NM, GPP programs, and PBFs.

First, as discussed in Section 2.3, except for EMR, all regulations under consideration are climate policies. EMR is primarily a competition regulation aimed at deregulating the electric utility industry rather than reducing emissions (Joskow 1997) and thus does not apply to our research question.

Second, regarding the policy impact on emissions, previous studies show that NBP, RPS laws, EMR, EERS, and PBFs all lead to a reduction in emissions, although the effect varies in magnitude. For example, Deschênes et al. (2017) find that, following the NBP's implementation, the NO_x emissions of the regulated states dropped dramatically, by almost 40%, during the summer months (i.e., the regulated period), while Greenstone and Nath (2020) document that RPS laws substantially reduced carbon emissions by between 10% and 25%. Grinstein and Larkin (2021) find that, following the passage of EMR, utilities in the restructured states emitted 18% less than those in unrestructured states. Yi (2015) shows that, after the adoption of EERS, carbon emissions in the electricity sector decreased by around 8%. Recent research also reveals that PBFs reduced carbon emissions by between 4% to 8% in the electric power sector (e.g., Prasad and Munch 2012, Yi 2015). However, the impact of NM on emissions is mixed (Prasad and Munch 2012), while there is limited research on the implications of GPP programs for emission control.

Third, in terms of the implications for the wider corporate sector and firms' financial decisions, we find that the NBP and RPS laws provide potentially the most relevant settings. As discussed in Section 2.2 of the manuscript, the NBP raised electricity prices by up to 9.8% for business users, including manufacturing firms, the subject of our study. Under RPS programs, retail electricity suppliers incurred significant direct and indirect costs of renewable energy production, which they partially passed on to their customers. Greenstone and Nath (2020) find that, in the industrial sector, electricity prices rose by 10.5% seven years after the passage of these laws. The impact of the remaining policies on electricity prices and hence the corporate sector is less clear, however. First, although EMR was designed to deregulate electricity markets and reduce retail electricity prices, several studies question its effectiveness (e.g., Borenstein 2002, Knittel and Roberts 2005). Regarding the role of EERS, while their effect on electricity consumption and carbon emissions is well documented (e.g., Yi 2015), little is known about whether and how EERS affect energy prices and corporate behavior. Similarly, to the best of our knowledge, there is scarcely any evidence on NM, GPP programs, and PBFs having implications for the corporate sector. For example, NM is unlikely to directly and systematically affect manufacturing industries' electricity input, because it applies primarily to small-scale generation (Forsyth et al. 2002). Similarly, although PBFs impose a small surcharge on electricity prices to fund various green programs to promote energy efficiency and education and energy support for low-income households, the program's impact on electricity prices is likely to be modest compared to other programs, such as the NBP and RPS laws. For instance, the Illinois Renewable Energy Resources Trust Fund raised funds by charging $37.50 per month to large nonresidential electric services (Greenstone and Nath 2020).

Finally, as mentioned, the source of the policy shock is preferably a court ruling, that is, a judicial decision based on the merits of a specific court case, rather than a legislative or regulatory decision, which is more likely to be affected by political pressure and lobbying. This requirement offers a cleaner identification strategy based on difference-in-differences analysis. To the best of our knowledge, of all the policies under consideration, the NBP is the only one that meets this criterion, namely, a policy implemented following a court ruling (i.e., the case of *Michigan et al. v. EPA*). As discussed in Section 3.3 of the manuscript, the fact that this court ruling was unexpected further makes the NBP a relevant setting, since the policy shock is more likely to be exogenous. Meanwhile, the remaining electricity policies were adopted as the result of state legislation (RPS, EMR, EERS, NM, GPP programs, and PBFs), state public utility commissions' rules (EERS and NM), executive/regulatory orders (GPP programs and PBFs), or a combination of these actions; see Nadel and Kushler (2000), Forsyth et al. (2002), Nadel (2006), Fabrizio et al. (2007), Lyon and Yin (2010), Palmer et al. (2013), Davies and Carley (2017), and Greenstone and Nath (2020) for more details.

Overall, for the purpose of our study, the NBP provides arguably the most appropriate experiment to examine how climate policy affects corporate behavior. It meets all four main criteria required for our analysis (for a summary of our comparison, see Table OA3). Among the remaining policies, RPS laws also have merits, such as a strong impact on carbon emissions and possibly the wider corporate sector, making it an interesting setting for future research on climate policy risk too.

Online Appendix OA4. The Boundary between the Treated and Control Regions

Since the 1990s, the United States has experienced a series of electricity restructuring in both the wholesale and retail markets in several states. In this Online Appendix, we discuss those developments, with a focus on the wholesale markets, which are most relevant to the manufacturing users in our study.

As part of the restructuring, regulators decoupled the generation and distribution functions of previously vertically integrated power utilities. The restructured markets allow generators to trade electricity among themselves, with retail suppliers, and with other market participants before electricity is eventually sold to end-users. Independent system operators (ISOs) are responsible for monitoring the electricity system and providing suppliers with non-discriminatory network access to the market.

Electricity prices are set within ISOs. All ISOs employ a pricing scheme called locational marginal pricing (LMP) to set prices for electricity at each node. In general, electricity prices consist of three parts: an energy component, a loss component, and a congestion component. The energy component is the plant generation cost. The loss component represents thermal loss costs, which causes small price differences at each node. The congestion component represents the price increase due to operational limitations imposed on the transmission grid (such as voltage limits, thermal limits, and stability limits). ISOs and generators are required to respect transmission constraints to avoid exceeding limits and causing a blackout. Congestion costs arise when ISOs have to dispatch (i.e., use on demand) higher-cost generation rather than low-cost generation, which would otherwise be dispatched in the absence of transmission constraints. The presence of congestion costs, coupled with different institutional designs and rules for adjacent ISOs, create seams between neighboring electricity markets. Accordingly, each ISO imposes a fee for trading electricity outside its borders (Flores-Espino et al. 2016). Overall, several factors, including administrative obstacles, the costs of coordinating electricity flows across ISOs, and transmission constraints, limit manufacturing firms' access to the electricity markets of adjoining ISOs. As confirmed in recent research (Curtis 2018), the impact of the NBP on electricity prices is unlikely to spread across ISO boundaries.

