Political ‘Colour’ and Firm Behaviour: Evidence from U.S. Power Plants’ Pollution Abatement[*]

Corrado Di Maria[†] Emiliya Lazarova[‡] Ian Lange[§]

January 15, 2024

Abstract

We ask whether firms behave differently depending on the political party in charge, above and beyond responding to any actual differences in policy. We use the pollution abatement behaviour of U.S. Steam Electric Power Plants under the Clean Water Act as our case study. Exploiting the variation provided by the outcome of tightly contested gubernatorial elections, we provide causal evidence that large firms respond to the political ‘colour’ of the governor in the state they operate, even when neither the stringency nor the enforcement of the rules depend on it. Within a theoretical model of the interaction between the regulator and the regulated firms, we show that multiple equilibria arise, and the outcomes of the election provide an effective coordination device. This unexpected behaviour has real-world consequences and leads to significant differences in pollution levels.

JEL Classification: H32, H76, Q25, Q53, Q58

Keywords: Regression Discontinuity, Environmental Policy, Pollution Abatement, Political Economy, Enforcement, Power Plants, Water Pollution.

Declarations: The authors have no relevant financial or non-financial interests to disclose. The authors did not receive support from any organization for the submitted work.

---

[*] We would like to thank Harrison Fell, Ed Balistreri, Pete Maniloff, Mirko Moro, seminar participants at the University of East Anglia’s Centre for Competition Policy, the Oxford Centre for the Analysis of Resource Rich Economies, the University of Stirling, the Colorado School of Mines, the University of Georgia, and Tohoku University, as well as conference participants for their helpful comments on previous versions of this paper.

[†] School of Economics, University of East Anglia. c.di-maria@uea.ac.uk.

[‡] School of Economics, University of East Anglia. e.lazarova@uea.ac.uk.

[§] Division of Economics and Business, Colorado School of Mines. ilange@mines.edu.

1 Introduction

As economic agents respond to the incentives they face, it is clear that the policies that shape such incentives would have real economic effects. From this point of view, it is obvious that economic activities and political context are closely interrelated. One would indeed expect politicians to affect business behaviour both directly – by devising policies and regulations – and indirectly – by swaying perceptions, expectations, and public opinion.

Much research has been devoted to the analysis of the links between politics, policies, and economic performance, with recent efforts aimed to causally attribute differences in economic outcomes to the political orientation of the executive (e.g. Pettersson-Lidbom, 2008). Two-party elections, in particular, have provided a rich body of data to test theories about the impact of the political orientation of the party in power on a number of relevant economic outcomes.[1]

In this paper, our attention is focussed on a different, but equally intriguing question. We ask whether economic agents facing fundamentally similar regulatory frameworks behave differently when they operate under politicians of different political orientation. In other words, we ask whether in the interplay between economic agents and politicians the 'who' matters quite independently from the 'what'. Our analysis is inspired by a number of attempts in economics to investigate how the identity of the interacting agents matters for their decision-making and ultimately determines economic outcomes.[2]

Empirically, the challenge we face is to isolate the effect of the 'identity' – and specifically the party affiliation – of the executive from the impact of the 'rules' they impose. In what follows, we focus on the water pollution abatement decisions made by large power generators (Steam Electric Power Plants, or SEPPs) regulated under the Clean Water Act (CWA, henceforth).[3] The peculiar nature of our case study – where emissions standards are set at the federal level and state authorities play a limited enforcement role – allows us to isolate the impact of the Governors' party affiliation, which we refer to as their political 'colour', from any actual difference in policy in this context.

Our identification strategy exploits narrow victories in closely contested gubernatorial elections in US states to investigate changes in pollution abatement by SEPPs. We find robust causal evidence that power plants located in states governed by a Democratic governor spend only a fraction of

---

[1] Alt and Lowry (2000) find that Democrats tend to have larger public spending, Caplan (2001) and Reed (2006) show that taxes tend to be higher under Democratic governments. Using U.S. gubernatorial elections data, Leigh (2008) finds significant partisan impacts on the level of the minimum wage, the level of post-tax income inequality, and the rate of unemployment, as well as incarceration rates and welfare caseloads. Beland (2015) identifies a significant increase in the annual work hours of blacks versus whites under Democratic governors, and Beland and Oloomi (2017) show similar positive impacts of democratic governors in terms of the job market outcomes of immigrants. An emerging literature further suggests that the governor's political affiliation has significant impacts on the provision of environmental public goods (e.g. List and Sturm, 2006; Fredriksson et al., 2011; Beland and Boucher, 2015; Pacca et al., 2021).

[2] This is the 'hedonic aspect' discussed by Drèze and Greenberg (1980), for example, in whose model the identity of the counterpart is all that matters. Similar insights underpin the literature on 'identity economics' spawned by the seminal contribution of Akerlof and Kranton (2000)

[3] In what follows, we see each SEPP as an independent firm and use the terms SEPP, power plants, and firm interchangeably. We find this to be a plausible assumption because our focus is on pollution abatement spending under the CWA, where regulation and compliance are at the plant level.

the amount spent by their counterparts in Republican states on water pollution control. In this respect, similarly to Jens (2017) – who investigates the impact of political uncertainty on the timing of firm investment – and Raff et al. (2022) – who instead focus on how local politics affect the state-level implementation of federal policy – our first contribution is to complement the existing macro literature with robust micro-econometric evidence of the impact of the political environment on the behaviour of firms.[4]

The major contribution of this paper, however, is to show that the political colour of the Governor matters for firms' behaviour, even when neither the stringency nor the enforcement of the regulation depends on it. To the best of our knowledge, we are the first to document this aspect.

In the second part of the paper, then, we focus on the potential mechanisms underpinning this surprising empirical result. We develop a novel theoretical model of the interaction between the regulator and the entire population of firms, which builds on the premise that the political colour of the executive plays no role in determining either the stringency or the enforcement of the policy. We contribute to the theoretical literature by introducing spillovers among firms in their strategic behaviour vis-à-vis the enforcement agency. We show that once these links are accounted for, the abatement decisions of the different firms become strategic complements so that the possibility of multiple equilibria emerges naturally, much as it does in investment games (e.g. Lee and Wilde, 1980; Milgrom and Roberts, 1990; Athey and Schmutzler, 2001).[5]

In this context, the possibility of correlated equilibria arises as a means to improve on the Nash outcomes, even in the absence of pre-game communication or commitment (e.g. Fudenberg and Tirole, 1986). Such correlation, however, requires the existence of an exogenous, salient and non-manipulable device to be attained. We suggest that firms might be using the outcome of closely contested gubernatorial elections as a signal to improve their expected payoffs. Thus, while ideology plays no direct role in our theoretical model, the political colour of the executive can still matter for the decisions of firms. We therefore identify a novel channel through which the outcomes of elections impact the real economy and provide a compelling rationale for the results found in our empirical analysis.

In the final part of the paper, we investigate the implications that changes in firms' behaviour in response to electoral outcomes have on pollution. Given the large environmental footprint of power generation – the electricity sector withdraws more freshwater than any other sector in the U.S. economy (Dieter et al., 2018) – this question is both relevant and salient. We show that adjustments in pollution abatement spending have significant impacts on the release of pollutants. Even changes to political colour that do not imply a shift in policy are thus shown to have real – if unintended – environmental consequences and welfare implications. This is the final contribution of

---

[4] To stay within the public economics literature at the macro level, Fredriksson et al. (2011) show that the level of public spending on environmental public goods in Republican states exceeds that in Democratic states, while Pacca et al. (2021) find the opposite result and show that the share of spending going to environmental items is larger under Democratic Governors. Beland and Boucher (2015) focus directly on environmental outcomes and provide evidence that the level of air pollution is lower in states with a Democratic Governor.

[5] Milgrom and Roberts (1990) is the classic reference on games with strategic complementarities; Vives (2005) provides an insightful overview of the literature on complementarities in games.

this paper.

The rest of the paper proceeds as follows, section 2 gives a brief overview of the technical and regulatory framework in which the SEPPs operate. Section 3 describes the institutional context, the identification strategy, and the methodology, as well as the data for our regression discontinuity analysis. It also presents the result of our analysis of pollution abatement across treatments, including a discussion of its validity and robustness. Section 4 delves into the mechanisms that underlie the RD results. We first rule out differences in enforcement as potential drivers of our results. We then develop a novel game of enforcement and compliance with externalities across firms and discuss its implications in section 5. Section 6 traces out the implications of our analysis in terms of environmental outcomes. Finally, section 7 summarizes our results, discusses the implications of our analysis, and concludes.

2 Steam-electric power plants, water pollution and its regulation

The basic operation of SEPPs is conceptually simple: they generate electricity by heating water, turning it into steam, and letting the high pressure steam spin a turbine, which drives an electrical generator. After passing through the turbine, the steam is brought back to liquid form in a condenser and discharged into the environment. SEPPs therefore withdraw large quantities of water and subsequently release it back into the environment, normally at a higher temperature. Both water withdrawal and the discharge of treated, heated water in the natural environment have significant impacts on a wide range of organisms in the aquatic ecosystem, from tiny photosynthetic organisms to fish, shrimp, crabs, birds, and marine mammals.[6]

The negative environmental impact of individual SEPPs crucially depends on both the specificity of the power plant design and on the way in which it is operated. In terms of thermal pollution, for example, the type of cooling system installed is the critical dimension. Once-through cooling systems, whereby water is withdrawn directly from a source, diverted through a condenser, and then discharged back into the body of water at high temperature, are the most damaging for the environment. Closed-cycle recirculating cooling systems and dry cooling ones are more modern, have lower environmental impacts and are required as part of New Source Performance Standards in the context of the Clean Water Act.

While thermal pollution is mostly a function of past and current investment, however, other types of environmental impacts are more directly related to the day-to-day operation of the power plant and specifically to the level of care taken to maintain the plant's operating conditions. For example, SEPPs routinely add chlorine and other toxic chemicals to their cooling water to decrease

---

[6] Aquatic organisms are killed by intake structures as they entrain them through the plants' heat exchangers where they succumb to physical, thermal and toxic stresses. Larger animals are killed when they are trapped against the intake screens by the pressure of the intake flow. The thermal pollution caused by the discharge of heated water from cooling systems also harms wildlife, as the oxygen supply decreases and the ecosystem composition is affected. The negative impacts of chlorinated water released in the environment have been well documented since at least the late 1970s. (e.g. Sung et al., 1978).

algal growth in heat exchangers. Since these chemicals eventually find their way into the natural environment with considerable environmental damage, better maintenance – which reduces the need for such treatment – alleviates the environmental impact of the plant's operations. Therefore, power plants have significantly more latitude to reduce some environmental impacts than others.

Due to their considerable potential for environmental degradation, SEPPs tend to be heavily regulated. In the U.S., they are regulated according to the CWA and are subject to effluent limitations via discharge permits issued under the National Pollutant Discharge Elimination System (NPDES).[7] According to the U.S. Environmental Protection Agency (EPA) "Effluent limitations guidelines and standards are established by the EPA for different nonmunicipal (i.e., industrial) categories. These guidelines are developed based on the degree of pollutant reduction attainable by an industrial category through the application of pollutant control technologies" (EPA, 2023a).

Permits generally require the facility to sample its discharge and notify the EPA of these results. Facilities are also required to flag up any instance of failed compliance with the requirements of their permits. The U.S. EPA – or an authorized state agency on its behalf, see below – may also send inspectors to SEPPs in order to determine if they are in compliance with the conditions imposed by their permits. Upon reception of the facilities' reports or following inspections, the enforcement agency may issue administrative orders, which require facilities to correct violations and assess monetary penalties. The EPA is also allowed by law to pursue "civil and criminal actions that may include mandatory injunctions or penalties, as well as jail sentences for persons found willfully violating requirements and endangering the health and welfare of the public or environment" (EPA, 2023b).

One important point to make is that, while the permitting process under the CWA remains largely a federal prerogative, the program enforcement has over time been delegated by the U.S. EPA to state authorities via the NPDES State Program Authorization process. Authorized states perform inspections under the CWA and are, therefore, at least in principle in a position to influence the degree to which the environmental standards set under the CWA are locally enforced.

3 Pollution abatement spending and gubernatorial elections

In this section, we investigate whether the political affiliation of the elected governors has an impact on the environmental behaviour of firms. Specifically, our goal is to identify the causal impact of a governor's political party affiliation on the water pollution abatement spending of SEPPs. Empirically, this poses a significant challenge since a number of the unobserved characteristics of both the candidates and the States that are correlated with the election outcomes might also affect the abatement behaviour exhibited by power plants. In this sense, election outcomes might

---

[7] Permits contain limits on what can be discharged, as well as requirements on monitoring and reporting protocols, and all other provisions that ensure that the discharge does not hurt water quality or people's health.

be endogenous to abatement decisions.

