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Waiting for the Courts: Effects of Policy Uncertainty on Pollution and Investment

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Abstract
Legal challenges and transitions of political power cause the future of regulatory policies to be uncertain. In this article, I investigate how uncertainty about environmental policy affects investment and emissions at coal-fired power plants. I exploit a legal challenge to the Clean Air Interstate Rule (CAIR) that created variation in the probability that individual plants would need to comply with the new policy. I use a difference-in-differences approach to compare pollution reductions at power plants located in states subject to more uncertainty to plants in states that that were not. I find that plants with a lower probability of being regulated invested in fewer capital-intensive pollution controls and reduced pollution by 13% less on average. Many of these plants did switch to capital-intensive pollution controls after the court upheld CAIR. Policy uncertainty increased compliance costs by $124 million by delaying efficient investments.

Keywords Policy uncertainty · Investment · Environmental regulation · Air pollution · Electricity

JEL Classification Q40 · Q52 · D22 · D92 · L94

1 Introduction

Most environmental regulations are subject to considerable uncertainty. Policy uncertainty can arise through several channels. Policies risk being changed after elections because a new administration can overhaul preexisting policies through executive orders and staff changes at regulatory agencies.[1] Policy uncertainty also stems from legal challenges of new federal agency rules. For example, in the United States, the Clean Power Plan, the Mercury and Air Toxics Standards, the Clean Air Interstate Rule, and the Oil and Natural

[1] For instance, after the 2016 presidential election, Donald Trump withdrew from the Paris Climate Agreement, scrapped the Clean Power Plan, approved orders to build major pipelines, and signed an order to remove regulations on wetlands and waterways that were introduced by the Obama administration.

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Gas Air Pollution Standards have all been challenged repeatedly in court. Periodic policy revisions create an additional source of uncertainty. As an example, the European Union Emissions Trading System has faced uncertainty over the program's emissions caps and about how permit trading rules may change over time. Policy uncertainty can make future market conditions less predictable and plausibly delays firms' investments.Policy uncertainty may have especially pernicious effects in environmental policymaking because these regulations often require significant capital investments, such as overhauls in electricity generation infrastructure. Delayed investment is particularly problematic because later investment strictly increases total pollution, which, for a stock pollutant like CO_2, could mean that we reach a strictly higher level of CO_2 in the atmosphere and therefore higher global temperatures.

Although economic theory suggests that uncertainty should cause firms to delay making irreversible investments (Bernanke 1983; McDonald and Siegel 1986; Pindyck 1988; Dixit and Pindyck 1994), we have limited empirical evidence on how policy uncertainty affects pollution abatement and investment.[2] Measuring the causal impacts of policy uncertainty is difficult for two reasons: 1) policy uncertainty is difficult to measure, and 2) in most cases, all firms in an industry or country are simultaneously exposed to policy uncertainty, so establishing a credible comparison group or counterfactual is difficult.

In this paper, I take advantage of a unique quasi-experiment to estimate how policy uncertainty affects pollution abatement and firm investment decisions. Specifically, I use the rollout of the EPA's Clean Air Interstate Rule (CAIR). The EPA announced CAIR in 2005 with the goal of further reducing sulfur dioxide (SO_2) emissions from coal-fired power plants in the Eastern United States starting in 2010.[3] After the EPA announced CAIR, several states and electric utilities filed lawsuits challenging the legality of the rule. To identify the effects of policy uncertainty on emissions and investment choices, I exploit variation created by a legal challenge levied by the states of Florida, Minnesota, and Texas, that were located on the border of the CAIR-regulated area. These "challenger" states argued that they should not be subject to the new policy because their geographic location meant that they did not significantly contribute to other states' noncompliance with National Ambient Air Quality Standards. As a result of the legal challenge, coal plants in these "challenger" states had to wait for the court to decide if they would actually have to comply with CAIR.[4]

This article's primary contribution is to provide empirical evidence of how firms react to environmental policy uncertainty. Although industry groups, politicians, and media often suggest that policy uncertainty can be a drag on investment and economic growth, few studies have provided empirical evidence supporting this theory.[5]

I show that coal plants exposed to more uncertainty due to the legal challenge were less likely to invest in capital-intensive technologies like flue-gas desulfurization systems (commonly referred to as scrubbers). Plants located in "challenger" states were instead more

[2] There is a recent empirical literature that investigates the effects of policy uncertainty (Born and Pfeifer 2014; Fernandez-Villaverde et al. 2015; Baker et al. 2016; Gulen and Ion 2015). I use a different identification strategy than these related studies because I can leverage quasi-experimental variation in exposure to policy uncertainty. I discuss the existing literature in more detail below.

[3] SO_2 is harmful to the human respiratory system and is a precursor to acid rain which can damage natural ecosystems. CAIR also introduced a program to reduce NO_x emissions.

[4] To mitigate concerns about selection bias, I provide evidence that compliance costs and environmental preferences were not systematically different in these "challenger" states.

[5] One notable exception is Fabrizio (2012).

likely to purchase costly emissions permits or to use lower-fixed-cost abatement strategies like switching to lower-sulfur coal.[6] This allowed them to maintain flexibility and avoid making an irreversible investment before they knew their regulatory status. This behavior is consistent with the theoretical predictions from the real options literature: that uncertainty should cause firms to delay making sunk investments. Furthermore, I find that plants in two of the "challenger" states were relatively more likely to install scrubbers in the years immediately following a ruling that they would have to comply with CAIR.[7] If the judicial challenge had never occurred, plants in Florida and Texas could have installed pollution controls sooner and saved as much as $216 million in permit expenses.

I also identify the effect of policy uncertainty on emissions by using a difference-in-differences approach. Namely, I compare changes in emission rates after the policy was announced between coal generators in states exposed to increased policy uncertainty to coal generators in states that were not. I find that units with a lower probability of being regulated due to the legal challenge reduced their sulfur dioxide emission rates by 13% less than units in states more certain to be regulated under CAIR. Moreover, these differences are unique to the CAIR policy: I show that plants in the "challenger" states did not make systematically different abatement choices when complying with previous policies. I also demonstrate that selection into the "challenger" group does not explain the differences in emission reductions. In particular, I find similar results when I control for operating company fixed effects and restrict the sample to only firms that operate power plants in both "challenger" states and other CAIR states.

The uncertainty generated by court cases is very similar to many other types of policy uncertainty such as uncertainty stemming from elections or upcoming policy reviews. In all of these cases, there is a future date at which firms will learn more about the future regulatory environment which creates additional incentive to delay current investment decisions. The advantage of studying the legal challenge to CAIR is that it affected some firms differentially which makes it possible to infer the effects of uncertainty empirically. The results are informative about how firms' investments may respond to uncertainty about future carbon regulation that emanates from court cases and also from elections, staff changes at the EPA, and periodic policy revisions. If firms expect uncertainty will be resolved at some point in the future, then they may delay making investments that are otherwise worthwhile. Thus the expected resolution of uncertainty creates option value to waiting, which is true regardless of whether it's a judge's decision or an election that is going to resolve the uncertainty. Climate policy uncertainty could have serious implications because delayed investment means that firms emit carbon pollution for longer leading to a higher stock of greenhouse gases and higher global temperatures.

Previous literature has theoretically investigated the effects of policy uncertainty. Viscusi (1983) provides a framework for analyzing the effects of risk and environmental regulations on firm investment decisions. He shows that if investment decisions are irreversible (such as an investment in a pollution control technology), uncertainty about regulatory policy causes firms to invest in fewer pollution controls and make fewer investments to expand output capacity (i.e., build new power plants). I expand Viscusi's theoretical framework by allowing firms multiple options for reducing pollution, a capital-intensive option

[6] All coal units were regulated under a cap and trade program so they needed to hold permits for each ton of SO_2 they emitted.

[7] The court ruled that plants in Minnesota would not be required to participate in CAIR, but plants in Florida and Texas would be required to comply.

(i.e., installing a pollution control device), and also a reversible option (i.e., purchasing cleaner burning fuel). I use the theory to develop novel theoretical predictions that I am then able to test empirically.[8] In more recent work, Stokey (2016) develops a model of investment decisions in which uncertainty about a one-time change in tax policy induces the firm to temporarily stop investing in order to wait and see how the policy unfolds. After the uncertainty is resolved, the firm exploits the tabled projects, generating a temporary investment boom.[9] In the current article, I test empirically whether reducing the probability that a fixed policy will be enacted causes firms to stop investing temporarily. I also test whether firms increase investment after the uncertainty is resolved. This paper also relates to several strands of theoretical literature in environmental economics, public economics, development economics, and industrial organization that explore the effects of uncertainty on investment.[10]

Many economists have also become interested in empirically identifying the consequences of uncertainty. More specifically, a recent literature seeks to empirically measure the effects of policy uncertainty on macroeconomic variables like aggregate investment and unemployment (Born and Pfeifer 2014; Fernandez-Villaverde et al. 2015; Baker et al. 2016) or to price political uncertainty (Pástor and Veronesi 2013; Kelly et al. 2014). There is also an emerging body of work that measures the impact of elections and political events on firm level investments (Gulen and Ion 2015; Kim and Kung 2016; Jens 2017). In industrial organization, Collard-Wexler (2013) and Pakes (1986) study the effects of uncertainty on market entry and research and development. And several papers provide empirical evidence of the effect of non-policy uncertainty on individual actors (Moel and Tufano 2002; List and Haigh 2010; Hurn and Wright 1994). In particular, Kellogg (2014) empirically tests both the direction and magnitude of the effect of price uncertainty on investment using oil drilling decisions.

I extend the existing empirical literature by taking advantage of a unique event to identify the effects of policy uncertainty on both firm investment decisions and pollution. I show that in the case of CAIR, policy uncertainty decreased capital investment, and increased both emissions and abatement costs.[11] In a related paper, Fabrizio (2012)

8 Specifically, I show how policy uncertainty affects a firm's incentive to choose a capital-intensive abatement option (scrubber) relative to a reversible abatement option (buying low-sulfur coal). Viscusi (1983) only allows for a non-reversible abatement option.

9 This work is distinct from the literature that considers a decision maker with a potential investment project, and the expected net return from the project evolves over time according to a known stochastic process (McDonald and Siegel 1986; Pindyck 1988; Dixit and Pindyck 1994). The decision maker must decide when and how much to invest. This literature shows that increases in the variance of the stochastic process increase the incentives to delay investment. In practice, policy uncertainty rarely involves increases in the variance of a stochastic process (holding mean fixed) but instead involves changes in the probability that a fixed policy will be enacted.

10 A growing literature in environmental economics compares the theoretical effects of different regulatory policies for inducing investment and R&D in new technologies (Requate and Unold 2003; Requate 2005; Krysiak 2008; Laffont and Tirole 1996; Chao and Wilson 1993). Notably, Zhao (2003) develops a rational expectations general equilibrium model of irreversible abatement investment to show how uncertainties about costs affect investment under permit trading versus emissions taxes. In public economics, Hassett and Metcalf (1999) simulate the impact of tax policy uncertainty on the level of aggregate investment. In development economics, Rodrik (1991) shows that policy uncertainty can act as a tax on investment in developing countries attempting to enact reforms. In industrial organization, Teisberg (1993) presents a model of capital investment choices by regulated firms under uncertain regulation. The model justifies utilities delaying investment and choosing shorter-lead-time technologies.

11 Emissions were higher at plants in the "challenger" states relative to other plants regulated under CAIR.

examines the effects of policy uncertainty on investment in the context of state renewable energy mandates. She finds that state-level renewable portfolio standards increased investment in renewable generating assets on average but investment increased significantly less in states with a history of regulatory reversal. Fabrizio (2012) uses past state-level regulatory reversals as a proxy for firm's current exposure to uncertainty. One advantage of my research design is that I am able to clearly identify which firms were exposed to more uncertainty. I also contribute to the literature by measuring the effects of policy uncertainty on the type of investments that are made, in addition to the level of investment. Using detailed microdata, I am able to determine if uncertainty caused firms to use less capital-intensive abatement strategies. Detailed data on regulatory compliance and investment allow me to build on related work such as Gulen and Ion (2015) and Jens (2017) that has typically focused on measuring the impact of policy uncertainty on aggregate measures of firm investment such as capital expenditures (CAPX). Furthermore, I am able to show how firm behavior changes after uncertainty is resolved and to quantify the additional compliance costs attributable to policy uncertainty.

In the next section, I discuss the institutional background of the electric-power industry, the history of air pollution regulation in the United States, and specific details of the Clean Air Interstate Rule (CAIR). In Sect. 4, I develop a two-period model of compliance under policy uncertainty that I use to develop predictions that can be tested empirically. In the fourth section, I explain the empirical methods and data sources used for the analysis. Section 5 discusses the results and Sect. 6 concludes.

2 Policy and Institutional Background

In 1989, the George H.W. Bush Administration proposed new amendments to the Clean Air Act. As part of the amendments, the United States would institute the first large-scale cap and trade program to reduce SO_2 emissions from electric power plants. The Acid Rain Program (ARP) began in 1995, regulating only the largest polluting facilities at first and introducing nearly all coal-fired power plants in the lower 48 states to the program by 2000. Many consider the program as hugely successful and even regard ARP as a benchmark model for quantity-based instruments for pollution control. ARP reduced SO_2 emissions by over 40% in the first 10 years and previous analyses suggest that the net benefits of the program were between \$58 and \$114 billion per year (Schmalensee and Stavins 2013).

Despite the large benefits achieved from ARP, the EPA determined that many states were still significantly contributing to non-attainment of National Ambient Air Quality Standards (NAAQS) for fine particles and/or 8-h ozone in downwind states. In May 2005, the EPA introduced the Clean Air Interstate Rule (CAIR) in order to further reduce NO_x and SO_2 emissions from power plants located in 28 states in the eastern United States. CAIR would include three new cap and trade programs, including a new program effectively replacing the Acid Rain Program (ARP) for eastern states. The program would continue to use permits from the Acid Rain program, but starting in January 2010, eastern states under CAIR would now have to surrender two permits for each ton of SO_2 emitted instead of one.