Figure OA1 maps the NBP affected regions based on Figure 1 in the main text. The green area is the Midcontinent ISO (MISO) region. The purple area represents the ISO in New England (ISO-NE) region. Manufacturers that purchase electricity from the markets operated by the MISO or the ISO-NE are also likely to be affected by the NBP even they did not locate in the NBP-compliant region. Figure OA2 maps the MISO region (the green area). We use state borders instead of ISO borders to select the treated sample. The reason is that the boundaries of ISOs are determined by the utilities rather than a fixed set of geographic regions. Since there is no clear geographic discontinuity for ISOs, using ISO border is short of being an appropriate methodology to identify the impact of the NBP (Curtis 2018).

Online Appendix OA5. Additional Analysis and Robustness Checks

OA5.1. The NBP's Effect on Electricity Prices

Table OA1 uses utility-level data and reports the estimates of the NBP's effect on electricity prices. We find a 9.1%–9.8% increase in electricity prices following the implementation of the NBP.

OA5.2. Robustness: Standard Errors Clustered at Different Levels

Table OA4 presents the baseline results as in columns (4) and (8) of Table 2 in the manuscript, with standard errors clustered at the state level, the NBP region-industry level, and the state-industry level, respectively. Industries are defined by the 4-digit SIC code.

OA5.3. Robustness: Is the NBP's Adoption an Exogenous Policy Shock?

In Table OA5, we directly examine whether state-level factors affect the likelihood of the NBP's implementation. Following Serfling (2016), we use a Cox proportional hazard model, where a failure event is the NBP's implementation in 2004. In columns (1) and (2) of Table OA5, we only include company sales-weighted book and market leverage, respectively, aggregated at the state level. The insignificant coefficients on leverage suggest that the aggregated firm leverage at the state level does not influence the NBP's adoption. Column (3) includes three predictors relating to state-level pollutant emissions, namely the NO_x, SO_2, and carbon emission rates. Consistent with the purpose of the NBP to contain ozone transport from upwind states to downwind states, the coefficient on the NO_x emission rate is significantly positive at the 10% level, whereas the coefficients on the SO_2 and carbon emission rates are insignificant. In column (4), we add several state-level economic and political variables, including energy intensity, the GDP growth rate, unemployment rate, GDP per capita, and political balance ratio. We find that none of these state-level factors is related to the adoption of the NBP. Columns (5) and (6) further include the sales-weighted book and market leverage ratios aggregated at the state level, respectively. Again, the results are generally consistent with those reported in columns (1)–(4). Overall, the evidence in Table OA5 suggests that state-level economic and political factors as well as aggregate firm leverage are unlikely to affect the NBP's adoption in 2004.

OA5.4 Robustness: Controlling for Potentially Confounding Regulations

In Table OA6, we account for several energy industry regulations that we have identified earlier and may have an impact on electricity consumption and pricing during our sample period, namely, RPS laws, EMR, EERS, NM, and PBFs.[34] In so doing, we address the concern that these policies may confound our main inference based on the NBP's implementation.

As discussed in Section 2.3 and Online Appendix OA3, among the most relevant electricity policies are RPS laws, which led to an increase in electricity prices by 10.5% seven years after the passage of these laws. Nevertheless, the impact of RPS laws on the financing decisions of manufacturers in NBP-compliant states is likely to be limited. Table OA2 shows that only four of the 11 treated states (in bold), namely Illinois, Michigan, Ohio, and North Carolina, were affected by these laws, and only toward the end of our sample period (i.e., 2007 and 2008). Our discussion in Online Appendix OA3 suggests that the impact of the remaining four state-level electricity policies on corporate behavior during our sample period is either ambiguous or limited. In short, a priori, our main inference is unlikely to be affected by these regulatory interventions.

Nevertheless, in additional empirical analysis, we control for the potential impact of the above electricity policies, following Greenstone and Nath (2020). In each model presented in columns (1) to (10) of Table OA6, we add to our DID specification an indicator variable representing the passage of each of the five potentially confounding policies. Across those models, the impact of the NBP on manufacturers' capital structure remains negative and significant. In columns (11) and (12), the results persist after we control for all policies in the same specification. Overall, the analysis suggests that our statistical inferences are not affected by these policies.[35]

[34] As mentioned in Section 3.3, GPP programs were adopted by six northeastern (downwind) states from 2001 to 2015. Thus, our research design is unlikely to be confounded by this because these states are excluded from our treated and control samples.
[35] In further analysis (untabulated), we examine whether and how these five policies affect the two channels (i.e., operating leverage and distress risk) that drive our main finding. We estimate the same DID specification as in Table OA6, with operating leverage or distress risk as the dependent variable. We document mixed and weak effects. Together with the analysis in Table OA6, the results suggest that these policies have a limited impact on firms' financing decisions and hence are unlikely to confound our statistical inferences.

OA5.5. Univariate Analysis of the NBP's Effect

Table OA7 reports univariate analysis comparing the differences (before vs. after the NBP's implementation, and the treated group vs. the control group) in book leverage (Panel A) and market leverage (Panel B). The results of the univariate analysis are consistent with our baseline findings.

OA5.6. Further PSM and EB Results

In Table OA8, we check the condition of covariate balance between the treated and control firms. Panels A and B report the results for the PSM and EB samples, respectively. Under both matching schemes, the treated and control firms exhibit no significant difference in their covariates.