Our identification strategy relies on the use of a regression discontinuity (RD) design (Lee, 2001, 2008; Pettersson-Lidbom, 2008; Ferreira and Gyourko, 2009). The discontinuity emerges from the fact that in a two-party system, a majority vote share makes one party the winner of the election. In tightly contested elections, the variation in electoral outcome near the 50% vote threshold can be effectively considered as a random assignment to the treatment group. As politicians cannot precisely manipulate voters' behaviour, causal inference may be therefore directly drawn from the observed differences in outcome variables between the treated and control groups, close to the discontinuity.

In what follows, we build on a series of recent contributions in the literature, and use gubernatorial races across US states to identify the causal effect of the governor's party affiliation.[8] The outcome variable of interest is the water pollution spending undertaken by SEPPs across US states. The treatment status is assigned based on the (normalized) vote share for the democratic candidate, $\widetilde{V}_i = V_i - 50$, where V_i is a random variable measuring the absolute vote share in favor of the democratic candidate in percentage points. We define SEPPs as belonging to the 'treated' group whenever the observed normalized vote share is positive, i.e. when $\widetilde{V}_i > 0$, and to the control group otherwise.

3.1 Data

Data on Governor party affiliation, vote margin, and state legislative party shares are all publicly available.[9] Gubernatorial elections occur every four years with the exception of New Hampshire and Vermont, that have two-year terms, and Virginia, which elects a Governor every five years. Additionally, about two-thirds of states hold elections for governor on one cycle of even number years (e.g., 1988, 1992, 1996, etc.). Others hold elections on a different cycle of even number years (e.g., 1990, 1994, 1998, etc.) and six states hold elections during odd number years.[10]

As our main focus is on gubernatorial elections won by either party by a small margin, it is important to provide a sense of the range of states included in our estimations. Table A.1 in the Appendix provides a list of elections won by either a Democratic or a Republican candidate by less than 3% of

---

[8] The seminal contributions of Lee (2001, 2008), that make use of U.S. House elections, spawned a rich literature and generated some controversy in the political science literature. Caughey and Sekhon (2011), Grimmer et al. (2011), and Sekhon and Titiunik (2012) all point out that the assumptions for causal identification might be violated in RD designs based on close U.S. House of Representative elections. More recently, however, Eggers et al. (2014) examine whether such violations may be shown to occur in other electoral settings – including the U.S. House in different time periods – and conclude that "*the assumptions behind the RD design are likely to be met in a wide variety of electoral settings*". Importantly, Caughey et al. (2017) explicitly state that "*Unlike US House elections, where incumbents appear to have an advantage in very close elections, our analysis of state legislative and gubernatorial elections uncovers no statistically significant pre-treatment discontinuities.*" By following Leigh (2008), Fredriksson et al. (2011), Beland (2015), Meyer (2019), Pacca et al. (2021), and Raff et al. (2022), who all use close gubernatorial elections for causal identification, we are on safe ground.

[9] We thank Le Wang for providing the election data from their paper – Fredriksson et al. (2011) – which formed the basis and provided the template for our own data collection. The original data only reached 2005, we extended the dataset to 2015. Table 1 provides the summary statistics for all the data used in this paper.

[10] New Jersey and Virginia (1985, 1989, etc.) and California, Kentucky, Louisiana, and Mississippi (1987, 1991...). California only joined the list of states with elections in odd years in 2003, when the state held a recall vote.

the votes. Shaded cells highlight the states that appear on both sides of the table. As easily gauged from the table, the two groups include a broad range of different states, as well as exhibiting a significant overlap.

Data collected by the Energy Information Agency via its Forms EIA-767 and EIA-923, an annual plant-level panel dataset covering the years 1985-2015, provides us with information on the SEEPs' characteristics as well as their pollution abatement behaviour. These data contain both plant-level investment and current expenditures in dollars on water pollution abatement.

In what follows, we focus on the sum of the amount spent on water pollution abatement capital and current expenditures on water abatement. For the former, the EIA-767 instructs the respondent to:

> *"Report new structures and/or equipment purchased to reduce, monitor, or eliminate waterborne pollutants, including chlorine, phosphates, acids, bases, hydrocarbons, sewage, and other pollutants. Examples include structures or equipment used to treat thermal pollution; cooling, boiler, and cooling tower blow-down water; coal pile runoff; and fly ash waste water".*

As refers to the latter, instead, the following guidance is provided:

> *"Expenditures cover all operation and maintenance costs for material and/or supplies and labor costs including equipment operation and maintenance (pumps, pipes, settling ponds, monitoring equipment, etc.), chemicals, and contracted disposal costs. Collection costs include any expenditure incurred once the water that is used at the plant is drawn from its source. Begin calculating expenditures at the point of the water intake. Disposal costs include any expenditures incurred once the water that is used at the plant is discharged. Begin calculating disposal expenditures at the water outlet (i.e., cooling costs)."*

Table 1 presents the summary statistics for the data in our dataset, distinguishing between Republican and Republican states. The top half of the table refers to the annual observations across the 1,714 SEPPs in our data. Since our goal is to gauge the impact of the party affiliation of the governor in charge on the SEPPs' spending on water pollution abatement, however, we aggregate the data to the electoral cycle level. The second part of the table presents the summary statistics for this version of our data, for completeness. In the analysis that follows, we use the electoral cycle data unless otherwise specified.

3.2 Graphical evidence

Before delving into the formal analysis of the causal impact of the political affiliation of the State Governor on firms' behaviour, we provide a graphical description of the situation. The left-hand

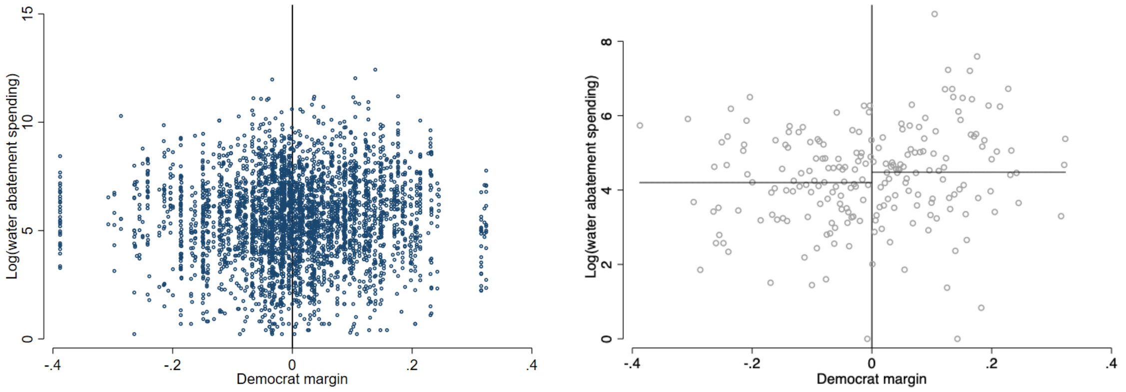
Table 1: Summary statistics

	Yearly observations					
	All Governors		Democrat Governor		Republican Governor	
	Mean/(SD)	N	Mean/(SD)	N	Mean/(SD)	N
Democrat Governor	0.44	25,149	1.00	10,956	0.00	14,193
	(0.50)		–		–	
Water pollution abatement spending (,000 US$)	1,312.40	16,164	1,474.04	7,194	1,182.76	8,970
	(9,631.94)		(10,461.18)		(8,909.78)	
Thermal pollution (Δ°F x discharge in cbf)	6,360.93	11,797	5,951.59	5,316	6,696.69	6,481
	(12,317.27)		(8,940.75)		(14,503.86)	
Chlorine (,000 lbs)	3,270.15	13,426	3,327.90	5,570	3,229.20	7,856
	(13,840.57)		(15,119.93)		(12,857.43)	
Plant nameplate capacity (MW)	732.92	16,159	733.97	7,308	732.06	8,851
	(720.68)		(723.29)		(718.55)	
Plant generation rate (MWh)	32.86	21,068	32.86	9,279	32.85	11,789
	(40.79)		(40.19)		(41.27)	
Heat rate (Btu/KWh)	12.46	17,300	11.41	7,575	13.28	9,725
	(92.15)		(11.35)		(122.49)	
Electricity deregulation status	0.22	25,149	0.16	10,956	0.27	14,193
	(0.41)		(0.37)		(0.44)	
NO_x trading participant	0.16	25,149	0.17	10,956	0.15	14,193
	(0.37)		(0.38)		(0.36)	
Inspections	1120.65	25,149	1,160.34	10,956	1,090.01	14,193
	(1,307.63)		(1,289.20)		(1,320.91)	
NPDES authorization	0.87	25,149	0.86	10,956	0.88	14,193
	(0.34)		(0.35)		(0.33)	
	Electoral-cycle observations					
Democrat Governor	0.47	6,252	1.00	2,962	0.00	3,290
	(0.50)		–		–	
Water pollution abatement spending (,000 US$)	4,864.00	3,933	5,266.76	1,906	4485.28	2,027
	(22,485.29)		(24,091.78)		(20,861.09)	
Thermal pollution (Δ°F x discharge in cbf)	6,158.50	2,845	5653.78	1,377	6,631.93	1,468
	(9,692.02)		(7,915.99)		(11,084.61)	
Chlorine (,000 lbs)	3,051.69	3381	3176.61	1,515	2950.26	1,866
	(13,558.10)		(16,490.22)		(10,600.66)	
Plant nameplate capacity (MW)	772.87	3,710	749.27	1,881	797.13	1,829
	(721.15)		(724.71)		(716.86)	
Plant generation rate (MWh)	34.00	4,990	33.15	2,427	34.81	2,563
	(40.80)		(40.14)		(41.42)	
Heat rate (Btu/KWh)	11.52	4,243	11.85	2,012	11.22	2,231
	(14.98)		(19.10)		(9.89)	
Electricity deregulation status	0.20	6,252	0.10	2,962	0.29	3,290
	(0.40)		(0.31)		(0.45)	
NO_x trading participant	0.14	6,252	0.17	2,962	0.12	3,290
	(0.35)		(0.37)		(0.32)	
Inspections	1,099.46	6,252	1,013.78	2,962	1,176.60	3,290
	(1,299.63)		(971.72)		(1,532.17)	
NPDES authorization	0.87	6,252	0.83	2,962	0.90	3,290
	(0.34)		(0.38)		(0.30)	

Notes: The table reports the sample means, the standard deviations (in parenthesis) and the non-missing observations for all the variables used in our analyses, for each of the subsamples. The top panel refers to the dataset with yearly observations; the bottom panel to the dataset aggregated to the electoral-cycle level.

panel of Figure 1 presents the scatter plot of (the logarithm of) water pollution abatement spending among SEPPs in US states between 1985 and 2015 against the normalized vote share for the

Democratic candidate. The vertical line identifies the point of discontinuity, where the normalized Democratic vote margin equals 0. To the right of this line the election is therefore won by the Democratic party's candidate, to the left by the Republican candidate. Figure 1a suggests that a lot of heterogeneity exists in the level of spending across SEPPs in our data, but otherwise provides no indication of an emerging pattern linking the political affiliation of the Governor, or the margin of victory in the last election, to water pollution abatement spending. In Figure 1b each dot represents the average spending per SEPP within a margin-of-victory bin, while the horizontal lines show the sample means on each side of the discontinuity. Considering the whole sample, the average spending on water pollution abatement per power plant is statistically significantly larger among SEPPs operating in States run by a Democratic Governor than in those run by a Republican.[11,12]



(a) Scatter plot

(b) Sample means

Figure 1: Plots of (the log of) water pollution abatement spending by steam electric power plants, against the democrat vote margin (1985-2015).

Figure 2 shows alternative regression discontinuity plots for water pollution abatement spending against the normalized vote share for the democratic candidate. Once again, the points represent average spending per SEPP within each margin-of-victory bin, and a different function is fitted on each side of the cut-off. A linear function is graphed in panel 2a, whereas we plot a quadratic one in 2b.[13] Visually, both panels suggest that there exists a discontinuity at the threshold. Interestingly, a Democratic win in a closely contested election seems to be associated with a lower level of spending on water pollution abatement by SEPPs in the State. Since the functions plotted in these figures use the whole set of observations, however, this discussion is merely suggestive of

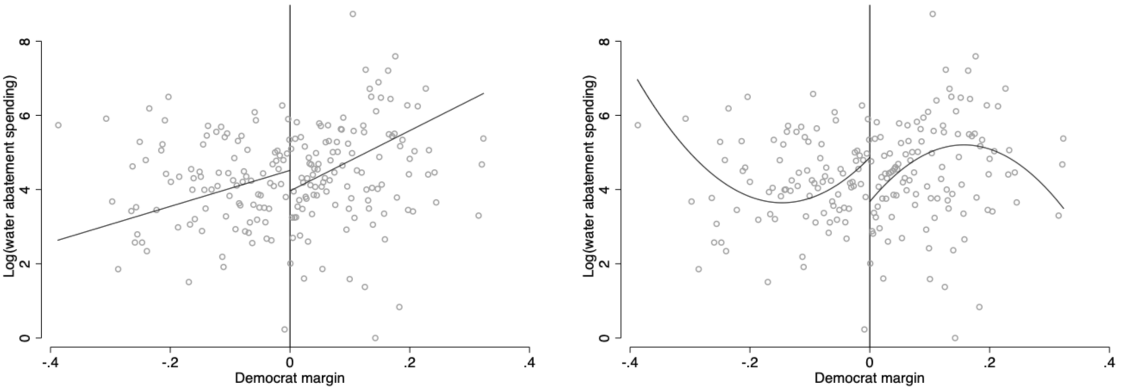
---

[11] To produce these graphs and those that follow, we used the rdplot command provided with the rdrobust STATA package (Calonico et al., 2017). In the current version of this paper, all estimations were performed on a MacOS Monterey 12.0.1 system, running a STATA MP 17 installation.