Since all ARP permits of vintage 2009 or earlier could be used to offset 1 ton of SO_2 emissions under the new CAIR program, plants had an incentive to start making immediate emission reductions before the new program took effect. If firms made emission reductions between 2005 and 2010, they could "bank" their extra emissions permits to use or sell under the new more stringent policy. The increased incentive to reduce emissions was immediately reflected by increased ARP allowance prices, which can be seen in Fig. 7 of Appendix 4. Sources that were included in the CAIR SO_2 program reduced their emissions by over 50% in the 5 years between the initial CAIR announcement and the start of the new program (EPA 2016).

2.1 Regulatory Challenge and Uncertainty

Shortly after the announcement of CAIR, several states and industry groups filed a series of lawsuits challenging the legality of the new EPA rule. The collection of lawsuits was aggregated into a single case called *North Carolina vs. Environmental Protection Agency*. Most of these lawsuits affected all CAIR states, however, three states located along the borders of the policy footprint (Florida, Minnesota, and Texas) claimed that their emissions did not significantly affect downwind states' compliance with National Ambient Air Quality Standards.[12] They argued that the EPA should therefore not include them in the new CAIR program. This legal challenge subjected power plants located in these three states to a higher level of policy uncertainty than power plants in the other states since they would need to wait to see if the court would overturn the current rule. If the court did vacate the rule, these states would be operating under a significantly less stringent regulatory regime.[13]

The D.C. Circuit Court made a final ruling on CAIR in December 2008. The court granted a permanent stay of the rule for Minnesota but decided that Texas and Florida would be required to participate in the program.[14] The court agreed with Minnesota's claim that their emissions were not significantly affecting downwind states' compliance with NAAQS. On the other hand, the court rejected Texas and Florida's similar claims. In 2010, the CAIR SO_2 program took effect for all of the initially planned states except for Minnesota. Figure 2 includes a timeline of critical events in the rollout of CAIR.

Throughout the rest of the article, I focus on measuring the effects of policy uncertainty that arose from the border states' legal challenge to the CAIR program. The legal challenge exposed plants to varying levels of uncertainty. I exploit this variation to identify the effects of policy uncertainty on pollution abatement, investment in control technologies, and coal purchases.

For the empirical analysis, it would be ideal if the legal challenge levied by Florida, Minnesota, and Texas occurred in isolation. However, *North Carolina vs. Environmental Protection Agency* also contained other challenges to CAIR. The D.C. Circuit Court of Appeals separated petitions into several categories that were relevant to the CAIR SO_2 program: (1) Texas, Florida, and Minnesota argued against their inclusion in CAIR; (2) North Carolina wanted to limit permit trading across geographic areas so that specific plants that were contributing to nonattainment in downwind states would be required to reduce

[12] Jet streams in North America typically cause the wind to blow from west to east.

[13] Figure 1 depicts the states included in the CAIR SO_2 program and separately identifies the "challenger" states which explicitly challenged their inclusion in the program.

[14] The D.C. Circuit Court made an initial ruling in July 2008 to stay implementation of CAIR for all states. The court ruled that EPA did not do a satisfactory job accounting for the effects of pollution in particular downwind states. In December, the court changed the ruling to allow CAIR to stand while the EPA fixed issues with the rule.

emissions;[15] (3) Several electric utility companies contested EPA's authority under Title I and Title IV to require firms to retire more than one allowance for each ton of emissions. Because several petitions were filed concurrently, one might think that it would be difficult to identify the causal effect of the border states' challenge (1). In particular, the estimated effect of the border states' challenge could be confounded by the effects of the other petitions (2) and (3). This possibility is impossible to rule out with certainty. However, the latter two petitions were broad challenges to the structure of the CAIR policy, either of these challenges could lead to the entire CAIR program being overturned.[16] Therefore, uncertainty stemming from these two challenges likely affected all power plants within CAIR equally (including plants in FL, MN, and TX), whereas, the challenge levied by the three border states generated additional uncertainty for plants within those states. For this reason, the variation created by the border states' challenge is still valuable for identifying the effects of policy uncertainty. If the concurrent legal challenges affected all states in CAIR equally, the difference-in-differences approach would still provide unbiased estimates of the effect of policy uncertainty associated with (1). In Sect. 4, I discuss threats to identification in more detail and propose several robustness checks and alternative models to address these potential concerns.

In the next section, I develop a two-period model of firm compliance with pollution regulation. I use the model to establish testable predictions about firm behavior under uncertainty. I then test the theoretical predictions using unit-level data in the following sections.

3 Model of Compliance Under Policy Uncertainty

Consider a two-period model. In each period, firms must pay a fee for each unit they emit. In the first period, the regulator sets the emission price equal to P_1. The emission price could also arise indirectly through an emission cap set by the regulator. However, the second-period emission price is not revealed until after the first period is completed. With probability $\rho \in [0, 1]$, the regulator will impose a more stringent price P_2^H (more stringent emissions cap) and with probability $1-\rho$, she will impose a less stringent price P_2^L (less stringent emissions cap), where $P_2^H > P_2^L$.[17] This uncertainty could result from a pending judicial review or from an upcoming election.

There are M risk-neutral firms and every firm emits pollution as a byproduct of each unit of output. For the case of an emission tax, M can be arbitrarily large. For an emission cap, assume that M represents a small subset of firms in the permit market such that each firm's abatement and investment has no influence on the equilibrium permit price.[18] Reducing emissions is costly for firms. However, firms can reduce their marginal abatement cost by

15 North Carolina argued that the program did not mandate emissions reductions from sources that were contributing to non-attainment of NAAQS in downwind states. Under a cap-and-trade program, upwind plants could avoid making emissions reductions by instead choosing to purchase more allowances.

16 In fact, the court agreed with parts of both of these challenges, and as a result CAIR was replaced by the Cross-State Air Pollution Rule (CSAPR) in 2014. The CSAPR program did not use ARP permits and limited interstate trading, which contributed to the eventual collapse of SO_2 allowance prices.

17 In the case of the CAIR program, $P_2^L = P_1$ and $P_2^H = 2P_1$. Each unit included in CAIR had to surrender two permits for each ton of SO_2 emissions starting in 2010, units not include in CAIR would continue to submit one permit per ton of SO_2.

18 For the empirical application, I focus on uncertainty that affected a small group of firms and was unlikely to have a substantial effect on the equilibrium permit market price.

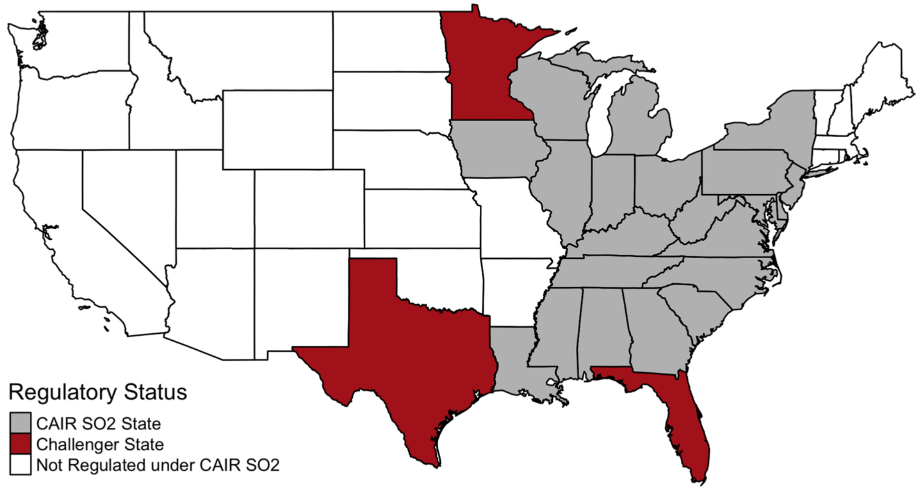

Fig. 1 CAIR SO_2 regulatory footprint

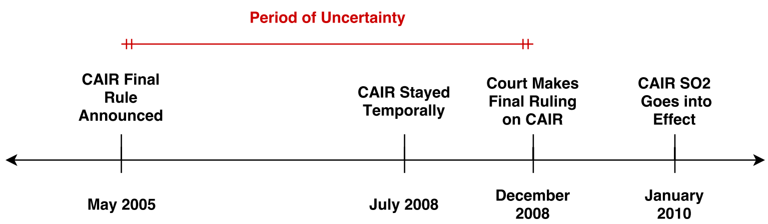

Fig. 2 CAIR regulatory timeline

investing in a capital-intensive technology (i.e., a scrubber for removing SO_2 emissions). Firms that invest in the technology must incur a fixed cost K^i. The cost of capital varies for each firm. In particular, capital costs are drawn from $K^i \sim F(K)$, where F is the cumulative distribution function of K. I assume $F(K)$ is continuous and differentiable. Investing in the technology is irreversible. Firms can reduce their pollution without installing the capital-intensive technology, but they must incur higher marginal abatement costs (e.g., purchasing low-sulfur coal).[19]

Absent any emission reductions, each firm would emit $\overline{e}$ units of pollution. A firm's realized emissions in period t, e_{it}, are equal to the baseline emissions level net of abatement a_{it}, so $e_{it} = \overline{e} - a_{it}$. Abatement cost $C(a_{it}, I_{it})$ is a function of the level of abatement a_{it} and $I_{it} \in \{0, 1\}$, an indicator function that is equal to one if the firm has installed the capital technology and zero otherwise. In particular, $C_a(a_{it}, 1) \leq C_a(a_{it}, 0)$ for all a_{it}, where the subscript a signifies partial derivative with respect to a_{it}. This means the marginal cost

[19] In practice, switching to low-sulfur coal does incur a fixed cost to retrofit boilers and equipment; however, these costs are generally very small in comparison to the capital cost of installing a scrubber.

of abatement is lower once the capital technology is installed.[20] Additionally, assume that the abatement cost function is strictly increasing and convex in the level of abatement: $C_a(a_{it}, I_{it}) > 0$ and $C_{aa}(a_{it}, I_{it}) > 0$, where C_{aa} denotes the second-order partial derivative of C with respect to a_{it}. Finally, normalize $C(0, I_{it}) = 0$. This normalization implies that choosing zero abatement is costless regardless of the technology decision. Also, notice that conditional on the decision to adopt the capital technology, the abatement cost function is identical across firms. Figure 3 shows an illustration of how capital investment in a technology could shift marginal abatement cost.

Each period, t, firms choose whether to install the technology. The installation decision is permanent: if installed; the technology will remain in any future periods. In addition, firms choose a_{it} and an output quantity q_{it} to maximize expected profits. For this analysis, I assume that output quantity is fixed at $q = \overline{q}$. This is a reasonable assumption for coal-fired power plants during the time period of this study since coal plants were almost always inframarginal and were already operating at a capacity constraint. This assumption is also common in literature (Fowlie 2010) and allows the firm's abatement decision to be modeled independently of the output decision. Omitting the firm subscript i and superscript i for readability, the firm's problem can be written as:

$$\begin{aligned} \min_{a_1, I_1} \; & P_1 \cdot (\overline{e} - a_1) + C(a_1, I_1) + K \cdot I_1 + \tfrac{1}{1+r} \mathbb{E}\left[\min_{a_2, I_2} \{ P_2 \cdot (\overline{e} - a_2) + C(a_2, I_2) + K \cdot (I_2 - I_1) \} \right] \\ & s.t. \quad a_t \in [0, \overline{e}], \quad I_t \in \{0, 1\}, \quad I_2 \geq I_1, \end{aligned} \tag{1}$$

where $\mathbb{E}$ is the expectation operator taken over the uncertain emission price in period 2 and r is the firm's per-period discount rate. The firm's problem is to choose capital investment and abatement to minimize the sum of current costs and expected costs in the next period, subject to the constraint that abatement must be weakly greater than zero and less than the baseline emissions level. The firm must also consider the irreversibility of the capital-investment decision.

The firm's optimal level of first-period abatement is determined by the following first order condition for an interior solution:

$$C_a(a_1, I_1) = P_1 \tag{2}$$

This first order condition is consistent with the standard intuition that firms should set their marginal cost of abatement equal to the permit price. All firms that do not install the technology will choose the same optimal abatement level, a_1^N, and all firms that do install the technology will choose a_1^I as their optimal abatement level. Furthermore, it must be true that $a_1^N \leq a_1^I$, which follows from the assumption that $C_a(a_t, 1) \leq C_a(a_t, 0)$ for all a_t. Graphically, Fig. 3 shows that the optimal abatement levels, a_1^I and a_1^N, are determined by finding the points where the marginal abatement cost curve intersects with the emission price.

[20] Modeling a new investment as reducing marginal abatement cost is standard in the theoretical literature investigating environmental policy instruments and technology adoption (Jung et al. 1996; Milliman and Prince 1989; Requate and Unold 2003; Shittu et al. 2015). Amir et al. (2008) and Baker et al. (2008) provide additional discussion about modeling technical change and the marginal cost of abatement. In the context of SO_2 abatement, a coal unit that has not installed a scrubber (capital technology) can reduce emissions by purchase low-sulfur coal, which entails large shipping costs for plants located in the Midwest and East. Units that have installed the technology can reduce pollution by simply running their scrubber. This entails some operation and maintenance costs, but these costs are relatively small compared to buying more expensive low-sulfur coal.

