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

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Abstract

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 find that plants with a lower probability of being regulated invested in fewer capital-intensive pollution controls and reduced pollution by less overall. After the court ruled to enforce CAIR, many of these plants switched to capital-intensive pollution controls. Regulatory uncertainty increased compliance costs by $386 million.

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Most environmental regulations are subject to considerable uncertainty. Policies implemented by regulatory agencies are almost always challenged in court, which can lead to rules being delayed, altered, or canceled. 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 Gas Air Pollution Standards have all been challenged repeatedly. As another example, the European Union Emissions Trading System has seen much uncertainty about the process for setting the program's emissions cap and about how permit allocations may change over time. Regulatory uncertainty can make future market conditions less predictable and plausibly alters firms' investments.

Regulatory uncertainty may have especially pernicious effects in environmental policymaking because these policies often require large capital investments, such as overhauls in electricity generation infrastructure. As a particularly important example, the U.S. Clean Power Plan would establish carbon emissions requirements for each state and thus redirect capital investment throughout the electricity sector. However, the United States Supreme Court recently stayed implementation of the rule, which means the Environmental Protection Agency (EPA) must halt enforcement while the rule undergoes additional review. The long legal review process subjects market participants to persistent uncertainty about the future regulatory environment. Economic theory suggests that uncertainty should cause firms to delay making irreversible investments (Bernanke, 1983; Dixit and Pindyck, 1994; McDonald and Siegel, 1986; Pindyck, 1988). Regulatory uncertainty could therefore cause firms to delay investment in cleaner technologies or alter the types of investments they make. In a statement opposing the stay, one group of power producers claimed that "[the stay is] preventing them from moving forward with major investments at this time" (Ayres, 2015). Despite these concerns, we have limited empirical evidence on how policy uncertainty affects pollution abatement and investment. 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.

I take advantage of a unique quasi-experiment to estimate how policy uncertainty affects pollution abatement and firm investment decisions. This experiment arose during 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.[1] 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, who were located on the border of the CAIR-regulated area. These "challenger" states argued that they should not be subject to the

[1] 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.

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 would have to wait for the court to decide if they would actually have to comply with CAIR.[2] 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.

I show that coal plants with a lower probability of being regulated 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 in these states were instead more likely to purchase costly emissions permits or to use lower-fixed-cost abatement strategies like switching to lower-sulfur coal.[3] 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.[4] If the judicial challenge had never occurred, plants in Florida, Minnesota, and Texas could have installed pollution controls sooner and saved as much as $386 million in permit expenses.

I also identify the effect of regulatory 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 regulatory 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 significantly 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 the differences in emission reductions are not explained by selection into the "challenger" group. In particular, I find similar results if 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.

Previous literature has theoretically investigated the effects of policy uncertainty. Stokey (2016) develops a model of investment decisions in which uncertainty about a one-time change

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

[3] 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.

[4] 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.

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.[5] In the current article, I test empirically whether reducing the probability that a fixed policy will be enacted causes firms to temporarily stop investing. I also test whether firms increase investment after the uncertainty is resolved.

In other related work, Hassett and Metcalf (1999) consider the impact of tax policy uncertainty on the level of aggregate investment. 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 provides a justification for utilities delaying investment and choosing shorter-lead-time technologies.[6]

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 (Baker et al., 2016; Born and Pfeifer, 2014; Fernandez-Villaverde et al., 2015) or to price political uncertainty (Kelly et al., 2014; Pástor and Veronesi, 2013). 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 (Hurn and Wright, 1994; List and Haigh, 2010; Moel and Tufano, 2002). 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.[7] In a related paper, Fabrizio (2012) examines the effects of regulatory 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

[5]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 (Dixit and Pindyck, 1994; McDonald and Siegel, 1986; Pindyck, 1988). 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.

[6]A growing literature in environmental economics compares the theoretical effects of different regulatory policies for inducing investment and R&D in new technologies (Chao and Wilson, 1993; Krysiak, 2008; Laffont and Tirole, 1996; Requate, 2005; Requate and Unold, 2003). 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.

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

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. Furthermore, I am able to quantify the additional compliance costs attributable to regulatory 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 Section 3, 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 Section 6 concludes.

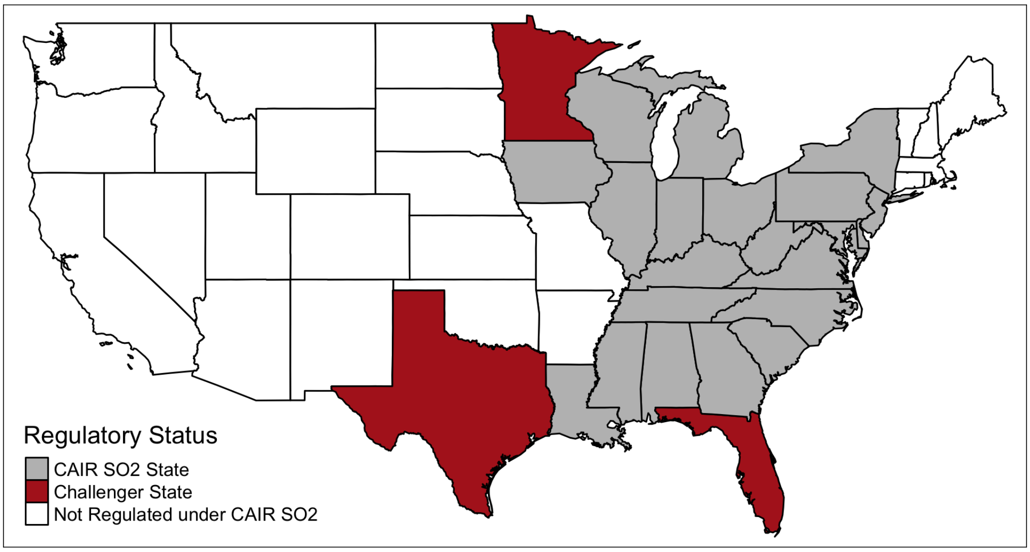
2 Policy and Institutional Background

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 ten years and previous analyses suggest that the net benefits of the program were between \$58-\$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-hour 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 one 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. Sources that were included in the CAIR SO_2 program reduced their emissions by over 50% in the five years between the initial CAIR announcement and the start of the new program (EPA, 2016).

