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CESIFO WORKING PAPER NO. 6127
CATEGORY 9: RESOURCE AND ENVIRONMENT ECONOMICS
OCTOBER 2016

ISSN 2364-1428

CESifo
Center for Economic Studies & Ifo Institute

CESifo *Working Paper No. 6127*

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Abstract

Researchers have utilized the fact that many states have term limits (as opposed to being eligible for re-election) for governors to determine how changes in electoral incentives alter state regulatory agency behavior. This paper asks whether these impacts spill over into private sector decision-making. Using data from gubernatorial elections in the U.S., we find strong evidence that power plants spend less in water pollution abatement if the governor of the state where the plant is located is a term-limited democrat. We show that this evidence is consistent with compliance cost minimization by power plants reacting to changes in the regulatory enforcement. Finally, we show that the decrease in spending has environmental impacts as it leads to increased pollution.

JEL-Codes: H320, H760, Q250, Q530, Q580.

Keywords: political economy, electoral incentives, term limits, environmental policy, pollution abatement, compliance costs, power plants, water pollution, regression discontinuity.

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September 30th, 2016
We would like to thank Harrison Fell, Ed Balistreri, Pete Manilo., Mirko Moro, seminar participants at the University of East Anglia, Oxford Centre for the Analysis of Resource Rich Resource Economies, University of Stirling, Colorado School of Mines, and conference participants at SURED 2016 for their helpful comments.

1 Introduction

Two-party elections provide a rich set of data to test theories about the impact of politicians on a number of relevant economic outcomes. Using U.S. gubernatorial elections data Besley and Case (1995), for example, have shown that democratic governors have an impact on income taxation and state spending, while Leigh (2008) finds significant gubernatorial partisan impacts on the minimum wage, post-tax income inequality and the rate of unemployment. Within the field of environmental economics, an emerging literature suggests that governors' electoral incentives have a significant impact on the budgets of environmental agencies (e.g. List and Sturm, 2006; Fredriksson et al., 2011).

In this paper we take the logical next step and investigate whether potential differences in environmental policies between democrat and republican governors spill over to the private sector, for example whether they affect the environmental practices of firms. Specifically, we study whether the governor's party affiliation has a causal impact on the amount of pollution abatement spending undertaken by steam electric power plants (SEPPs) in the U.S.

To identify the causal effect of the governor's party affiliation, a regression discontinuity design (RDD) is used, where the discontinuity comes from the fact that 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, since politicians cannot precisely manipulate voters' behavior. In this situation, causal inference may be directly drawn from the observed differences between outcome variables across the treated and control groups.[1]

The large environmental footprint from power generation makes for an interesting and salient case study.[2] Using data on water pollution abatement spending between 1985 and 2005, we find that for elections where the winner is re-electable, the winner's party makes no difference to the amount plants spend on pollution abatement during the governor's term. When the winner is term-limited, however, a divergence in pollution abatement spending occurs. These results are consistent with existing empirical findings that the governor's re-electable status is a key determinant of the impacts (Fredriksson et al., 2011).

We argue that these results are consistent with a model of compliance cost minimization, which suggests that SEPPs do in fact respond to the change in the governors' behavior. We take the predictions of this model to the data and show that the impact of party and re-election status on abatement spending persists even after controlling for number of inspections and whether the state, rather than

[1] Notice that the use of a regression discontinuity framework is appropriate in this context because the re-election status and party of a particular state's governor is unlikely to be correlated with variables related to pollution abatement decisions. See Angrist and Pischke (2009); Imbens and Wooldridge (2009); Lee and Lemieux (2010) for comprehensive discussions of the use of Regression Discontinuity methods in Economics.

[2] The electricity sector withdraws more freshwater than any other sector in the U.S.

the federal, government is authorized to carry out those inspections. We do find that more inspections lead to more spending on water pollution abatement. Furthermore, the evidence suggests that the increase in spending is larger under a democratic governor, and when the regulatory enforcement is delegated to the state. Conversely, we find that inspections are less effective in increasing water pollution abatement spending when the governor is term-limited.

Finally, we analyze whether less spending in pollution abatement actually leads to more pollution. The results of this final empirical effort show that decreases in pollution abatement spending leads to higher chlorine levels, but does not seem to have a significant effect on thermal pollution. Under a term-limited democratic governor, however, thermal pollution increases as a result of a drop in the number of inspections.

Our work is related to the emerging literature on electoral incentives and environmental spending. List and Sturm (2006) use an interaction model and find a significant negative effect on state environmental agency budgets for lame duck (i.e., term-limited) governors located in *green* states. They argue that the governors in *green* states looking for re-election are more likely to favor environmental agency budgets in order to illicit votes from their *green* voters, but during a governor's lame duck term she or he will revert to pleasing the base constituents. Fredriksson et al. (2011) use an RD to investigate how state environmental agency budgets varies depending on the political affiliation of the governor. They show that the average republican governor tends to expand the state's environmental agency budget more rapidly than their democratic counterpart, but only in periods where she cannot be re-elected. Outside of this term-limited period, there is no statistical difference between the growth rate of environmental agency budgets under democrats and republicans. We contribute to this literature in two ways. On the one hand, we provide causal evidence that also the enforcement efforts made by regulatory agencies are affected by the party affiliation of the governor. In particular, we show that in states where the governor is a term-limited democrat, the number of inspections is lower than in a comparable state with a republican governor. Our main contribution to this literature, however, is to convincingly show that the impacts of the governor's party affiliation and eligibility status spill over onto the private sector's decision making.

Our investigation also contributes to the literature on party affiliation and environmental outcomes and provides additional insights into the mechanism through which the enforcement of environmental regulations impacts the levels of pollution (Earnhart, 2004). Beland and Boucher (2015), for example, shows, within an RD framework, that air pollution tends to be lower under democratic governors. Moreover, Hanna and Oliva (2010) estimate that inspections lead to a 15% reduction in a plant's level of air pollution and that inspections are more likely to happen under democratic governors. We complement these papers by looking at water pollution rather than air. Our results suggest that, following tight elections, both the number of inspections and the level of water pollution abatement spending are lower under a term-limited democratic governor. We are also able to link both type of changes to increase in water pollution. Additionally, our results suggest that not only the quantity but also the quality of inspections matter. For example, we find that the impact of inspections on SEPPs' abatement spending is heightened under a democratic governor, but muted when a term-limited governor is in office. Another factor in determining the effectiveness of inspections on pollution levels is whether the state has the authority to administer the Clean Water Act (CWA). Grooms (2015) shows that polluters are less likely to be found in violation of the CWA after the state takes over its administration, if that state is considered 'corrupt'. While not looking at corruption explicitly, our analysis shows a negative correlation between SEPP abatement spending and a state's authorization to enforce the CWA.