In Table OA9, we conduct two tests to establish the robustness of our PSM and EB analysis. First, in columns (1)–(2) and (3)–(4) of Panel A, we perform EB to match firms on the first moment (i.e., the mean) and on all the three moments (i.e., the mean, variance, and skewness), respectively. Across all models, the NBP's effect on capital structure remains negative and significant. A noticeable trade-off exists, however, between the size of the balanced sample and the number of moments used therein. Overall, the results from our robustness check suggest that matching on two moments, as employed in the main EB analysis, is a reasonable compromise given the tradeoff between the sample size and the stringency of the matching criteria (i.e., the number of moments). Importantly, the findings show that the treatment effect remains qualitatively the same, regardless of the matching choices.

As a second robustness check, we repeat our PSM and EB analysis using a different industry classification, namely, two-digit SIC codes instead of the EIA industry classification used in Table 4 in the main body of the paper.[36] In Panel B of Table OA9, we find that the NBP's impact remains significant and negative across all models. In further robustness tests (untabulated), we find that our PSM and EB analysis is insensitive to the use of alternative calipers (0.01, 0.025, and 0.05) and tolerance levels (0.025, 0.05, and 0.1).

[36] A drawback to matching or balancing within two-digit SIC codes is that, given the increase in the number of clusters and the resulting decrease in cluster size, the size of the propensity score matched and entropy balanced samples decreases substantially, for the same reason as articulated above.

OA5.7 Placebo Tests

Although we have controlled for several time-varying and time-invariant factors at various levels, it is still possible that our results are driven by unobserved shocks that coincided with the implementation of the NBP. To alleviate this concern, we perform two placebo tests. First, in Table OA10, we conduct a placebo test based on a sample of non-manufacturing firms, consisting of Compustat firms in non-manufacturing industries, as well as those in the commercial and transportation sectors. Since these non-manufacturing firms have lower electricity intensities than their manufacturing counterparts, we expect the NBP's impact on their capital structure to be less pronounced. The results reported in Table OA10 support this prediction.

In Table OA11, we present the results from another placebo test using a group of pseudo (treated) firms. We use firms headquartered in randomly selected control states as the pseudo treated group and firms headquartered in the remaining control states as the control group. This approach enables us to pick up any unobserved common shocks in 2004 that might drive our baseline results. The results show an insignificant relationship between the pseudo treatment event and financial leverage, suggesting that our baseline results are unlikely to be driven by confounding effects relating to broad unobserved events.

OA5.8 Robustness: Using Plant Locations

In Table OA12, we repeat our baseline analysis, identifying our treated and control firms based on plant locations rather than firm headquarters locations. In our baseline tests in the main body, we use the CRSP–Compustat merged database on firm historical headquarters locations to proxy for manufacturers' exposure to the NBP's implementation. Our rationale is similar to that of Serfling's (2016) in that the bulk of a firm's operations should be close to its headquarters. However, electricity price shocks may be specific to the locations where manufacturing firms set up their plants. To alleviate this concern, we hand-collect data on manufacturing plant locations from the 10-Ks for 1,433 manufacturing firms in 2003 and use this data to redefine the treated and control groups.[37] We set the

[37] Firms usually disclose the locations of their manufacturing plants, corporate offices, distribution centers, warehouses, and research and development facilities in counties, states, or countries in "Item 2. Properties" of their 10-K filings.

indicator, *NBP Operations*, to one if the firm had manufacturing plants only in the 11 NBP-compliant states in 2003, and zero if the firm had all plants outside the NBP regions or had no recorded plant at all in the same year. We exclude firms with plants in both NBP-compliant and non-compliant states from our sample. Table OA12 reports the regression results using our hand-collected 10-K data and the full set of control variables as in columns (4) and (8) of Table 2 of the manuscript. The coefficients on *NBP Operations* × *Post* are significantly negative in both the book and market leverage regressions, suggesting that our main results are robust to using plant locations data.

OA5.9. Robustness: Alternative Measures of Financial Leverage

In Table OA13, we check the robustness of our main results using alternative measures of financial leverage. Following recent capital structure studies (e.g., Serfling 2016), we adopt several measures, including the natural logarithm of total debt, net leverage, and leverage with leases. Using the natural logarithm of the total debt as an alternative measure of financial leverage alleviates the concern that an increase in a firm's total assets might cause the debt ratio to decrease.[38] Because firms may hoard liquid assets (e.g., cash) in response to increased energy input costs, we next use the net book and market leverage to account for this potential effect on liquid assets. Since ignoring operating lease commitments may underestimate the magnitude of financial leverage (Eisfeldt and Rampini 2009, Rauh and Sufi 2012), we also use the book and market leverage with leases as alternative measures to deal with this concern. Table OA13 reports the regression results based on these five additional measures of financial leverage. All results report significant and negative coefficients on *NBP* × *Post*, suggesting that the negative impact of the NBP's implementation on corporate capital structure is insensitive to alternative definitions of financial leverage.

OA5.10. Robustness: Additional Control Variables and Fixed Effects

In Table OA14, we add several additional control variables to our preferred specifications: 1) cash holdings (*Cash Holdings*), which accounts for firms' precautionary motive for holding cash due to the risks related to the NBP, 2) the total payout ratio (*Payout Ratio*), which controls for the potential

[38] We also find that using the natural logarithm of one plus the total value of debt yields qualitatively similar results.

changes in corporate payout policies that may also be affected by the operating leverage channel, and 3) investment-related factors, namely, total investment (*Total Investment*) and the natural logarithm of the number of employees (*Log Employment*), which control for potential investment opportunities and decisions in response to the NBP's adoption (Borghesi et al. 2015, Calel and Dechezlepretre 2016, Curtis 2018). These additional control variables do not alter our baseline results.

Table OA15 reports the results controlling for state × industry and industry × year fixed effects (industries defined at the four-digit SIC code level). These fixed effects mitigate the concern that time-invariant characteristics across state-industries and time-variant industry-level factors may explain our results. We find that our main inference is robust to these additional fixed effects.

OA5.11. Robustness: Various Subsamples Excluding Potentially Confounding Observations

Table OA16 reports additional robustness tests based on several subsamples that exclude certain observations that may confound our analysis. In columns (1) and (2), we exclude the financial crisis period (i.e., 2007 and 2008) from our sample and report the regression results. Our baseline findings remain unchanged.