[12] The difference between the mean of (the logarithm of) water pollution abatement spending in Democratic- vs Republican-run states is 0.30, meaning that on average SEPPs spend 34% more in states with a Democratic governor, than in states with a Republican one. The t-statistic for the difference-in-means test is equal to -2.84, implying rejection of the null hypothesis that the means in the two sub-samples are identical at the 99% confidence level.

[13] Our choice of lower-order polynomials here is consistent with the guidance provided by Gelman and Imbens (2019), who advise against higher order functions. Such polynomials lead, according to Gelman and Imbens (2019), to problems with noisy estimates, sensitivity to the degree of the polynomial, as well as poor coverage of the confidence intervals.

a potential discontinuity and cannot be interpreted causally in any meaningful sense. To further investigate the existence of this discontinuity and to causally attribute any differences in the outcome variable to the electoral outcomes, we now turn to a more rigorous analysis.



(a) Linear interpolation

(b) Quadratic interpolation

Figure 2: Plots of (the log of) water pollution abatement spending by steam electric power plants, against the democrat vote margin (1985-2015).

3.3 Local-linear regressions with bias correction

Our case study presents a sharp regression discontinuity in that the opportunity for causal identification of the impact of the Governor's party affiliation on SEPP abatement spending comes from the discontinuity that occurs when one party earns a marginal majority of the vote, i.e. they win the office by a small margin (e.g. Lee, 2008; Lee and Lemieux, 2010). Letting the treatment assignment be $T_{it} = \mathbb{1}(\widetilde{V}_{it} > 0)$ – where $\mathbb{1}$ is the indicator function – we indicate the potential outcomes in terms of water pollution abatement spending for plant i and time t as $Y_{it}(T_{it})$. In other words, $Y_{it}(1)$ denotes water pollution abatement spending undertaken by a SEPP which operates in a State that at time t has a Democratic Governor, whereas $Y_{it}(0)$ indicates the spending incurred under a Republican Governor. The challenge in this context is to estimate the average treatment effect at the threshold, τ, given by

$$\tau = \mathbb{E}\left[Y_{it}(1) - Y_{it}(0) \middle| \widetilde{V}_i = 0 \right], \tag{1}$$

without actually ever observing either value at the threshold. As discussed in Cattaneo et al. (2019a), indeed, the crucial feature of the sharp RD design is that there are no observations for which the score is exactly equal to the cutoff value, and the RD analysis fundamentally relies on extrapolation towards this cutoff point. The central goal of empirical RD analysis is therefore to adequately perform this local extrapolation in order to compare control and treatment units.

Following the latest recommendations in the literature, we estimate the (local, RD) average treatment effect via local-linear regressions with bias correction (Calonico et al., 2014; de la Cuesta and Imai, 2016; Cattaneo et al., 2019a). We select the optimal bandwidth by minimizing the mean square error of the local linear estimator (Imbens and Kalyanaraman, 2012). As is good practice, we place more weight on observations closer to the cut-off, by adopting a triangular kernel (Cattaneo et al., 2019a). Finally, we adopt the bias-correction algorithm developed by Calonico et al. (2014) to estimate the bias due to the estimation close to the threshold and correct the RD point estimates accordingly. The baseline estimates for the RD effect are presented in Table 2.[14]

Table 2: Non-parametric RD estimates: Log of water pollution abatement spending

	Average treatment effect at the threshold	
	(I)	(II)
Conventional	-1.81***	-1.57***
	(0.53)	(0.55)
Bias-corrected	-2.05***	-1.73***
	(0.53)	(0.55)
Robust	-2.05***	-1.73**
	(0.64)	(0.68)
Bandwidth est.	0.060	0.059
Observations	3,933	3,933
Effective obs. est.	1,922	1,868

Notes: This table reports the estimated coefficients from non-parametric RD estimations of the effect of a democratic governor winning the election on the amount spent on water pollution abatement. The coefficients are estimated following Calonico et al. (2014). Column (I) reports the estimated ATE without covariates; in column (II), plant and year fixed effects are included, as well as dummies for the State's electricity deregulation status, NO_x trading scheme participation, and its NDPES authorization status. Standard errors clustered at the state level in parentheses. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 2 reports the estimated average treatment effect for two different specifications of the RD. Column (I) does not include any co-variates in the estimation, whereas for the estimates in column (II) we include plant and year fixed effects, together with dummies for the state's electricity deregulation status, its participation in a NO_x trading system, and its status under the NDPES Authorization programme.[15] The latter specification is run to control for time-invariant differences amongst SEPPs (e.g. coal vs gas, presence of a scrubber, etc.) that might be driving the variation in water pollution abatement spending, as well as for other (time-variant) institutional differences across States. The NPDES control is added to account for the fact that, as discussed in Section 2 above, only States authorized under the NPDES Authorization programme have responsibilities for monitoring and enforcement under the CWA. For each specification, the conventional and bias-corrected estimates are reported in the first two rows. The third row further reports robust stan-

---

[14] To run these estimations, we used the rdrobust package due to Calonico et al. (2017).

[15] The 'electricity deregulation' variable assumes the value of 1 if the State's electricity system has already been deregulated at the time of the election the observation refers to and zero otherwise. The 'NO_x trading' variable is one if the plant under consideration participates in a NO_x trading scheme at that time and zero otherwise – thanks to Dan Kaffine for sharing this data with us. Finally, the 'NPDES dummy' gets a value of 1 for each observation recorded after the authorization status has been agreed. This last variable has been constructed using the information found in Grooms (2015).

dard errors that are rescaled to incorporate the contribution of the bias correction step to the variability of the bias-corrected point estimator, as detailed in Cattaneo et al. (2019a). The bottom half of the table provides information on the optimal bandwidth selection for both the point estimation of the discontinuity and the estimation of the bias, and informs us that the estimations use 1,922 and 1,868 plant-level expenditure observations to estimate the discontinuity, respectively.

The estimates in Table 2 suggest the presence of a substantial causal impact at the cutoff. Indeed, the coefficients indicate that the average SEPP operating under a Democratic governor who barely won an election is likely to spend for water pollution abatement only about one fifth of the amount spent by the average plant operating under a Republican governor.[16] While this result appears rather striking at first sight, one must keep in mind that our outcome variable – the sum of current expenditure and investment in water pollution abatement – is rather lumpy and any new investment decision may lead to large jumps. Additionally, given that the variable we use is the sum of both expenditure and investment over the whole electoral cycle, our results in reality point to a difference of around a hundred thousand dollars per year for firms whose revenues are in the billions. For further context, this is around 1% of the yearly average for air pollution new capital expenditure reported by Raff et al. (2022).

In conclusion, whereas the average SEPPs operating in states with a Democratic governor tend to spend more on pollution abatement than their counterparts in Republican states – see Figure 1b and footnote 12 – our regressions discontinuity analysis suggests that for SEPPs close to the threshold, the causal impact of operating under a Democrat Governor is, coeteris paribus, to spend substantially less than under a Republican one.

3.4 Validity

When gauging the validity of any RD design, a first necessary step is to show that there is continuity in the running variable, in our case the vote share. This ensures that the assignment to treatment or control is essentially random and that treated units are not able to manipulate which side of the discontinuity they fall on (see McCrary, 2008, for the seminal discussion). In our sample, the plants' location is clearly predetermined and not easily changed, therefore we only need to test whether the party which wins the election shows evidence of sorting using the methodology proposed by Cattaneo et al. (2019b). Several authors have already convincingly argued that close US gubernatorial elections are essentially random and therefore represent a suitable case study for RD designs (e.g. Fredriksson et al., 2011; Beland, 2015; Pacca et al., 2021; Raff et al., 2022). In this sense, the results in Table 3 come as no surprise.

Neither the conventional nor the robust, bias-corrected tests à la Cattaneo et al. (2019b) provide any evidence of sorting (see Table 3). Additionally, as discussed above, there is both a good geographical spread (both North-South and East-West) and a significant overlap in the sets of States on

---

[16] From the bias-corrected point estimate in column (II) of Table 2, we get that the expected ratio in spending between treated and controls is $\mathbb{E}(Y_{it}(1))/\mathbb{E}(Y_{it}(0)) = \mathrm{e}^{-1.73} = 0.18$.

Table 3: Non-manipulability tests

	RD manipulation test
Conventional	-1.33
	(0.19)
Robust	-0.90
	(0.37)
Bandwidth estimation	0.11-0.12
Observations	326
Effective obs. estimation	216

Notes: This table reports the results of the conventional and robust, bias-corrected RD manipulation tests, using local polynomial density estimations based on Cattaneo et al. (2019b). The parentheses report *p*-values. The data are aggregated to the electoral term level for each State.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

each side of the discontinuity. California, Connecticut, Florida, Illinois, Indiana, Iowa, Minnesota, Mississippi, Montana, Nebraska, New Jersey, New York, Pennsylvania, Vermont and West Virginia all appear on both sides of the discontinuity suggesting that there is little reason to be concerned about a bias due to sorting. To conclude, we can think of these close elections as being as good as random.

Based on this discussion, we are confident that our strategy is sound. Moreover, as our particular interest is in trying to isolate the impact of the political colour of the Governor from their broader political agenda, we carefully selected our case study to limit the potential for policy spillovers. Indeed, as argued in section 2, we believe that the potential role of actual State-level policy changes on the water pollution abatement decisions by SEPPs is very limited. To assuage any remaining concerns about our identification strategy, however, we run a few additional tests.

First, rather than focusing on spending over whole electoral cycles, we restrict our attention to spending that takes place in the first year following a tightly contested election. This reduces the risk that policy changes taking place after the election may impact the SEPPs' decisions. Column (I) of Table 4 presents the result of this analysis and confirms that a large discontinuity in spending arises between Republican and Democratic states.

Our second step is to follow Innes and Mitra (2015) who, in their study on the causal effect of a Congressman's party affiliation on local enforcement of the Clean Air Act, only use data for elections without an incumbent. As incumbency plays an important role in elections, removing it helps to isolate the effect of interest. Also in this case, a significant and sizable discontinuity emerges from the data, as shown in Table 4, column (II).

Finally, since the spending decisions taken by SEPPs located in states without NPDES authorization should not be influenced in any way by any actual difference in policy, limiting the sample to elections in states without such authorization provides an even cleaner case study for the influence of political colour. The results of this exercise are reported in column (III) of Table 4 and suggest that,

Table 4: Non-parametric RD estimates: Log of water pollution abatement spending

	Average treatment effect at the threshold		
	(I)	(II)	(III)
Conventional	-2.10***	-1.65**	-1.54**
	(0.36)	(0.66)	(0.70)
Bias-corrected	-2.32***	-1.80***	-2.51***
	(0.36)	(0.66)	(0.70)
Robust	-2.32***	-1.80**	-2.51**
	(0.39)	(0.79)	(1.04)
Bandwidth est.	0.052	0.042	0.050
Observations	4,563	1,075	2,485
Effective obs. est.	2,193	529	1,648

Notes: This table reports the estimated coefficients from non-parametric RD estimations of the effect of a democratic governor winning the election on the amount spent on water pollution abatement. The coefficients are estimated following Calonico et al. (2014). Column (I) reports results for spending that takes place only in the first year after each election, using yearly data. Following Innes and Mitra (2015), column (II) reports the estimated ATE for elections without incumbents. Column (III) refers to elections that take place in state without NPDES authorization. Standard errors clustered at the state level in parentheses. Columns (II) and (III) use data aggregated to the electoral cycle level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

even when neither the stringency nor the enforcement of the regulation falls within the Governor's remit, a large and significant effect emerges from our analysis.

As is customary in empirical applications, further support for the results of the RD analysis performed above may be garnered from both predetermined covariates tests and placebo outcomes. The idea behind this practice is simply that if units lack the ability to precisely manipulate the score value they receive, there should be no systematic differences between units with similar values of the score. Analyzing both predetermined characteristics and placebo outcomes in the same way as the outcome of interest is very informative: since predetermined covariates (or placebo outcomes) could not have been affected by the treatment, failing to reject the null hypothesis of no treatment effect lends credibility to the idea that the RD design is indeed valid.