The rest of the paper proceeds as follows, Section 2 gives a brief overview of the issues, whereas Section 3 describes the methodology, the data, and presents the result of our RD analysis of pollution abatement across treatments. Section 4 delves into the mechanisms behind the results so far. There we first introduce a conceptual framework and then take it to the data. In Section 5 we trace out the implications of our analysis in terms of environmental outcomes. Finally, Section 6 discusses the implications of our analysis and concludes.

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

Conceptually, the basic operation of SEPPs is 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 condensed in a condenser and discharged into the environment. SEPPs thus withdraw large quantities of water and subsequently release them, normally at higher temperature. Both the water withdrawal and the discharge of 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.[3]

The negative environmental impact of individual SEPPs crucially depends on the specificities of the power plant design and, to a lesser extent, on the way in which they are 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 body of water, diverted through a condenser, and then discharged back into the water at higher temperatures are the most damaging for the environment. Closed-cycle recirculating cooling systems and dry cooling ones are more modern and required as part of New Source Performance Standards in the CWA, which means that older SEPPs have not installed them.

[3] 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.

While thermal pollution is mostly a function of past and current investment, other types of environmental impacts are more directly related to the day-to-day operation of the power plant and specifically of the level of care taken to maintain the plant's operating conditions. SEPPs routinely add chlorine or other toxic chemicals to their cooling water to decrease algal growth in heat exchangers, for example. Since these chemicals eventually find their way into the natural environment with considerable environmental damages, better maintenance, which reduces the need for such treatment, also alleviates the environmental impact of the SEPP's operations.

Due to their considerable potential for environmental degradation, SEPPs are generally heavily regulated. In the U.S., SEEPs are regulated according to the CWA and are subject to effluent limitations via discharge permits issued under the National Pollutant Discharge Elimination System (NPDES). According to the U.S. Environmental Protection Agency "Effluent limitations guidelines and standards are established by EPA for different non-municipal (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."[4]

Permits generally require the facility to sample its discharges 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 EPA or an authorized state agency 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 regulator may issue administrative orders which require facilities to correct violations and assess monetary penalties. The EPA is also allowed by the 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."[5]

It is interesting to note that, while the permitting process remains by and large a federal prerogative, the program enforcement has over time been delegated by the EPA to state authorities via the NPDES State Program Authorization process. States are thus in a position to influence the degree to which environmental standards set under the CWA are enforced. Correspondingly, state politicians, in particular participants in gubernatorial elections might be tempted to use this type of secondary policy to influence their electoral prospects (e.g. List and Sturm, 2006).

3 Pollution abatement spending and gubernatorial elections

In this section we start our investigation into the impact that the political affiliation of the elected governor has on the environmental behavior of firms. We capture the causal impact of the party al-

[4]See https://www.epa.gov/npdes/npdes-permit-limits, last accessed August 31, 2016.
[5]See https://www.epa.gov/npdes/npdes-frequent-questions, last accessed August 31, 2016.

legiance of governors on water pollution abatement spending by SEPPs using a RD design, following Lee (2008). Our identification strategy relies on the imprecise control exerted over the assignment variable (i.e. the vote share) for politicians, thereby removing the potential endogeneity of elections resulting from unobserved characteristics of states and candidates (e.g. Lee and Lemieux, 2010).

In our analysis $Y_i(0)$ and $Y_i(1)$ represent the potential outcomes in terms of pollution abatement spending for plant i, depending on its assignment to the control or treated group, respectively. As elsewhere in the literature, assignment status is 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. SEPPs are assigned 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. Our main estimation uses the robust non-parametric approach introduced by Calonico et al. (2014) to estimate the average treatment effect at the threshold, τ, given by:

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

For completeness, we also specify and estimate a parametric regression discontinuity model:

$$Y_{it} = \alpha_i + \beta_1 D_{it} + F(\widetilde{V}_{it}) + \epsilon_{it}, \tag{2}$$

where Y_{it} represents the water pollution abatement measure of interest at SEPP i and at time t, D_{it} is a dummy variable that takes a value of one if a democratic governor is in power at time t in the state where SEPP i operates, and $F(\widetilde{V}_{it})$ is a polynomial function of the normalized vote share. Mindful of the advice of Gelman and Imbens (2016), we estimate the party effect, β_1, controlling for the margin of victory $\widetilde{V}$ using a linear and a quadratic specification of the function $F(\widetilde{V}_{it})$.

3.1 Data

The analyses described above use the EIA form 767, an annual-plant level panel dataset covering the years 1985-2005. Given that our treatment is about the party and re-election status of the governor, in this section and the next we aggregate the data up to election cycles. These data contain both plant level investment and current expenditure in dollars on water pollution abatement.

The dependent variable for the estimation of (1) and (2) is the sum of the amount spent on water pollution abatement capital and the current expenditure on water abatement. The instructions for the former entry in the EIA-767 read:

> *Report new structures and or equipment purchased to reduce, monitor, or eliminate water-borne pollutants, including chlorine, phosphates, acids, bases, hydrocarbons, sewage, and*

other pollutants. Examples include structures/equipment used to treat thermal pollution; cooling, boiler, and cooling tower blowdown water; coal pile runoff; and fly ash waste water.

For 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).

Governor party affiliation, vote margin, state legislative party shares, and state term limit legislation is all publicly available.[6] Gubernatorial elections occur every four years with the exception of New Hampshire and Vermont, which have only two year terms, and Virginia, which is 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...). Others hold elections on a different cycle of even number years (e.g., 1990, 1994, 1998...) and six states hold elections during odd number years.[7] Thirty-six states have term limit legislation for their governors and these are geographically spread across the U.S., while Virginia is the only state that limits its governor to a single term. Figure 1 shows which states do and do not have term limit legislation. Summary statistics are given in Table 1.

As discussed, for example, by Lee (2008) and Lee and Lemieux (2010), the opportunity for causal identification of the impact of governor's party and re-election status on SEPP abatement spending comes from the sharp discontinuity that occurs when one party earns just over 50% of the vote, i.e. they win the office. As the vote margin gets closer to 50% from either side, it implies an electorate with similar viewpoints. Additionally, there is both spatial and temporal variation in the party and re-election status of each state's governor (the fact that Connecticut may have a re-electable democratic governor is uncorrelated with the status and party of Oregon's governor). Furthermore, there is variation as to when states hold elections for governor (many states are on one set of even years, some on a different set of even years, and others on odd years) so that a trend in election outcomes for one election cycle does not impact all of the governors.