Figure 1: CAIR SO_2 Regulatory Footprint



2.1 Regulatory Challenge and Uncertainty

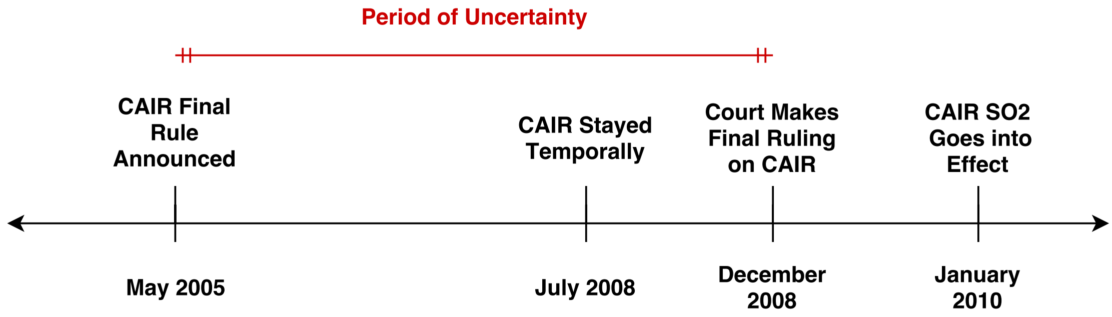
Shortly after the announcement of CAIR, several states and industry groups filed a series of lawsuits challenging the legality of the new EPA rule. The collection of lawsuits was aggregated into a single case called *North Carolina vs. Environmental Protection Agency*. Three states located along the 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. 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 regulatory 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.[8]

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.[9] The court agreed with Minnesota's claim that their emissions were not significantly affecting down wind states' compliance with NAAQS. On

[8] *North Carolina vs. Environmental Protection Agency* also contained other challenges to CAIR. The majority of these challenges claimed that CAIR *did not* adequately address specific states' contribution to non-attainment of downwind states with federal air quality standards. 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.

[9] 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.

Figure 2: CAIR Regulatory Timeline



the other hand, the court rejected Texas and Florida's similar claims.[10] 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 important events in the rollout of CAIR.

Throughout the rest of the article, I focus on measuring the effects of regulatory uncertainty that arose from the 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.

In the next section, I develop a two-period model of firm compliance with a 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 $P_2^H > P_2^L$. 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.[11] Reducing

[10]For a more detailed discussion of the judicial challenge, please see Appendix A.

[11]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 overall permit market.

emissions is costly for firms. However, firms can reduce their marginal abatement cost by 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., switching to low-sulfur coal).[12]

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 of abatement is lower once the capital technology is installed.[13] 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 $C_{aa}(a_{it}, I_{it}) > 0$. Finally, normalize $C(0, I_{it}) = 0$. This normalization implies that choosing zero abatement is costless regardless of the technology decision.

Each period, firms choose whether to install the technology at time t. 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:

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

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

[12]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.

[13]Modeling a new investment as reducing marginal abatement cost is standard in the theoretical literature investigating environmental policy instruments and technology adoption, see Jung et al. (1996); Milliman and Prince (1989); Requate and Unold (2003), and see Amir et al. (2008) for a discussion. This assumption is consistent with SO_2 compliance costs at coal plants, because the marginal costs of running a scrubber after installation are very low.

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 equilibrium permit price. All firms that do not install the technology will choose the same abatement level a_1^N, and all firms that do install the technology will choose a_1^I. 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.

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 opportunity cost 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.[14] 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:

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

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.*

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

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 emissions 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 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. However, many firms will still want to reduce emissions immediately while also maintaining the option to adopt the technology next period. In the context of CAIR, firms could choose a lower-fixed-cost abatement strategy like switching to 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 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.

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 decrease the sulfur content of 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.

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.[15] 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. I am interested in reductions in SO_2 emissions so I restrict the sample to only coal-fired boilers.[16] The CEMS database includes all generators with nameplate capacity over 25 MW and thus includes essentially all coal units in the contiguous United States. The EPA also provides descriptive data for each unit including the capacity, geographic coordinates, beginning date of operation, name of operating company, and a description of any pollution control technology installed.

To better understand how uncertainty affects coal-purchasing decisions, I also obtain plant level fuel receipts data from the Energy Information Agency (EIA) and the Federal Energy Regulatory Commission (FERC). From 2002-2007, 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 2007-2011, all coal plants reported fuel purchases on a single form, EIA Form 923. The fuel receipts data include the quantity of coal purchased (short tons), 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, less likely to have a scrubber installed, and produced less gross output. Distance to

[15] 2002-2011 is the time frame for the main analysis. I also collect data going back as far 1996 that is used for an additional test.

[16] Coal boilers emit over 99.5% of all SO_2 emissions from the electric-power industry.

Table 2: Coal Unit Summary Statistics by Group Before CAIR

	CAIR	Non-CAIR	Challenger	Total
Gross Load (TWH)	1.956	2.583	3.162	2.194
	(1.847)	(1.792)	(1.917)	(1.881)
Distance to PRB (Miles)	1904.9	991.7	1747.1	1701.2
	(389.2)	(593.0)	(627.5)	(589.8)
Regulated (0,1)	0.723	0.915	0.778	0.768
	(0.448)	(0.279)	(0.418)	(0.422)
Age (Years)	39.91	31.85	26.68	37.06
	(10.79)	(10.28)	(11.83)	(11.71)
Capacity (Max HI bil. btu)	3.491	4.301	5.232	3.815
	(2.764)	(3.053)	(3.197)	(2.916)
Scrubber (0,1)	0.152	0.418	0.395	0.229
	(0.359)	(0.495)	(0.492)	(0.420)
SO_2 2004 (lbs/MMBtu)	1.406	0.574	0.705	1.171
	(0.948)	(0.507)	(0.437)	(0.914)
SO_2 2009 (lbs/MMBtu)	1.077	0.532	0.625	0.923
	(0.891)	(0.489)	(0.413)	(0.824)
SO_2 Difference (lbs/MMBtu)	-0.329	-0.0420	-0.0801	-0.248
	(0.696)	(0.234)	(0.264)	(0.611)
N	880	200	88	1,168

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. Standard deviations are in parentheses.

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.

Comparing the 2004 SO_2 emission rates in the bottom of Table 2 to the emission rates for 2009, it's clear that emission reductions were much larger in CAIR states. The average emission rate dropped by 0.33 lbs. per MMBtu in CAIR states, while it only dropped by 0.04 lbs. per MMBtu in non-CAIR states and 0.08 lbs. per MMBtu in "challenger" states. CAIR states were also more likely to install scrubbers between 2004 and 2009. These descriptive results are consistent with the theory that regulatory 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 regulatory uncertainty. Another possibility is 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 discussion, 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 an analogous empirical approach can be used to test Proposition 3. Specifically, I test if uncertainty generated by the legal challenge caused firms that didn't 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 SO_2 emission rates after the policy announcement at units subject to additional legal uncertainty to other units in CAIR and to 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 "treatment" subgroup composed of units located in one of the states subjected to additional regulatory 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:

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

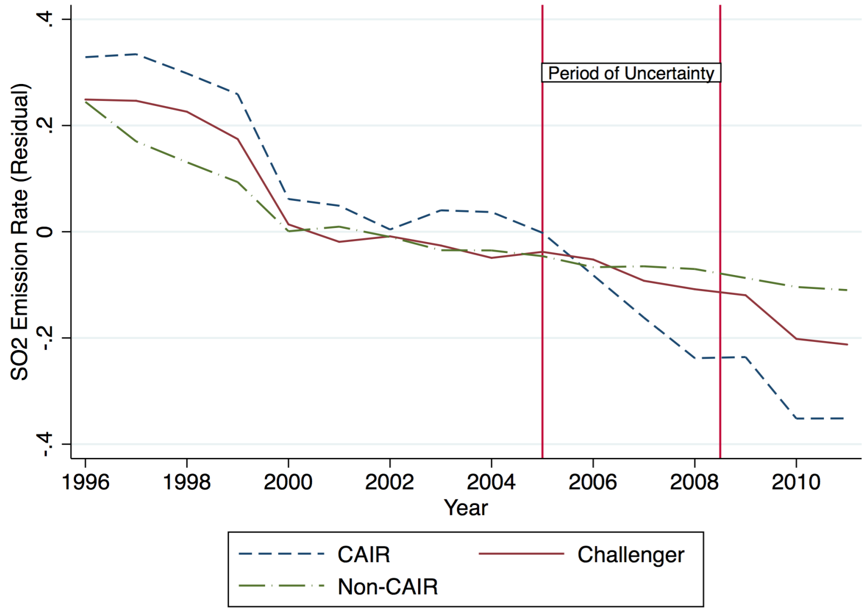
The dependent variable is unit $i's$ SO_2 emission rate in lbs. per MMBtu[17] in year t. $\mathbb{1}[Challenger_{it}]$ is an indicator variable, equal to one if the year is 2005-2009 and the unit is located in a Minnesota, Florida, or Texas. The period of 2005-2009 includes years after the policy was 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, regulatory status, distance to the Powder River Basin, and emission rate in 2004 before the policy was announced. 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 variable proxies for a firm's ability to purchase low-sulfur coal. Finally, I control for each unit's emission rate in 2004, since abatement opportunities may be limited if a unit already has a very low emission rate.

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 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.

An important identifying assumption is unconfoundedness; it must be true that after controlling for observed covariates (unit fixed effects), average emission rates for the treatment groups and the control group would have followed parallel trends absent the intervention. Figure 3 shows the average 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 four years before the policy was announced. 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 unconfoundedness assumption, I estimate the DID model (4) with the pre-period as 1996-1999, and the post-period as 2000-2001. The results of these regressions can be found in Panel A of Table 8 of the appendix. In all specifications, I fail to reject the null hypothesis that $\beta_1 = 0$. The point estimates are also small in magnitude. Panel B of Table 8 of the appendix presents regressions with the dependent variable as a binary choice to install a scrubber. I

[17] 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 per MWH and annual SO_2 emissions (tons) as the outcome variable.

Figure 3: SO_2 Emission Rates Trends by Group



The plotted trend lines represent the mean 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.

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 regulatory 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 regulatory 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 for 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%.[18] 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 had an effect on permit prices, the effect would likely bias against finding $\beta_1 > 0$. Since the legal challenge reduced the probability that firms in the "challenger" states would have to comply with CAIR, this would reduce expected demand for permits and tend to drive down permit prices. Lower permit prices should cause other firms in CAIR to be less likely to make early investments in pollution control. 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 Proposition 1 and 3

In addition to measuring the impact of regulatory 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 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.

[18] 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.

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:

$$\mathbb{1}[Scrubber]_{it} = \beta_t \mathbb{1}[Challenger]_{it} \cdot \mathbb{1}[year]_t + \lambda_t \mathbb{1}[CAIR]_{it} \cdot \mathbb{1}[year]_t + \mathbf{x_i'}\boldsymbol{\gamma} + \gamma_t + \epsilon_{it}, \tag{5}$$

where $\mathbb{1}[Scrubber_{it}]$ is an indicator variable, set equal to one if unit i has a scrubber installed in year t and zero otherwise, and $\mathbb{1}[year]_t$ is a year dummy. Again, $\mathbf{x_i'}$ controls for unit observable characteristics and also contains a dummy for if the unit had a scrubber installed during year $t-1$. Conditional on not already having installed a scrubber, β_t is the average additional probability that units in "challenger" states in year t 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.[19] 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.[20] Finding β_t is negative for years before the court decision, and β_t 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.

4.2 Alternative 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. 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). This estimator has two main steps. First, I flexibly estimate a propensity score function. Secondly, I reweight the observa-

[19]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.

[20]I also investigate abatement and investment trends for each state individually in the appendix. There were only 9 units without scrubbers in MN after the court ruling and none installed scrubbers in 2010-2011.

tions in the treatment and control groups using the estimated propensity scores.

More formally, 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.[21] 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, I require $P(D=1|X) < 1$, in addition to the unconfoundedness assumption.[22] This overlap condition is satisfied for the observed covariates.[23] The average treatment on the treated is given by:

$$
\begin{aligned}
&ATT = E[Y^1(i,2009) - Y^0(i,2009)|D(i)=1] \\
&= \int E[Y^1(i,2009) - Y^0(i,2009)|X(i), D(i)=1]dP(D=1|X) \\
&= \int E[\rho_o \cdot (Y(i,2009) - Y(i,2004))|X(i)]dP(D=1|X) \\
&= E\big[\rho_0 \cdot (Y(i,2009) - Y(i,2004)) \cdot \frac{P(D=1|X)}{P(D=1)}\big] \\
&= E\left[\frac{(Y(i,2009) - Y(i,2004))}{P(D=1)} \cdot \frac{D - P(D=1|X)}{(1-P(D=1|X))}\right] \\
&where \quad \rho_0 = \tfrac{D-P(D=1|X)}{P(D=1|X)(1-P(D=1|X))}
\end{aligned} \tag{6}
$$

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 (6). 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.

In addition to the propensity-score-weighted estimator, I also implement a nearest-neighbor-

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

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

[23] The data appendix includes marginal kernel density plots of the continuous covariates for each group.

matching estimator (Abadie and Imbens, 2006). 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.[24] I also use the Abadie and Imbens (2006) bias correction to adjust for inexact matches in the control group. The nearest-neighbor estimator is:

$$\widehat{ATT} = \frac{1}{N_1} \sum_{i \in \Upsilon_1} \Big\{ \big(Y(i,2009) - Y(i,2004)\big) - \sum_{k \in \Upsilon_0} w_{ik} \big(Y(k,2009) - Y(k,2004)\big) \Big\} \tag{7}$$

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 (Prop. 2)

Table 3 presents regression results from equation 4. The primary outcome of interest is SO_2 emission rate in pounds per unit of heat input. The last three columns include year fixed effects, column (3) includes state fixed effects and column (4) includes unit fixed effects. In all specifications, standard errors are clustered at the unit level.[25]

In each specification, $CAIR$ (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 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 all specifications. This provides evidence that units exposed to increased regulatory uncertainty were less likely to reduce their emissions relative to other states included in CAIR. This is consistent with Proposition 2 from Section 3. 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, I can compare emission reductions in the "challenger"

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

[25] The results are also robust to clustering at the plant level, operating-company level, state-year level, and state level.

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

	(1)	(2)	(3)	(4)	(5)
Challenger (β_1)	0.137***	0.136***	0.135***	0.129***	0.262**
	(0.0224)	(0.0223)	(0.0223)	(0.0236)	(0.120)
CAIR (β_2)	-0.147***	-0.147***	-0.146***	-0.142***	-0.256**
	(0.0214)	(0.0214)	(0.0214)	(0.0230)	(0.113)
Controls	Yes	Yes	Yes	No	Yes
Year FE	No	Yes	Yes	Yes	Yes
State FE	No	No	Yes	No	No
Unit FE	No	No	No	Yes	No
Operator FE	No	No	No	No	Yes
Restricted Sample	No	No	No	No	Yes
N	8097	8097	8097	8279	470
R^2	0.847	0.852	0.857	0.906	0.866

The first four columns allow for different combinations of year, state, and unit fixed effects. 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 2002-2009. The fifth 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 $p < 0.01$

states relative to states never included in CAIR by summing the coefficients $Challenger$ (β_1) and $CAIR$ (β_2). This estimate is small in magnitude and not statistically different from zero. 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 possible endogeneity of managerial preferences, I restrict the sample to only operating companies that run facilities in both "challenger" states and in other states. I then estimate equation 4 including operator company fixed effects. The results of these regressions can be found in column 5 of Table 3. While the magnitude of the estimates are slightly changed, the coefficient signs are consistent with the baseline estimates. $CAIR$ (β_2) is still negative and significant at the 5% level, and $Challenger$ (β_1) is positive and significant at the 5% level. These estimates have less power due to smaller sample size.