We have a limited number of continuous predetermined variables that we can for the SEPPs in our data, but we do have information on each SEPP's nameplate generating capacity (i.e. its 'size') and we use this as our predetermined co-variate. In terms of possible placebo outcomes, we consider both the effective electricity generation (in MWh) and the actual heat-rate (in Btu/KWh) for each plant, under the assumption that both energy generation and fuel inputs' choice are unlikely to be influenced by the party allegiance of the Governor. The first three columns of Table 5 present the results of the RD estimations for each of these variables, controlling for the same covariates included in Table 2. In all cases, our estimations emphatically rule out the existence of any discontinuity across the threshold, further supporting the credibility of our exercise.[17]

---

[17] In each case, similar results are obtained by estimating the discontinuity without the additional covariates. The full set of results are available from the authors upon request.

Table 5: Non-parametric RD estimates: name-plate capacity, generation, heat rate and inspections

	Average treatment effect at the threshold			
	Capacity	Generation	Heat rate	Inspections
Conventional	-39.21	-2.80	-1.62	0.11
	(82.56)	(5.03)	(1.80)	(0.33)
Bias-corrected	-24.56	-1.48	-2.11	0.16
	(82.56)	(5.03)	(1.80)	(0.33)
Robust	-24.56	-1.48	-2.11	0.16
	(97.91)	(6.09)	(2.28)	(0.37)
Bandwidth est.	0.079	0.069	0.082	0.091
Observations	3,710	4,990	4,243	326
Effective obs. est.	2,163	2,648	2,471	191

Notes: This table reports the estimated coefficients from non-parametric RD estimations of the effect of a democratic governor winning the election on the rated capacity, the amount of electricity generated and the heat-rate of power plants in the State. The coefficients are bias-corrected, robust estimates following Calonico et al. (2014). The standard errors in parentheses are clustered at the state level. All data are aggregated to the electoral term level, the inspection data are further aggregated by State.

All estimates are computed including plant and year fixed effects, as well as dummies for the State's electricity deregulation status, its NO_x trading scheme participation, and its NPDES authorization status.

*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

One final aspect we want to address to be confident of the validity of our design refers to the possibility that some other discontinuous variable may be driving the jump in pollution abatement spending. Given that the stringency of the regulation is set at the federal level, as discussed above, the remaining obvious suspect for this role is the intensity of the enforcement. Elsewhere in the literature, it has indeed been suggested that enforcement may be laxer in states with Democratic governors (Konisky, 2007; Elrod et al., 2019). To satisfy ourselves that this is not the actual driver of our results above, we test for the presence of a discontinuity in the number of inspections to SEPPs undertaken during each electoral cycle within the framework of the CWA by the authority in charge of compliance in each state.[18] The last column of Table 5 reports the results of this estimation and suggests that, contrary to the results in Elrod et al. (2019), the SEPPs in our sample do not experience differences in enforcement under Democratic governors relative to Republican ones. This difference might be explained by the different set of emitters used in the two analyses. Whereas we focus on SEPPs, Elrod et al. (2019) analyze only the so-called 'major discharging facilities', which tend to be larger facilities such as water treatment plants and attract the majority of inspections. The fact that the number of inspections to SEPPs is not discontinuous in our data, strengthens our conclusions that any differences in abatement are linked to political colour, not to policy differences.[19]

---

[18] We are grateful to Dietrich Earnhart for generously sharing his data on the number of CWA inspections with us.

[19] Also in this case, our conclusions do not change when the estimation excludes the additional covariates. These results are available from the authors upon request.

3.5 Robustness

Having identified the existence of a sizeable discontinuity and provided supporting evidence that the design we employ is sound, we conclude this first part of the paper with a few additional estimations to shore up our confidence in the robustness of the results discussed above.

Table 6: Non-parametric RD estimates: Log of water pollution abatement spending, without observations close to the cut-off

	Average treatment effect at the threshold	
	(I)	(II)
Conventional	-2.48***	-2.34***
	(0.60)	(0.63)
Bias-corrected	-2.80***	-2.62***
	(0.60)	(0.63)
Robust	-2.80***	-2.62***
	(0.70)	(0.75)
Bandwidth est.	0.059	0.053
Bandwidth bias	0.113	0.105
Observations	3854	3854
Effective obs. est.	1789	1679

Notes: This table reports the estimated coefficients from non-parametric RD estimations of the effect of a democratic governor winning the election on the amount spent on water pollution abatement. The coefficients are estimated following Calonico et al. (2014). All estimation are estimated dropping observations within 0.25% the cut-off. In column (I) the estimated ATT is computed without additional covariates. The estimation in column (II) includes plant and year fixed effects, as well as dummies for the State's electricity deregulation status, its NO_x trading scheme participation, and its NPDES authorization status. The standard errors in parentheses are clustered at the state level. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Mostly, we want to rule out that a few observations very close to the discontinuity may unduly bias our conclusions. As suggested in the recent compendium by Cattaneo et al. (2019a), we re-estimate our RD regressions dropping all observations within a very small radius from the threshold. Table 6 report the results of the same local-linear regressions with bias correction performed in Section 3.3, having excluded the 79 observations within a 0.25% radius around the threshold. Despite the significant number of observations dropped, the bandwidth does not change too much and the results continue to indicate a strong local impact of the Governor's political colour on water pollution abatement spending.

4 Analysing the mechanisms

Our analysis so far provides causal evidence that SEPPs react to the political colour of the Governor in charge in their state by changing their overall spending on water pollution abatement. Given the nature of our case study, this result is surprising for at least two reasons. On the one hand, water regulatory standards are set at the federal level and are therefore independent of the party currently in power in the State – in this sense, the stringency of the regulation does not change when the political party in power does. Moreover, compliance with water pollution regulation is relatively inexpensive for the large power plants we analyze. SEPPs tend to place much greater emphasis on compliance with air quality regulation, for example. Together, these two factors suggest that water pollution abatement is a relatively minor problem for large SEPPs, and should not be strongly correlated with the political affiliation of the Governor. It is therefore surprising that the power plants in our sample exhibit such marked differences in behaviour when 'treated' with governors of different political parties. As economists, we are of course interested in understanding the mechanisms that underlie our empirical results, and it is to this aspect that we now turn.

In principle, the results so far could reflect a context in which SEPPs aim to minimize their cost of compliance while faced with differentiated enforcement efforts across political parties. As it happens, while the permitting process remains by and large a federal prerogative, the program enforcement has over time been delegated by the U.S. EPA to state authorities via the NPDES State Program Authorization process – see the discussion in Section 2. Thus, governors in states with NPDES authorization could therefore be at least in principle in a position to influence the degree to which environmental standards are enforced.

Our analysis in Section 3.4 already suggests that regulatory enforcement – as measured by the number of inspections carried out under the CWA within each state – does not depend on political colour. The results in last column of table 5 indeed indicate that in our data the number of inspections does not differ across Republican and Democratic states. To investigate this aspect in greater detail, however, we test whether this result continues to hold once we allow the effect of enforcement to vary across political environments and institutional arrangements. Below, we run a series of regressions that explain the water pollution abatement spending undertaken by SEPPs as a function of the political environment, the enforcement efforts made by the environmental enforcement agency, the NDPES authorization status and a number of other controls. Specifically, we estimate the following equation,

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 I_{it} + \beta_3 N_{it} + \beta_4 \left(A_{it} \times B_{it}\right) + X'_{it}\gamma + \phi_i + \psi_t + \varepsilon_{it}. \qquad (2)$$

As before, Y_{it} is (the logarithm of) water pollution abatement spending by plant i in period t. D_{it} is the treatment dummy, which takes a value of one if a Democratic governor has won the election which took place at time t in the state where plant i operates, and zero otherwise. I_{it} is our proxy for the intensity of enforcement, which is given by the number of inspections per year undertaken within the framework of the CWA by the authority in charge of compliance in each state. N_{it} is the dummy that indicates whether the State where plant i is located had received the authorization to enforce the CWA under the NPDES programme by the time the election took place in period t. In some of the regressions, we also include interaction terms between the treatment dummy, the effort measure and the NPDES dummy – which we indicate in general terms by $(A_{it} \times B_{it})$ in (2) – to allow for differential effects for each of the variables. Our empirical specification also accounts for observable heterogeneity across states using a vector, X, of control variables. Two dummy variables are used here to indicate if the plant is located in a state that has a restructured electricity market or that participates in a NO_x trading program. To further control for unobservable heterogeneity at the plant level, we include plant and year fixed effects in all our regressions. Following Bertrand et al. (2004), we cluster the standard errors at the level of the treatment, in our case the state.

To retain as much as possible of the RD flavour from Section 3, along with the opportunity it offers to provide a causal interpret of our results, we restrict our sample to the available subset of close elections. We thus drop from the sample used in the estimations that follow all data points that refer to SEPPs operating in states where the last election would be excluded from our RD estimations based on the optimal bandwidth selection approach proposed by Cattaneo et al. (2019b).[20,21]

Table 7: Water pollution abatement spending

	Log of water pollution abatement spending				
	(I)	(II)	(III)	(IV)	(V)
Democrat	-0.72**	-0.74**	0.35	-0.75**	-0.53***
	(0.33)	(0.35)	(1.85)	(0.35)	(0.14)
$\ln$(Inspections)	–	0.24	0.31	-1.09	0.26
	–	(0.28)	(0.33)	(2.05)	(0.29)
$\ln$(Inspections) × Democrat	–	–	-0.16	–	–
	–	–	(0.28)	–	–
$\ln$(Inspections) × NPDES	–	–	–	1.36	–
	–	–	–	(1.97)	–
NPDES × Democrat	–	–	–	–	-0.33
	–	–	–	–	(0.53)
Constant	-77.65	-69.83	-73.08	-71.07	-73.71
	(60.74)	(66.63)	(67.74)	(66.90)	(65.45)
Observations	1,379	1,379	1,379	1,379	1,379
R-squared	0.07	0.07	0.07	0.07	0.07
$\partial Y_{it}/\partial D_{it}$	-0.72	-0.74	-0.73[a]	-0.75	-0.86[b]
Test for Treatment Effect	F=4.58**	F=4.60**	F=4.51**	F=4.66**	F=4.36**
Test for Treatment Effect (NPDES=0)	–	–	–	–	F=14.27***

Notes: This table reports the estimated coefficient from fixed-effects panel regressions. The standard errors in parentheses are clustered at the state level. Year fixed-effects and additional control variables included in all regressions but not shown (Electricity deregulation status, NO_x trading). All data are aggregated to the electoral term level.

[a] This marginal effect is computed at the sample mean value of $\ln$(Inspections), i.e. 6.73.

[b] This marginal effect is computed assuming NDPES=1.

*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 7 presents the results of our estimates. Column (I) reports the estimates obtained by regress-

---

[20] To compute the cut-off points we run the *rdbwselect* command provided with the *rdrobust* STATA package (Calonico et al., 2017). We choose the bandwidth computed with the *cerrd* option. Following this procedure, we retain only these observations where the margin of victory by either party at the last election did not exceed 3.6%.

[21] We note here that, while technically an unbalanced panel of observations related to SEPPs across states and over time, each of the 679 SEPPs contributing observations to this analysis appear on average twice, mostly not in consecutive elections. From this point of view, the dataset is probably better interpreted as a cross-section.

ing abatement spending on the treatment dummy alone, and shows a negative effect of having a (narrowly victorious) Democratic Governor on pollution abatement spending thereby confirming the results from Section 3 above. In the remaining columns, we add more variables, including a control for the number of inspections. The coefficients for the (log of) inspections count are consistently insignificant from the statistical point of view across all regressions.[22] In column (III) we include an interaction term to control for the possible differential effect of inspections performed under a Democratic governor. The treatment dummy loses significance and the coefficient of the interaction term is not statistically significant and we are thus unable to reject the null of no difference. The treatment dummy and the interaction term are highly correlated, however, and their joint significance test is significant at the 99% level, suggesting that inspections have no impact on the (very stable) treatment effect.[23] Columns (IV) and (V) look into the impact of the delegation of inspections to state authorities. These results suggest that inspections delegated to the states are not more effective than federal ones, and that delegated inspections undertaken under a Democratic governor are not different from those carried out under a Republican Governor.

Overall, these results suggest that enforcement efforts do not affect the abatement spending undertaken by SEPPs, irrespective of the political affiliation of the Governor. These results are in line with the existing literature. Elrod et al. (2019), for example, show that while officials in states with Democratic governors inspect a smaller percentage of their major water polluters, and carry out less stringent inspections than their counterparts in Republican states, they induce similar levels of compliance. That CWA inspections play no role in determining water pollution abatement spending might as well reflect the fact that for the power plants in our sample, water pollution regulation represents a second-order problem compared to other types of environmental controls such as those mandated by the Clean Air Act, as suggested above.