When using an RD methodology, it is necessary to show that there is continuity in the running variable (vote share). This ensures that the discontinuity point is essentially random and that plants are not able to manipulate which side of discontinuity they fall on. In our sample, plants are fixed

[6]We thank Le Wang for providing the election data from their paper (Fredriksson et al., 2011).

[7]NJ and VA (1985,1989,...) and CA,KY,LA, and MS (1987,1991...). CA only joined the list of states with elections in odd years in 2003, when the state held a recall vote.

in location and cannot change states. Thus we test for whether the party which wins the election shows evidence of sorting using the McCrary (2008) test with standard errors corrections suggested by Cattaneo et al. (2016). The results are given in Table 2. We find no statistical evidence of sorting. Additionally, one might wonder whether certain states always fall on one side of the discontinuity. The states where a democratic governor won their term-limited term by 1% or less are CO, FL, GA, KS, MD, MO, MS, NC, and NV and for republican governors the states are CA, IN, ME, MS, NC, NM, OR, PA, and WY. Among the list, two states overlap (NC and MS) and generally they are states that often have competitive elections (CO, ME, MD, FL, NC, NM, NV and PA). The results of the McCrary (2008) test and list of states near the cutoff imply that their is little reason to be concerned about a bias due to sorting and we can think of these close elections as essentially random.[8]

There may also be concerns that the change in water pollution abatement spending as a result of the party and re-election status of the governor could be instead driven by other observable variables. A balance of covariates test is often performed to ensure that other observables are not changing with the discontinuity. In this case, our parametric models include a plant fixed effect to control for time invariant differences in SEPP (coal vs gas, presence of a scrubber, etc). The amount of electricity generated by each SEPP is an observable, time-varying metric thus it was tested for discontinuities at the cutoff value. For term-limited governors, the estimate of the change in generation at the discontinuity point had a p-value of 0.21; for re-electable governors, the p-value was 0.87. The lack of statistical difference in generation is further assurance that the cutoff point is essentially random.

3.2 Results

Table 3 reports the results of our estimation of the average effect of the treatment on the treated at the discontinuity according to Equation (1). As mentioned above, we follow the methodology suggested by Calonico et al. (2014) to correct for potential biases in the estimation of the coefficient and the standard errors.[9] The estimate performed on the entire sample seems to suggest that SEPPs in the U.S. do not approach their environmental spending differently, depending on the party affiliation of the Governor in the state they operate.

A growing literature exists, however, that emphasizes that governors in the U.S. exhibit significant differences in behavior when they face the possibility of re-election in the following term relative to when they are, instead, serving their last allowable term (List and Sturm, 2006; Fredriksson et al., 2011). List and Sturm (2006), for example, emphasize that re-electable politicians in 'green' states have an incentive to bias their secondary policy – e.g. by boosting the state's environmental budget – to establish their environmental credentials in the hope to woe environmentally minded voters

[8] A convincing argument in favour of the appropriateness of an RD design in this context was already made by Fredriksson et al. (2011), in one of the seminal papers in this literature.

[9] The estimations were run in STATA using the rdrobust package, see Calonico et al. (2016).

into voting for them in the upcoming elections.[10] Such incentives, however, do not arise for term-limited, 'lame duck' politicians who cannot seek re-election. Term-limited politicians are therefore more likely to implement policies in line with their own preferences. Since similar incentives face all politicians, irrespective of their party affiliation, List and Sturm (2006) conclude that pooling equilibria, whereby democrats and republicans pursue similar policies during re-electable periods, are a likely outcome of the electoral competition.[11] These insights, which have been shown to hold empirical relevance for governors in the U.S. (List and Sturm, 2006; Fredriksson et al., 2011) lead us to conjecture that plants operating in states with term-limited governors would face different incentives relative to plants located in states with re-electable governors. Based on this, we re-estimate (1) separately on the two subsets of plants facing term-limited governors and on the complementary set of plants in state with re-electable governors. As shown in Table 3 the results confirm that the treatment has a significant (causal) impact only for plants facing term-limited governors. Among such SEPPs, those operating under a democrat seem to spend less on water pollution abatement, relative to their counterparts facing a term-limited republican.

Figure 2 provides a graphical representation of the effect of the winning candidate's vote margin on the amount of pollution abatement spending by power plants during the term. The x-axis shows the democratic vote share where a zero or above means the democrat won and a negative democratic vote share means the republican won. The two panels of Figure 2 provide a visual illustration of the results discussed above. They suggest that the difference among treated and not treated plants in the term-limited context arise from a change in behavior that emerge under republican governors. Indeed, the levels of expenditures incurred by plants operating in states run by a democrat seem rather similar across the two pictures.

Table 4 reports the results of the estimation of the model in (2) where the function $F(\widetilde{V})$ is first specified as linear and subsequently as quadratic. Additionally, the table reports the estimation of an additional model where, besides the treatment dummy, D_{it}, another dummy, LD_{it}, is included that indicates whether the governor is term-limited. The model also includes an interaction term between the two, which provides temporal and spatial variation that makes it unlikely that our treatment is correlated with unobservables. Formally, the interaction model we estimate is:

$$Y_{it} = \alpha_0 + \alpha_1 D_{it} + \alpha_2 LD_{it} + \alpha_3 D_{it} \times LD_{it} + F(\widetilde{V}_{it}) + \varepsilon_{it}. \quad (3)$$

Each model is estimated using two different bandwidths, as an additional check of the robustness of the results. The estimates presented in Table 4 paint an interesting picture that in part confirms and in part complements the results obtained above.

[10] Symmetrically, in 'brown' states the opposite happens and politicians across the political spectrum are tempted to set low environmental budgets.

[11] List and Sturm (2006) show that such outcomes are more likely to occur in tightly contested elections taking place in states with sizable minorities of voters with polarized beliefs.

Focussing first on the estimates obtained using the 7.5 percentage point bandwidth, the results confirm that when governors are term-limited there is a large, statistically significant and negative effect of the treatment on the water pollution abatement spending of SEPPs. The effect of the treatment, however, vanishes when SEPPs are faced with a re-electable governor.

The results obtained doubling the bandwidth, however, paint a somewhat different picture. While there is still no evidence of a treatment effect in the case of power plants operating under a re-electable governor, it is interesting to notice that the sign of the estimate for the coefficient of the treatment dummy, α_1, switches from negative to positive. This suggests that SEPPs that operate in states where democrats are likely to win by a large margin spend more on pollution abatement than their counterparts in states that experience republican landslides.