In columns (3) and (4) of Table OA16, we exclude the directly regulated manufacturers. Curtis (2018) shows that less than 2% of manufacturing plants operated in-house emission sources and thus were regulated directly by the NBP. While this is a small fraction, there might be a concern that our results are driven by those regulated firms, who were directly affected by the program. To account for this possibility, we use hand-collected data on regulated manufacturing plants from the NBP Compliance and Environmental Results Reports from 2003 to 2007. We identify 68 regulated manufacturing firms, 27 of which are present in our sample. We then exclude these 27 regulated firms and re-estimate our regressions. Our baseline result persists.

In columns (5) and (6) of Table OA16, we exclude the firms that relocated their headquarters during our sample period. Concerned about high electricity prices, firms may have responded to the NBP's implementation by relocating their headquarters and operations to other states (Bartram et al. 2021). This would, however, bias against finding the hypothesized treatment effect. We find that during our sample period from 2000 to 2008, around 7% (i.e., 140 out of 1,974) of manufacturing firms relocated their headquarters at least once. We drop these 140 firms from our sample. Our baseline finding remains unchanged, suggesting that our inference is robust to relocation risks.

We also follow Klasa et al. (2018) to discard the observations with positive foreign income or foreign taxes, because including the firms with international operations may confound the treatment effect of the NBP on financial leverage. We present the regression results in columns (7) and (8) of Table OA16. Our baseline result still holds.

Because the states of Michigan and Alabama were not covered completely by the NBP in 2004, we drop them from our sample and report the results in columns (9) and (10) of Table OA16. Our baseline result is not affected.

Manufacturers headquartered in California and Texas constitute almost half of our sample observations (i.e., 5,680 of 12,454), which raises a concern that these two non-NBP states alone might drive our baseline finding. We discard the manufacturing firms headquartered in California and Texas and re-estimate the regressions. Our baseline result continues to hold in columns (11) and (12) of Table OA16.

In columns (13) and (14) of Table OA16, we follow Crane and Koch (2018) to remove the event year 2004 from our sample. Our baseline results are robust to this alteration.

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Figure OA1. NBP Affected Regions