At the bottom of Table 7, we report our estimates on the size of the treatment effect, i.e. $\partial Y_{it}/\partial D_{it}$, and the results of the tests for its statistical significance. A significant and negative difference in spending across plants in Democratic and Republican states emerges very clearly, is extremely robust, and its magnitude is similar to the effects found with our RDD estimations in Section 3.3. Importantly, this significant difference persists after controlling for enforcement efforts and even among plants that operate in states where the local EPA is not delegated to enforce the CWA rules.[24] Even when states have no authority to enforce the CWA, and therefore the views of the Governor ought to play no role in the choice of water pollution abatement spending, the governor's political colour still makes a significant difference to power plants' decisions. These results suggest that some other mechanism, beyond any potential differences in policy and enforcement between political parties, is at play.

---

[22] The joint significance (F-)test on the coefficients of the $\ln$(Inspections) and $\ln$(Inspections) $\times$ Democrat variables in column (III) is 0.52, with an associated p-value of 0.60. The equivalent test for the coefficients of $\ln$(Inspections) and $\ln$(Inspections) $\times$ NPDES in column (IV) is 1.36, with a p-value of 0.27.

[23] The overall treatment effect at the sample mean of the inspection variable (6.73) is equal to -0.73. The F-test statistic for this to be significantly different from zero is 4.51 – as indicated in Table 7, column (III) – with a p-value of 0.039.

[24] In column (V) – where we are able to explicitly test for the treatment effect across NDPES status – we indeed find evidence of a significant treatment effect in both cases.

This last finding is difficult to reconcile with economic theory because one would in general not expect payoff-irrelevant aspects of the economic environment to determine in such a robust way the behaviour of economic agents. Since none of the existing models that explain the compliance behaviour of regulated firms (see Shimshack, 2014, for a review) are able to explain the results discussed above, in the next section we put forward a new model of enforcement and compliance. In that context, we show that the political affiliation of the governor – while not directly relevant to the firms' payoffs – may yet play a role as a signal that allows businesses to coordinate on alternative strategies, therefore bridging the gap between theory and empirical evidence.

5 A game of enforcement and compliance

In order to model the regulatory environment in place under the CWA, where permits are issued at the federal level by the U.S. EPA while the subsequent enforcement is mostly delegated to state authorities, we develop a framework that focuses on the strategic interaction between the enforcement agency and a group of firms regulated under fault-based liability.

The structure and the timing of the game may be succinctly described as in Figure 3 below, where the nodes indicate the decision point for each of the agents, whereas the ticks denote the moments in time when outcomes are realized.

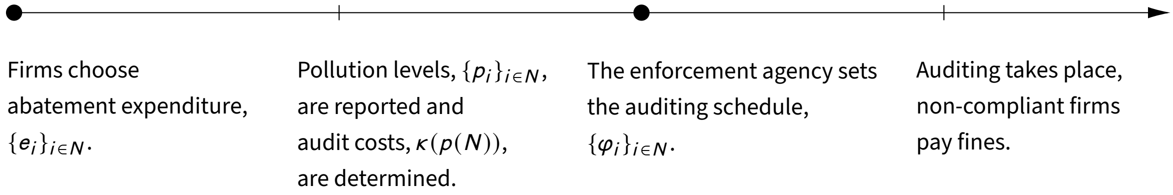


Figure 3: Timeline of the game

In the first stage of the game, each regulated firm – given its pollution permit – chooses the optimal level of abatement spending trading off the opportunity cost of abating pollution against the expected fines that it would accrue if it were found to be non-compliant. The firms then engage in production, which results in a certain level of pollution being realized. All firms are required to monitor their emissions and to report their realized pollution levels to the enforcement agency.

It is important for our discussion that pollution is partly stochastic, so that it cannot be perfectly controlled by the firm, despite its best efforts. In line with the principle of fault-based liability, therefore, a firm may still be compliant with the regulation, despite reporting a level of pollution in excess of its permitted level, as long as it can show to have operated with 'due diligence'. Accordingly, the true compliance status of the firm can only be determined once its abatement activities have been carefully scrutinized. This process takes place via a process of thorough (and costly) audits conducted by the enforcement agency.

In the second stage of the game, the enforcement agency uses the emissions reports it receives from the firms to devise an auditing schedule to identify non-compliant firms and collect the associated fines. This auditing process is the only tool at the agency's disposal to enforce the regulation. Following the discussion in Heyes (2002) for an exogenous trigger policy, we acknowledge that auditing power plants is not the only task the enforcement agency is mandated to perform, however. Therefore the (opportunity) cost of auditing may vary with the priority attached to regulating power plants. We go a step further in our theoretical modelling of the implementation side of policy by incorporating the fact that the resources available to the enforcement agency to conduct inspections are likely to be linked to the perceived need for auditing, which depends on the total pollution being reported (similar points were made by Dion et al., 1998; Stafford, 2002; Gray and Shadbegian, 2004). To this end, we need to recognize the reciprocal externalities that firms impose on one another. The main theoretical contribution here is that we explicitly model these externalities, whereas others have completely abstracted away from them. Our results are novel and suggest that the theoretical literature has so far missed an important feature of the problem.

The final step in the game is for the selected firms to be audited and, once found to be at fault, to be subjected to enforcement action in the form of hefty fines.

In the remainder of this section, we formally introduce the setting, provide a stylized solution to the model, and discuss the empirical implications that emerge from the equilibrium behaviour.

5.1 Building blocks and the timing of the game

We consider a sector consisting of N firms that are subject to environmental regulations and assume that emissions standards are set optimally according to some welfare criterion by a central regulator and are exogenous to our model. In accordance with the regulations, firm i is given a permit that allows it to release pollutants up to the level $\hat{p}_i$.[25] To ensure that the pollution level generated by its operations is within the permissible range, the firm undertakes abatement activities that entail abatement spending equal to e_i.[26]

The realized level of pollution, p_i, comprises a deterministic component, $g(e_i)$, which links abatement spending to polluting emissions, and a random component, ϵ_i, according to,

$$p_i = g(e_i) + \epsilon_i. \tag{3}$$

We assume that subsequent increases in abatement spending reduce pollution, *coeteris paribus*, albeit at decreasing rates, so that $g_e < 0$, and $g_{ee} > 0$.[27] We also assume that the random compo-

---

[25] In actual fact, under fault based liability, a firm is assigned a minimum standard of due diligence in terms of the mitigation activities it is expected to undertake. The fault standard is determined by the opportunity cost of these activities. It is helpful to think of the permit level $\hat{p}$ as the expected level of pollution corresponding to the fault standard. We revisit this point later on in our discussion of the model.

[26] In section 2 we discussed the types of abatement activities available to the electricity generators that are the focus of our case study. The theoretical model presented here, however, is intended to be more general and to apply to other sectors that are similarly regulated.

[27] As is common practice, we denote the first and second (partial) derivatives of function f with the respect to variable x by $f_x \equiv \partial f / \partial x$ and $f_{xx} \equiv \partial^2 f / \partial x^2$, respectively.

nent of the pollution process, $\epsilon_i : N \to \mathbb{R}$, has mean 0, variance σ^2, and cumulative distribution function Φ. The firms monitor their emissions and the realized level of pollution; once realized and observed by the firm, the level of pollution is truthfully reported by the firm to the enforcement agency.

Equation (3) implies that a pollution report $p_i > \hat{p}_i$ is only an imperfect signal of the firm's compliance status. No level of abatement fully eliminates the risk of sending a non-compliant signal: for an adverse realization of the stochastic term, the level of pollution may well end up exceeding the regulatory threshold despite the best efforts of the firm.[28] Under fault liability, compliance depends in fact only on whether a firm's abatement spending is at least as large as the fault standard – defined as the level of abatement spending that would satisfy the permit standard in the absence of any stochastic disturbance, i.e. $\hat{e}_i \equiv g^{-1}(\hat{p}_i)$. Given the realized level of pollution p_i, the probability that a firm i is compliant is given by Prob $\left[g^{-1}\left(p_i - \epsilon_i\right) \geq \hat{e}_i\right]$. The true compliance status for a firm can therefore only be established with certainty after a costly audit of the firm by the enforcement agency. Audits entail additional costs for the inspected firms due to legal and administrative expenses, as well as the possible stoppage time required to allow the auditors access to the machinery and/or to perform additional tests. We denote these costs with the positive constant m. We also assume that the firm cannot alter its compliance status at the time of the audit and that, following the inspection, the firm's true compliance status is indeed revealed.

The enforcement agency's task is to enforce the regulatory standards by carrying out inspections and collecting fines from non-compliant firms. The punishment schedule is determined by the regulator and is therefore exogenous from the point of view of the enforcement agency. Following Heyes (2002), who suggests that the most relevant cost associated with non-compliance in this context is the loss of reputation, we model fines – denoted by F – as (large) lump-sum payments. A more general model, where both the inspection costs, m, and the fines, F, could be assumed to be firm-specific and the latter be linked to the degree of non-compliance, would arguably be a closer approximation to the actual enforcement environment. Given that neither change would make a qualitative difference for our analysis, and in the interest of a more transparent exposition, we err on the side of simplicity here.

Auditing firms is just one of the many activities the enforcement agency is required to perform, and the (opportunity) cost of auditing depends on the overall resources allocated to this part of its operations. Realistically, we assume that the auditing budget increases with the need for enforcement – measured by the aggregate level of reported pollution, $p(N) \equiv \sum_{i \in N} p_i$ – but at a rate that is slower than the increase in reported pollution (Stafford, 2002; Gray and Shadbegian, 2004). The opportunity cost of using resources for auditing therefore increases with the aggregate level of pollution: the overall envelope is finite and other activities need to be foregone in exchange for more inspections. Formally, we let the unit cost of inspections, $\kappa(p(N))$, be a strictly increasing and convex function of aggregate pollution: i.e., $\kappa_{p(N)} > 0, \kappa_{p(N)p(N)} > 0$. Given that pollution is a

---

[28] This assumption follows the modelling in Feng and Hennessy (2009) and differentiates our work from that of Heyes (2002), for example, who assumes that abatement effort determines compliance perfectly.

decreasing and convex function of abatement expenditure, we have that $\kappa_e < 0$, $k_e(0) \to \infty$, and $\kappa_{ee} > 0$.[29]

Since auditing is a costly activity, the enforcement agency needs to decide – based on the reported levels of pollution – which of the firms to audit in order to maximize the expected revenue from fines.[30] Taking the actions of the firms as given, the agency trades off its own auditing costs against the expected revenue from fines, which, for a given set of realized pollution reports, $\{p_1, \ldots, p_N\}$, is simply $F \cdot \sum_{i \in N} \text{Prob}\left[g^{-1}(p_i - \epsilon_i) < \hat{e}_i\right]$. We denote the probability with which firm i is audited by $\varphi_i(p_i(e_i), p_{-i}(e_{-i}))$, where $-i$ is our shorthand to indicate the vector of variables indexed by all firms except for i. Therefore, the agency's problem is to choose the φ's that solve

$$\min_{\{\varphi_i\}_{i \in N}} \sum_{i \in N} \varphi_i(p_i(e_i), p_{-i}(e_{-i}))\kappa(p(N)) - \sum_{i \in N} \varphi_i(p_i(e_i), p_{-i}(e_{-i}))\, F \; \text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right]. \tag{4}$$

Within this framework, each firm chooses its abatement effort, e_i, to minimize its expected compliance costs $C(e_i, e_{-i})$ that also depend on all other firm's abatement decisions e_{-i}, the probability of being audited, $\varphi_i(p_i(e_i), p_{-i}(e_{-i}))$, the fixed cost of audit, m, and the fine F, that is,

$$\min_{e_i} C(e_i, e_{-i}) = e_i + \varphi_i(p_i(e_i), p_{-i}(e_{-i}))\left(m + F \cdot \mathbb{1}_{\{e_i < \hat{e}_i\}}\right), \tag{5}$$

where $\mathbb{1}_{\{e_i < \hat{e}_i\}}$ is an indicator function that takes on the value 1 if $e_i < \hat{e}_i$, i.e. if the firm is not compliant, and 0 otherwise.