4 Exploring the mechanisms

Our analysis so far suggests that SEPPs behave differently when facing politicians of different political orientation, who are not eligible for re-election. Up to this point, however, we have provided little, if anything, in the way of a conceptual framework that might shed light on the mechanisms at play. In this section we put forward such a framework and present suggestive empirical evidence to support it.

In the context of water pollution abatement spending by SEPPs, it is natural to cast our thinking within a regulatory compliance framework (see Gray and Shimshack, 2011, for a discussion). Firms face an environmental regulator who mandates water pollution concentration limits or other ambient standards. The regulator also decides on the level of effort, e, exerted in enforcing these rules. If firms are found to be in violation of the environmental regulations they face a cost in the form of a pecuniary fine, χ.

Our firms are assumed to make their generation decisions independently from the environmental regulation and to subsequently choose their level of abatement to minimize their compliance costs. This assumption is realistic within the context of water pollution regulation of large SEPPs for whom water pollution abatement costs represent just a small fraction of total costs. For our purposes here, the firms' only relevant decision is thus their choice of abatement effort. Each firm optimally chooses its abatement effort (empirically proxied by their water pollution abatement spending) in order to minimize its compliance costs. We let a indicate this abatement level and $c(\cdot)$ be the monetary cost associated with each level of abatement effort. The effort cost function is assumed to be strictly increasing and convex, i.e. we let $c_a > 0$ and $c_{aa} > 0$. The probability of being found compliant with the regulations, π, depends positively on the firms abatement effort, a, but is a decreasing function of the enforcement efforts on the part of the regulator, e. We also assume that the marginal returns to abatement are decreasing with abatement itself, but increasing in the effort invested by the regulator, i.e. we let $\pi_{aa} < 0$ and $\pi_{ae} > 0$. As mentioned before, if found non-compliant the firm suffers a financial penalty equal to χ. Formally, the model reads

$$\min_{a} \; c(a) + \Big(1 - \pi(a, e)\Big)\chi. \tag{4}$$

The first order necessary and sufficient condition is then simply,

$$c_a(a) - \pi_a(a, e)\chi = 0,$$

which implicitly define the optimal level of effort as a function of regulatory enforcement, $a^*(e)$.

Totally differentiating the first-order condition above and rearranging gives:

$$\frac{\mathrm{d}a^*}{\mathrm{d}e} = \frac{\pi_{ae}\chi}{c_{aa} - \pi_{aa}\chi} > 0.$$

This expressions shows how from the point of view of the firms, an increase in enforcement induces an increase in abatement. This leads naturally to our first testable implication, i.e.

Hypothesis 1. *Pollution abatement effort should be positively correlated with the enforcement effort exerted by the regulating agency.*

One of the key insights that emerges from the work of List and Sturm (2006) is that politicians facing a tightly contested re-election campaign have incentives to behave similarly – irrespective of their ideology – since they try to tailor their secondary policies to suit the demands of key electoral groups. It follows that we would not expect to detect much difference in the policies set by politicians of different parties in re-electable periods. For example, we would not expect there to be much difference between the level of enforcement taking place under a republican and a democrat re-electable governor in hard-fought states. As a consequence, according to our model above, we would not expect a treatment effect to emerge during a period in which the governor may be re-elected. Based on List and Sturm (2006), however, term-limited politicians should instead introduce policies that better reflect their own political convictions. Thus, if there exists a systematic difference in attitudes to the environment between republicans and democrats, we would expect a significant divergence of policies between the two types in term-limited periods. These observations lead us to the second empirically relevant implication of our conceptual framework, namely that

Hypothesis 2. *In as far as politicians differ in their environmental preferences, we would expect a treatment effect to only emerge in term-limited periods.*

Not all pollution abatement spending, however, can be simply assumed to affect the probability of detection within one electoral cycle. In particular, one might want to allow for the possibility that abatement spending takes the form of investment and affects the probability of detection with a lag. In the starkest case, one can assume that environmental investment only affects the future probability of detection. Since the probability of being found compliant with the regulation also depends on the level of enforcement spending in the future, $\mathbb{E}[i_{+1}]$, the minimization problem in (4) can be rewritten as:

$$\min_{e} \; c(e) + \Big(1 - \pi(e_{-1}, i)\Big)\chi + \beta\Big[c(e_{+1}) + \Big(1 - \pi\Big(e, \mathbb{E}[i_{+1}]\Big)\Big)\,\chi\Big].$$

The associated first-order condition is now

$$c_e - \beta\pi_e(e, \mathbb{E}[i_{+1}])\chi = 0,$$

which allows us to study the comparative statics with respect to the expected level of enforcement and obtain

$$\frac{\mathrm{d}e^*}{\mathrm{d}\mathbb{E}[i_{+1}]} = \frac{\pi_{e\mathbb{E}[i_{+1}]}\chi}{c_e e - \pi_{ee}\chi} > 0.$$

Transparently, in this case it is the expectation of tighter enforcement standards in the future that increase the effort level by firms.

In contrast to the previous case in this context observing the political affiliation of a term-limited governor does not provide useful information about future policies: any newly elected governor that aims to be re-elected is likely to implement a similar environmental platform. Conversely, knowing the political affiliation of a governor and observing their policy allows firms to update their beliefs on future policies when the current governor has a positive probability of re-election (see List and Sturm, 2006). It follows that when spending affects the probability of detection with a lag, a treatment effect would only emerge under re-electable governors. Also in this case, however, the emergence of a treatment effect depends on the existence of systematic differences in policy between politicians of different parties. Seen in this light, our empirical test of Hypothesis 2 can also be interpreted as a test of the existence of differences in environmental preferences between republican and democratic governors.

Notice that the key feature in our discussion here is that firms adjust their pollution abatement in response to the enforcement efforts exerted by environmental regulators. The latter, however, adjust their *modus operandi* based on the policies pressed upon them by the political authorities in charge. Thus, the predictable pattern in the policies implemented by governors is predicated on their having both the incentives and the opportunity to influence the behavior of the environmental regulator. Should the enforcement effort of the regulatory agency be outside of the control of the governor, the mechanism behind Hypothesis 2 would need not exist. In this case, neither re-electable nor term-limited politicians would be able to influence the behavior of the environmental agency and would thus not behave differently in their final term in office that they do in previous ones. This observation leads us to the final prediction we take to the data:

Hypothesis 3. *When the enforcement effort is outside of the control of the political authorities, there should be no systematic difference between the behavior of firms in re-electable vs. term-limited periods.*

4.1 Empirical implementation

Our empirical strategy to test the hypotheses derived above revolves around a series of regressions aimed at explaining the pollution water spending undertaken by SEPPs as a function of the political environment, the enforcement efforts made by the environmental regulatory agency, and a number of controls. In what follows, we estimate:

$$Y_{it} = \beta_0 + \beta_1 D_{it} + \beta_2 LD_{it} + \beta_3 D_{it} \times LD_{it} + \mathbf{Z}'\gamma + \mathbf{X}'\delta + \phi_i + \varepsilon_{it}. \quad (5)$$

As before, Y_{it} is (the logarithm of) water pollution abatement spending by plant i and time t, D_{it} and LD_{it} are the treatment and term-limited dummies that take a value of one if a democratic governor is in power at time t in the state where plant i operates, and whether the governor is serving his last allowable term, respectively.