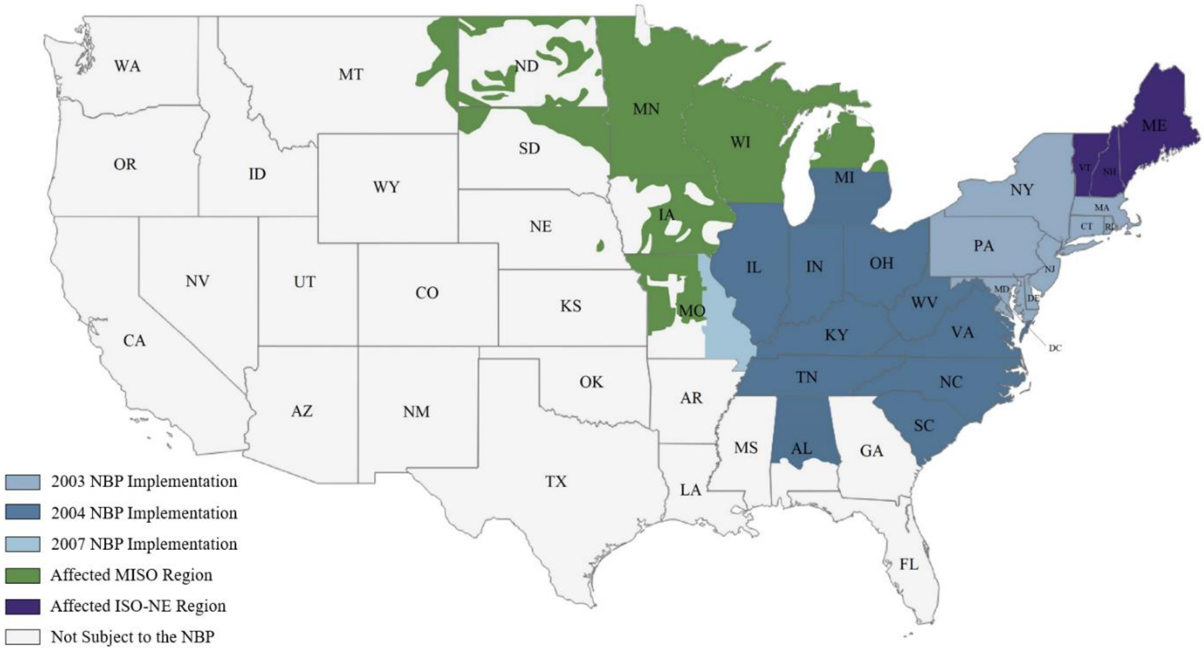

Figure OA2. Midcontinent Independent System Operator (MISO) Region

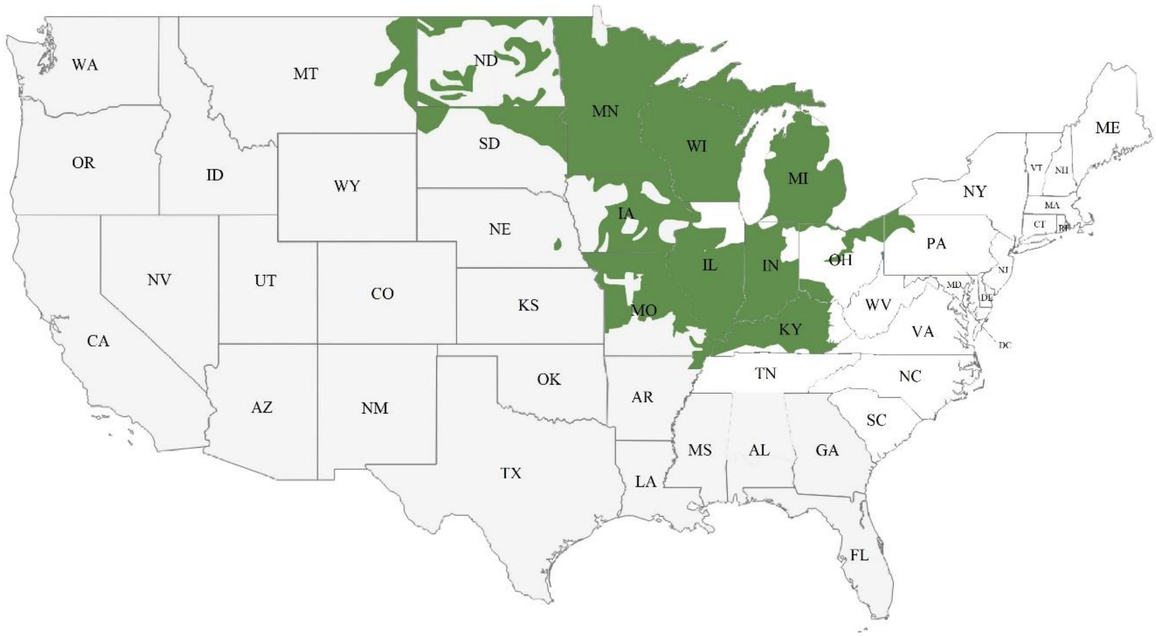

Table OA1. The NBP's Implementation and Electricity Prices

This table reports the results from the regressions relating electricity prices to the NBP's implementation based on the U.S. power utilities from 1999 to 2008. We use the utility-year electricity data constructed from the Energy Information Administration (EIA) form 861. The variable *NBP* is an indicator variable equal to one if a utility serves its industrial customers in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. Following Curtis (2018), we interact the average annual prices of three types of fuels (i.e., coal, oil, and natural gas; variables named *CoalPrice*, *OilPrice*, and *NatGasPrice*, respectively) with the percent of electricity derived from each type of fuel in the North American Electric Reliability Corporation (NERC) region (variables named *PerCoal*, *PerOil*, and *PerNatGas*, respectively), and include these three interaction terms in the regression. Observations are weighted by their 1998 revenue to ensure that the utilities with little or no electricity sales do not drive the results. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the utility level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)
	ln(Electricity Price)	ln(Electricity Price)
NBP × *Post*	0.098*	0.091*
	(1.66)	(1.65)
PerCoal × CoalPrice	0.076***	0.087***
	(4.35)	(6.15)
PerOil × OilPrice	0.012***	0.014***
	(5.45)	(8.79)
PerNatGas × NatGasPrice	0.009	0.039
	(0.18)	(1.27)
Utility Fixed Effects	No	Yes
State Fixed Effects	Yes	No
Year Fixed Effects	Yes	Yes
East/West Trends	Yes	Yes
Observations	13,391	13,391
R-Squared	0.466	0.732

Table OA2. The Adoption of Other Relevant State-Level Electricity-Sector Policies

This table reports the years in which the six policies, namely renewable portfolio standards (RPS) laws, electricity market restructuring (EMR), energy efficiency resource standards (EERS), net metering (NM), green power purchasing (GPP) programs, and public benefit funds (PBFs), were first adopted for each state. The state names in boldface also adopted the NBP. The number following the term *Sus.* is the year that a state suspended the corresponding policy. Data on RPS laws are based on the work of Greenstone and Nath (2020). Data on EMR are based on the work of Fabrizio et al. (2007) for the period up to 1999 and from Craig and Savage (2013) for the period after 1999. Data on EERS are from the American Council for an Energy-Efficient Economy's State and Local Policy Database. Data on NM, GPP, and PBFs are from the North Carolina Clean Technology Energy Center's Database of State Incentives for Renewables & Efficiency (DSIRE).

State	RPS	EMR	EERS	NM	GPP	PBF
Alabama						
Alaska			2010	2010		
Arkansas			2010	2001		
Arizona	2001		2010	2008		
California	2002	1998 (Sus. 2001)	2006	1995		1996
Colorado	2004		2007	2004		
Connecticut	1998	2000	2011	1998	2004	1998
Delaware	2005	2001	2009	1999		1999
D.C.	2005	2001	2008	2000		1999
Florida				2008		
Georgia				2001		
Hawaii	2004		2009	2001 (Sus. 2015)		2006
Idaho						
Illinois	**2007**	**1999**	**2007**	**2007**		**1997**
Indiana				**2004**		
Iowa	1991		2008	1984		
Kansas	2009			2009		
Kentucky				**2004**		
Louisiana				2003		
Maine	1999	2000	2009	1998 (Sus. 2017)	2003	1997
Maryland	2004	2000	2008	1997	2001	
Massachusetts	2002	1998	2008	1982	2007	1997

Michigan	**2008**	**2001**	**2008**	**2008**		**2001**
Minnesota	2007		2007	1983		1999
Mississippi				2015		
Missouri	2008		2009	2007		
Montana	2005			1999		1997
Nebraska				2009		
Nevada	1997		2005	1997		
New Hampshire	2007	1998	2016	1998		1996
New Jersey	1999	1999	2018	1999		1999
New Mexico	2004		2008	2008		2005
New York	2004	1998	2007	1997		1996
North Carolina	**2007**		**2008**	**2005**		
North Dakota				1991		
Ohio	**2008**	**2001**	**2008**	**1999**		**1999**
Oklahoma				1988		
Oregon	2007		2016	1999		1999
Pennsylvania	2004	1999	2008	2004		1999
Rhode Island	2004	1998	2006	2011	2015	1996
South Carolina				**2015**		
South Dakota						
Tennessee						
Texas	1999	2002	1999			
Utah				2017		
Vermont			1999	1998		1999
Virginia		**2002 (Sus. 2007)**	**2007**	**2009**		
Washington	2006		2006	1998		
West Virginia				**2009**		
Wisconsin	1999		2006	1982	2006	2007
Wyoming				2001		

Table OA3. Summary of Policy Comparison

This table provides a summary of a comparison of the NBP (the 2004 adoption) and the six electricity policies, namely, renewable portfolio standards (RPS) laws, electricity market restructuring (EMR), energy efficiency resource standards (EERS), net metering (NM), green power purchasing (GPP) programs, and public benefit funds (PBFs), based on the criteria set out in Online Appendix OA3.