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. The first column presents the estimate using the propensity-score-weighted estimator from equation 6. 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 1, 3, and 5 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

Table 4: Propensity-Score-Weighting and NN-Matching Estimates

	(1)	(2)	(3)	(4)
ATT (Challenger-CAIR)	0.116^{**}	0.103^{***}	0.0935^{**}	0.0966^{**}
	(0.0510)	(0.0353)	(0.0402)	(0.0382)
N Treated	81	81	81	81
Model	P-Score Weight	NN Match	NN Match	NN Match
# of Neighbors	-	1	3	5

This table 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 $p < 0.01$

robust to the number of matches chosen, and the propensity-score-weighted estimator yields a similar estimate.

5.2 Additional Robustness Checks

To address additional identification concerns, I conduct several robustness checks. In Panel A of Table 9 in the appendix, I estimate the model, excluding 2009 from the sample to account for the possibility that firms reacted quickly to the court decision.[26] Dropping 2009 does not cause any noticeable changes to the estimated effects. In Panel B of Table 9, I restrict the sample to only units within 600 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.

It is also possible that other political or legal factors are driving differences in pollution abatement and not regulatory uncertainty. I attempt to address some of these potential concerns in Appendix Table 10. For instance, during the time frame of this study some power plants were required to install pollution controls due to 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 10 reports estimates of the baseline model on restricted sample that excludes any plant that was subject to NSR litigation related to SO_2 emissions. The results are robust to 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 10 shows estimates of the baseline model from equation 4

[26] 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 one year contracts so this is unlikely but possible.

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,[27] 75% of the observation are dropped. As a result, the estimated coefficient is no longer statistically significant. However, the point estimate is still positive and of similar magnitude. This result suggests that the baseline result are not being driven by confounding political factors.

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 natural logarithm of emission rate as the outcome variable. The results of all of these regressions are consistent with the baseline model and are presented in Table 11 in the Appendix.

Finally, I present estimates with alternative standard error clusters. In Table 12, I allow for clustering at the plant level, operating company level, and state level. These alternative clusters do not change the significance of the estimated effects.

5.3 Mechanisms (Props. 1 & 3)

Table 5: Decision to Install Scrubber, Sulfur Content of Coal

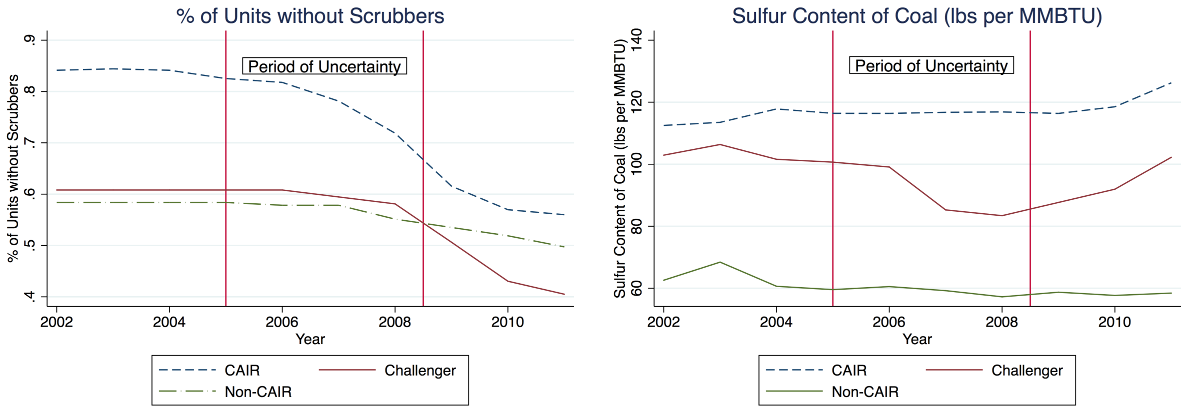
	(1) Scrubber	(2) Scrubber	(3) Sulfur	(4) Sulfur	(5) Sulfur
Challenger (β_1)	-0.120**	-0.525**	-14.36*	-14.21*	-12.12
	(0.0543)	(0.264)	(8.306)	(8.386)	(8.180)
CAIR (β_2)	0.0814**	0.329	4.200	4.405	3.105
	(0.0411)	(0.217)	(2.928)	(2.723)	(2.242)
Model	OLS	Probit	OLS	OLS	OLS
Controls	Yes	Yes	Yes	Yes	No
Year FE	-	-	Yes	Yes	Yes
State FE	-	-	No	Yes	No
Plant FE	-	-	No	No	Yes
N Challenger	54	54	585	585	585
N	769	769	3466	3450	3466
R^2	0.152		0.102	0.331	0.936

Scrubber installation regressions only include units that did not already have a scrubber installed as of 2004. "Sulfur Content" regressions (columns 3-5) are run at the plant level and standard errors are clustered by plant. "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 $p < 0.01$.

[27] Many states elect PUC commissioners.

I now turn to looking 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 8% more likely to install a scrubber compared to units that were not regulated under CAIR. In addition, units in "challenger" states were 12% 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.

Figure 4: 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.

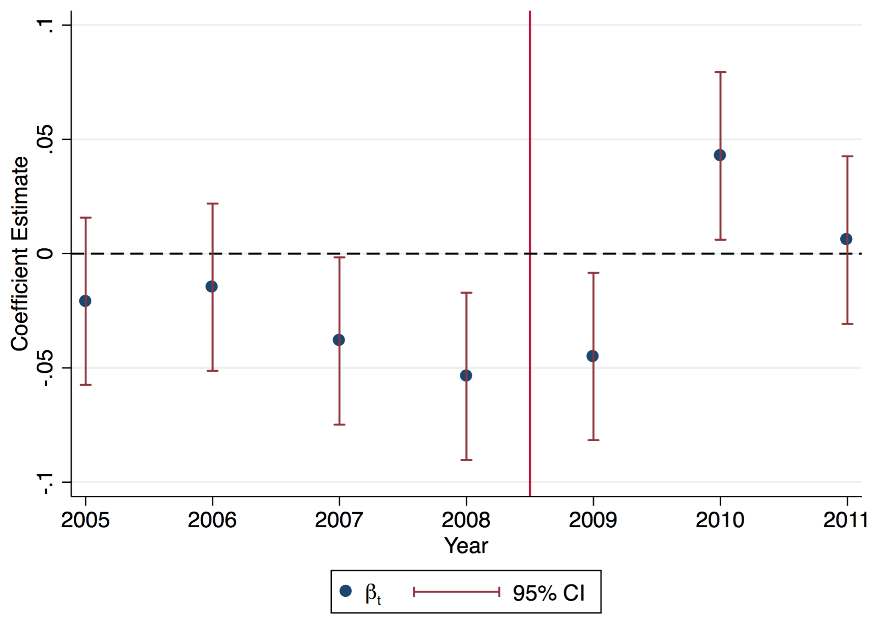
Columns 3-5 present the estimated coefficients for the DID model with the sulfur content of coal purchases as the outcome variable. The coefficient on $CAIR$ (β_2) is not significant, meaning there is no evidence that plants in CAIR states reduced the sulfur content of their coal purchases compared to non-CAIR states after the policy was announced. I do find $Challenger$ (β_1) to be negative and significant at the 10% level for the specifications in column 3 and 4, indicating plants in "challenger" states were more likely to reduce the sulfur content of their fuel purchases relative to other plants regulated under CAIR. When I include plant fixed effects, the result is no longer statistically significant but the point estimate is similar.