The vector $\mathbf{Z}$ is shorthand for all the variables we use to capture the enforcement effort in the state where plant i operates, at time t. In what follows, our main proxy for the intensity of enforcement is given by the number of inspections undertaken within the framework of the CWA by the authority in charge of compliance in each state per year.[12] As discussed in Section 2 above, the responsibility to verify compliance with the CWA mandates at plant level rests in principle with the federal EPA. Over time, however, such verification activities have been progressively delegated to state agencies within the framework of the NPDES program. In what follows, we use a dummy variable, NPDES, that takes the value of one in each state that has received the necessary authorization to perform inspections, after the authorization has taken place. This variable is built using the information found in Grooms (2015).

Our empirical specification also accounts for observable heterogeneity across states using a vector, $\mathbf{X}$, of control variables. Three dummy variables are used to indicate if the plant is located in a state that (1) has a restructured electricity market, (2) participates in the Regional Greenhouse Gas Initiative (RGGI), and (3) participates in a NO_x trading program. Other controls are the share variable ranging from zero to one, showing the portion of the election cycle that the majority party of the state legislative branch is the same as the governors political party (e.g. 1 = same party throughout the governors term, 0 = split or opposing party throughout the governors term, .5 = same party for

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

half of the governor's term) and the logged share of Sierra Club membership to state population.[13] These two variables account for the ease of passing state regulatory legislation and as a control for the environmental conscience of the states' constituents. To further control for the unobservable heterogeneity at the plant level, we include plant fixed effects in all our regressions. Following Bertrand et al. (2004), moreover, we cluster the standard errors at the level of the treatment, in our case the state.

Finally, to retain as much as possible the flavor of the RDs from Section 3, and the opportunity if offers to interpret the results in a causal manner, 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 has been won by either party with a majority of more than 5 percentage points.

4.2 Results

Our analysis in this section hinges on the assumption that the level of regulatory enforcement is affected by the party affiliation of the winner of the electoral competition. Before delving in our empirical estimation of the model in (5), we thus find it necessary to verify whether this is indeed the case. Using the information at our disposal on the number of inspection per state in any given year, we conduct a non-parametric RD estimation similar to the one discussed in Section 3 above. Table 5 reports on these estimations. Similarly to our previous results, the analysis suggests that, following closely contested elections, the number of inspections at the state level is lower if the winner is a term-limited democrat. The data show that no such discrepancy emerges under re-electable governors.

This evidence provides support to our conceptual framework, in that it suggests that there is a predictable pattern to the change of enforcement efforts across states with different party majority and re-electable status of their governors. With this point made, we now move to the second part of our empirical analysis.

Table 6 reports the results of our estimations of equation (5). We start with a simple model that does not control for the level of enforcement in the states. The estimates in column (1) are consistent with the results of the RD estimations in the previous section and provide empirical support for our Hypothesis 2 above. Indeed, it clearly emerges from the tests reported at the bottom of the table that a statistically significant treatment effect (TE) only emerges in periods where the elected governor is term-limited. During these periods SEPPs operating in states with a democratic governor are shown to be spending less on water pollution abatement than their counterparts in states where a republican holds the governor post. We test for changes in spending by plants within republican and

[13] Thank you to Daniel Kaffine for providing the data to us.

democratic states across re-electable and term-limited terms, as an indirect test that governors react to electoral incentives (EI in the table). As can be evinced from the significance of the tests and the sign of the coefficients, our estimates suggest that the discrepancy between spending in democratic and republican states emerges as the result of a clear increase in spending by plants under republicans and a parallel, albeit less clear-cut, drop experienced by plants under democratic governors. The results of the tests at the bottom of the table further suggests that these patterns are very robust to changes in the regression set-up. These findings support the electoral incentives mechanism proposed by List and Sturm (2006) and lend credence to our conceptual framework above, especially in its one-period variant. Additionally, it provides suggestive evidence of systematic differences in environmental preferences across the political spectrum.

We next tackle our main question, i.e. we test the channels through which differences in policy spill over into private sector behavior. In column (2) we present the estimates of a model that includes the (logarithm of) the number of inspections carried out in the state where the power plant is located in any given year. We interpret the number of inspections as a proxy for the 'quantity' of enforcement activities. The results accord with our theoretical priors formalized in Hypothesis 1, as an increase in the number of inspections is associated with an increase in spending in water pollution abatement. It is interesting to note that the elasticity of abatement spending with respect to the number of inspections is close to one and remarkably stable across specifications.[14]

The following two columns, (3) and (4), address the question whether inspections might affect spending differently in term-limited terms and under a democratic governor, respectively. The question is aimed at understanding whether controlling for the quantitative aspect, namely whether more or fewer inspections make a difference, there might be an additional qualitative side to the issues. One can conjecture, for example, that the state governor has the means to pressurize the state enforcement agency into instructing its inspectors to be tougher against violations, while keeping the number of inspections unchanged. Conversely, a larger budget devoted to enforcement, might not lead to the hoped for compliance benefits if the inspectors may be swayed to be more lenient in prosecuting violations. The evidence suggests, that at least to a certain extent, the quality of the inspections matters. Column (3) shows, for example, that inspections performed when a governor is in their final term in office are less effective than inspections carried out during terms in which the incumbent may seek re-election. No such pattern seems to emerge, *prima facie*, when looking at the interaction between the number of inspections and the democrat dummy. It is worth noting, however, that the p-value of the coefficient of the interaction term log(Inspections) Democrat is only 0.11, and that its sign suggests that inspections carried out under democratic governors are indeed more effective in spurring pollution abatement efforts than those under a republican. To

[14] Across all the specifications in columns (2)-(6), we cannot reject the null hypothesis that the coefficient on ln(inspections) is equal to one.

summarize, our evidence so far suggests that the mechanisms proposed in our theoretical model and the implications formalized in both Hypothesis 1 and 2 are borne out in the data.