	NBP	RPS	EMR	EERS	NM	GPP	PBFs
Climate Policy	Yes	Yes	No	Yes	Yes	Yes	Yes
Impact on Emissions	Yes	Yes	Yes	Yes	Unclear	Unclear	Yes
Impact on Corporate Sector	Yes	Yes	Unclear	Unclear	Unclear	Unclear	Unclear
Policy Shock by Court Ruling	Yes	No	No	No	No	No	No

Table OA4. Baseline Results with Standard Errors Clustered at Different Levels

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. Continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the state, NBP region–industry (four-digit SIC level), and state–industry level (four-digit SIC level), respectively (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage			Market Leverage		
	(1)	(2)	(3)	(4)	(5)	(6)
NBP × *Post*	–0.033***	–0.033**	–0.033***	–0.033**	–0.033**	–0.033***
	(–3.78)	(–2.47)	(–3.00)	(–2.35)	(–2.48)	(–2.79)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Clustering	State	NBP Region-Ind	State-Ind	State	NBP Region-Ind	State-Ind
Observations	12,454	12,454	12,454	12,454	12,454	12,454
R-Squared	0.746	0.746	0.746	0.753	0.753	0.753

Table OA5. Determinants of the NBP's Implementation

This table reports the results from a Cox proportional hazard model, where a "failure event" is the NBP's implementation in a state. The sample period is from 2000 to 2008. The observations of a state are excluded from the sample after the NBP's implementation in 2004. Independent variables are measured as of year $t-1$. Online Appendix OA1 provides all the variable definitions. All the independent variables, except indicator variables, are standardized to have a mean of zero and a standard deviation of one. Dollar values are expressed in 1999 dollars. Standard errors are clustered at the state level (z-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Sales-Weighted State Book Leverage	0.053				0.197	
	(0.32)				(0.75)	
Sales-Weighted State Market Leverage		0.110				0.091
		(0.58)				(0.48)
State NO_x Emission Rate			1.441*	1.418*	1.400	1.455*
			(1.71)	(1.73)	(1.61)	(1.66)
State SO_2 Emission Rate			–0.317	–0.460	–0.421	–0.468
			(–0.58)	(–0.88)	(–0.77)	(–0.87)
State Carbon Emission Rate			–0.383	–0.309	–0.291	–0.316
			(–0.91)	(–0.74)	(–0.67)	(–0.73)
State Energy Intensity				0.187	0.099	0.185
				(0.45)	(0.22)	(0.46)
State GDP Growth				–0.651	–0.561	–0.611
				(–1.38)	(–1.08)	(–1.22)
State Unemployment Rate				0.502	0.463	0.491
				(0.97)	(0.92)	(0.94)
State GDP Per Capita				–0.719	–0.794	–0.667
				(–1.19)	(–1.28)	(–1.16)
Political Balance				–0.436	–0.363	–0.465
				(–1.54)	(–1.19)	(–1.62)
Observations	415	415	397	387	387	387

Table OA6. Controlling for Electricity Market Programs

This table reports the results of the OLS regressions estimating the NBP's effects on financial leverage, for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. *RPS* is an indicator variable set to one if a firm is headquartered in a state that has adopted the renewable portfolio standards (RPS) laws in a given year, and zero otherwise. *EMR* is an indicator variable set to one if a firm is headquartered in a state that has experienced electricity market restructuring (EMR) in a given year, and zero otherwise. *EERS* is an indicator variable set to one if a firm is headquartered in a state that has adopted the energy efficiency resource standards (EERS) in a given year, and zero otherwise. *NM* is an indicator variable set to one if a firm is headquartered in a state that has adopted a net metering (NM) policy in a given year, and zero otherwise. *PBFs* is an indicator variable set to one if a firm is headquartered in a state that has adopted public benefit funds (PBFs) in a given year, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in the 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) Book Leverage	(2) Market Leverage	(3) Book Leverage	(4) Market Leverage	(5) Book Leverage	(6) Market Leverage	(7) Book Leverage	(8) Market Leverage	(9) Book Leverage	(10) Market Leverage	(11) Book Leverage	(12) Market Leverage
NBP × Post	–0.032***	–0.031***	–0.031***	–0.030***	–0.030***	–0.031***	–0.031***	–0.031***	–0.032***	–0.034***	–0.027***	–0.025**
	(–3.11)	(–2.87)	(–3.02)	(–2.84)	(–2.99)	(–2.93)	(–3.04)	(–2.89)	(–3.18)	(–3.25)	(–2.62)	(–2.32)
RPS	0.007	0.023***									–0.001	0.017**
	(0.94)	(3.23)									(–0.10)	(2.20)
EMR			–0.014*	–0.023***							–0.010	–0.015**
			(–1.88)	(–3.33)							(–1.32)	(–2.00)
EERS					0.022***	0.021***					0.022**	0.014*
					(2.71)	(2.62)					(2.56)	(1.74)
NM							–0.009	–0.014			–0.008	–0.018*
							(–1.03)	(–1.45)			(–0.87)	(–1.73)
PBFs									–0.014	0.031	–0.008	0.039
									(–0.54)	(1.27)	(–0.29)	(1.56)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454	12,454
R-Squared	0.746	0.754	0.746	0.754	0.746	0.754	0.746	0.753	0.746	0.753	0.746	0.754

Table OA7. Univariate Analysis

This table reports the mean and the *t*-statistics of mean differences in *Book Leverage* and *Market Leverage* between the treated and the control groups in the pre- and post-NBP years. For the definitions of all the variables and the details of their construction, see Online Appendix OA1. All the continuous variables are winsorized at their first and 99th percentiles *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Book Leverage				
	Treatment Group (TG)	Control Group (CG)	Mean Difference (TG−CG)	*t*-stat.
Pre-NBP Period	0.284	0.223	0.062***	7.97
Post-NBP Period	0.243	0.217	0.026***	3.45
Mean Difference (Post−Pre)	−0.041***	−0.006	**−0.035*****	**−3.22**
t-stat.	−5.02	−1.01		