The capital investment choice is a dynamic decision. A profit-maximizing firm must consider not only the direct costs and benefits of investing today, but also the option value of waiting until next period to decide after the uncertainty has been resolved. The solution to the problem will consist of a cutoff rule for investment; all firms with a capital investment cost $K^i \leq K_1^*$ will install the technology, and all firms with higher capital costs will not.[21] A firm should install the capital technology in the first period if the expected net costs from installing immediately are less than the expected costs from waiting until the second period to decide. In particular, firms should invest if:

$$\begin{aligned} & P_1 \cdot (\bar{e} - a_1^I) + C(a_1^I, 1) + K + \mathbb{E}[\min_{a_2}\{P_2 \cdot (\bar{e} - a_2) + C(a_2, 1)\}] \\ & \quad \leq P_1 \cdot (\bar{e} - a_1^N) + C(a_1^N, 0) + \mathbb{E}[\min_{a_2, I_2}\{P_2 \cdot (\bar{e} - a_2) + C(a_2, I_2) + K \cdot I_2\}] \end{aligned} \tag{3}$$

We now consider the testable predictions regarding firm behavior implied by the model. Proofs of all the propositions are provided in the Appendix.

3.1 Theoretical Predictions

The first proposition considers how a change in the probability of the more stringent policy, ρ, affects firms' decision to invest in the capital technology in the first period.

Proposition 1 *Reducing the probability ρ that the stringent emission price will occur will reduce investment in the capital technology in the first period. Formally, $F(K_1^*)$ (weakly) increases in ρ.*

The first result is intuitive. Reducing the probability of the high emission price decreases the expected future payoff from investing in the capital abatement technology. This causes a smaller share of firms to invest. In the Appendix, I provide a proof by explicitly writing out the cutoff rule as function of ρ and differentiating to obtain a comparative static. In the context of CAIR, we would expect units located in Florida, Minnesota, and Texas to be less likely to install scrubbers during the period before the court made a ruling.

The second proposition shows how changes in the probability of a high emission price impact emissions during the first period.

Proposition 2 *Reducing the probability ρ that the stringent emission price will occur (weakly) increases aggregate emissions in period one. Formally, $\frac{d\mathbf{e}_1}{d\rho} \leq 0$, where $\mathbf{e}_1 = \sum_i e_{i1}$*

.

This proposition follows closely from Proposition 1. Because smaller ρ leads fewer firms to adopt the technology and firms that install the technology will choose to emit less, emissions will be higher in period 1. The second proposition suggests that units that were less likely to be regulated under CAIR, such as those units located in the three "challenger" states, should have higher emissions during the period before the court's ruling.

The next proposition considers the behavior of firms who choose to not install the capital technology. Uncertainty about the future emission price will impact the total

[21] See Requate and Unold (2003) for more details.

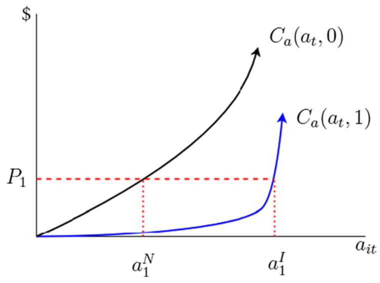

Fig. 3 Marginal abatement cost with and without technology investment. $C_a(a_{it}, 1)$ is the marginal abatement cost for units that have invested in the capital technology (i.e., coal units that have installed a scrubber). $C_a(a_{it}, 0)$ is the marginal abatement cost for units that have not invested in the technology (coal units can still reduce emissions by buying low-sulfur coal). Given an emission price of P_1, a_1^I is the optimal abatement level for units that have invested in the technology and a_1^N is the optimal abatement level for units that have not

amount of abatement these firms undertake (e.g., change the amount of low-sulfur coal they decide to purchase).

Proposition 3 *Aggregate abatement by firms that do not adopt the technology (who choose to purchase low-sulfur coal) weakly decreases with* ρ*. Formally,* $\frac{d\mathbf{a_1^N}}{d\rho} \leq 0$*, where* $\mathbf{a_1^N} = \sum_i a_{i1} \cdot \mathbb{1}(I_{i1} = 0)$*.*

As the probability of the stringent emission price decreases from one, fewer firms will adopt the capital technology in period 1. However, because there is an emissions price in period 1, many firms will still want to reduce emissions (relative to $\overline{e}$) while maintaining the option to adopt the technology in period 2. Figure 3 shows graphically that the firms that do not adopt the technology will still choose a positive level of abatement. In the context of CAIR, firms could choose a lower-fixed-cost abatement strategy like burning lower-sulfur coal to reduce emissions without making a sunk-cost investment in a scrubber.

Proposition 3 follows closely from Proposition 1. As the probability of the high emission price decreases, more firms will decide not to adopt the technology. Since all firms that do not adopt the technology will choose to abate a_1^N, there will be more aggregate abatement by non-adopters. In the context of CAIR, I can test whether the legal challenge by the three border states led plants in those states to purchase more low-sulfur coal than plants in other CAIR states during the period before the court's ruling.

The final proposition shows how uncertainty during the first period can alter capital investment in the second period.

Proposition 4 *Reducing the probability* ρ *that the stringent emission price will occur will cause more capital investment in the second period if the stringent emission price happens to be realized.*

As ρ decreases, fewer firms will adopt the technology in period 1. In the case that the high emission price P_2^H does occur, a larger share of firms will then choose to adopt in the second period. This proposition suggests that after the court ruled to include Texas and

Florida in CAIR, we should see relatively more scrubbers installed in those states than in other CAIR-regulated states after the court decision.

Testing Proposition 2 is the central focus of the empirical section of this paper. In particular, I test if reductions in the probability of regulation increased emissions during the period of uncertainty. The judicial challenge of CAIR by three states generated variation in firms' probability of having to comply with CAIR. Specifically, plants in these three states were less likely to have to comply with the new regulation than firms in other states under CAIR. I use this variation to test for differences in emission reductions, and also to directly test for differences in investment and abatement methods (Propositions 1 and 3). Furthermore, I test whether investment in scrubbers (capital technology) increased more in these states after the court ruled that they would need to comply (Proposition 4).

Table 1 summarizes the propositions. Column 2 provides a short description of the theoretical result and the third column describes the associated empirical prediction that can be taken to the data. The fourth column provides the theoretically predicted regression coefficients, which are discussed in detail in the following sections.

4 Data and Empirical Methods

In order to test the propositions from the previous section, I collect source-level data from the EPA's Continuous Emissions Monitoring System (CEMS) for the years 2002–2011.[22] CEMS is a nation-wide database used to monitor compliance with federal emissions programs such as the Acid Rain Program and the Clean Air Interstate Rule SO_2 Trading Program.

The EPA Clean Air Markets Program database allows users to collect source-level emissions data at the hourly level. For this study, I aggregate the data to the annual level. The CEMS data include gross output (MWh), NO_x emissions (tons), CO_2 emissions (tons), SO_2 emissions (tons), and heat input (MMBtu) at the boiler level. The CEMS database includes all generators with nameplate capacity over 25 MW and thus includes nearly all coal units in the contiguous United States. The EPA also provides descriptive data for each unit including geographic coordinates, beginning date of operation, operating company, and a description of any pollution control technology installed.

I restrict the sample to include only coal-fired boilers because coal boilers emit over 99.5% of all SO_2 emissions from the electric-power industry. Furthermore, I limit the sample to include only coal units that *did not* already have a scrubber installed before the CAIR announcement in 2004. Coal units that already had a scrubber installed did not have an investment decision to make, so they are not helpful for testing the propositions from the previous section. Additionally, I focus on coal units that operated throughout the sample.[23]

To better understand how uncertainty affects coal-purchasing decisions, I also obtain plant level fuel receipts from the Energy Information Agency (EIA) and the Federal Energy Regulatory Commission (FERC). From 2002 to 2007, plants that were subject to cost-of-service regulation reported fuel purchases annually on FERC Form 423 and deregulated plants reported on EIA Form 423. From 2007 to 2011, all coal plants reported fuel

[22] 2002–2011 is the time frame for the primary analysis. I also collect data going back as far as 1996 that I use for an additional test.

[23] As a robustness check, I also run regressions for the entire population of coal units (including those that were scrubbed before 2004) and for the complete unbalanced panel of units.

Table 1 Theoretical predictions and associated empirical predictions

	Theoretical prediction	Empirical prediction	Predicted sign
Proposition 1	$\rho \downarrow \Rightarrow \sum_i I_{i1} \downarrow$	Units in "challenger" states should be less likely to install a scrubber♦ #	$\beta_1 < 0$
Proposition 2	$\rho \downarrow \Rightarrow \mathbf{e_1} \uparrow$	Units in "challenger" states should have higher emissions♦ #	$\beta_1 > 0$
Proposition 3	$\rho \downarrow \Rightarrow \mathbf{a_1^N} \uparrow$	Plants in "challenger" states should have lower sulfur content in their coal purchases♦ #	$\beta_1 < 0$
Proposition 4	If $P_2 = P_2^H$, then $\rho \downarrow \Rightarrow \sum_i I_{i2} \uparrow$	Units in "challenger" states should be more likely to install a scrubber after the court ruled to enforce the high emission price#	$\beta_t > 0, \forall\, t > 2009$

♦Before the court ruling. #Relative to other units regulated under CAIR

Table 2 Coal unit summary statistics by group before CAIR

	CAIR	Non-CAIR	Challenger	Total
Gross load (TWH)	1.881	2.247	2.649	1.994
	(1.683)	(1.662)	(1.730)	(1.695)
Distance to PRB (Miles)	1906.0	1142.6	1658.7	1764.1
	(392.3)	(581.8)	(580.4)	(524.5)
Regulated (0,1)	0.740	0.913	0.818	0.773
	(0.439)	(0.284)	(0.390)	(0.419)
Age (years)	41.00	34.13	32.95	39.32
	(9.557)	(9.144)	(9.937)	(9.986)
Scrubber installed by 2004 (0,1)	0	0	0	0
	(0)	(0)	(0)	(0)
Scrubber installed by 2009 (0,1)	0.217	0.0874	0.0909	0.187
	(0.413)	(0.284)	(0.291)	(0.390)
Log SO_2 rate 2004	0.271	− 0.363	− 0.178	0.136
	(0.601)	(0.428)	(0.470)	(0.619)
Log SO_2 rate 2009	− 0.191	− 0.539	− 0.326	− 0.257
	(1.001)	(0.732)	(0.566)	(0.945)
Difference (2009–2004)	− 0.462	− 0.175	− 0.148	− 0.393
	(0.958)	(0.645)	(0.526)	(0.898)
N	518	105	44	677

The descriptive statistics describe boiler characteristics in 2004. Distance to PRB is the unit's distance to the Powder River Coal Basin in Wyoming, this serves a proxy for the unit's ability to purchase lower-sulfur subbituminous coal. Capacity is measured as the unit's maximum heat input in btu (billions). "SO_2 Difference" is the change in emission rates between 2004 and 2009. The sample only includes units that did not already have a scrubber installed by 2004. Standard deviations are in parentheses

purchases on a single form, EIA Form 923. The fuel receipts data include the quantity of coal purchased (short tons), the sulfur content of fuels (percentage of weight), heat content (MMBtu), and ash content. Price and other fuel contract details are provided for regulated plants. A useful feature of the fuel receipts data is that it indicates if a plant is under cost-of-service regulation. I merge the EIA data with the EPA data using unique Plant ID numbers included in both data sets. This allows me to identify the regulatory status of each unit in the EPA data.

Table 2 provides summary statistics for units located in each of the three groups of states in 2004, right before CAIR was announced. The first column includes all units included in the CAIR SO_2 trading program except the three "challenger" states. The third column summarizes units located in Florida, Minnesota, and Texas and the second column includes all other coal units. Units in CAIR had higher emission rates on average than non-CAIR units and units in the three "challenger" states. Units in CAIR also were older than other units, less likely to be regulated and produced less gross output. Distance to PRB is the unit's distance to the Powder River Coal Basin in Wyoming. Units in Non-CAIR states were much closer to the Powder River Basin on average and likely had greater access to low-sulfur coal. I limit the sample to only units in each group that did not have a scrubber installed in 2004 when CAIR was announced.

Comparing the 2004 natural log of SO_2 emission rates in the bottom of Table 2 to the natural log of emission rates for 2009, it's clear that emission reductions were much larger in CAIR states. The average log emission rate dropped by 0.46 in CAIR states, while it only dropped by .148 in non-CAIR states and 0.175 in "challenger" states. CAIR states were also more likely to install scrubbers. 21% of CAIR units installed scrubbers between 2004 and 2009, compared to only 9% of both non-CAIR and "challenger" units. These descriptive results are consistent with the theory that policy uncertainty delays abatement and investment, but on their own are not proof of a causal relationship. It is possible the difference between emission reductions in "challenger" states and other CAIR states were driven by differences in unit characteristics between the two groups and not by policy uncertainty. To account for this possibility, I also employ a nearest-neighbor estimator to obtain a more balanced sample across the two groups.[24]

Another threat to identification is that selection into the "challenger" group was itself endogenous. This would be the case if generation companies located in Florida, Minnesota, and Texas had a particular preference against reducing emissions and decided to file the lawsuit for that reason. An additional possibility is that emission rates in each of these groups were already following different time trends not associated with CAIR at all. In the next section, I describe the empirical model used to address these potential concerns.

4.1 Empirical Model and Identification

In this section, I discuss the econometric model used to test the predictions from the theoretical model. First, I describe the difference-in-differences (DID) approach used to test if the legal challenge of CAIR caused a decrease in pollution abatement (Proposition 2). I then describe how a similar empirical approach can be used to test Proposition 3. Specifically, I test if uncertainty generated by the legal challenge caused firms that did not invest in scrubbers to increase purchases of low-sulfur coal. Next, I explain how a slightly modified but simple framework can be used to test whether the legal challenge reduced investment in capital-intensive pollution controls (scrubbers) during the period before the uncertainty was resolved (Proposition 1). Finally, I introduce a regression framework to test if relative investment in scrubbers changed after the court ruling (Proposition 4).