5.2 Solving the game

In line with the timing of the game set out above – see Figure 3 – the enforcement agency moves last, taking as given the firms' choices on abatement expenditure. It is straightforward to show that the optimal auditing schedule satisfies the following properties:

Lemma 1. *Let $\{\varphi_i^*(p_i, p_{-i})\}_{i \in N}$ be the enforcement agency's optimal choices of auditing probabilities, then:*

1. *Given a set of pollution signals $\{p_1, \ldots, p_n\}$, for each $i \in N$,*

$$\varphi_i^*(p_i, p_{-i}) = \begin{cases} 0, & \text{if } \kappa(p(N)) > F \; \text{Prob}\left[e_i' < \hat{e}_i \,|\, p_i\right], \\ 1, & \text{if } \kappa(p(N)) \leq F \; \text{Prob}\left[e_i' < \hat{e}_i \,|\, p_i\right]. \end{cases} \tag{6}$$

---

[29] Notice that the assumption that the cost of auditing diverges to infinity as the abatement effort goes to zero, together with the assumption that the level of fine is finite and equal to F, implies that the enforcement agency cannot guarantee the respect of the rules by threatening to inspect and fine every single firm in the sector. This implication reflects most actual regulatory environments and makes for an interesting economic problem.

[30] Once again we distinguish the objective of an enforcement agency from that of a regulator. While a regulator seeks to trade off pollution/abatement incentives with the cost of enforcement, the enforcement agency, whose decision on auditing occurs after abatement activities have taken place and pollution is realized, has no instrument to impact on incentives. Its task is therefore merely focused on the implementation of penalties and the effective cost of detection.

2. *Given a set of pollution signals* $\{p_1, \ldots, p_n\}$ *and two firms i and j sending signals* $p_i \leq \hat{p}_i$ *and* $p_j > \hat{p}_j$*, then* $\varphi_i^*(p_i, p_{-i}) \leq \varphi_j^*(p_j, p_{-j})$.
3. *Given two signals' profiles* $\{p_j, p_{-j}\}$ *and* $\{p_j', p_{-j}\}$ *that differ only by the signal sent by firm j, with* $p_j \leq \hat{p}_j$ *and* $p_j' > \hat{p}_j$*, then* $\varphi_i^*(p_j, p_{-j}) \geq \varphi_i^*(p_j', p_{-j})$ $\forall i \in N \setminus \{j\}$.

Proof. See Section B.1 in the Appendix. □

The first part of the lemma follows trivially from the agency's problem, in equation (4). It informs us that all firms for which the expected fine, Prob $\left[e_i' < \hat{e}_i \mid p_i\right] F$, exceeds the cost of the audit, $\kappa(p(N))$, will be selected for audit with certainty. Conversely, all firms for which the unit cost of the audit is higher than the expected fine will not be audited. The second part of the Lemma confirms, instead, the intuitive result that a firm is less likely to be audited if it sends a 'compliant' signal, all else equal. The third part is less obvious and deserves some discussion. Point 3 above states that, irrespective of the signal sent by any given firm, its probability of being audited decreases (weakly) with the number of other firms that send non-compliant signals. Intuitively, as the number of non-compliant signals, and therefore the total reported level of pollution, increases, so does the (opportunity) cost of auditing. As a consequence, from the point of view of the individual firm, the probability of being selected for audit cannot increase, *coeteris paribus*. This is a key insight of our analysis as it highlights the reciprocal externalities created across firms by their reporting to the enforcement agency.

These externalities have crucial implications for the behaviour of the regulated firms in the economy and some more discussion of the implications of Lemma 1 is therefore warranted. We first note that each firm's abatement spending decisions impact both on its expected compliance status and on the overall level of pollution, thereby also feeding into the agency's auditing costs. Considering the first part of Lemma 1, it is easy to see how the probability with which each firm gets audited depends on the aggregate abatement in the economy. Thus, for a given level of abatement spending by firm i, it is possible that i be audited when aggregate pollution is low, but not when it is high. Indeed, it may well be the case that, for $e\prime_j < e_j$, we get both

$$\kappa(p_i(e_i), p_j(e_j), p_{-ij}) \leq F \text{ Prob}\left[e_i < \hat{e}_i \mid p_i\right], \text{ and, } \kappa(p_i(e_i), p_j(e_j'), p_{-ij}) > F \text{ Prob}\left[e_i < \hat{e}_i \mid p_i\right], \tag{7}$$

where now $-ij$ indicates all firms other than i and j. In the interest of the internal consistency of the model, and given the strict convexity of κ, we require that if (7) holds for some e_i, it must also hold for all $e_i' \leq e_i$. Intuitively, with this consistency requirement, we impose that if the increase in auditing costs due to an increase in total pollution suffices to change the decision of the regulator when pollution is 'low', the same must happen when pollution is 'higher'.

More importantly, however, we want to rule out the situation where a firm is able, by reducing its own abatement spending, to increase pollution sufficiently to drive the auditing cost, κ, above the (increased) expected revenue from its own fine. Such a scenario would imply that the firm is able to 'pollute its way out' of enforcement, a clearly undesirable feature for a model of enforcement. To rule out this counter-intuitive outcome, we impose that the impact of each firm on the overall cost of pollution be 'small enough'. Specifically, for any two profiles of abatement expenditure (e_i, e_{-i}) and (e_i', e_{-i}) such that $e_i \leq e_i'$, we require that the following holds:

$$\kappa\left(p_i(e_i') + \sum_{j\in N\setminus\{i\}} p_j(e_j)\right) - \kappa\left(p_i(e_i) + \sum_{j\in N\setminus\{i\}} p_j(e_j)\right) \leq \text{Prob}\left[e_i' < \hat{e}_i \mid p_i\right] F - \text{Prob}\left[e_i < \hat{e}_i \mid p_i\right] F. \quad (8)$$

We can now prove the following intermediate step in solving the game:

Lemma 2. *Consider the simultaneous-move abatement expenditure game between N firms with payoffs given by* (5) *discussed above. This game is super-modular.*

Proof. See Section B.2 in Appendix B. □

To understand this result, we may look back to equation (5) and substitute in the optimal φ^* from Lemma 1 to get

$$\min_{e_i} C(e_i, e_{-i}) = e_i + \varphi_i^*(p_i(e_i), p_{-i}(e_{-i}))\left(m + F \cdot \mathbb{1}_{\{e_i<\hat{e}_i\}}\right). \quad (9)$$

This expression clearly drives home the idea that the benefits from an increase in abatement effort for each firm depend on the effort exerted by all other firms in the economy, so that each firm's abatement behaviour creates externalities on all other firms in the economy. Lemma 2 shows that in our game, the strategic interaction among the actions of the players is such that an increase in abatement by the other players in the game increases the returns to the individual firm of increasing its own abatement spending, so that the game exhibits stategic complementarity (Fudenberg and Tirole, 1986).

Finally, the fact that the abatement expenditure game described above is super-modular, allows us to directly invoke the following result due to Milgrom and Roberts (1990):

Proposition 1. *Consider the super-modular abatement expenditure game between N firms with payoffs given by* (5) *where firms choose abatement expenditure $\{e_i\}_{i\in N}$ simultaneously. For all firms $i \in N$, there exist a smallest expenditure level, $\underline{e}_i$, and a largest expenditure level, $\overline{e}_i$, such that the expenditure profiles $(\underline{e}_i)_{i\in N}$ and $(\overline{e}_i)_{i\in N}$ constitute pure Nash equilibria of the game.*

Proof. See Milgrom and Roberts (1990), Theorem 5. □

It follows immediately from this proposition that our game admits a (bounded) set of multiple non-cooperative equilibria, due to the reciprocal externalities created by the regulator's budget constraint.

The presence of such equilibria suggests that firms might be able to resort to a correlated equilibrium to improve upon the Nash outcomes at their disposal (Fudenberg and Tirole, 1986). For this to happen, the crucial element is the existence of a visible, salient and non-manipulable correlating device that firms might use to condition their strategy selection upon. We conjecture that tightly contested gubernatorial elections may play such a role and allow firms to choose a high-effort strategy when one party wins and a low-effort one when the other party prevails.

That tightly-contested elections are highly visible and non-manipulable seems beyond doubt; the empirical evidence on inspections further suggests that they are also salient for SEPPs. Indeed, Elrod et al. (2019) show that among large emitters regulated by the CWA, relatively more inspections are carried out under Republicans than under Democrats. While this pattern does not carry over to the smaller emitters we work with, it stands to reason that power plants treated under the CWA, being aware of this pattern of behaviour by the regulator, may see a Republican victory as a signal to coordinate on the high-expenditure strategy. Conversely, a narrow Democratic win could be seen by the power plants as a signal to coordinate on the low-spending strategy.

To conclude, the results of our discussion in this section provide a useful bridge between the empirical results and our theoretical understanding of the behaviour of regulated power plants. Our model offers a theoretically consistent explanation for the puzzling behaviour that emerges from our empirical analysis, whereby power plants choose different abatement strategies when operating under governors of different political colour, even when there are no differences in policy or enforcement efforts.[31]

6 Abatement spending and environmental outcomes

Our analysis so far shows that across U.S. states, SEPPs tend to spend relatively less on water pollution abatement when they operate in a state where the winner of a closely contested election is affiliated with the Democratic party. We have also shown that a conceptual framework that emphasizes the role of externalities among abating firms in the presence of a resource-constrained enforcement agency leads to theoretical implications that resonate with both the data and the stylized facts. We now continue our discussion by looking at what consequences, if any, differences in pollution abatement spending make in terms of environmental outcomes at the power plant level.

We tackle this issue within a panel regression framework in which the measured levels of two key water pollutants, thermal pollution and chlorine, are regressed on the SEPPs' water pollution abatement spending, controlling for their power generation rate, which obviously correlates with both water use and the rate at which the heat exchangers are used. Thermal pollution here is the product of the annual average rate of discharge in cubic feet per second and the difference between the intake temperature and the outflow temperature. Chlorine use is the amount of chlorine added to the water in a year in thousands of pounds. These pollution data are available from the EIA Forms 767 and 923. In this section, we use yearly data and control for year fixed-effects by including year

---

[31] Our game here is cast within a static framework, which is fully consistent with the static nature of the empirical work in the previous sections. One could in principle develop a dynamic alternative and focus on the outcome of a repeated game. Recent results from the literature on regret-based heuristics suggest that the results would not be too dissimilar to what one gets with the correlation framework discussed above (e.g. Hart and Mas-Colell, 2000; Hart, 2005).

dummies in all our specifications. Given the significant degree of non-linearity displayed by the data, moreover, we include squares of the explanatory variables across our regressions.

Table 8 shows the outcome of these estimations. The first two columns of the table show that water pollution abatement spending has no significant impact on thermal pollution and neither do additional inspections. As expected, however, a clear positive correlation emerges between the degree of generation and thermal pollution.

Table 8: Water pollution and abatement efforts

	Thermal pollution		Chlorine	
	(1)	(2)	(3)	(4)
Water ab. spend	2.04	1.17	-19.25***	-59.72***
	(7.59)	(12.66)	(7.13)	(17.93)
(Water ab. spend)^2	–	0.00	–	0.15***
	–	(0.05)	–	(0.05)
Generation rate	119.89***	119.87***	63.37**	62.80**
	(24.10)	(24.10)	(29.49)	(29.43)
(Generation rate)^2	-0.34***	-0.34***	-0.21	-0.21
	(0.12)	(0.12)	(0.13)	(0.13)
Inspections	0.14	0.14	0.23	0.24
	(0.41)	(0.41)	(0.60)	(0.60)
(Inspections)^2	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	2,096.44***	2,097.17***	2,156.42**	2,188.28**
	(755.66)	(754.10)	(958.16)	(952.08)
Observations	10,154	10,154	10,581	10,581
R-squared	0.02	0.02	0.01	0.01

Notes: This table reports the estimated coefficient from fixed-effects panel regressions. The standard errors in parentheses are clustered at the state level. Year dummies are included in all regressions albeit not shown. All data are yearly data.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Interestingly, the results for chlorine use are rather different, however. Indeed, the second set of regressions show that for chlorine water pollution abatement spending significantly reduces current polluting emissions. Once again the squared terms exhibit the expected convex relationship, as increased expenditures reduce chlorine use, albeit at decreasing rates.[32] As before, the coefficients of inspections are statistically insignificant, while generation rates are positively correlated with higher levels of pollution. The difference between the two sets of results can be explained by recalling that thermal pollution is – given the installed cooling system – to a large extent proportional to the amount of energy generated. While there are a limited number of measures that power plants can implement to prevent exceeding their mandated limits – e.g., keeping their boiler from running over its maximum capacity – they mostly imply costs that are unlikely to be classified as 'pollution abatement'. On the other hand, the more SEPPs spend to maintain their heat exchangers, an expenditure likely to be classed at least partly as pollution abatement, the less al-

---

[32] Note that the estimated inflection point is 199.06, compared to a sample mean of 1.58.

gal build-up they will experience, and the lower the need to chlorinate the water they run through their cooling systems to flush out contaminants.

This exercise shows that the level of spending on pollution abatement on the part of SEPPs has important effects on at least some environmental outcomes. Having previously provided robust evidence that electoral outcomes affect abatement spending, we conclude that differences in the executive's political colour have relevant implications for the private sector's behaviour, even when they do not result in policy changes, as well as tangible knock-on effects on environmental outcomes.