Panel B: Market Leverage				
	Treatment Group (TG)	Control Group (CG)	Mean Difference (TG−CG)	*t*-stat.
Pre-NBP Period	0.336	0.188	0.148***	19.24
Post-NBP Period	0.251	0.149	0.102***	16.23
Mean Difference (Post−Pre)	−0.084***	−0.038***	**−0.046*****	**−4.66**
t-stat.	−8.59	−8.11		

Table OA8. Comparison of Firm Characteristics for the Propensity Score Matched (PSM) and Entropy Balanced (EB) Samples

Panel A of this table tabulates the means of the propensity scores and the firm-level control variables across the treated and control groups in the propensity score matched sample used in Table 4 of the manuscript. Panel B tabulates the mean, variance, and skewness of firm characteristics for the treated and control groups of the entropy balanced sample used in Table 4 of the manuscript. For the definitions of all the variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in the 1999 dollars. The *p*-values in Panel A are based on two-sample *t*-tests. In Panel B, for the difference in means, we calculate the significance levels based on *t*-tests, whereas, for the difference in variances we use *F*-tests. *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Propensity Score Matched Sample

	Control Group (Obs.=261)	Treated Group (Obs.=261)	Mean Difference	*p*-value
Propensity Score	0.290	0.290	0.001	0.952
Log Total Assets	5.059	4.935	0.123	0.561
Market-to-Book	3.206	3.431	–0.225	0.727
ROA	–0.129	–0.110	–0.018	0.857
Fixed Assets	0.234	0.232	0.002	0.889
Dividend Payer	0.276	0.284	–0.008	0.846

Panel B: Entropy Balanced Sample

	Control Group (Obs.=798)		Treated Group (Obs.=217)		Difference in Means	Difference in Variances
	Mean	Variance	Mean	Variance		
Log Total Assets	5.167	7.343	5.175	7.302	–0.007	0.041
Market-to-Book	3.735	84.018	3.732	83.366	0.002	0.652
ROA	–0.149	1.520	–0.149	1.507	0.000	0.012
Fixed Assets	0.231	0.025	0.231	0.024	0.000	0.000
Dividend Payer	0.350	0.228	0.350	0.229	0.000	–0.001

Table OA9. Entropy Balancing (EB) and Propensity Score Matching (PSM) Analysis:

Additional Robustness Checks

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation, using alternative propensity score matched and entropy balanced samples. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. In Panel A, we balance the treated and control firms in 2003 using all firm-level controls, namely, *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, and *Dividend Payer*, within each EIA manufacturing industry cluster (as depicted in Figure R1). In columns (1) to (2) of Panel A, we balance the treated and control firms on the first moment (i.e., the mean), whereas in columns (3) and (4) of Panel A, we balance the treated and control firms on all three moments (i.e., the mean, variance, and skewness). In Panel B, we repeat the same procedure as in Table 4 of the manuscript, but, instead of using the EIA manufacturing industry classification (as depicted in Figure 2 of the manuscript), we use the two-digit SIC industry classification. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Balancing on EIA Manufacturing Industry Classification

	Entropy Balancing on the First Moment		Entropy Balancing on All Three Moments	
	(1)	(2)	(3)	(4)
	Book Leverage	Market Leverage	Book Leverage	Market Leverage
NBP × *Post*	–0.025**	–0.028**	–0.030**	–0.026*
	(–2.21)	(–2.39)	(–2.17)	(–1.75)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	9,841	9,841	7,626	7,626
R-Squared	0.738	0.767	0.752	0.774

Panel B: Matching and Balancing on Two-digit SIC Industry Classification

	Propensity Score Matching		Entropy Balancing	
	(1)	(2)	(3)	(4)
	Book Leverage	Market Leverage	Book Leverage	Market Leverage
NBP × *Post*	–0.041**	–0.049***	–0.063***	–0.054***
	(–2.35)	(–2.67)	(–2.92)	(–2.81)
Control Variables	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	2,807	2,807	4,414	4,414
R-Squared	0.764	0.759	0.741	0.701

Table OA10. Placebo Test 1:

The NBP's Effect for the Sample without Manufacturing Industries

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat firms in non-manufacturing industries from 2000 to 2008. Specifically, the sample includes industrial firms in non-manufacturing industries, as well as those in two non-manufacturing sectors, namely, commercial and transportation. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in the 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) Book Leverage	(2) Market Leverage
NBP × *Post*	–0.016	–0.017
	(–1.44)	(–1.45)
Control Variables	Yes	Yes
Firm Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	14,702	14,702
R-Squared	0.750	0.747

Table OA11. Placebo Test 2:

The NBP's Effect for Randomly Assigned Treated and Control Groups

This table reports the placebo test results using randomly assigned treated and control groups. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *Pseudo Treatment* is an indicator variable equal to one if a firm is headquartered in a randomly assigned treatment state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets, Market-to-Book, ROA, Fixed Assets, Dividend Payer, Default Spread, State GDP Growth*, and *Political Balance*. The coefficients on all control variables are omitted for brevity. For the definitions of all the control variable and the details of their construction, see Online Appendix OA1. Continuous variables, except for macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in the 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) Book Leverage	(2) Market Leverage
Pseudo Treatment × Post	−0.005	−0.009
	(−0.41)	(−0.68)
Control Variables	Yes	Yes
Firm Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	12,454	12,454
R-Squared	0.745	0.753

Table OA12. The NBP's Effect Estimated Using the Locations of Manufacturing Plants