The right panel of Figure 4 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 increase again after uncertainty is resolved in late 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.

5.4 Investment After the Court Ruling (Prop. 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 9 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 equation 5. This regression allows me to identify how the relative probability of units installing a scrubber in Texas and Florida changed over time.

Figure 5: Relative Probability of Installing a Scrubber by Year



Estimates of equation 5 are reported in Figure 5. 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 Figure 4, 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 regulatory 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 Section 5.6.

5.5 Cost-of-Service Regulation and Heterogeneous Effects

Regulators often grant utilities exclusive rights to sell electricity in a service area but also control the rates that utilities can charge. The price allowed by the regulator depends on the capital stock owned by the utility and an allowed rate of return on capital. Because the price of retail electricity can directly depend on capital investments, regulated power plants may face a much different profit motive compared to merchant generators. If regulated firms can add pollution-control investments to their rate base, they may not have a high option value to wait to install pollution controls. To test if there is heterogeneity in the treatment effect of regulatory uncertainty, I estimate equation 4, allowing for interactions between the "Regulated" variable and treatment variables[28]. The regression results can be found in Table 6. The coefficient on $Challenger$ is again positive and statistically significant; however, the interaction of $Regulated \times Challenger$ is negative and statistically significant. This indicates that regulatory uncertainty reduced abatement by less at regulated units compared to deregulated units. This result also suggests environmental policy uncertainty could be a larger concern in states with deregulated wholesale electricity generation.

5.6 Discussion: The Costs of Policy Uncertainty

To assess the additional compliance cost that resulted from delayed abatement, I combine the unit-level estimates of increased SO_2 emission rates due to regulatory uncertainty from my preferred specification (β_1 from equation 4) with unit-level heat-input data. This provides

[28]Texas is the only state in the "challenger" group that had any deregulated plants. However, this is not equivalent to interacting the "challenger" indicator with a Texas dummy because there were both regulated and deregulated plants operating in Texas.

Table 6: Heterogeneous Effects under Cost-of-Service Regulation

Dep. Var. : SO_2 (lbs./MMBtu)	(1)	(2)	(3)	(4)
Challenger	0.241***	0.243***	0.241***	0.238***
	(0.0441)	(0.0442)	(0.0444)	(0.0470)
CAIR	-0.179***	-0.182***	-0.177***	-0.173***
	(0.0483)	(0.0484)	(0.0482)	(0.0511)
Regulated X Challenger	-0.161***	-0.165***	-0.165***	-0.170***
	(0.0517)	(0.0516)	(0.0516)	(0.0562)
Regulated X CAIR	0.0860	0.0884	0.0854	0.0882
	(0.0542)	(0.0543)	(0.0539)	(0.0588)
Controls	Yes	Yes	Yes	No
Year FE	No	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes
Unit FE	No	No	No	Yes
N	8098	8098	8098	8098
R^2	0.742	0.746	0.752	0.812

The above regressions allow for treatment effect heterogeneity by interacting the $CAIR$ and $Challenger$ indicators with an indicator for if the unit is regulated under cost-of-service regulation. All standard errors are listed in parenthesis and are clustered at the unit level. * indicates $p < 0.10$, ** indicates $p < 0.05$, and *** indicates $p < 0.01$

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 2005-2009.[29] 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, they could have installed controls sooner and reduced permit expenditures. The aggregate permit costs from delayed abatement are:

$$TotalCost = \sum_{t=2005}^{2009} \sum_{i \in challenger} \Delta \widehat{SO2rate} \cdot HeatInput_{it} \cdot PermitPrice_t \qquad (8)$$

where $PermitPrice_t$ 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 regulatory 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

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

over all the units in Texas, Florida, and Minnesota and aggregate across all years from 2005 to 2009.

I find that policy uncertainty increased compliance costs by $386 million over five years. 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, Florida, or Minnesota 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.[30]

The costs of regulatory uncertainty appear to 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.[31] The increased abatement costs resulting from the legal challenge would then be equivalent to increasing the nationwide cost of CAIR by 5% over the five year period. Because the legal challenge only affected a small part of the SO_2 permit market, this highlights the potential importance of regulatory 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. Regulatory uncertainty is likely to have even larger costs on a national or global scale.

6 Conclusions

In recent years, regulatory 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 largely on if the new policy remains in place. The recent election of Donald Trump as U.S. president demonstrates the potential fragility of regulatory commitments. Theoretical work has shown that policy uncertainty should cause firms to delay making sunk investments and anecdotal evidence suggest this could be a real issue. However, little empirical evidence has been provided to support this theory.

This article provides some of the first empirical evidence that regulatory 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), regulatory uncertainty delayed reductions of sulfur dioxide emissions. In particular, firms that faced more uncertainty were more likely

[30]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.

[31]See Federal Register Vol. 70 (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.

to delay investment in capital infrastructure to reduce emissions. Furthermore, these firms were more likely to use abatement strategies that didn't require large fixed costs. In order to maintain flexibility many firms decided to purchase emissions allowances as a means of compliance. I estimate delayed investment stemming from regulatory uncertainty increased firm's expenditures on allowances by $386 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 the Clean Power Plan remains largely uncertain due to unknown future political conditions and unknown outcomes of the judicial review process. Many states initially filed lawsuits challenging the legality of the policy. These lawsuits culminated in the Supreme Court ordering the EPA to halt enforcement of the rule. Now the rule must be reviewed again in court and could be altered or scrapped by the new Trump administration. In the end, it could be several years between the proposal of the policy and the time when firms actually know the final status and details of the regulation. 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

A Judicial Review: North Carolina vs. EPA

This section provides additional details on CAIR and the judicial review of the program. All challenges to the CAIR program were combined into a single case: North Carolina vs. EPA.[32]

The CAIR rule was initially introduced in 2005 to aid several states in complying with National Ambient Air Quality Standards (NAAQS) required under the Clean Air Act. NAAQS were initially imposed to reduce ambient levels of particulate matter, ground-level ozone and other pollutants. These pollutants can be harmful to human health and can damage both agriculture and natural ecosystems. Ozone levels can be difficult to regulate locally because several precursors to ozone such as SO_2 and NO_x can be generated from sources in other states. The EPA introduced CAIR in order to limit SO_2 and NO_x emissions that were contributing to non-attainment of air quality standards in many downwind states. The program would require emission reductions from most eastern states. States that were regulated under CAIR could propose their own plan for compliance or join the new CAIR cap and trade program for electric generating units that would be administered by the EPA.