7 Conclusions

In this paper, we asked whether firms respond to the colour of the political executive in charge, over and beyond any actual difference in policy. A regression discontinuity design based on the outcome of closely contested gubernatorial races in the U.S. enabled us to show that steam electric power plants regulated under the Clean Water Act respond to changes in the party affiliation of the executive by modifying their water pollution abatement spending in both statistically and economically significant ways. Power plants facing a Republican governor were shown to spend substantially more on water pollution abatement than their counterparts operating under a Democrat.

This result is exciting in its own right, as little empirical evidence exists about the interaction between firms' behaviour and political environments. From our point of view, its interest is heightened by the fact that our case study allows us to gauge the effect of political colour in situations where one would not expect any. In fact, the political colour of the executive is shown to significantly affect the behaviour of power plants even when the state executive is neither involved in the setting of the regulation nor in its enforcement. The colour of the executive itself matters!

The evidence we uncovered led us to investigate the mechanisms that might be driving these results. In the second part of the paper, therefore, we developed a novel game theoretical model which realistically extended existing frameworks to allow the enforcement agency to interact strategically with the whole population of firms. In this extended set-up, strategic complementarities emerge due to the enforcement agency's budget constraint. We show that the game admits multiple equilibria and argue that the firms may have an advantage in coordinating their strategies using non-manipulable coordination devices. In this context, and irrespective of whether the enforcement is conducted by federal or state authorities, regulated firms have an incentive to use the signal offered by the outcome of the gubernatorial elections to coordinate their abatement strategies. Our empirical results are consistent with this interpretation, which also chimes well with the stylized facts on enforcement found elsewhere in the literature (Elrod et al., 2019).

Our work provides evidence that the political environment matters greatly to the behaviour of firms, therefore adding to the literature on this important issue. More than that, we show that the role played by the political environment is subtler and more pervasive than one would expect. In our example, different environmental outcomes emerge from the political colour of the executive, even in the absence of actual differences in policy and enforcement. From this point of view, we believe that the main take-away from our paper is the realization of the importance of elections and electoral outcomes for a whole range of economic and social issues, well beyond those discussed in electoral manifestos and pledges, and even beyond the intentions of the political agents. Overall, it provides a stark reminder of the voters' responsibility in taking a comprehensive view of the possible consequences of their electoral choices, and of the researchers' role in informing them.

References

Akerlof, G. A. and R. E. Kranton (2000): "Economics and Identity," *The Quarterly Journal of Economics*, 115, 715–753.

Alt, J. and R. Lowry (2000): "A dynamic model of state budget outcomes under divided partisan government," *The Journal of Politics*, 62, 1035–1069.

Athey, S. and A. Schmutzler (2001): "Investment and Market Dominance," *The RAND Journal of Economics*, 32, 1–26.

Beland, L.-P. (2015): "Political Parties and Labor-Market Outcomes: Evidence from US States," *American Economic Journal: Applied Economics*, 7, 198–220.

Beland, L.-P. and V. Boucher (2015): "Polluting politics," *Economics Letters*, 137, 176 – 181.

Beland, L.-P. and S. Oloomi (2017): "Party affiliaciton and public spending: evidence from U.S. Governors," *Economic Inquiry*, 55, 982–995.

Bertrand, M., E. Duflo, and S. Mullainathan (2004): "How Much Should We Trust Differences-in-Differences Estimates?" *The Quarterly Journal of Economics*, 119, 249–275.

Calonico, S., M. D. Cattaneo, M. H. Farrell, and R. Titiunik (2017): "rdrobust: Software for Regression Discontinuity Designs," *The Stata Journal*, 17, 372–404.

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014): "Robust Nonparametric Confidence Intervals for Regression-Discontinuity Designs," *Econometrica*, 82, 2295–2326.

Caplan, B. (2001): "Has Leviathan been bound? A theory of imperfectly constrained government with evidence from the states," *Southern Economic Journal*, 67, 825–847.

Cattaneo, M. D., N. Idrobo, and R. Titiunik (2019a): *A Practical Introduction to Regression Discontinuity Designs: Foundations*, Cambridge Elements: Quantitative and Computational Methods for Social Science, Cambridge University Press.

CATTANEO, M. D., M. JANSSON, AND X. MA (2019b): "Simple Local Polynomial Density Estimators," *Journal of the American Statistical Association*, 1–7.

CAUGHEY, D. AND J. S. SEKHON (2011): "Elections and the Regression Discontinuity Design: Lessons from Close U.S. House Races, 1942–2008," *Political Analysis*, 19, 385–408.

CAUGHEY, D., C. WARSHAW, AND Y. XU (2017): "Incremental Democracy: The Policy Effects of Partisan Control of State Government," *The Journal of Politics*, 79, 1342–1358.

DE LA CUESTA, B. AND K. IMAI (2016): "Misunderstandings About the Regression Discontinuity Design in the Study of Close Elections," *Annual Review of Political Science*, 19, 375–396.

DIETER, C., M. MAUPIN, R. CALDWELL, M. HARRIS, T. IVAHNENKO, J. LOVELACE, N. BARBER, AND K. LINSEY (2018): "Estimated Use of Water in the United States in 2015," Tech. rep., U.S. Geological Survey Circular 1441.

DION, C., P. LANOIE, AND B. LAPLANTE (1998): "Monitoring of Pollution Regulation: Do Local Conditions Matter?" *Journal of Regulatory Economics*, 13, 5–18.

DRÈZE, J. H. AND J. GREENBERG (1980): "Hedonic Coalitions: Optimality and Stability," *Econometrica*, 48, 987–1003.

EGGERS, A. C., A. FOWLER, J. HAINMUELLER, A. HALL, AND J. M. SNYDER (2014): "On the Validity of the Regression Discontinuity Design for Estimating Electoral Effects: New Evidence from over 40,000 Close Races," *American Journal of Political Science*, 59, 259–274.

ELROD, A., S. KARADAS, AND K. C. THEYSON (2019): "The Effect of Gubernatorial Political Parties on Monitoring and Enforcement of Federal Environmental Regulation: Evidence from the Clean Water Act," *Environmental Economics and Policy Studies*, 21, 171–202.

ENVIRONMENTAL PROTECTION AGENCY (2023a): "NPDES Permit Basics," `https://www.epa.gov/npdes/npdes-frequent-questions`, last accessed November 1, 2023.

——— (2023b): "NPDES Permit Limits," `https://www.epa.gov/npdes/npdes-permit-limits`, last accessed November 1, 2023.

FENG, H. AND D. A. HENNESSY (2009): "Production and Abatement Distortions under Noisy Green Taxes," *Journal of Public Economic Theory*, 11, 37–53.

FERREIRA, F. AND J. GYOURKO (2009): "Do Political Parties Matter? Evidence from U.S. Cities," *The Quarterly Journal of Economics*, 124, 399–422.

FREDRIKSSON, P. G., L. WANG, AND K. A. MAMUN (2011): "Are politicians office or policy motivated? The case of U.S. governors' environmental policies," *Journal of Environmental Economics and Management*, 62, 241–253.

FUDENBERG, D. AND J. TIROLE (1986): *Dynamic Models of Oligopoly*, Hardwood Academic Publishers.

GELMAN, A. AND G. IMBENS (2019): "Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs," *Journal of Business & Economic Statistics*, 37, 447–456.

GRAY, W. B. AND R. J. SHADBEGIAN (2004): "'Optimal' pollution abatement–whose benefits matter, and how much?" *Journal of Environmental Economics and Management*, 47, 510–534.

GRIMMER, J., E. HERSH, B. FEINSTEIN, AND D. CARPENTER (2011): "Are close elections random?" Mimeo.

GROOMS, K. (2015): "Enforcing the Clean Water Act:The Effect of State-level Corruption on Compliance," *Journal of Environmental Economics and Management*, 73, 50–78.

HART, S. (2005): "Adaptive Heuristics," *Econometrica*, 73, 1401–1430.

HART, S. AND A. MAS-COLELL (2000): "A Simple Adaptive Procedure Leading to Correlated Equilibrium," *Econometrica*, 68, 1127–1150.

HEYES, A. (2002): "A Theory of Filtered Enforcement," *Journal of Environmental Economics and Management*, 43, 34–46.

IMBENS, G. AND K. KALYANARAMAN (2012): "Optimal Bandwidth Choice for the Regression Discontinuity Estimator," *The Review of Economic Studies*, 79, 933–959.

INNES, R. AND A. MITRA (2015): "Parties, Politics and Regulation: Evidence from Clear Air Act Enforcement," *Economic Inquiry*, 53, 522–539.

JENS, C. E. (2017): "Political uncertainty and investment: Causal evidence from U.S. gubernatorial elections," *Journal of Financial Economics*, 124, 563 – 579.

KONISKY, D. M. (2007): "Regulatory Competition and Environmental Enforcement: Is There a Race to the Bottom?" *American Journal of Political Science*, 51, 853–872.

LEE, D. S. (2001): "The Electoral Advantage to Incumbency and Voters' Valuation of Politicians' Experience: A Regression Discontinuity Analysis of Elections to the U.S.." Working Paper 8441, National Bureau of Economic Research.

——— (2008): "Randomized experiments from non-random selection in US House elections," *Journal of Econometrics*, 142, 675–697.

LEE, D. S. AND T. LEMIEUX (2010): "Regression Discontinuity Designs in Economics," *Journal of Economic Literature*, 48, 281–355.

LEE, T. AND L. L. WILDE (1980): "Market Structure and Innovation: A Reformulation," *The Quarterly Journal of Economics*, 94, 429–436.

LEIGH, A. (2008): "Estimating the impact of gubernatorial partisanship on policy settings and economic outcomes: A regression discontinuity approach," *European Journal of Political Economy*, 24, 256 – 268.

List, J. A. and D. M. Sturm (2006): “How Elections Matter: Theory and Evidence from Environmental Policy,” *The Quarterly Journal of Economics*, 121, 1249–1281.

McCrary, J. (2008): “Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test,” *Journal of Econometrics*, 12, 698–714.

Meyer, A. G. (2019): “Elite Influence on Climate Change Skepticism: Evidence from Close Gubernatorial Elections,” *Journal of the Association of Environmental and Resource Economists*, 6, 783–822.

Milgrom, P. and J. Roberts (1990): “Rationalizability, Learning and Equilibrium in Games with Strategic Complementarities,” *Econometrica*, 58, 1255–1277.

Pacca, L., D. Curzi, G. Rausser, and A. Olper (2021): “The Role of Party Affiliation, Lobbying and Electoral Incentives in Decentralized U.S. State Support of the Environment,” *Journal of the Association of Environmental and Resource Economists*, 8, 617 – 653.

Pettersson-Lidbom, P. (2008): “Do Parties Matter For Economic Outcomes? A Regression-discontinuity Approach,” *Journal of the European Economic Association*, 6, 1037–1056.

Raff, Z., A. Meyer, and J. M. Walter (2022): “Political differences in air pollution abatement under the Clean Air Act,” *Journal of Public Economics*, 212, 104688.

Reed, W. (2006): “Democrats, Republicans, and Taxes: Evidence that Political Parties Matter,” *Journal of Public Economics*, 90, 725–750.

Sekhon, J. S. and R. Titiunik (2012): “When Natural Experiments Are Neither Natural nor Experiments,” *American Political Science Review*, 106, 35–57.

Shimshack, J. P. (2014): “Regulator reputation, enforcement, and environmental compliance,” *Annual Review of Resource Economics*, 4, 5339–360.

Stafford, S. (2002): “The Effect of Punishment on Firm Compliance with Hazardous Waste Regulations,” *Journal of Environmental Economics and Management*, 44, 290–308.

Sung, R., D. Strehler, and C. Thorne (1978): “Assessment of the Effects of Chlorinated Seawater from Power Plants on Aquatic Organisms,” Tech. rep., U.S. Environmental Protection Agency – Interagency Energy/Environment R&D Program Report.

Vives, X. (2005): “Complementarities and Games: New Developments,” *Journal of Economic Literature*, 43, 437–479.