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP Operations* is an indicator variable equal to one if a firm only had manufacturing plants in the treated region, and zero if it only had manufacturing plants outside of the treated region or it did not own or lease any manufacturing plants in 2003. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. Continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1) Book Leverage	(2) Market Leverage
NBP Operations × *Post*	–0.031*	–0.052**
	(–1.81)	(–2.48)
Control Variables	Yes	Yes
Firm Fixed Effects	Yes	Yes
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
Observations	6,965	6,965
R-Squared	0.736	0.705

Table OA13. Alternative Measures of Financial Leverage

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables in columns (1) to (5) are as follows: *Log (Total Debt)* is the natural logarithm of the book value of long-term debt plus debt in current liabilities and is expressed in 1999 dollars; *Net Book (Market) Leverage* is the book value of long-term debt plus debt in current liabilities less the book value of cash and short-term investments divided by the book value of total assets (market value of debt and equity); *Book (Market) Leverage with Leases* is the book value of long-term debt plus debt in current liabilities plus the value of leases (xrent*10) divided by the book value of total assets plus the value of leases (market value of debt and equity plus the value of leases). In columns (4) and (5), we adjust all the control variables by adding the value of leases (xrent*10) to the book value of total assets. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. The continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Log (Total Debt)	Net Book Leverage	Net Market Leverage	Book Leverage with Leases	Market Leverage with Leases
NBP × *Post*	–0.184**	–0.048***	–0.041***	–0.029***	–0.034***
	(–2.25)	(–3.46)	(–2.70)	(–3.20)	(–3.13)
Control Variables	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	9,879	12,454	12,454	11,403	11,403
R-Squared	0.890	0.807	0.728	0.773	0.775

Table OA14. Baseline Results with Additional Control Variables

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. In Columns (1), (3), (5), and (7), the control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. In Columns (2), (4), (6), and (8), the control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. Continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage				Market Leverage			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
NBP × *Post*	–0.031***	–0.032***	–0.028***	–0.026***	–0.032***	–0.032***	–0.030***	–0.028***
	(–3.09)	(–3.13)	(–2.81)	(–2.67)	(–3.07)	(–3.06)	(–2.87)	(–2.69)
Cash Holdings	–0.223***			–0.227***	–0.162***			–0.158***
	(–9.68)			(–9.74)	(–9.25)			(–8.80)
Payout Ratio		–0.006		0.004		–0.047***		–0.033**
		(–0.30)		(0.23)		(–3.49)		(–2.49)
Total Investment			0.002	0.005			–0.018***	–0.016***
			(0.28)	(0.52)			(–3.13)	(–2.67)
Log Employment			0.024***	0.008			0.006	–0.005
			(2.93)	(0.92)			(0.94)	(–0.76)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	12,454	12,427	12,014	12,014	12,454	12,427	12,014	12,014
R-Squared	0.754	0.746	0.750	0.758	0.758	0.753	0.763	0.767

Table OA15. Baseline Results with Additional Fixed Effects

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. State × industry and industry × year fixed effects are both defined at the four-digit SIC level. The coefficients on all the control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. Continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Book Leverage			Market Leverage		
	(1)	(2)	(3)	(4)	(5)	(6)
NBP × *Post*	–0.034**	–0.031***	–0.034**	–0.027**	–0.033***	–0.028**
	(–2.53)	(–2.94)	(–2.40)	(–2.18)	(–2.99)	(–2.17)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	No	No	Yes	No	No
Year Fixed Effects	No	Yes	No	No	Yes	No
State × Industry Fixed Effects	No	Yes	Yes	No	Yes	Yes
Industry × Year Fixed Effects	Yes	No	Yes	Yes	No	Yes
Observations	12,135	12,424	12,102	12,135	12,424	12,102
R-Squared	0.773	0.754	0.781	0.793	0.761	0.800

Table OA16. Additional Robustness Tests: Various Subsamples Excluding Potentially Confounding Observations

This table reports the results from the OLS regressions relating financial leverage to the NBP's implementation for Compustat manufacturing firms from 2000 to 2008. The dependent variables are *Book Leverage* and *Market Leverage*. The variable *NBP* is an indicator variable equal to one if a firm is headquartered in a treated state, and zero otherwise. *Post* is an indicator variable set to one for 2004 and later, and zero otherwise. The control variables include *Log Total Assets*, *Market-to-Book*, *ROA*, *Fixed Assets*, *Dividend Payer*, *Default Spread*, *State GDP Growth*, and *Political Balance*. The coefficients on all control variables are omitted for brevity. For the definitions of all the control variables and the details of their construction, see Online Appendix OA1. Continuous variables, except macroeconomic ones, are winsorized at their first and 99th percentiles, and the dollar values are expressed in 1999 dollars. Standard errors are clustered at the firm level (*t*-statistics are in parentheses). *, **, and *** denote significance at the 10%, 5%, and 1% levels, respectively.

	Excluding Financial Crisis Period of 2007–2008		Excluding Directly Regulated Firms		Excluding Relocating Firms		Excluding Firms with International Operations		Excluding Michigan and Alabama		Excluding California and Texas		Excluding the Implementation Year 2004	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
	Book Leverage	Market Leverage	Book Leverage	Market Leverage	Book Leverage	Market Leverage	Book Leverage	Market Leverage	Book Leverage	Market Leverage	Book Leverage	Market Leverage	Book Leverage	Market Leverage
NBP × *Post*	–0.030***	–0.036***	–0.031***	–0.033***	–0.028***	–0.031***	–0.041**	–0.045***	–0.032***	–0.040***	–0.027**	–0.026*	–0.036***	–0.032***
	(–3.05)	(–3.68)	(–3.03)	(–3.01)	(–2.69)	(–2.85)	(–2.21)	(–2.70)	(–3.04)	(–3.49)	(–2.01)	(–1.89)	(–3.14)	(–2.70)
Control Variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	9,871	9,871	12,279	12,279	11,500	11,500	6,999	6,999	11,919	11,919	6,756	6,756	11,028	11,028
R-Squared	0.776	0.797	0.746	0.750	0.744	0.763	0.747	0.734	0.746	0.743	0.751	0.766	0.750	0.755