North Carolina, as well as several other states and electric utility companies challenged the legality of the CAIR rule after its announcement in 2005. The D.C. Circuit Court of Appeals separated legal challenges into four different categories: (1) SO_2 and NO_x budgets (2) altering the allocation of Title IV allowances (3) the exclusion of some border states (4) the Phase I compliance deadline.

This article focuses on the uncertainty generated by the third category, the exclusion of some border states. Although there was uncertainty generated by all four categories of North Carolina vs. EPA, the exclusion of border states challenge is the only category that differentiated some plants in their probability of being regulated.

[32]See Kruse (2009) for an even more comprehensive background.

In July 2008, the D.C. Circuit Court agreed with parts of three of the four arguments from North Carolina. The court ruled that CAIR was unlawful under the Clean Air Act because it did not specifically address any one state's contribution to another individual states ambient air quality. Instead it formulated emission reductions as a group, meaning any particular state could avoid making actual emission reductions by instead purchasing permits from the cap and trade pool.

The court also ruled in response to utility objections to SO_2 and NO_x budgets under CAIR. They ruled that the EPA's allocation rules did not specifically target pollution reduction in specific upwind states. Furthermore, the EPA had initially proposed reducing SO_2 permit allocations from the Title IV Acid Rain program. However, the court decided that the EPA did not have authority to reduce firm's allocations of Title IV permits or to remove permits from circulation. The court also ruled that the Phase 1 of CAIR did not require sufficient emission reductions that were required by 2010 under the Clean Air Act.

The court also approved one border state challenge, the motion to exclude Minnesota; however, they rejected the motions to exclude Texas and Florida. The court ruled that the EPA had not provided sufficient evidence of Minnesota's contribution of non-compliance for downwind states.

After an initial court ruling in July 2008, it appeared that CAIR SO_2 would not be implemented in 2010 as planned. The EPA would need to significantly change many of the CAIR provisions in order for it to become law. However, in December 2008, the D.C. Circuit Court decided that both the CAIR SO_2 and NO_x programs would proceed as scheduled and remain in place while the EPA constructed a new rule to replace CAIR. The court did not provide a definitive time table for EPA to complete the new replacement regulation.

All states that were initially scheduled to participate in the CAIR SO_2 program with the exception of Minnesota were forced to comply with CAIR during the period of 2010-2014. In 2015, phase 1 of a replacement program called the Cross-State Air Pollution Rule went into effect. The Cross-State Air Pollution Rule addressed the court's initial concerns by limiting permit trading to within specific air basins and not altering allocations of Title IV permits.

B 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:

$$\min_{a_1, I_1} \quad P_1 \cdot (\bar{e} - a_1) + C(a_1, I_1) + K \cdot I_1 + \mathbb{E}\Big[\min_{a_2, I_2}\{P_2 \cdot (\bar{e} - a_2) + C(a_2, I_2) + K \cdot (I_2 - I_1)\}\Big]$$
$$s.t. \quad a_t \in [0, \bar{e}], \quad I_t \in \{0, 1\}, \quad I_2 \geq I_1$$

B.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) and a group of 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: $P_1 \cdot a_1^I - C(a_1^I, 1) - K + E[min\{P_2 \cdot a_2 - C(a_2, 1)\}|I_1 = 1]$.[33] If the firm installs the technology in period 1, they save on permit costs and abatement costs in period 1 and anticipate saving on permit costs and abatement costs in period 2; however, they 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 their expected benefit will be $P_1 \cdot a_1^N - C(a_1^N, 0) + E[min\{P_2 \cdot a_2 - C(a_2, I_2) + K \cdot I_2\}|I_1 = 0]$. 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:

[33] 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.

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

differentiating (11) with respect to ρ we have:

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

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 $\frac{dF(K_1^*)}{d\rho} \geq 0$

■

B.2 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:

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

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

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

where f is the probability density function of K. We know that the first term in the brackets, $f(K^*)\frac{dK^*}{d\rho}((\bar{e} - a_1^I) - (\bar{e} - a_1^N))$, is negative since f is non-negative by definition,

$\frac{dF(K_1^*)}{d\rho}$ is positive as shown above, and $((\bar{e} - a_1^I) - (\bar{e} - a_1^N))$ 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 $\frac{d\mathbf{e_1}}{d\rho} \leq 0$.

■

B.3 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:

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

As the probability of the high price regime increases, more firms adopt the technology, which works to reduce total abatement by no adopters, this effect is labeled [1] in equation 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 equation 17 equals zero. Therefore, $\frac{d\mathbf{a}_1^N}{d\rho} \leq 0$.

B.4 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 adopters in period 2 must increase as ρ decreases, conditional on the high price regime occurring.

■

C Robustness Checks and Additional Figures

Since Minnesota was eventually excused from complying with CAIR and Florida and Texas were not, there is a possibility that there were different expectations in each state prior to the court's decision. In order to allow for possible heterogeneity in the effect of uncertainty across states, I estimate the baseline emissions model from 4 and allow for DID estimates to vary for each state by interacting a state dummy with the "Post" dummy. This means $Texas/Florida$ can have a different estimate than $Minnesota$, this is in contrast to the baseline model which only estimates a pooled average treatment effect for plants in all three states. Table 7 presents the results. The estimated effect is positive and significant for each of the groups and the point estimates for each group appear to be very similar. This result suggests there was not largely different expectations regarding the probability of regulation across these three states.

Table 7: Differential Effects by State - Dep Var: SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Minnesota	0.133***	0.128***	0.128***	0.114***
	(0.0378)	(0.0372)	(0.0374)	(0.0396)
Florida/Texas	0.125***	0.126***	0.125***	0.119***
	(0.0248)	(0.0248)	(0.0251)	(0.0278)
CAIR	-0.138***	-0.138***	-0.137***	-0.130***
	(0.0230)	(0.0231)	(0.0234)	(0.0261)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	Yes	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	8098	8098	8098	8280
r2	0.741	0.745	0.750	0.818

This table presents regression results from an estimating equation analogous to equation 4 except the "challenger" group is split into two different groups: the first group including units in Florida and Texas, units that the court eventually required to comply with CAIR, and the other group being units in Minnesota which were eventually excused from compliance. Standard errors are clustered at the unit level. * indicates $p < 0.10$, ** indicates $p < 0.05$, and *** indicates $p < 0.01$