Next, we turn our attention to our final hypothesis and augment our regression model to include a dummy variable, NPDES, which identifies all plants operating in states that have been authorized to perform inspections within the framework of the CWA. Column (5) presents an extension of the regression in column (2), which controls for the role of the authorization status and the interaction with the quantity of inspections. Despite the difficulty of individually identifying the effect, due to the high correlation between NPDES and the interaction effect, one can conclude that the authorization status is a relevant dimension, as the two coefficients are jointly significant.[15] The negativity of the sign of the NPDES dummy might indicate that states with low level of pollution abatement self-select into the NPDES pool, and the coefficient of the interaction term suggests that direct enforcement by the state is more conducive to pollution abatement than enforcement by the federal EPA.

According to Hypothesis 3, enforcement activities should respond to electoral incentives only in situations where the governor has a direct influence over the enforcement of environmental regulation. As a consequence, significant differences in treatment effects by re-election status should only emerge when the NPDES dummy is equal to one. The results in column (6) confirm this hypothesis, as they show that controlling for NPDES authorization status, significant treatment effects can be identified in states without NPDES authorization both under term-limited and re-electable governors.

5 Environmental outcomes

Our analysis so far has shown that across U.S. states, SEPPs tend to spend relatively less on water pollution abatement when they operate in a state governed by a democrat in their last term of office. We have also shown that a conceptual framework that emphasizes the role of pollution regulation enforcement and compliance cost minimization leads to theoretical implications that resonates with the data. At this stage, we want to conclude our discussion by looking at what consequences, if any, these differences in both enforcement and 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 level of two key water pollutants, thermal pollution and chlorine, are regressed on the SEPP's power generation rate, which obviously correlates with water use and the degree to which the heat exchangers are used, water pollution abatement spending, and the number of inspections in the state in any given year.

[15] The value of the F-test of the joint significance hypothesis is 3.14, with a p-value of 0.05.

Thermal pollution, 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 the year in thousands of pounds. These pollution data are available from the EIA form 767. In this context, we use yearly data and control for year fixed effects by including year dummies in all specifications. To limit concerns over potential endogeneity, we use the lagged value for both abatement spending and inspections. Given the significant degree of non-linearity displayed by the data, all explanatory variables enter as second degree polynomials.

5.1 Results

Table 7 shows the outcome of the estimations described above. The first two columns of the table show that water pollution abatement spending has no significant impact on thermal pollution, whereas lagged inspections and their squared value are statistically significant and have the theoretically correct signs: an increase in enforcement leads to a reduction in thermal pollution. The marginal effect of additional inspections is, however, decreasing as one would expect given that low hanging fruit is progressively exhausted and the technical limits of the installed machinery eventually kick in.[16]

Interestingly, we get slightly different results for chlorine use. Indeed, the second set of regressions show that for chlorine, past water pollution abatement spending significantly reduces current polluting emissions. Once again the squared terms indicates the expected, convex relationship.[17] While individually only marginally significant, the coefficients on inspections are jointly strongly statistically significant, and provide similar insights as the ones discussed for thermal pollution.[18] The difference between the two sets of results can, however, 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 algal build-up they will experience, and the lower the need to chlorinate the water they run through their cooling systems to flush out contaminants.

Despite the asymmetries in the results, the picture that emerges from this exercise is one whereby both the level of spending on pollution abatement on the part of SEPPs and the number of inspections

[16] From the estimated coefficients we compute the inflection point, which obtains at Inspections_{t-1}=6713.45. The mean of the variable is 1277.82 and its standard deviation 1386.52.

[17] The estimated inflection point is 462.72, compared to a sample mean of 1.34, with a standard deviation of 8.79.

[18] The F-test for the null of joint significance of the two coefficients in this case is 10.06 and the corresponding p-value is 0.0003. The inflection point in this case is 5386.40.

carried out under the CWA affect environmental outcomes. In the preceding sections, we have provided convincing and robust evidence that electoral incentives affect both dimensions. The natural conclusions that we draw from the analysis in this section is that distortions in policy conduct and in private sector behavior due to electoral incentives have tangible implications for environmental quality.

6 Conclusions

In democratic systems, electoral incentives play a fundamental role as they hold politicians accountable to their constituents. A growing body of evidence, however, suggests that these electoral incentives my be diminished by term limits, as politicians alter their priorities and behavior. In this paper we provide compelling, causal evidence that these electoral incentives spill over into private sector decision making.

We first examine the impact of governor's party and re-election status on spending for water pollution abatement by SEPPs in the state. Within a RDD framework, we show that the level of water pollution abatement spending is statistically smaller when the governor of the state that the plant is located in is a term-limited democrat.

Next, we develop a conceptual framework to explain the behavior of power plants in this context and put forward a number of testable implications that we then take to the data. The first step in this process is to show that electoral incentives cause changes in the level of enforcement effort exerted by the environmental regulators across U.S. states.

Using a sample of tightly contested elections, we then test the implications of our theory. Our results show that the predictions of our compliance cost minimization model are borne out into the data. In particular, we show that the number of inspections is positively correlated with the pollution abatement effort exerted by firms, as also suggested by Earnhart (2004). Moreover, our results indicate that enforcement may be more effective under democratic governors – a finding in line with the analysis of Hanna and Oliva (2010) – and less effective when the governor is term-limited, which provides a novel insight. The presence of electoral incentives emerges clearly from our analysis, as governors are shown to pursue different policies when they are re-electable than when they are in their final term in office. As these differences vanish when the governor is not in control of the relevant policy enforcement levers, we conclude that our empirical findings are strongly aligned with the theory proposed by List and Sturm (2006).

Our final empirical effort is reserved to tracing out the implications of electoral incentives for environmental quality, directly via the level of enforcement and indirectly via its impacts on pollution abatement. We show that both thermal pollution and chlorine emissions are inversely related to the number of inspections and the level of abatement spending. Given our previous analysis of the causal impacts of electoral incentives on both variables, we conclude that electoral incentives have clear, causal impacts also on environmental outcomes.

These results are in line with a number of contributions across the economics and political science literature which show that re-election status of the executive branch of government influences the policies implemented as well as many economic outcomes (e.g. Besley and Case, 1995; Leigh, 2008; Beland, 2015). The main conclusions from this literature are that there is an *accountability* effect of re-election that encourages the efforts of re-electable incumbents to create higher voter payoffs (e.g. Alt et al., 2011). The scope of this concept is not limited to pollution abatement and further research can explore the links between term limits and various other private sector decisions.