4.1.1 Empirical Test of Proposition 2

In order to test Proposition 2, I compare changes in the natural logarithm of SO_2 emission rates after the policy announcement at units subject to additional legal uncertainty to other units in CAIR and units not regulated by CAIR. I use both regression and matching approaches to control for observable characteristics of the coal units. The states never regulated under CAIR serve as a natural "control" group. All states initially intended to be regulated under CAIR are defined as the "treatment" group. Additionally, I define a

[24] Table 4 in the next section shows that for the matched sample, CAIR units and Challenger units look very similar in age, baseline emissions, distance to PRB, and regulatory status.

"treatment" subgroup composed of units located in one of the states subjected to additional policy uncertainty.

A DID approach relaxes the assumption that the average level of the dependent variable would have been the same absent "treatment". Instead, it must be true that trends in the dependent variable would have been the same absent "treatment". By adding controls, we are ensured that the estimated effect is only being identified off of units with similar observable characteristics. I start by estimating the following regression:

$$ln(Y_{it}) = \beta_1 \mathbb{1}[Challenger]_{it} + \beta_2 \mathbb{1}[CAIR]_{it} + \mathbf{x_i'}\boldsymbol{\eta} + \gamma_t + \epsilon_{it} \tag{4}$$

The dependent variable is the natural log of unit i's SO_2 emission rate in lbs. per MMBtu in year t. I use a log-transformed dependent variable in the main specification because investment in a scrubber leads to a proportional reduction in emission rate at a coal boiler.[25] $\mathbb{1}[Challenger]_{it}$ is an indicator variable, equal to one if the year is 2005–2009 and the unit is located in a Minnesota, Florida, or Texas. The period of 2005–2009 includes years after the policy was announced, but before the court made its ruling. $\mathbb{1}[CAIR]_{it}$ is an indicator variable, set equal to one if the unit is located in a CAIR state *including* Florida, Minnesota, and Texas, and the year is 2005–2009. γ_t is a set of year fixed effects and $\mathbf{x_i'}$ contains unit fixed effects in my preferred specification. For specifications without unit fixed effects, $\mathbf{x_i'}$ contains a vector of controls such as the unit's age in 2004, regulatory status, distance to the Powder River Basin, and log emission rate in 2004 before the policy announcement. Several studies have shown compliance choice can be affected by a plant's regulatory status (Cicala 2015; Fowlie 2010), this control ensures only differences between plants under the same regulatory regime are being compared. I also control for the unit's distance to the Powder River Basin, the Powder River Basin is the primary mining location for low-sulfur coal, so this control proxies for a firm's ability to purchase low-sulfur coal. Finally, I control for each unit's age in 2004 and log emission rate in 2004, since a plant's time until retirement and baseline emissions rate are likely to affect their incentive to invest in pollution reductions.

The coefficient of interest is β_1. I include units in Florida, Minnesota, and Texas in both the "CAIR" and "Challenger" groups. Therefore, β_1 can be interpreted as the average percentage change in emission rates in Florida, Minnesota, and Texas relative to units in other CAIR-regulated states. Proposition 2 predicts that units in the "challenger" states should be less likely to reduce emissions. If this is true, β_1 should be positive. On the other hand, we would expect β_2 to be negative because units in states regulated under CAIR should be more likely to reduce emissions relative to units in states that are not subject to the rule.

β_1 is the "differential effect" of the announcement of CAIR for plants exposed to additional policy uncertainty. In order to consistently estimate the differential effect, several assumptions must hold.[26] First, recall that units belong to one of three different groups: (1) units that were included in CAIR but were not in a "challenger" states (treatment 1), (2) units that were included in CAIR but were located in a "challenger" state (treatment 2), and (3) units that were not included in CAIR (control group). To identify the differential effect between (1) and (2) it must be true that after controlling for observed covariates (unit fixed effects), average emission rates of units that received either "treatment 1" or "treatment 2" would have followed parallel trends relative to the control group absent the intervention. More specifically, the trends in average emissions for each of these groups must have

[25] I use SO_2 per MMBtu instead of SO_2 per MWH because gross output data is missing for some units in the sample. As a robustness check, I also run the model with SO_2 emissions rate, total SO_2 emissions (levels) as the outcome variable.

[26] See Hotz et al. (2006) for a discussion of the identification of differential effects.

had equal slopes in the case that CAIR was never announced. This is the standard unconfoundedness assumption. Figure 4 shows the average log SO_2 emissions trends for each of the three groups in the years before the CAIR SO_2 program was announced in 2005. A visual inspection shows no systematic deviation in the slopes of the trend lines between the groups. Furthermore, the trends had been nearly flat for each of the groups during the 4 years before the policy was announced. Identifying the differential effect also requires an additional unconfoundedness assumption. We require that units in the "treatment 2" group would have behaved the same on average as "treatment 1" units in the case that they had instead received "treatment 1". In particular, if CAIR was introduced but there was no challenge by the border states, we require that plants in the "challenger" states would have made the same average emissions reductions as plants in other CAIR states (after controlling for observable unit characteristics). This assumption is not directly testable but there is indirect evidence that it is likely to hold. In Fig. 4, there is a decrease in emission rates for each of the groups at the end of the 1990s. This decrease was the result of compliance with Phase 2 of the Acid Rain Program. As an indirect test of the second unconfoundedness assumption, I estimate the DID model (4) with the pre-period as 1998–1999, and the post-period as 2000–2001. The results of these regressions can be found in Panel A of Table 6 of the Appendix. In all specifications of these falsification tests, I fail to reject the null hypothesis that $\beta_1 = 0$. The point estimates are also small in magnitude. Panel B of Table 6 of the Appendix presents regressions with the dependent variable as a binary choice to install a scrubber. I also fail to reject the null hypothesis that units in Texas, Florida, and Minnesota were equally likely to install scrubbers to comply with the Acid Rain Program Phase 2 relative to other CAIR states. This exercise provides evidence that units in Texas, Florida, and Minnesota were not systematically different from units in other CAIR states when complying with previous SO_2 regulations.

Even if compliance decisions in "challenger" states were not different in the past, it is still possible firm ownership has changed over time and utility executives located in these states now have a stronger preference against making emission reductions. To account for potential bias through this channel, I also run the model (4) on a restricted sample only including operating companies that owned plants in "challenger" states and also in other CAIR states. I also include operator fixed effects. This specification ensures I am comparing abatement choices in states subject to more policy uncertainty to abatement choices in other states, while holding managerial preferences constant.

Finally, to ensure that the regression estimates are unbiased, the stable unit treatment value assumption (SUTVA) must hold. This means that policy uncertainty in Texas, Florida, and Minnesota must not have changed the abatement choices of units outside of those states. This assumption may not hold if power plants in the "challenger" states made up a large enough portion of the SO_2 permit market to significantly affect allowance prices. There are two reasons why a violation of SUTVA is unlikely to cause problems in identifying a causal effect. First, since emissions from power plants in the three "challenger" states made up only 8.9% of total SO_2 emissions in 2004, the exclusion of these three states would have decreased permit demand by 4.8%.[27] This change would be unlikely to cause a large enough decrease in permit prices to drastically change abatement decisions in other states. Secondly, even if the legal challenge by Texas, Minnesota, and Florida affected permit prices, the effect would likely bias against finding $\beta_1 > 0$. Since the legal challenge

[27] Total demand for permits can be determined by multiplying 2004 SO_2 emissions by 2 for units included in CAIR, and multiplying 2004 SO_2 emissions by 1 for non-CAIR units and summing across all units.

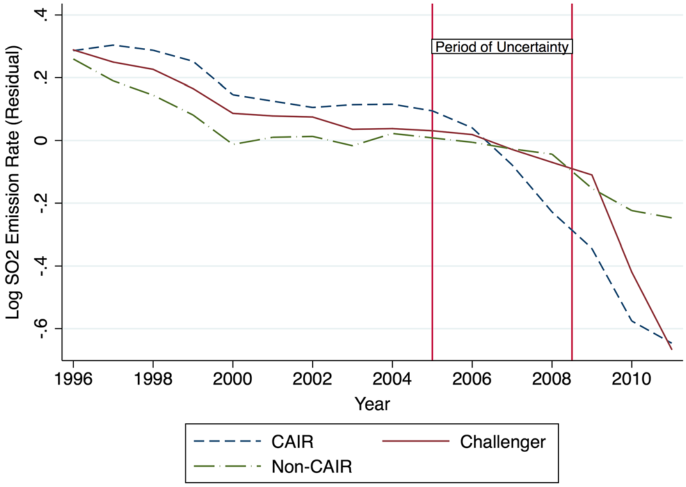

Fig. 4 Log SO_2 emission rates trends by group. The plotted trend lines represent the mean Log SO_2 emission rate (residual) for each group after controlling for unit fixed effects. The first red vertical line indicates the initial announcement of CAIR. The second vertical red line represents the date the court made its final ruling. (Color figure online)

reduced the probability that firms in the "challenger" states would have to comply with CAIR, this would reduce expected demand for permits. Also, because CAIR would use the same permits as ARP and ARP permit allocations were predetermined, any firm being included (or discluded) did not change the overall supply of permits available. That is, inclusion in CAIR only changed the number of permits that needed to be submitted to the regulator per ton of emissions but not the allocation of permits. Therefore, a reduction in expected demand for permits while holding supply fixed should drive down permit prices. Lower permit prices should cause other firms in CAIR to be less likely to make early investments in pollution controls. Furthermore, increased uncertainty about future permit prices should cause other plants regulated under CAIR to be more likely to delay their own investment. This would bias against finding the result that plants in "challenger" states were more likely to delay making pollution reductions relative to other CAIR-regulated plants.

4.1.2 Empirical Test of Propositions 1 and 3

In addition to measuring the impact of policy uncertainty on pollution outcomes, I am also interested in the mechanisms driving any differences in pollution. Coal units typically have two options for reducing SO_2 emissions. Units can install a flue-gas desulfurization system (scrubber) or they can switch to lower-sulfur coal. Installing a scrubber requires a relatively large fixed-cost investment and has low operating costs. In contrast, switching coal rank typically requires a relatively smaller fixed cost and higher variable costs. The theoretical model predicts that coal units that were subjected to additional legal uncertainty should increase total purchases of low-sulfur coal (low fixed-cost abatement). For example, we may see firms in the "challenger" states making more non-scrubber abatement than firms in states where the probability of regulation was closer to one. I estimate the direction and magnitude of this effect by running the same DID regression with log sulfur content of coal purchases as the dependent variable. Since coal purchase data is recorded at the plant level, I run these regressions with observations at the plant-year level instead of the boiler-year level. If β_1 is negative, it would provide evidence that plants located in the states challenging the ruling were more likely to reduce the sulfur content of their coal during the period of uncertainty.

I can also use a similar framework to test if units with a lower probability of being regulated under CAIR were less likely to install scrubbers during the period of uncertainty (Proposition 1). To test the first proposition, it does not make sense to estimate a DID model with a binary irreversible decision as the outcome. Instead, I restrict the sample to only units that did not already have a scrubber installed in 2004. I then estimate both linear probability and probit models where the dependent variable is a binary variable, set equal to one if the unit installed a scrubber by 2009, and set equal to zero otherwise.

4.1.3 Empirical Test of Proposition 4

The fourth result from the analytic model predicts that we should see a relative increase in scrubber investment at plants in Florida and Texas after the court ruled to enforce CAIR. To test whether the relative probability that scrubbers were installed changed after the court's decision, I estimate the following model:

$$\mathbb{1}[Scrubber]_{it} = \beta_\tau \mathbb{1}[Challenger]_{it} \cdot \mathbb{1}[year^\tau]_t + \lambda_\tau \mathbb{1}[CAIR]_{it} \cdot \mathbb{1}[year^\tau]_t + \mathbf{x_i'}\boldsymbol{\eta} + \gamma_t + \epsilon_{it}, \tag{5}$$

where $\mathbb{1}[Scrubber]$ is an indicator variable, set equal to one if unit i has a scrubber installed in year t and zero otherwise, and $\mathbb{1}[year^\tau]$ is a year dummy that is set equal to one if the year t is equal to τ. Again, $\mathbf{x_i'}$ controls for the unit's distance to the Powder River Basin, boiler age in 2004, emission rate in 2004, and cost-of-service regulation status. I also drop each unit that had already had a scrubber installed during the year $t-1$. This is to account for the fact that only unscrubbed units are actually making an investment decision each year. Conditional on not already having installed a scrubber, β_τ is the average additional probability that units in "challenger" states in a year τ install a scrubber, relative to units in the other "CAIR" states.

In order to examine outcomes after the court made the decision to include Florida and Texas in CAIR, I expand the sample to include the years 2010 and 2011.[28] I also drop units in Minnesota since I want to test if the probability of installing a scrubber went up after the court ruled that the two states would have to comply.[29] Finding β_τ is negative for years before the court decision, and β_τ is positive after the court decision, would support the theoretical prediction that units that faced a lower probability of being regulated should be less likely to invest in pollution controls in the period before the court decision but should be relatively more likely to install a scrubber in the years following the decision.

[28] In 2011, the EPA announced a replacement policy for CAIR called the Cross State Air Pollution Rule (CSAPR), for that reason I do not consider any data beyond 2011 because any abatement choices beyond that point are likely related to the new policy.

[29] I also investigate abatement and investment trends for each state individually in the Appendix. There were only nine units without scrubbers in MN after the court ruling and none installed scrubbers in 2010–2011.

4.2 Propensity Score and Nearest-Neighbor Matching Estimators

A potential weakness of the DID estimator with a vector of linear controls is it implicitly assumes the treatment effect must be homogeneous. This assumption would be violated if abatement choices are different on average at plants with different characteristics. Table 2 shows that units in "challenger" states were younger than other CAIR units, closer to the Powder River Basin, and more likely to be subject to cost-of-service regulation. Previous work by Cicala (2015) and Fowlie (2010) show plants operating under cost-of-service regulation have been more likely to install capital-intensive technologies. There is also reason to believe pollution controls are less likely to be installed on older units closer to retirement.