Figure 6: Trends for MN,TX,FL

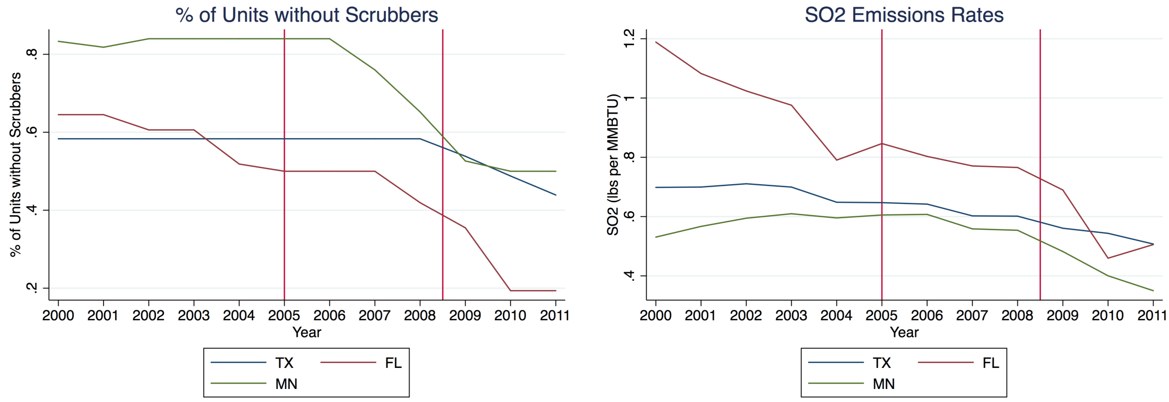


Table 8: Diff-in-Diff : Acid Rain Program Phase 2 (2000)

Panel A: SO_2 (lbs/MMBtu)				
	(1)	(2)	(3)	(4)
Challenger	0.0323	0.0326	0.0321	0.0248
	(0.0628)	(0.0628)	(0.0630)	(0.0688)
CAIR	-0.101**	-0.101**	-0.101**	-0.0943**
	(0.0405)	(0.0405)	(0.0408)	(0.0443)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	Yes	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	6239	6239	6239	6254
r2	0.862	0.863	0.865	0.904

Panel B: Decision to Install Scrubber		
	(1)	(2)
Challenger	0.0240	0.452
	(0.0154)	(0.424)
CAIR	-0.0265**	-0.822**
	(0.0127)	(0.388)
Model	OLS	Probit
Controls	Yes	Yes
N_Challenger	61	61
N	851	851
r2	0.0262	

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 9: Robustness Checks - Diff-in-Diff with Sample Restrictions 1 - Dep Var: SO2 (lbs. per MMBtu)

Panel A: Drop 2009				
	(1)	(2)	(3)	(4)
Challenger	0.105***	0.104***	0.103***	0.0976***
	(0.0242)	(0.0241)	(0.0244)	(0.0269)
CAIR	-0.110***	-0.110***	-0.108***	-0.103***
	(0.0231)	(0.0232)	(0.0233)	(0.0261)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	7120	7120	7120	7213
r2	0.756	0.758	0.760	0.821

Panel B: Only Plants Near MN,FL,TX				
	(1)	(2)	(3)	(4)
Challenger	0.0780**	0.0780**	0.0743*	0.0618
	(0.0383)	(0.0383)	(0.0380)	(0.0404)
CAIR	-0.123*	-0.124*	-0.120*	-0.111
	(0.0648)	(0.0649)	(0.0648)	(0.0698)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	1557	1557	1557	1587
r2	0.826	0.831	0.833	0.893

Panel A reports regressions results for the baseline Diff-in-Diff regression from equation 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 equation 4 but excluding all units that are located further than 600 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 10: Robustness Checks - Diff-in-Diff with Sample Restrictions 2 - Dep Var: SO2 (lbs. per MMBtu)

Panel A: Drop Units Subject to NSR Lawsuits	(1)	(2)	(3)	(4)
Challenger	0.110***	0.109***	0.108***	0.0992***
	(0.0248)	(0.0247)	(0.0252)	(0.0277)
CAIR	-0.119***	-0.120***	-0.118***	-0.112***
	(0.0239)	(0.0239)	(0.0243)	(0.0272)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	7606	7606	7606	7783
r2	0.744	0.747	0.749	0.819

Panel B: Only Units in States with Republican Appointed PUC Chairmen	(1)	(2)	(3)	(4)
Challenger	0.0822	0.0807	0.0808	0.0762
	(0.0552)	(0.0550)	(0.0552)	(0.0583)
CAIR	-0.125**	-0.124**	-0.124**	-0.121*
	(0.0579)	(0.0579)	(0.0579)	(0.0623)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	2160	2160	2160	2210
r2	0.839	0.843	0.844	0.903

Panel A reports regressions results for the baseline Diff-in-Diff regression from equation 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 equation 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 11: Robustness Checks - Alternative Dependent Variables

Panel A: Levels SO_2 (tons)	(1)	(2)	(3)	(4)
Challenger	715.4**	698.0**	700.1**	662.8*
	(344.1)	(342.8)	(344.0)	(365.8)
CAIR	-1245.6***	-1260.5***	-1241.7***	-1256.0***
	(198.3)	(199.1)	(199.9)	(219.5)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	8098	8098	8098	8281
r2	0.806	0.817	0.810	0.878

Panel B: Log(SO_2 (tons))	(1)	(2)	(3)	(4)
Challenger	0.178***	0.175***	0.178***	0.165***
	(0.0442)	(0.0443)	(0.0445)	(0.0483)
CAIR	-0.146***	-0.149***	-0.143***	-0.125**
	(0.0425)	(0.0429)	(0.0455)	(0.0574)
Controls	Yes	Yes	Yes	No
Year_FE	No	Yes	No	Yes
State_FE	No	No	Yes	No
Unit_FE	No	No	No	Yes
N	8097	8097	8097	8279
r2	0.756	0.777	0.761	0.842

Panel C: Log(SO_2 (lbs/MMBtu))	(1)	(2)	(3)	(4)
Challenger	0.103***	0.102***	0.102***	0.0948***
	(0.0319)	(0.0318)	(0.0319)	(0.0338)
CAIR	-0.0549*	-0.0560*	-0.0530	-0.0446
	(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	8097	8097	8097	8279
r2	0.788	0.797	0.794	0.865

This table reports regressions results for the baseline Diff-in-Diff regression from equation 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 logarithm of the emission rate SO_2 in lbs/MMBtu. All standard errors are clustered at the unit level. * indicates $p < 0.10$, ** indicates $p < 0.05$, and *** indicates $p < 0.01$

Table 12: Alternative Std. Error Clusters: DiD Dep Var: SO2 (lbs. per MMBtu)

	(1)	(2)	(3)	(4)
Challenger	0.118***	0.120***	0.118***	0.118***
	(0.0389)	(0.0437)	(0.0284)	(0.0361)
CAIR	-0.130***	-0.129***	-0.130***	-0.130***
	(0.0385)	(0.0452)	(0.0250)	(0.0389)
Year_FE	Yes	Yes	Yes	Yes
Unit_FE	Yes	Yes	Yes	Yes
SE_Cluster	Plant	Operator	State-Year	State
N	8280	8167	8280	8280
r2	0.818	0.815	0.818	0.818

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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