Our contribution is instructive in helping understand what drives compliance with environmental regulations. We suggest that there are predictable periods of relatively low monitoring followed by periods of higher enforcement, such that a forward looking firm will act on these expectations. These insights might be helpful to regulators and environmental advocacy groups alike. For example, our findings may help environmental non-governmental organizations (NGOs) understand where their work might be most useful in complementing the work of enforcement agencies. Indeed, Grant and Grooms (2016) find that environmental NGO operations may be effective as a substitute for state environmental agency behavior. Our results would suggest that environmental NGOs should step up their activity when the governor is a term-limited democrat.

A Tables and figures

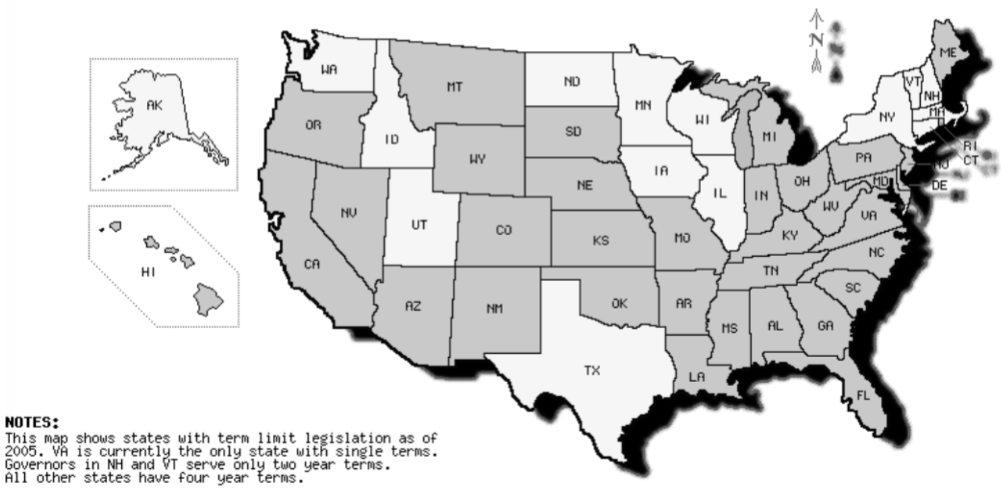
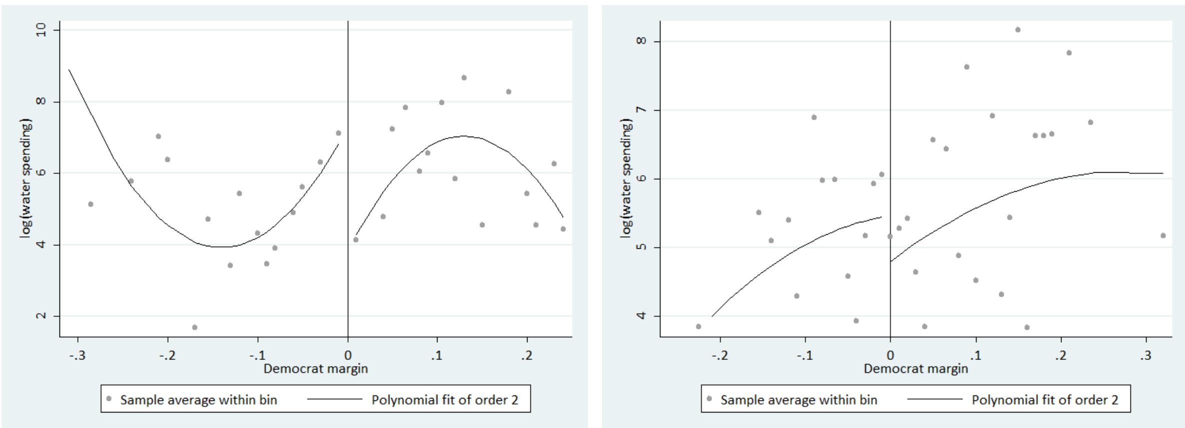


Figure 1: States with term limit legislation (shown in grey).



(a) Term-limited governors

(b) Not term-limited governors

Figure 2: Exogenous policy under induced technological change

Table 1: Summary Statistics

Variable	All Data	Democrat	Republican	Term-limited	Re-electable
log (Water abatement spending)	1348.06	1829.58	989.96	2253.55	1079.27
	(8824.75)	(12125.15)	(5116.42)	(15465.76)	(5450.61)
Thermal pollution (Δ°F × discharge per cbf)	5997.88	5900.64	6070.20	6380.99	5884.16
	(10084.16)	(8437.92)	(11152.39)	(13864.50)	(8648.02)
Clorine (1000s of lbs)	28.75	31.55	26.72	24.15	30.14
	(105.82)	(122.50)	(91.85)	(93.73)	(109.17)
Democrat	0.43	1.00	0.00	0.49	0.41
	(0.49)	(0.00)	(0.00)	(0.50)	(0.49)
Term-limited	0.23	0.26	0.20	1.00	0.00
	(0.42)	(0.44)	(0.40)	(0.00)	(0.00)
Inspections	1344.02	1398.61	1303.42	1225.03	1379.34
	(1443.19)	(1382.99)	(1485.24)	(982.58)	(1552.18)
NPDES	0.87	0.86	0.88	0.97	0.84
	(0.33)	(0.35)	(0.32)	(0.16)	(0.36)
Generation rate	34.84	37.54	32.83	38.58	33.73
	(37.60)	(38.84)	(36.53)	(40.31)	(36.69)
Electricity deregulation status	0.11	0.07	0.13	0.08	0.11
	(0.31)	(0.26)	(0.34)	(0.28)	(0.32)
RGGI	0.00	0.00	0.00	0.00	0.00
	(0.05)	(0.06)	(0.04)	(0.04)	(0.05)
NO_x trading	0.10	0.08	0.12	0.09	0.11
	(0.30)	(0.28)	(0.32)	(0.28)	(0.31)
Same party majority	0.37	0.49	0.29	0.44	0.35
	(0.48)	(0.50)	(0.45)	(0.50)	(0.48)
Sierra Club membership (%)	0.15	0.14	0.16	0.14	0.16
	(0.10)	(0.08)	(0.11)	(0.09)	(0.10)

Notes: This table reports the variables' means in each subsample, with the relative standard deviations in parentheses. All data are yearly observations.

Source: EIA-767 and public information on elections.