To relax the assumption of homogeneous treatment effects, I estimate a semi-parametric difference-in-differences estimator in the spirit of Abadie (2005). For this analysis, units in "challenger" states are the treated group and the pool of control units are all other coal units subject to CAIR. This estimator has two main steps. First, I flexibly estimate a propensity score function. Secondly, I reweight the observations in the treatment and control groups using the estimated propensity scores. I discuss the details regarding the propensity-score-weighting estimator in Appendix 2.

In addition to the propensity-score estimator, I also implement a nearest-neighbor-matching estimator (Abadie and Imbens 2006). Intuitively, this estimator matches treated units (units in challenger states) with other observably simlar control units (other units that were subject to CAIR). When contructing matches, I force units to be matched exactly on the binary "Regulated" variable and then I choose nearest-neighbor matches using Mahalanobis distance metric over the three continuous variables.[30] I also use the Abadie and Imbens (2006) bias correction to adjust for inexact matches in the control group. The nearest-neighbor estimate of the average treatment effect on the treated group is:

$$\widehat{ATT} = \frac{1}{N_1} \sum_{i \in \Upsilon_1} \left\{ (Y(i,2009) - Y(i,2004)) - \sum_{k \in \Upsilon_0} w_{ik}(Y(k,2009) - Y(k,2004)) \right\} \tag{6}$$

where Υ_1 is the set of all units in the treatment group, N_1 is the number of units in the treatment group, and Υ_0 includes all units in the control group. The weight placed on unit k when constructing the counterfactual estimate for treated facility i is w_{ik}.

5 Results

In this section, I present the primary empirical results and conduct a series of robustness checks.

5.1 Effects of Policy Uncertainty on Emissions (Proposition 2)

Table 3 presents regression results from Eq. 4. The primary outcome of interest is the Log SO_2 emission rate in pounds per unit of heat input. The last four columns include year

[30] The continous variables include the unit's distance to the Powder River Basin, boiler age, and baseline emission rate in 2004.

fixed effects, Column 2 includes state fixed effects, and Column 3 and 4 include unit fixed effects. In all specifications, standard errors are clustered at the unit level.[31]

In each of the first three specifications, *CAIR* (β_2) is negative and statistically significant at the 1% level. This means units in states that were scheduled to be part of the CAIR SO_2 program reduced emissions 16% more than units not scheduled to participate in the years before the program began, 2005–2009. Units anticipating the lower emissions cap under CAIR had an incentive to make early emission reductions because they could bank current allowances to use and sell under the new program. On the other hand, *Challenger* (β_1) is positive and statistically significant at the 1% level in each of the first four specifications. Units exposed to increased policy uncertainty reduce their emission rates by less relative to other states included in CAIR. This is consistent with Proposition 2 from Sect. 4. Since there was increased uncertainty as to whether units in these states would actually have to comply with the new regulations, they had a higher option value to delay abatement that required sunk irreversible investments. Additionally, we can compare emission reductions in the "challenger" states relative to states never included in CAIR by summing the coefficients *Challenger* (β_1) and *CAIR* (β_2). For specification 3, the sum of β_1 and β_2 is 0.03 and not statistically different from zero (p value = 0.43). Therefore, there is little evidence that units in "challenger" states made larger emission reductions than units that never anticipated regulation at all.

While these results are suggestive, they could be prone to several biases discussed in the previous section. To account for the possibility that the legal challenge was endogenous, I restrict the sample to only operating companies that run facilities in both "challenger" states and other states. I then estimate Eq. 4 including operator company fixed effects. The results of these regressions can be found in column 5 of Table 3. The magnitude of the estimates are only slightly changed, and the coefficient signs are consistent with the baseline estimates. However, the estimates have less power due to the much smaller sample size and *CAIR* (β_2) and *Challenger* (β_1) are no longer statistically significant.

Table 4 presents the results using the semi-parametric DID estimators of the average treatment effect on the treated (ATT). Recall that the treated group includes all units in Texas, Minnesota, and Florida while the control group includes all other units included in CAIR. Panel A shows the summary statistics for "challenger" plants and for the nearest-neighbor-matched sample of CAIR units (1 match per unit). The covariates are much more balanced than they were with the full sample. Furthermore, I fail to reject a t test of difference in means for any of the matching variables. The first column of Panel B presents the estimate using the propensity-score-weighted estimator from Eq. 21. To obtain standard errors, I bootstrap the entire two-step procedure of estimating the propensity score then calculating the weighted sample average. Columns 2–4 report the estimated ATT using the nearest neighbor matching estimator for one, three, and five matches respectively. I use the bias correction and standard errors from Abadie and Imbens (2006). All the estimates are positive and significant at conventional levels. The NN estimator appears to be robust to the number of matches chosen, and the propensity-score-weighted estimator yields a similar estimate.

To alleviate any remaining concerns that the estimated emissions differences are driven by other confounding factors, sample selection, or arbitrary modelling choices, I conduct several additional robustness checks that are described in detail in Appendix 3.

[31] The results are also robust to clustering at the plant level, operating-company level, state-year level, and state level. See Table 11 in the Appendix.

Table 3 Difference-in-difference: Dep Var: Log SO_2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Challenger (β_1)	0.134***	0.134***	0.134***	0.132
	(0.0371)	(0.0373)	(0.0371)	(0.263)
CAIR (β_2)	– 0.165***	– 0.165***	– 0.165***	– 0.197
	(0.0311)	(0.0312)	(0.0311)	(0.218)
Controls	Yes	Yes	No	Yes
Year FE	No	Yes	Yes	Yes
State FE	No	Yes	No	No
Unit FE	No	No	Yes	No
Operator FE	No	No	No	Yes
Restricted sample	No	No	No	Yes
N	5335	5335	5335	194
R^2	0.579	0.624	0.721	0.577

Specifications without unit fixed effects also control for unit observable characteristics. For the first four columns the sample includes all coal units that operated between 2002 and 2009 and did not have a scrubber installed by 2004. The fourth column restricts the sample to only units that were run by operating companies that ran plants in both "challenger" states and other states and include operating company fixed effects. All standard errors are listed in parenthesis and are clustered at the unit level. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

5.2 Mechanisms (Propositions 1 and 3)

I now turn to look at the mechanisms driving the observed differences in pollution abatement. The first two columns of Table 5 contain the estimated effects with the scrubber installation decision as the outcome variable. Units in CAIR states were more likely to install a scrubber compared to units that were not regulated under CAIR. In addition, units in "challenger" states were 20% less likely to install a scrubber in comparison to other CAIR states. This provides evidence in support of Proposition 1, that firms with a lower probability of being regulated should be less likely to make a sunk investment in pollution-control technologies.

Columns 3–5 present the estimated coefficients for the DID model with log sulfur content of coal purchases as the outcome variable. The coefficient on *CAIR* (β_2) is not significant, meaning there is no evidence that plants in CAIR states reduced the sulfur content of their coal purchases compared to non-CAIR states after the policy was announced. I do find *Challenger* (β_1) to be negative and significant for the specifications in column 3, 4, and 5, indicating plants in "challenger" states reduced the sulfur content of their fuel purchases by about 5% more relative to other plants regulated under CAIR.

The right panel of Fig. 5 plots the average sulfur content of fuel purchases for CAIR-regulated plants, non-CAIR plants, and plants in the "challenger" states. The sulfur content of coal for CAIR and non-CAIR plants remains relatively constant after the CAIR policy was announced in 2005. However, sulfur content noticeably declines at plants located in the "challenger" states. Sulfur content increases again after uncertainty is resolved in late

Table 4 Propensity-score-weighting and NN-matching estimates

	CAIR	Challenger	Difference in Means
Panel A: Summary Statistics for matched sample (1 Neighbor)			
Age (years)	32.82	32.95	0.13
	(10.10)	(9.94)	(1.81)
Log SO_2 rate 2004	− 0.16	− 0.18	0.02
	(0.46)	(0.47)	(0.06)
Distance to PRB (Miles)	1630.21	1658.72	28.51
	(528.38)	(580.42)	(93.26)
Regulated (0,1)	0.82	0.82	0
	(0.39)	(0.39)	(0)
N	44	44	

	(1)	(2)	(3)	(4)
Panel B: Propensity-score-weighting and NN-matching estimates				
ATT	0.267**	0.486***	0.376***	0.356***
	(0.106)	(0.115)	(0.106)	(0.108)
N treated	44	44	44	44
Model	P-score weight	NN match	NN match	NN match
Num. neighbors	–	1	3	5

Panel A contains summary statistics for the “Challenger” units and the nearest neighbor matched control group. Panel B presents estimates for the average treatment effect on the treated (ATT) where units located in TX, MN, and FL are the treated group and all other units in CAIR are the control group. The first columns provides the ATT for the propensity-score-weighted estimator and the last three columns include nearest-neighbor-matching estimates allowing for different numbers of neighbors. All standard errors are listed in parenthesis. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 5 Decision to install scrubber, sulfur content of coal

	(1) Scrubber	(2) Scrubber	(3) Log sulfur	(4) Log sulfur	(5) Log sulfur
Challenger (β_1)	− 0.199***	− 0.844***	− 0.0578*	− 0.0574**	− 0.0486**
	(0.0478)	(0.306)	(0.0297)	(0.0287)	(0.0235)
CAIR (β_2)	0.101**	0.538**	0.00526	0.00869	0.00434
	(0.0401)	(0.228)	(0.0198)	(0.0193)	(0.0160)
Model	OLS	Probit	OLS	OLS	OLS
Controls	Yes	Yes	Yes	Yes	No
Year FE	–	–	Yes	Yes	Yes
State FE	–	–	No	Yes	No
Plant FE	–	–	No	No	Yes
N challenger	44	44	585	585	585
N	667	667	3351	3340	3834
R^2	0.220		0.895	0.903	0.933

Scrubber installation regressions only include units that did not already have a scrubber installed as of 2004. The Scrubber regressions are cross sectional with the dependent variable equal to one if a scrubber was installed on the unit by 2009. Therefore, these regressions do not include fixed effects. "Sulfur Content" regressions (columns 3–5) are run at the plant level. "N Challenger" is the number of observations in the "challenger" group. Standard errors in parenthesis. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

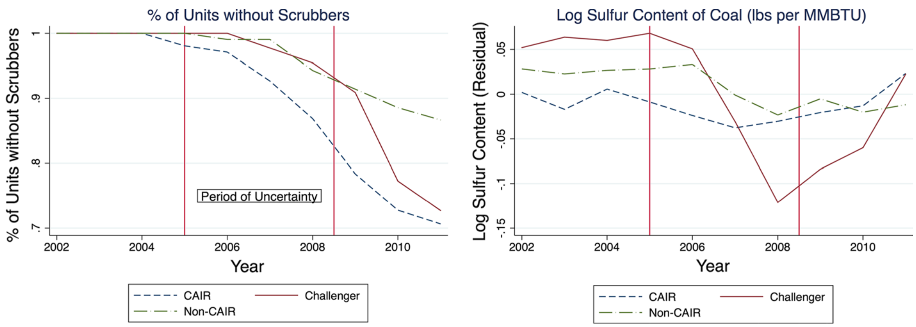

Fig. 5 Scrubber and sulfur content trends by group. In both graphs, the first red vertical line indicates the initial announcement of CAIR. The second vertical red line represents the date the court made its final ruling

2008. This is consistent with firms increasing abatement through the higher-variable-cost option while they delay investing in the high-fixed-cost option (installing a scrubber). As the probability of a more stringent emission price decreases from one, firms should be less likely to install a scrubber. Although, they still had some incentive to reduce emissions by switching to lower-sulfur fuels since permit prices increased after the announcement of CAIR.

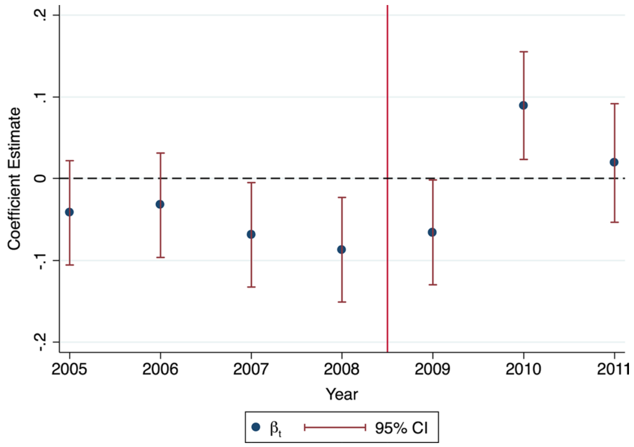

Fig. 6 Relative probability of installing a scrubber by year

5.3 Investment After the Court Ruling (Proposition 4)

In December 2008, the D.C. Circuit Court ruled that Florida and Texas would be required to participate in the CAIR program but plants located in Minnesota would be excluded. If plants were delaying investment to wait for the resolution of the uncertain policy, plants in Texas and Florida should be more likely to have installed pollution controls in the years immediately after the ruling, while plants in Minnesota would not. Minnesota only had nine coal units without pollution controls as of 2009 so it is difficult to make strong inferences from their behavior; however, none of these units installed scrubbers in the years immediately following the court ruling. To test if units in Texas and Florida were more likely to install pollution controls after the final ruling, I estimate the model from Eq. 5. This regression allows me to identify how the relative probability of units installing a scrubber in Texas and Florida changed over time.