A Appendix – List of narrowly-won elections

Table A.1: Elections won by small margin

Democratic win	Republican win
California (2003)	Alabama (1994, 1998)
Connecticut (2014)	Arizona (1994)
Delaware (2008)	California (1986, 1994)
Florida (1998)	Colorado (2002)
Georgia (1998)	Connecticut (1998)
Hawaii (1990, 2002)	Florida (2014)
Illinois (2014)	Illinois (1986, 1994, 2002)
Indiana (2000)	Indiana (2000)
Iowa (2002)	Iowa (1990)
Kentucky (1999)	Kansas (1990)
Louisiana (1999)	Maine (1994)
Maryland (1998)	Massachusetts (1994, 2002)
Minnesota (2014)	Michigan (1994)
Missouri (2004)	Minnesota (1994, 2010)
Mississippi (2003)	Mississippi (1995)
Montana (2008)	Montana (1992, 1996, 2004)
Nebraska (1986, 1994)	Nebraska (1990)
New Hampshire (2002)	New Jersey (1985, 1987, 2001, 2013)
New Jersey (2005)	New York (1998)
New Mexico (1986)	Ohio (2002, 2014)
New York (1986)	Oklahoma (1990)
North Carolina (2004, 2012)	Pennsylvania (1986)
Oregon (1990, 2014)	Rhode Island (1990)
Pennsylvania (1990)	South Carolina (1990, 1998, 2014)
Texas (1994)	South Dakota (1990)
Virginia (1993, 2005)	Utah (1992)
Vermont (1986)	Vermont (1992, 2004)
Washington (1996, 2008)	Wisconsin (2014)
West Virginia (2004, 2012)	West Virginia (2000)

Notes: The table reports the State and the Year in which an election took place, which was won by either party by a margin smaller than 3%. The shaded cells identify states that appear in both columns.

B Online Appendix – Proofs of results

B.1 Proof of Lemma 1

Ad 1. Consider the enforcement agency's minimization problem (4) and notice that the objective function is linear in all arguments $\{\varphi_1, \ldots, \varphi_n\}$. Furthermore, the function is strictly monotonically increasing in φ_i for any $i \in N$ if and only if

$$\kappa(p(N)) > F\,\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right];$$

and it is monotonically decreasing if and only if:

$$\kappa(p(N)) \leq F\,\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right].$$

Given that for all $i \in N$, $\varphi_i \in [0,1]$, we conclude that at the optimum we have a bang-bang[33] solution where for $i \in N$:

$$\varphi_i^*(p_i, p_{-i}) = \begin{cases} 0 & \text{if } \kappa(p(N)) > F\,\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right] \\ 1 & \text{if } \kappa(p(N)) \leq F\,\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right] \end{cases}$$

Ad 2. This follows from the fact that $\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i \leq \hat{p}\right] < \text{Prob}\left[e_j < \hat{e}_j \,|\, p_j > \hat{p}_j\right]$. Indeed, writing out the conditional probabilities we get:

$$\begin{aligned} \text{Prob}\left[e_i \leq \hat{e}_i \,|\, p_i \leq \hat{p}_i\right] &= \text{Prob}\left[g^{-1}(p_i - \epsilon_i) \leq \hat{e}_i \equiv g^{-1}(\hat{p}_i)\right] \\ &= \text{Prob}\left[\epsilon_i \leq p_i - \hat{p}_i\right] && \text{(B.1)} \\ &= \Phi(p_i - \hat{p}_i) \leq 0.5; && \text{(B.2)} \\ \text{Prob}\left[e_j \leq \hat{e}_j \,|\, p_j > \hat{p}_j\right] &= \text{Prob}\left[g^{-1}(p_j - \epsilon_i) \leq \hat{e}_j \equiv g^{-1}(\hat{p}_j)\right] \\ &= \text{Prob}\left[\epsilon_j \leq p_j - \hat{p}_j\right] && \text{(B.3)} \\ &= \Phi(p_j - \hat{p}_j) > 0.5; && \text{(B.4)} \end{aligned}$$

where the equality signs in (B.1) and (B.3) follow from the assumption that $g_e < 0$, while inequality (B.2) follows from $p_i \leq \hat{p}_i$. The expression in (B.4) follows from $p_j > \hat{p}_j$, together with the assumption that the expected value of ϵ_i is 0 for all i.

Ad 3. First, since $p_j < p'_j$, total pollution is smaller under profile $\{p_j, p_{-j}\}$ than under profile $\{p'_j, p_{-j}\}$, as $p_j + p(N\setminus\{j\}) < p'_j + p(N\setminus\{j\})$. Next, by recalling that the auditing cost $\kappa(p(N))$ is an increasing function of total pollution, we know that $\kappa\left(p_j + p(N\setminus\{j\})\right) < \kappa\left(p'_j + p(N\setminus\{j\})\right)$. Therefore auditing firm i is costlier when the set of pollution signals is $\{p'_j, p_{-j}\}$ than under profile $\{p_j, p_{-j}\}$. Since firm i's reported pollution is the same under both profiles, the expected fine as-

---

[33] For completeness, we acknowledge that when $\kappa(p(N)) = F\,\text{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right]$, the agency is indifferent for any $\varphi \in [0,1]$. We choose to adapt the convention that in this situation the agency choose to audit with certainty.

sociated with firm i is also the same. We conclude that, at the optimum, the enforcement agency audits firm i with a weakly lower probability under $\{p'_j, p_{-j}\}$ than under $\{p_j, p_{-j}\}$. $\square$

B.2 Proof of Lemma 2

We follow Milgrom and Roberts (1990) and define the game as supermodular if the payoff functions for all players are upper semi-continuous in the choice variables and exhibit increasing differences in their arguments.

Consider (5). It is straightforward to show that it is upper semi-continuous in e_i and e_{-i} for all $i \in N$. First, the first term e_i is clearly continuous, differentiable, and monotonically increasing. To see that the second term is upper semi-continuous, recall that, by assumption, p_i is a continuous, differentiable and monotonically decreasing function of e_i; $\kappa(p(N))$ is a continuous, differentiable and increasing function of $\{p_1, \ldots, p_n\}$ and therefore a continuous, differentiable and decreasing function of $\{e_1, \ldots, e_n\}$; and that Prob $[e_i < \hat{e}_i \mid p_i]$ is a continuous, differentiable and monotonically decreasing function of e_i. Therefore, $\varphi_i^*(p_i, p_{-i})$ defined in Lemma 1 is also upper-semi continuous. Finally, the term m is a constant and the indicator function $\mathbb{1}_{\{e_i \leq \hat{e}_i\}}$ is upper-semi continuous by definition.

We now need to show that $C_i(e_i, e_{-i})$ exhibits increasing differences in its arguments. For any $i, j \in N$, and any two abatement expenditure profiles $\{e_1, \ldots, e_n\} \equiv \left\{e_i, e_j, e_{-ij}\right\}$ and $\left\{e'_1, \ldots, e'_n\right\} \equiv \left\{e'_i, e'_j, e_{-ij}\right\}$ such that $e_i \geq e'_i$ and $e_j \geq e'_j$ and $e_k = e'_k$ for all $k \in N \setminus \{i, j\}$, the proof entails showing that:[34]

$$C_i(e_i, e_j, e_{-ij}) - C_i(e'_i, e_j, e_{-ij}) \leq C_i(e_i, e'_j, e_{-ij}) - C_i(e'_i, e'_j, e_{-ij}). \tag{B.5}$$

Plugging in the cost function given in (5) and the optimal auditing schedule from Lemma 1 into the expression above, simplifying and rearranging the terms, we get:

$$\left(\varphi^*(e_i, e_j, e_{-ij}) - \varphi^*(e_i, e'_j, e_{-ij})\right)\left(m + F \times \mathbb{1}_{\{e_i < \hat{e}_i\}}\right) \leq \left(\varphi^*(e'_i, e_j, e_{-ij}) - \varphi^*(e'_i, e'_j, e_{-ij})\right)\left(m + F \times \mathbb{1}_{\{e'_i < \hat{e}_i\}}\right).$$

Since $e_i \geq e'_i$, the following inequality holds with respect to the indicator functions,

$$\mathbb{1}_{\{e_i < \hat{e}_i\}} \leq \mathbb{1}_{\{e'_i < \hat{e}_i\}},$$

implying that to establish the supermodularity of the game, we can restrict our attention to the first terms in parentheses on each side of the inequality, and focus on proving that the following inequality holds:

$$\underbrace{\left(\varphi^*(e_i, e_j, e_{-ij}) - \varphi^*(e_i, e'_j, e_{-ij})\right)}_{A} \leq \underbrace{\left(\varphi^*(e'_i, e_j, e_{-ij}) - \varphi^*(e'_i, e'_j, e_{-ij})\right)}_{B}. \tag{B.6}$$

---

[34] We remind the reader that the objective of each firm is to *minimize* $C(e_i, e_{-i})$, which determines the sign of the inequality in (B.5).

In equilibrium, each of the $\varphi^*(\cdot)$ functions in the expressions indicated with A and B above takes on only two possible values, either 0 or 1 – see Lemma 1. It immediately follows that A and B can only take on three possible values in turn: 0, 1, and -1. Table B.1 summarizes the possible combinations of values that may obtain. We discuss each possible case below.

		A		
		-1	0	1
B	-1	✓	I	II
	0	✓	✓	III
	1	✓	✓	✓

Table B.1: Possible combinations of the values of A and B in (B.6).

It is straightforward to see that whenever $A = -1$, the inequality we are trying to prove holds trivially, this is reflected in the tick marks in the first column of Table B.1.

Similarly, the inequality is also immediately satisfied if $A = 0$ and $B > -1$, as well as when $A = 1$ and $B = 1$. This is also shown in Table B.1.

To complete the proof, therefore, we only need to address the less obvious cases at the top right corner of the Table. We start by considering the case marked as I, that emerges when $A = 0$ and $B = -1$.

Ad I. For this case to arise, we must have that $\varphi_i^*(e_i', e_j, e_{-ij}) = 0$ and $\varphi_i^*(e_i', e_j', e_{-ij}) = 1$. From the optimal auditing scheme in (6), we know that these equalities jointly imply:

$$\begin{aligned} \kappa(p_i(e_i'), p_j(e_j), p_{-ij}(e_{-ij})) &> F \operatorname{Prob}\left[e_i' < \hat{e}_i \,|\, p_i\right] \quad \text{and} \\ \kappa(p_i(e_i'), p_j(e_j'), p_{-ij}(e_{-ij})) &\leq F \operatorname{Prob}\left[e_i' < \hat{e}_i \,|\, p_i\right]. \end{aligned}$$

However, since $\kappa(\cdot)$ is an increasing value of reported pollution, it must be the case that

$$\kappa(p_i(e_i'), p_j(e_j), p_{-ij}(e_{-ij})) < \kappa(p_i(e_i'), p_j(e_j'), p_{-ij}(e_{-ij})),$$

which leads to a contradiction with the previous two. We conclude that this case cannot exist.

Ad II. Case II in Table B.1 once again requires $\varphi_i^*(e_i', e_j, e_{-ij}) = 0$ and $\varphi_i^*(e_i', e_j', e_{-ij}) = 1$, which we have ruled out above.

We are now left with the case where $A = 1$ and $B = 0$, to which we now turn.

Ad III. This case arises when $\varphi_i^*(e_i, e_j, e_{-ij}) = 1$, $\varphi_i^*(e_i, e_j', e_{-ij}) = 0$, and either $\varphi_i^*(e_i', e_j, e_{-ij}) = \varphi_i^*(e_i', e_j', e_{-ij}) = 0$ or $\varphi_i^*(e_i', e_j, e_{-ij}) = \varphi_i^*(e_i', e_j', e_{-ij}) = 1$.

Start with the first subcase and note that $\varphi_i^*(e_i, e_j, e_{-ij}) = 1$ and $\varphi_i^*(e_i', e_j, e_{-ij}) = 0$ jointly imply:

$$\begin{aligned} \kappa(p_i(e_i), p_j(e_j), p_{-ij}(e_{-ij})) &\leq F \operatorname{Prob}\left[e_i < \hat{e}_i \,|\, p_i\right] \quad \text{and} \\ \kappa(p_i(e_i'), p_j(e_j), p_{-ij}(e_{-ij})) &> F \operatorname{Prob}\left[e_i' < \hat{e}_i \,|\, p_i\right]. \end{aligned}$$

These two inequalities violate condition (8) and therefore this subcase case must be ruled out.

The final sub-case is therefore the one where $\varphi_i^*(e_i, e_j, e_{-ij}) = 1$ and $\varphi_i^*(e_i, e_j', e_{-ij}) = 0$, while $\varphi_i^*(e_i', e_j, e_{-ij}) = \varphi_i^*(e_i', e_j', e_{-ij}) = 0$. These combinations imply both

$$\kappa(p_i(e_i), p_j(e_j), p_{-ij}) \leq F \text{ Prob}\left[e_i < \hat{e}_i \mid p_i\right], \text{ and, } \kappa(p_i(e_i), p_j(e_j'), p_{-ij}) > F \text{ Prob}\left[e_i < \hat{e}_i \mid p_i\right],$$

and

$$\kappa(p_i(e_i'), p_j(e_j), p_{-ij}) \leq F \text{ Prob}\left[e_i' < \hat{e}_i \mid p_i\right], \text{ and, } \kappa(p_i(e_i'), p_j(e_j'), p_{-ij}) \leq F \text{ Prob}\left[e_i' < \hat{e}_i \mid p_i\right].$$

The latter expressions directly violate the internal consistency condition set out in equation (7) and the text below it, and this case can be ruled out. □