Figure 7: Map of Coal Plants

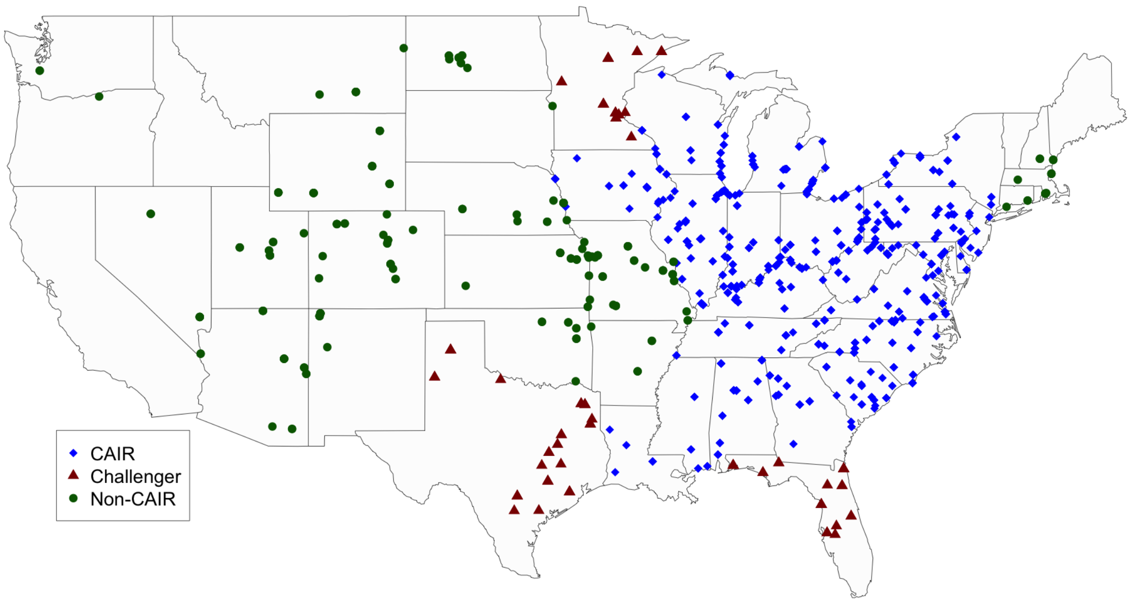
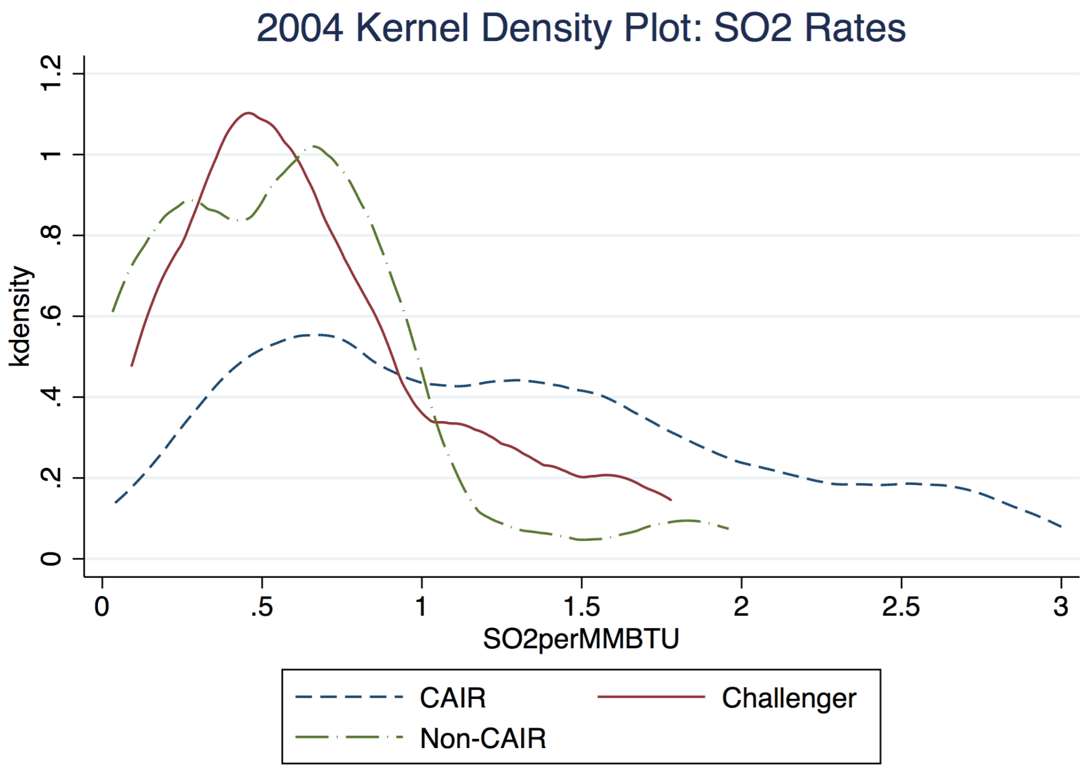


Figure 8: Marginal Kernel Density Plots for Observed Covariates



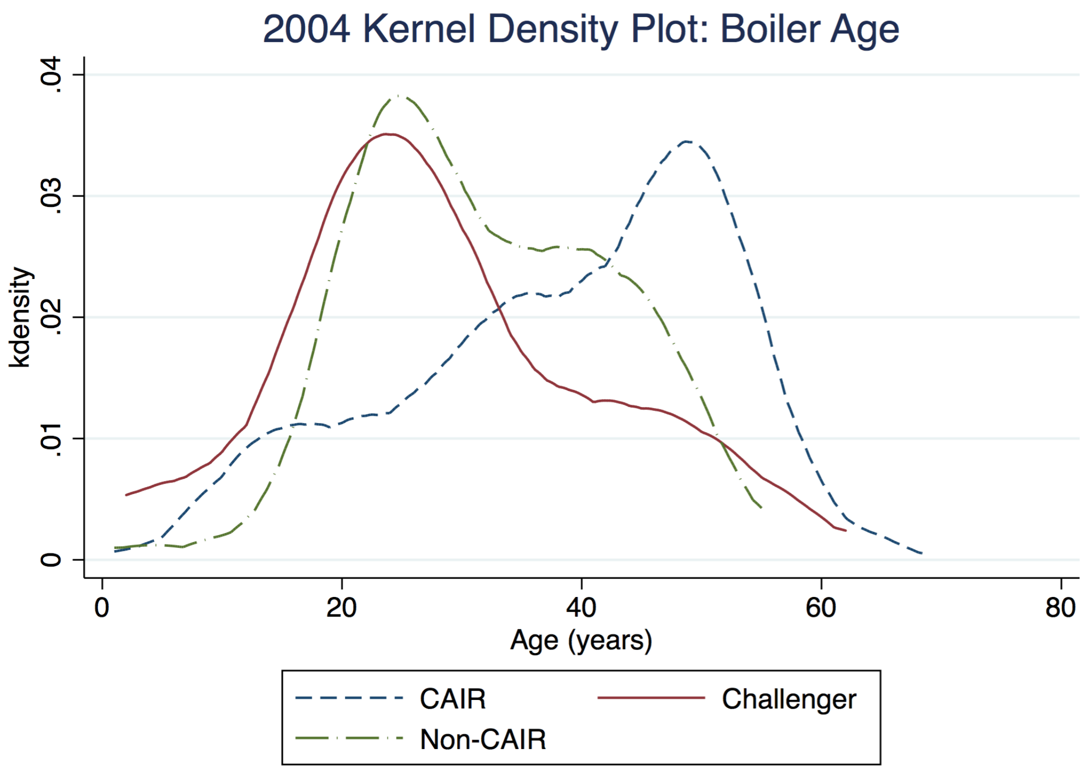


Figure 9: Marginal Kernel Density Plots for Observed Covariates