Estimates of Eq. 5 are reported in Fig. 6. β_t can be interpreted as the average additional probability that units in Texas and Florida installed a scrubber in year t relative to other units in CAIR. For the years 2007–2009, units in Texas and Florida were less likely to install scrubbers relative to other CAIR states. A notable change occurs in 2010, β_{2010} is positive and significant, meaning units in Texas and Florida were more likely to install scrubbers relative to other CAIR units. This is plausibly due to the installations that occurred in response to the court's ruling. The court made its final decision in December 2008 and scrubbers typically take 12–24 months to install. If firms decided to install pollution equipment in late 2008, these decisions would be expected to be reflected in the data around 2010. The point estimate for β_{2011} is also positive though not statistically significant. These results provide evidence in support of Proposition 4. Namely, firms should be more likely to install a scrubber after the uncertainty is resolved in the case that they have to comply with the more stringent emissions cap regime.

Referring back to the right panel of Fig. 5, we also see that plants in the "challenger" states switched away from using lower-sulfur coal after the court announcement in late 2008. This indicates policy uncertainty was costly for firms in these states. Many firms made costly purchases of low-sulfur coal during the period of uncertainty. If the firms had known initially they would be included in CAIR, they could have avoided these coal purchases and additional permit expenditures by immediately installing pollution controls. I discuss the costs of policy uncertainty in more detail in Sect. 5.4.

5.4 Discussion: The Costs of Policy Uncertainty

A comprehensive welfare analysis of the impact of policy uncertainty would account for both changes compliance costs and pollution damages. Pollution damages vary significantly across space (Muller and Mendelsohn 2009), therefore to measure changes in pollution damages, I would need detailed estimates of how policy uncertainty shifted emissions across space. Unfortunately, the empirical estimates in the previous sections only provide insight about how emissions changed on average in the “challenger” states relative to other CAIR states. Furthermore, I would also need to account for exactly how emissions shifted across time. Because I do not have precise measures of firm-level abatement costs and associated emissions damages, a counterfactual simulation of permit market outcomes with and without the policy uncertainty is beyond the scope of this paper.

In place of a comprehensive welfare analysis, I aim to quantify the additional compliance costs incurred by plants in the “challenger” states as a result of policy uncertainty. The court ultimately ruled that Texas and Florida would have to comply with CAIR and many plants in these states made investments in pollution controls between 2009 and 2011. This suggests that policy uncertainty delayed investment in these states. If the legal challenge had never occurred, plants could have installed pollution controls sooner and avoided buying costly emissions permits during the period of uncertainty. On the other hand, the legal challenge likely led to reductions in real investment costs for plants in Florida and Texas. That is, these plants benefited in net present value terms from delaying scrubber investment. In addition, plants in Minnesota benefited from the legal challenge. Minnesota plants were less likely to make a scrubber investment that they would later regret after the court excused the state from the CAIR regulation. Therefore, to assess the total additional compliance cost that resulted from the legal challenge, I add the change in real permit expenditures in challenger states to the change in real capital costs as shown in Eq. 7. It is important to note, this calculation is an ex-post analysis of the cost of uncertainty and depends on the policy realization.

$$TotalCost = \Delta PermitExpenditures + \Delta CapitalCosts \tag{7}$$

To calculate the additional permit cost, $\Delta PermitExpenditures$ in Eq. 7, I combine the unit-level estimates of increased SO_2 emission rates due to policy uncertainty from my preferred specification (β_1 from Eq. 4) with unit-level heat-input data. This provides an estimate of increased pollution levels in the “challenger” states during the interim period before the court made its final ruling. I then take this estimate and combine it with SO_2 allowance price data from 2005 to 2009.[32] After the announcement of CAIR in 2005, permit prices immediately increased as firms responded in anticipation of the stricter future cap. Firms that chose to maintain flexibility and delay investing in pollution controls had to buy more expensive permits in order to comply with the cap (or gave up the opportunity to sell their permits at the higher price). Many of these units eventually did install pollution controls after the court made the final ruling. If firms had initially known their regulatory status,

[32] Allowance price data were obtained from EIA and EPA, the price data are the market clearing prices from the annual EPA allowance auction.

they could have installed controls sooner and reduced permit expenditures. The aggregate permit costs from delayed abatement are:

$$\Delta PermitExpend. = \sum_{t=2005}^{2009} \sum_{i \in \{TX \cup FL\}} (1+r)^{(2010-t)} \cdot \Delta \widehat{SO2rate} \cdot HeatInput_{it} \cdot PermitPrice_t \tag{8}$$

where r is the discount rate and $PermitPrice_t$ is the average SO_2 allowance price at the EPA annual auction in year t. I multiply the estimated increase in emission rate due to policy uncertainty by the heat input for each unit i in year t and then multiply this by the permit price in year t. I then sum over all the units in Texas and Florida and aggregate across all years from 2005 to 2009. Annual permit expenditures are calculated in 2010 dollars assuming a risk-free discount rate of 3.98%.[33]

To assess how the legal challenge affected capital expenditures, $\Delta CapitalCosts$ in Eq. 7, I separate the challenger plants into two groups. The first group contains plants in Texas and Florida, states where the challenge was overruled. The second group contains plants in Minnesota. Several plants in Texas and Florida did eventually invest in scrubbers after the court made a ruling in 2008. These plants accrued a financial benefit from delaying investment. To calculate this benefit, I first assume that each plant paid \$319/KW of capacity to install a scrubber, this is the average scrubber cost reported in Sharpe (2009). Second, I assume that each plant that did install a scrubber would have installed the technology 2 years earlier in the absence of the legal challenge. I assume a 2-year difference because the average CAIR plant installed a scrubber in 2008 while the average challenger plant waited until 2010. Under these assumptions, the cost savings in 2010 for each unit i equals $ScrubCost_i - \frac{ScrubCost_i}{(1+r)^2}$ where r is the discount rate (equal to 0.0398) and $ScrubCost_i = Capacity_i * 319$. The total change in capital costs for plants in Texas and Florida is the sum of these cost savings across all units that installed after 2008. Additionally, I calculate the cost savings for plants in Minnesota as $\Delta ProbInstall * ScrubCost_i$ where $\Delta ProbInstall$ is the reduction in the probability that plants in Minnesota installed a scrubbed due the legal challenge. I use β_1 from the first column Table 5 as the estimate for the change in the probability of installing.

Calculating the expression in Eq. 7, we see that policy uncertainty increased permit compliance costs by \$216 million over 5 years in Texas and Florida. Since units in the "challenger" states comprised a relatively small percentage of the overall market, the installation of several additional scrubbers would be unlikely to have a large effect on prices in the nationwide allowance market. However, if installing scrubbers at several plants in Texas and Florida pushed permit prices down, then the above calculation may overestimate actual cost savings. On the other hand, any upward bias in the estimated costs would likely be offset by additional environmental and health benefits from these units installing pollution controls sooner.[34]

In contrast, many plants saved on capital expenses as a result of uncertainty about the CAIR policy. Under the assumptions described above, I find that firms in Texas and Florida saved \$1.1 million by delaying investment in scrubber technologies. Additionally, plants in

[33] 3.98% was the average rate for 10-Year U.S. Treasury bonds during this period.

[34] Measuring the additional health costs that arose from increases in emissions is not straightforward. Although the legal challenge increased emissions in the "challenger" states, these increases were partly offset by later decreases in emissions after the court ruling (since firms still had to comply with the cap). Increased health costs are therefore a result of spatial and temporal shifts in emissions.

Minnesota also benefited from delaying investment in pollution controls because they were eventually excluded from CAIR. I find that plants in Minnesota saved \$91.9 million in capital costs by delaying investment in pollution controls. The net effect of policy uncertainty was an increase in overall compliance costs of \$123 million.

The costs of policy uncertainty appear substantial, despite the small geographic area included in the judicial review. The EPA estimated that the annual compliance costs of the CAIR SO_2 program would be \$1.8 billion.[35] The increased compliance costs resulting from the legal challenge would then be equivalent to increasing the nationwide cost of CAIR by 1.4% over the 5-year period or a 15% increase in compliance costs for plants in Florida and Texas.[36] Because the legal challenge only affected a small part of the SO_2 permit market, this highlights the potential importance of policy uncertainty. Other legal challenges often affect a much larger geographic area. For instance, the Clean Power Plan lawsuit affects electric utilities in all 50 states. policy uncertainty is likely to have even larger costs on a national or global scale.

These results suggest an often neglected cost of the policymaking process. Specifically, if firms face uncertainty regarding when and how a new policy will be implemented, this can raise the overall cost of the policy. Before creating new rules, federal agencies prepare cost-benefit analyses to compare the merits of different policy alternatives. Typically though, these analyses only incorporate the explicit benefits and costs of a policy conditional on being enacted but ignore that some policy alternatives may be more likely to be challenged in court or scrapped by future administrations. Policymakers, politicians, and federal agencies could potentially improve welfare outcomes by consciously weighing the uncertainty associated with different possible rules before announcing a notice of proposed rulemaking or when finalizing policy specifics after comments from industry participants and the public.

6 Conclusion

In recent years, policy uncertainty has become more salient in many industries such as health-care, transportation, energy, manufacturing, telecommunications, trade, finance, and banking. New regulations often require specific investments by firms. From a firm's perspective, the overall value of an investment depends mainly on if the new policy remains in place. Theoretical work has shown that policy uncertainty should cause firms to delay making sunk investments. Also, anecdotal evidence suggests that policy uncertainty can lead to increases in pollution, lower industry profits, and higher unemployment. However, little empirical evidence has been provided to support these claims.

This article provides some of the first empirical evidence that policy uncertainty can cause firms to delay investment and to alter the types of investments they choose. In the context of the Clean Air Interstate Rule (CAIR), policy uncertainty delayed reductions of sulfur dioxide emissions. In particular, firms that faced more uncertainty were more likely to delay investment in capital infrastructure to reduce emissions. Furthermore, these firms

[35] See Federal Register Vol. 70 (Thursday, May 12, 2005). The EPA estimated that the average cost of each ton of SO_2 abated would be \$500 and that the program would reduce emissions by 3.6 million tons in 2010.

[36] These calculations are obtained by multiplying EPA's predicted average cost of compliance by the required emission reduction under CAIR.

were more likely to use abatement strategies that did not require substantial fixed costs. In order to maintain flexibility, many firms decided to purchase emissions allowances as a means of compliance. I estimate that policy uncertainty increased firms' compliance costs by $123 million.

In order to address concerns about climate change, new regulations will need to be introduced and existing policies will need to be updated in the energy sector and other industrial sectors. In the United States, the EPA proposed the Clean Power Plan in 2014 as a potential policy to reduce greenhouse gas emissions from the electricity sector. The policy would require substantial investment in renewable and natural gas generation and the retirement of many existing coal plants. However, the long-term implementation of U.S. climate policy remains largely uncertain due to unknown future political conditions and unknown outcomes of the judicial review process. We have seen that this political and legal uncertainty could hinder the effectiveness of government policies that aim to spur investment in cleaner capital infrastructure.

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Appendices

Proofs of Propositions

To simply exposition, I assume without loss of generality that the the discount rate $r = 0$. The firms' problem in the first period can then be written as:

$$\min_{a_1, I_1} P_1 \cdot (\overline{e} - a_1) + C(a_1, I_1) + K \cdot I_1 + \mathbb{E}\left[\min_{a_2, I_2}\{P_2 \cdot (\overline{e} - a_2) + C(a_2, I_2) + K \cdot (I_2 - I_1)\}\right]$$

$$s.t. \quad a_t \in [0, \overline{e}], \quad I_t \in \{0, 1\}, \quad I_2 \geq I_1$$

Proof of Proposition 1

Firms are differentiated by their costs of capital. Specifically, there are three groups of firms: (1) firms that will install the capital technology in the first period, (2) firms that will wait to install the capital technology only if the high emissions price is realized, (3) firms that will never install the capital technology. The third group will have the highest cost of capital and regardless of ρ they will never install the technology. Therefore, the share of firm's adopting the technology in the first period will be determined by the cutoff capital cost K_1^* that separates the first and second groups. I will show that K_1^* is increasing in ρ and therefore the the share of adopters in period one $F(K_1^*)$ will also be increasing in ρ.