Table 2: Non-manipulability tests

	RD manipulation test
All elections	-0.85
	(0.39)
Elections with term-limited winners	-0.35
	(0.73)
Elections with re-electable winners	-0.69
	(0.49)

Notes: This table reports the results of the robust, bias-corrected RD manipulation test, using local polynomial density estimations based on Cattaneo et al. (2016). The parentheses report p-values. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 3: Non-parametric RD estimates: Water pollution abatement spending

	Average Treatment Effect at the threshold
All elections	-2.04
	(1.35)
Elections with term-limited winners	-3.67***
	(0.62)
Elections with re-electable winners[a]	-1.36
	(1.20)

Notes: This table reports the estimated coefficients from a non-parametric RDs of the effect of a democratic governor winning the election on the yearly amount spent on water pollution abatement. The coefficients are bias-corrected, robust estimates following Calonico et al. (2014). Standard errors are clustered at the state level. Standard errors in parentheses. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.
a This line reports the conventional estimate, without bias-correction.

Table 4: Parametric RD estimates: Water pollution abatement spending

	All elections			
Bandwidth	Linear	Linear w/interaction	Quadratic	Quadratic w/interaction
7.5 Percentage Points	-1.47*	-1.43**	-1.36*	-1.40**
	(0.73)	(0.67)	(0.68)	(0.69)
15 Percentage Points	-0.61**	-0.63**	-0.63**	-1.09
	(0.27)	(0.28)	(0.28)	(0.65)
	Elections where the winner is term-limited			
Bandwidth	Linear	Linear w/interaction	Quadratic	Quadratic w/interaction
7.5 Percentage Points	-8.86***	-3.40***	-8.86***	-3.95***
	(0.00)	(0.00)	(0.00)	(0.89)
15 Percentage Points	2.01**	2.45***	2.27***	-0.23
	(0.75)	(0.58)	(0.64)	(1.55)
	Elections where the winner is not term-limited			
Bandwidth	Linear	Linear w/interaction	Quadratic	Quadratic w/interaction
7.5 Percentage Points	-0.69	-0.65	-0.60	-1.63*
	(0.69)	(0.67)	(0.67)	(0.97)
15 Percentage Points	-0.54	-0.60	-0.61	-0.66
	(0.34)	(0.37)	(0.37)	(0.77)

Notes: This table reports the estimated coefficients from a parametric RDs of the effect of a democratic governor winning the election on the yearly amount spent on water pollution abatement. Results are shown for four different functional forms with two different bandwidths (7.5 and 15 percentage points). All regressions include plant fixed effects and standard errors are clustered at the state level. Standard errors in parentheses. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 5: Non-parametric RD estimates: Inspections per year

	Average Treatment Effect at the threshold
All elections	-488.71*
	(258.86)
Elections with term-limited winners	-1004.21***
	(370.45)
Elections with re-electable winners	-399.16
	(322.74)

Notes: This table reports the estimated coefficients from a non-parametric RDs of the effect of a democratic governor winning the election on the yearly number of inspections carried out under the CWA. The coefficients are bias-corrected, robust estimates following Calonico et al. (2014). The standard errors in parentheses are computed using the heteroskedasticity-robust nearest-neighbor variance estimator with $n = 3$. All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 6: Water pollution abatement spending

	Log of water pollution abatement spending					
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat	-0.04	0.23	0.20	-3.38	0.09	1.44***
	(0.34)	(0.27)	(0.26)	(2.22)	(0.27)	(0.48)
Term-limited	1.15***	2.18***	6.00***	2.26***	2.07***	5.88***
	(0.38)	(0.63)	(2.04)	(0.57)	(0.62)	(2.12)
Democrat × Term-limited	-2.10***	-2.78***	-3.47***	-2.50***	-2.74***	-3.36***
	(0.52)	(0.68)	(0.69)	(0.69)	(0.70)	(0.65)
log (Inspections)	–	1.30**	1.31**	1.21**	1.02*	1.05**
	–	(0.55)	(0.55)	(0.51)	(0.57)	(0.48)
log (Inspections) × Term-limited	–	–	-0.47**	–	–	-0.49**
	–	–	(0.21)	–	–	(0.21)
log (Inspections) × Democrat	–	–	–	0.44	–	–
	–	–	–	(0.27)	–	–
log (Inspections) × NPDES	–	–	–	–	0.07	–
	–	–	–	–	(0.69)	–
NPDES	–	–	–	–	-1.44	-0.46*
	–	–	–	–	(5.72)	(0.27)
NPDES × Democrat	–	–	–	–	–	-1.75**
	–	–	–	–	–	(0.65)
Constant	19.17***	-1.34	-0.72	-2.32	4.93	6.53
	(3.25)	(9.31)	(9.33)	(9.37)	(8.64)	(8.04)
Observations	1368	1368	1368	1368	1368	1368
R-squared	0.06	0.11	0.12	0.12	0.12	0.14
Test for TE – re-electable	t=-0.08	t=0.63	t=0.57	F=0.17	t=0.24	F=1.89
(NPDES=0)						t=2.98***
Test for TE – term-limited	F=11.21***	F=11.29***	F=19.57***	F=3.67**	F=9.50***	F=40.20***
(NPDES=0)						F=5.46**
Test for EI – republican	t=3.02***	t=3.45***	F=13.18***	t=3.97***	t=3.33***	t=2.77***
Test for EI – democrat	F=5.57**	F=2.54	F=3.86*	F=0.17	F=2.43	F=2.65

Notes: This table reports the estimated coefficient from fixed-effects panel regressions. The standard errors in parentheses are clustered at the state level. Additional control variables are included in all regressions but not shown (Electricity deregulation status, RGGI, NO_x trading, Same party majority, Sierra Club membership). All data are aggregated to the electoral term level.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

Table 7: Polluting emissions

	Thermal pollution		Chlorine	
	(1)	(2)	(3)	(4)
(Water ab. spend$_{t-1}$)	14.00	13.57	-53.48***	-54.37***
	(10.67)	(10.35)	(16.02)	(16.21)
(Water ab. spend$_{t-1}$)^2	-0.03	-0.03	0.12**	0.12**
	(0.03)	(0.03)	(0.06)	(0.06)
Generation rate	118.56***	112.35***	112.58***	107.40**
	(33.07)	(32.45)	(40.63)	(41.11)
(Generation rate)^2	-0.30**	-0.28*	-0.34***	-0.33**
	(0.14)	(0.14)	(0.13)	(0.13)
(Inspections$_{t-1}$)	–	-0.72**	–	-0.55
	–	(0.32)	–	(0.48)
(Inspections$_{t-1}$)^2	–	0.00***	–	0.00*
	–	(0.00)	–	(0.00)
Constant	2921.39***	3476.47***	1852.33	2255.63
	(895.00)	(884.35)	(1450.01)	(1355.56)
Observations	7510	7510	6841	6841
R-squared	0.01	0.01	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.
*, **, *** indicate 10%, 5% and 1% statistical significance, respectively.

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