The expected net benefits from installing in period 1 are equal to: $P_1 \cdot a_1^I - C(a_1^I, 1) - K + E[\min_{a_2, I_2}\{P_2 \cdot a_2 - C(a_2, I_2)\}|I_1 = 1]$.[37] If the firm installs the technology in period 1, it saves on permit costs and abatement costs in period 1 and anticipates saving on permit costs and abatement costs in period 2; however, it also must pay the capital cost K and the abatement cost $C(a_1^I, 1)$. If the firm waits until period 2 and only installs the capital technology if the stringent price is enacted, then its expected benefit will be $P_1 \cdot a_1^N - C(a_1^N, 0) + E[\min_{a_2, I_2}\{P_2 \cdot a_2 - C(a_2, I_2) + K \cdot I_2\}|I_1 = 0]$. Firms should invest if:

$$\begin{aligned} &P_1 \cdot a_1^I - C(a_1^I, 1) - K + \mathbb{E}[\min_{a_2, I_2}\{P_2 \cdot a_2 - C(a_2, I_2)\}|I_1 = 1] \\ &\geq P_1 \cdot a_1^N - C(a_1^N, 0) + \mathbb{E}[\min_{a_2, I_2}\{P_2 \cdot a_2 - C(a_2, I_2) + K \cdot I_2\}|I_1 = 0] \end{aligned} \tag{9}$$

Let a_2^{IH} be the optimal level of abatement in period 2, conditional on having installed the capital technology ($I_2 = 1$) and emission prices being high. Let a_2^{IL} be the optimal abatement level if permit prices are low and the firm has installed the technology. Furthermore, let a_2^{NH} and a_2^{NL} be the optimal abatement levels for firms who have not installed the technology for the high and low emission price cases respectively. Expanding the expectations, we have:

$$\begin{aligned} &P_1 \cdot a_1^I - C(a_1^I, 1) - K + \rho(P_2^H \cdot a_2^{IH} - C(a_2^{IH}, 1)) \\ &+ (1-\rho)(P_2^L \cdot a_2^{IL} - C(a_2^{IL}, 1)) \geq P_1 \cdot a_1^N - C(a_1^N, 0) \\ &+ \rho(P_2^H \cdot a_2^{IH} - C(a_2^{IH}, 1) - K) + (1-\rho)(P_2^L \cdot a_2^{NL} - C(a_2^{NL}, 0)) \end{aligned} \tag{10}$$

To understand the the right-hand side of the inequality, recall that firms in the second group will install the capital technology in the second period only if the high permit price is realized, which will occur with probability ρ. The cutoff cost K_1^* for investment in the first period is determined by the capital cost at which a firm would be indifferent between investing and waiting. Setting the right and left hand sides of (10) equal to each other and solving for K we obtain:

$$\begin{aligned} K_1^* = &\frac{\left(P_1 \cdot a_1^I - C(a_1^I, 1)\right) - \left(P_1 \cdot a_1^N - C(a_1^N, 0)\right)}{1-\rho} \\ &+ \left(P_2^L \cdot a_2^{IL} - C(a_2^{IL}, 1)\right) - \left(P_2^L \cdot a_2^{NL} - C(a_2^{NL}, 0)\right) \end{aligned} \tag{11}$$

differentiating (11) with respect to ρ we have:

$$\begin{aligned} \frac{dK_1^*}{d\rho} = &\frac{P_1}{1-\rho}\left(\frac{da_1^I}{d\rho} - \frac{da_1^N}{d\rho}\right) + \frac{1}{1-\rho}\left(C_a(a_1^N, 0)\frac{da_1^N}{d\rho} - C_a(a_1^I, 1)\frac{da_1^I}{d\rho}\right) \\ &+ \frac{1}{(1-\rho)^2}\left([P_1 \cdot a_1^I - C(a_1^I, 1)] - [P_1 \cdot a_1^N - C(a_1^N, 0)]\right) + P_2^L\left(\frac{da_2^{IL}}{d\rho} - \frac{da_2^{NL}}{d\rho}\right) \\ &+ \left(C_a(a_2^{NL}, 0)\frac{da_2^{NL}}{d\rho} - C_a(a_2^{IL}, 1)\frac{da_2^{IL}}{d\rho}\right) \end{aligned} \tag{12}$$

[37] Recall a_1^I is the optimal first period abatement conditional on installing the capital technology, and a_1^N is the optimal first period abatement choice for firms that do not install the technology.

Substituting in the equilibrium conditions, $C_a(a_1, I_1) = P_1$ and $C_a(a_2, I_2) = P_2$, and canceling terms we are left with:

$$\frac{dK_1^*}{d\rho} = \frac{1}{(1-\rho)^2}\big([P_1 \cdot a_1^I - C(a_1^I, 1)] - [P_1 \cdot a_1^N - C(a_1^N, 0)]\big) \tag{13}$$

We know that the first term in brackets must be larger than the second term in brackets. To see this, notice $P_1 \cdot a_1^I - C(a_1^I, 1) \geq P_1 \cdot a_1^N - C(a_1^N, 1)$ since a_1^I is the optimal abatement choice conditional on having $I_1 = 1$ by definition. Additionally we know that $P_1 \cdot a_1^N - C(a_1^N, 1) > P_1 \cdot a_1^N - C(a_1^N, 0)$ which follows from the assumption that marginal cost of abatement is lower once the capital technology is installed. This means the term in the large parentheses is positive, and since $\frac{1}{(1-\rho)^2}$ is positive this implies $\frac{dK_1^*}{d\rho} > 0$. Finally, since the cumulative distribution function F must be non-decreasing in its argument it follows that $\frac{dF(K_1^*)}{d\rho} \geq 0$ □

Proof of Proposition 2

We will show that $\frac{d\mathbf{e_1}}{d\rho} < 0$. Total emissions is equal to the sum of emissions from firms that invest in the technology and emissions from those that do not:

$$\mathbf{e_1} = M\big(F(K_1^*)(\overline{e} - a_1^I) + (1 - F(K_1^*))(\overline{e} - a_1^N)\big) \tag{14}$$

We next differentiate with respect to ρ to obtain a comparative static:

$$\frac{d\mathbf{e_1}}{d\rho} = M\left[f(K^*)\frac{dK^*}{d\rho}((\overline{e} - a_1^I) - (\overline{e} - a_1^N)) - F(K^*)\frac{da_1^I}{d\rho} - (1 - F(K^*))\frac{da_1^N}{d\rho}\right] \tag{15}$$

where f is the probability density function of K. We know that the first term in the brackets, $f(K^*)\frac{dK^*}{d\rho}((\overline{e} - a_1^I) - (\overline{e} - a_1^N))$, is negative since f is non-negative by definition, $\frac{dF(K_1^*)}{d\rho}$ is positive as shown above, and $((\overline{e} - a_1^I) - (\overline{e} - a_1^N))$ is negative because firms that install the technology will have lower emissions. The next two terms in the brackets are equal to zero because $\frac{da_1^I}{d\rho} = 0$ and $\frac{da_1^N}{d\rho} = 0$, this can be shown by differentiating the first order condition $C_a(a_1, I_1) = P_1$ with respect to ρ. Therefore, since M is also non-negative, it must be the case that $\frac{d\mathbf{e_1}}{d\rho} \leq 0$. □

Proof of Proposition 3

Let $\mathbf{a_1^N}$ denote total abatement by firms that do not adopt the technology in the first period, $\mathbf{a_1^N} = \sum_i a_{i1} \cdot \mathbb{1}(I_{i1} = 0)$. Total abatement by non-adopters is equal to:

$$\mathbf{a}_1^N = M\big((1 - F(K_1^*))(a_1^N)\big) \tag{16}$$

Differentiating with respect to ρ we have:

$$\frac{d\mathbf{a}_1^N}{d\rho} = M\left[\underbrace{-f(K_1^*)\frac{dK_1^*}{d\rho}(a_1^N)}_{[1]} + \underbrace{(1 - F(K_1^*))\frac{da_1^N}{d\rho}}_{[2]}\right] \tag{17}$$

As the probability of the high price regime increases, more firms adopt the technology, which works to reduce total abatement by non-adopters, this effect is labeled [1] in Eq. 17. This term is negative since $f(K_1^*)$, a_1^N are positive and $\frac{dK_1^*}{d\rho}$ is positive by Proposition 1. Since $\frac{da_1^N}{d\rho} = 0$, the term labeled [2] in Eq. 17 equals zero. Therefore, $\frac{d\mathbf{a}_1^N}{d\rho} \leq 0$.

Proof of Proposition 4

Define K_2^* as the the cutoff capital cost that a firm would be indifferent to installing the technology in the second period, conditional on the high price regime occurring:

$$K_2^* = (P_2^H \cdot a_2^{IH} - C(a_2^{IH}, 1)) - (P_2^H \cdot a_2^{NH} - C(a_2^{NH}, 0)) \tag{18}$$

Notice the cutoff does not depend on ρ since the uncertainty has already been resolved at this point. The number of firms that adopt is the number of firms that have capital costs smaller than K_2^* but have capital costs larger than K_1^* (i.e., did not invest in the first period). This number of firms can be expressed as:

$$M * \max\{0, (F(K_2^*) - F(K_1^*))\} \tag{19}$$

Let $\hat{\rho}$ be defined such that $K_2^* = K_1^*(\hat{\rho})$. Then differentiating (19) we obtain:

$$\frac{d[M * \max\{0, (F(K_2^*) - F(K_1^*(\rho)))\}]}{d\rho} = \begin{cases} 0, & \text{if } \rho > \hat{\rho} \\ M(-f(K_1^*)\frac{dK_1^*}{d\rho}), & \text{if } \rho < \hat{\rho} \end{cases} \tag{20}$$

It follows from the proof of Proposition 1 that $M(-f(K_1^*)\frac{dK_1^*}{d\rho}) < 0$. Therefore, the number of adopters in period 2 must increase as ρ decreases, conditional on the high price regime occurring. □

Propensity-Score Weighting Details

Let $Y^0(i,t)$ represent the emission rate unit i would attain at time t in absence of treatment. Similarly, let $Y^1(i,t)$ represent the emission rate unit i would attain at time t if exposed to the treatment. The effect of the treatment on the outcome for unit i at time t is defined as $Y^1(i,t) - Y^0(i,t)$. Additionally, let $D(i)$ be an indicator function determining if unit i receives the treatment. Also define $P(D = 1|X)$ as the propensity score, the probability a unit receives treatment conditional on observed covariates. For this analysis, the treatment group will be all units located in the “challenger” states and the control group will be all other units in CAIR.[38] I use the year 2004 emission rate as the pre-period observation and the 2009 emission rate as the post-period observation. The objective is to estimate the average treatment effect on the treated group (ATT): $E[Y^1(i,2009) - Y^0(i,2004)|D(i) = 1]$. Estimation of the ATT requires a weaker assumption on distribution of covariates than would be required to estimate the population average treatment effect (ATE). For identification, require that for all X, $P(D = 1|X) < 1$, in addition to the unconfoundedness

[38] The semi-parametric DID estimator only allows for one treatment group and one control group so I omit units outside CAIR.

assumption.[39] Appendix 4 includes marginal kernel density plots of the continuous covariates for each group which demonstrate that this overlap condition is satisfied. Therefore, the average treatment on the treated is given by:

$$
\begin{aligned}
ATT =& E[Y^1(i,2009) - Y^0(i,2009)|D(i) = 1] \\
=& \int E[Y^1(i,2009) - Y^0(i,2009)|X(i), D(i) = 1]dP(D = 1|X) \\
=& \int E[\rho_o \cdot (Y(i,2009) - Y(i,2004))|X(i)]dP(D = 1|X) \\
=& E\left[\rho_0 \cdot (Y(i,2009) - Y(i,2004)) \cdot \frac{P(D = 1|X)}{P(D = 1)}\right] \\
=& E\left[\frac{(Y(i,2009) - Y(i,2004))}{P(D = 1)} \cdot \frac{D - P(D = 1|X)}{(1 - P(D = 1|X))}\right] \\
& where \quad \rho_0 = \frac{D - P(D = 1|X)}{P(D = 1|X)(1 - P(D = 1|X))}
\end{aligned}
\tag{21}
$$

The third line follows from the unconfoundedness assumption, after controlling for observed covariates, the treatment and control groups would have followed parallel paths absent the intervention. The estimator is the sample analog of the fifth line in (21). Intuitively, the estimator is down weighting the distribution of $Y(i,2009) - Y(i,2004)$ for the untreated group for values of the covariates which are over-represented among the untreated and weighting-up $Y(i,2009) - Y(i,2004)$ for those values of the covariates under-represented among the untreated. I estimate the propensity score using a flexible logit model that includes interactions of all the covariates and quadratic terms.

Robustness Checks

To address additional identification concerns, I conduct several robustness checks. In Panel A of Table 7 in Appendix 4, I estimate the model, excluding 2009 from the sample to account for the possibility that firms reacted quickly to the court decision.[40] Dropping 2009 does not cause any noticeable changes to the estimated effects. In Panel B of Table 7, I restrict the sample to only units within 1000 miles of the centroid of Texas, Florida, or Minnesota. It is possible that the variable "Distance to the Powder River Basin" is not sufficiently controlling for coal purchasing opportunities. For example Florida and New Hampshire may be similar distance to the Powder River Basin but may face significantly different opportunity cost of buying low-sulfur coal. Restricting the sample to only nearby plants does not change the direction or statistical significance of the coefficients of interest.

[39] Estimation of the ATE requires the overlap condition: $0 < P(D = 1|X) < 1$.

[40] 2009 was after the court ruling so it is possible firms could have reduced emissions after the ruling was made. Scrubbers usually take over a year to install, and coal is usually purchased on 1-year contracts so this is unlikely but possible.

It is also possible that other political or legal factors are driving differences in pollution abatement and not policy uncertainty. I attempt to address some of these potential concerns in Table 8 of Appendix 4. For instance, during the time frame of this study, some power plants were required to install pollution controls due to the New Source Review (NSR) lawsuits. It is a priori possible that NSR requirements are driving results if many of these lawsuits occurred in CAIR states. Panel A of Table 8 reports estimates of the baseline model on a restricted sample that excludes any plant that was subject to NSR litigation related to SO_2 emissions. The results are robust to the exclusion of these plants, which mitigates concerns that NSR lawsuits are impacting the results.

Another potential concern is that other political or institutional factors impacted emission reductions. Panel B in Table 8 shows estimates of the baseline model from Eq. 4 but only including units in states that had a Republican governor in 2006 and choose PUC chairmen by appointment. In 2006, Texas, Minnesota, and Florida all had Republican governors and appointed PUC chairmen. This restricted sample attempts to deal with possible confounding political factors that would make installing pollution controls more feasible in some states. Since most states did not have both a Republican governor and an appointed PUC commission,[41] 75% of the observation are dropped. However, the point estimate is still positive, statistically significant, and of similar magnitude. This result suggests that the baseline result is not being driven by confounding political factors.

An additional potential concern with the analysis is that the border states' legal challenge initially only challenged the inclusion of Texas plants that were located west of the north-south I-35/I37 corridor. In the baseline analysis, I included all Texas units in the challenger group because EPA almost always levies regulations for entire states to avoid in-state pollution havens. As a robustness check, I have also run the baseline regressions with plants in East Texas, discluded from the "Challenger" group. The results are shown in Table 10 and the results are robust to this change.

I also account for the possibility that firms were changing electricity output as a method of compliance. In particular, I estimate the model with total annual SO_2 emissions in tons as the dependent variable. I also estimate the baseline DID regression with the natural logarithm of SO_2 in tons as the dependent variable and also with the emission rate as the outcome variable. The results of all of these regressions are consistent with the baseline model and are presented in Table 12 in the Appendix 4.

Finally, I present estimates with alternative standard error clusters. In Table 11, I allow for clustering at the unit level, plant level, state-year level, and state level. These alternative clusters do not change the significance of the estimated effects. In Table 9 also shows that the results are robust to using the entire sample of coal units (including scrubbed units) and also to using an unbalanced panel (including units that did not operate in each year of the sample).

[41] Many states elect PUC commissioners.

Tables and Figures

See Figs. 7, 8 and Tables 6, 7, 8, 9, 10, 11 and 12.

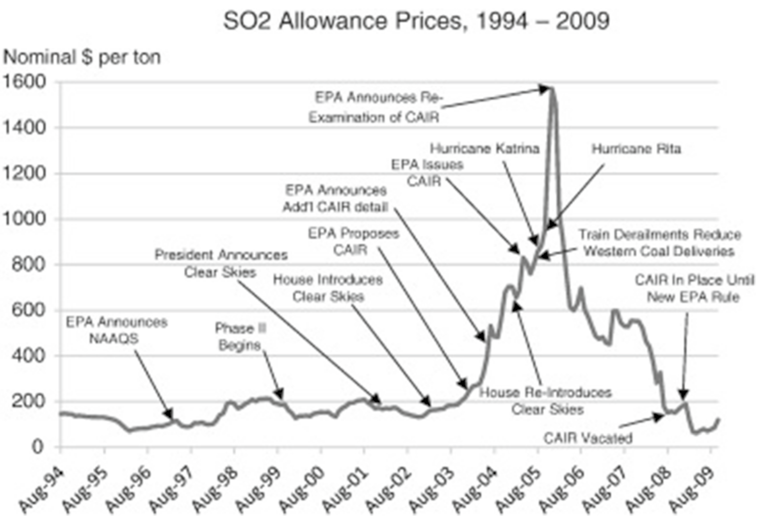

Fig. 7 SO_2 Allowance price history. *Source*: Hitaj and Stocking (2016)

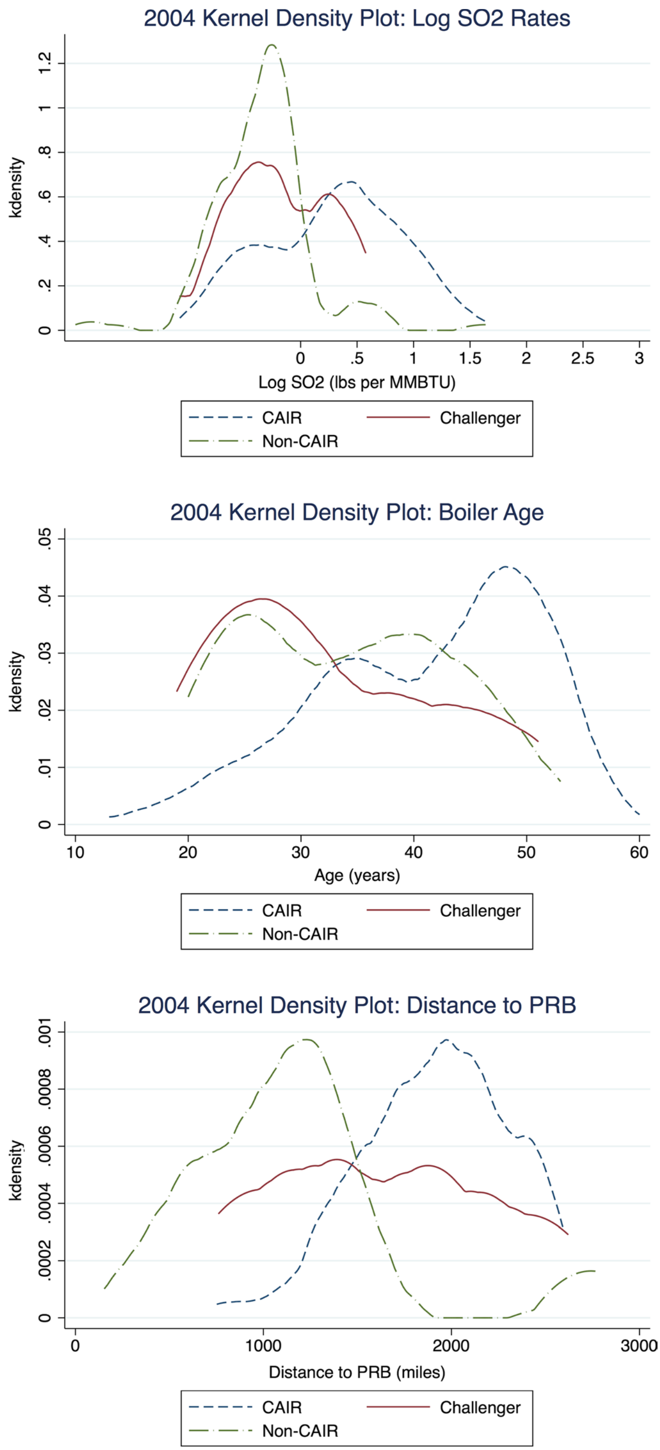

Fig. 8 Marginal kernel density plots for observed covariates

Table 6 Diff-in-diff: Acid Rain Program Phase 2 (2000)

	(1)	(2)	(3)	(4)
Panel A: Log SO_2 (lbs/MMBtu)				
Challenger	− 0.0541	− 0.0541	− 0.0541	− 0.0541
	(0.0666)	(0.0666)	(0.0671)	(0.0768)
CAIR	0.0272	0.0272	0.0272	0.0272
	(0.0393)	(0.0393)	(0.0396)	(0.0453)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	Yes	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	2704	2704	2704	2704
r2	0.874	0.874	0.881	0.921

	(1)	(2)
Panel B: Decision to install scrubber		
Challenger	0.00463	0.342
	(0.0266)	(0.359)
CAIR	− 0.0548**	− 0.602**
	(0.0218)	(0.301)
Model	OLS	Probit
Controls	Yes	Yes
N_Challenger	46	46
N	676	676
r2	0.0714	

Panel A reports regressions results for a diff-in-diff regression with 1996–1999 as the pre-period and 2000–2001 as the post-period and SO_2 emission rate as the dependent variable. For Panel A, standard errors are clustered at the unit level. Panel B reports estimates for both OLS and Probit models where the dependent variable is a binary decision to install a Scrubber by 2001, the sample includes all units that did not already have a scrubber installed in 1996, *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 7 Robustness checks—diff-in-diff with sample restrictions 1—Dep Var: Log SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Panel A: Drop 2009				
Challenger	0.0930***	0.0930***	0.0930***	0.0930***
	(0.0298)	(0.0298)	(0.0299)	(0.0321)
CAIR	− 0.131***	− 0.131***	− 0.131***	− 0.131***
	(0.0257)	(0.0257)	(0.0258)	(0.0277)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	4669	4669	4669	4669
r2	0.702	0.716	0.712	0.797
	(1)	(2)	(3)	(4)
Panel B: Only plants near MN,FL,TX				
Challenger	0.113***	0.113***	0.113***	0.113***
	(0.0404)	(0.0405)	(0.0405)	(0.0432)
CAIR	− 0.197***	− 0.198***	− 0.197***	− 0.198***
	(0.0331)	(0.0332)	(0.0332)	(0.0354)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	3327	3327	3327	3327
r2	0.510	0.538	0.528	0.668

Panel A reports regressions results for the baseline diff-in-diff regression from Eq. 4 excluding data from 2009. For this regression, the pre-period is 2002–2004 and 2005–2008 is the post-period. The court made a ruling in December 2008, so it is possible that firms could react to the announcement by reducing emissions in 2009. Panel B estimates the baseline model from Eq. 4 but excluding all units that are located further than 1000 miles from the centroid of TX, MN, or FL. All standard errors are clustered at the unit level, *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 8 Robustness checks—diff-in-diff with sample restrictions 2—Dep Var: Log SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Panel A: Drop units subject to NSR Lawsuits				
Challenger	0.107***	0.107***	0.107***	0.107***
	(0.0369)	(0.0370)	(0.0371)	(0.0395)
CAIR	− 0.138***	− 0.138***	− 0.138***	− 0.138***
	(0.0308)	(0.0309)	(0.0310)	(0.0330)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	5007	5007	5007	5007
r2	0.609	0.633	0.629	0.741
	(1)	(2)	(3)	(4)
Panel B: Only units in states with republican appointed PUC chairmen				
Challenger	0.155**	0.155**	0.155*	0.155*
	(0.0785)	(0.0787)	(0.0787)	(0.0839)
CAIR	− 0.223***	− 0.223***	− 0.223***	− 0.223**
	(0.0829)	(0.0831)	(0.0831)	(0.0886)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	1376	1376	1376	1376
r2	0.578	0.601	0.588	0.709

Panel A reports regressions results for the baseline diff-in-diff regression from Eq. 4 excluding any unit that were subject to New Source Review litigation related to SO_2 emissions. During the time frame of this study, some power plants were required to install pollution controls due to NSR regulations, all of these plants are excluded from the sample. I thank Ian Lange for supplying NSR information. Panel B estimates the baseline model from Eq. 4 but only including units in states that had a Republican governor in 2006 and choose PUC chairmen by appointment. In 2006, TX, MN, and FL all had Republican governors and appointed PUC chairmen. This restricted sample attempts to deal with possible confounding political factors that would make installing pollution controls more feasible in some states. All standard errors are clustered at the unit level. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 9 Robustness checks 3—diff-in-diff with sample changes—Dep Var: Log SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Panel A: Unbalanced panel				
Challenger	0.142***	0.138***	0.141***	0.127***
	(0.0321)	(0.0317)	(0.0326)	(0.0334)
CAIR	− 0.130***	− 0.131***	− 0.122***	− 0.113**
	(0.0361)	(0.0364)	(0.0409)	(0.0459)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	6329	6329	6329	6464
r2	0.603	0.623	0.618	0.757
	(1)	(2)	(3)	(4)
Panel B: All units (including scrubbed)				
Challenger	0.104***	0.103***	0.103***	0.0959***
	(0.0319)	(0.0317)	(0.0319)	(0.0338)
CAIR	− 0.0558*	− 0.0568*	− 0.0539	− 0.0457
	(0.0326)	(0.0328)	(0.0333)	(0.0379)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	8096	8096	8096	8278
r2	0.789	0.798	0.795	0.865

Panel A reports regressions results for the baseline diff-in-diff regression from Eq. 4 but includes all units even units that were not operating in each time period throughout the sample. Panel B reports regressions results for the diff-in-diff regression from Eq. 4 but includes the full population of coal units including units that already had scrubbers installed before 2005. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 10 Robustness check—disclude east texas plants from challenger group

	(1)	(2)	(3)	(4)
Challenger	0.134***	0.134***	0.134***	0.134***
	(0.0371)	(0.0371)	(0.0372)	(0.0397)
CAIR	− 0.165***	− 0.165***	− 0.165***	− 0.165***
	(0.0311)	(0.0311)	(0.0312)	(0.0332)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	5335	5335	5335	5335
r2	0.579	0.606	0.596	0.721

This table reports regressions results for the diff-in-diff regression from Eq. 4 but but does not include plants in East Texas in the Challenger Group. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 11 Alternative SE clusters: DiD Dep Var: Log SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Challenger	0.134***	0.134***	0.134***	0.134***
	(0.0397)	(0.0514)	(0.0431)	(0.0455)
CAIR	− 0.165***	− 0.165***	− 0.165***	− 0.165v
	(0.0332)	(0.0467)	(0.0364)	(0.0519)
Year_FE	Yes	Yes	Yes	Yes
Unit_FE	Yes	Yes	Yes	Yes
SE_Cluster	Unit	Plant	State-Year	State
N	5335	5335	5335	5335
r2	0.721	0.721	0.721	0.721

This table reports regressions results for the baseline diff-in-diff regression from Eq. 4 allowing for alternative standard error clustering by Unit, Plant, State-Year, and State. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

Table 12 Robustness checks—alternative dependent variables

	(1)	(2)	(3)	(4)
Panel A: Levels SO_2 *(tons)*				
Challenger	755.9*	759.4*	771.6*	677.4
	(437.9)	(437.4)	(433.8)	(500.2)
CAIR	− 1808.5***	− 1810.4***	− 1812.5***	− 1787.9***
	(246.4)	(246.5)	(246.1)	(271.7)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	5335	5335	5335	5335
r2	0.798	0.812	0.804	0.869
	(1)	(2)	(3)	(4)
Panel B: Log(SO_2 *(tons))*				
Challenger	0.134***	0.134***	0.134***	0.131**
	(0.0371)	(0.0371)	(0.0372)	(0.0620)
CAIR	− 0.169***	− 0.169***	− 0.169***	− 0.239***
	(0.0311)	(0.0311)	(0.0312)	(0.0357)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	5335	5335	5335	5335
r2	0.826	0.837	0.833	0.829
	(1)	(2)	(3)	(4)
Panel C: SO_2 *(lbs/MMBtu)*				
Challenger	0.133***	0.133***	0.133***	0.133***
	(0.0309)	(0.0309)	(0.0310)	(0.0330)
CAIR	− 0.165***	− 0.166***	− 0.165***	− 0.166***
	(0.0258)	(0.0258)	(0.0258)	(0.0275)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	5335	5335	5335	5335
r2	0.799	0.808	0.808	0.874

This table reports regressions results for the baseline diff-in-diff regression from Eq. 4 with alternative dependent variables. Panel A uses the level of emissions, SO_2 in tons, Panel B uses the natural logarithm of SO_2 in tons as the outcome variable, and Panel C uses the emission rate of SO_2 in lbs/MMBtu. All standard errors are clustered at the unit level. *indicates $p < 0.10$, **indicates $p < 0.05$, and ***indicates $p < 0.01$

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