9-2011

Are Politicians Office or Policy Motivated? The Case of U.S. Governors' Environmental Policies

Per G. Fredriksson
University of Louisville

Le Wang
University of New Hampshire - Main Campus

Khawaja Mamun
Sacred Heart University, mamunk@sacredheart.edu

Recommended Citation

Fredriksson, Per, Le Wang, and Khawaja Mamun. "Are Politicians Office or Policy Motivated? The Case of U.S. Governors' Environmental Policies." Journal of Environmental Economics and Management 62.2 (2011): 241-253.

Are Politicians Office or Policy Motivated? The Case of U.S. Governors' Environmental Policies†

Per G. Fredriksson

Department of Economics, University of Louisville

Le Wang

Department of Economics, University of New Hampshire

Khawaja A. Mamun

Department of Economics, Sacred Heart University

Running Title: Are Politicians Office or Policy Motivated?

---

† We thank John List, Jim Snyder, Daniel Sturm, and Patrick Warren for kindly providing data for this research, and two knowledgeable and helpful referees, Raj Chetty, Karen Conway, Jose Fernandez, Steve Gohmann, Guido Imbens, Jennifer Jensen, Angeliki Kourelis, Essie Maasoumi, Daniel Millimet, Daniel Phaneuf, seminar participants at the University of New Hampshire and session participants at presentations at the NARSC conference in Brooklyn, NY, and at the MPSA conference in Chicago, IL, for helpful comments and discussions of this research program. The usual disclaimers apply. Corresponding Author: Per G. Fredriksson, Department of Economics, College of Business, University of Louisville, Louisville, KY 40292. Email: Per.Fredriksson@Louisville.edu; Phone: 502-852 4858; Fax: 502-852 7672.

Are Politicians Office or Policy Motivated? The Case of U.S. Governors' Environmental Policies

Abstract

Are elected politicians primarily motivated by holding office, thus choosing environmental policies accordingly? Or are they motivated by the chance to implement their preferred environmental policies? Do governors have character, in the sense that they promise and implement environmental policies consistent with their own preferences? To answer these questions, we study the differences in environmental spending across both re-electable and lame duck governors from the two main political parties. In our empirical analysis, we make use of parametric and non-parametric regression-discontinuity approaches. While re-electable governors do not set significantly different policies, lame duck governors do. We argue that in the area of environmental policy governors appear to be primarily office motivated and lack character.

Keywords: Political Economy, Term Limits, Candidate Motivation, Elections, Environmental Spending, States, Regression Discontinuity.

JEL Codes: Q58, H7, D7, C21

"I will fight for the environment. Nothing to worry about."

- Gubernatorial candidate Arnold A. Schwarzenegger in August, 2003.[1]

1. Introduction

Are politicians primarily motivated by holding office or by the chance to implement their preferred policies? Theoretical contributions to the literature have generally exogenously assumed either of these two driving forces.[2] Persson and Tabellini (2000), e.g., find such opposing views unsatisfactory, and an improved empirical understanding of politicians' motivations thus appears helpful in the evaluation of existing theory, and in future model building including analysis of environmental policy formation (Persson and Tabellini, 2000; Callander, 2008).[3] In addition, since public office comes with significant powers to set environmental policy, it appears of significant importance to empirically discern politicians' motivations for seeking office. Analysis of politicians' underlying motivations should help us better understand instances of political failure in the area of environmental policy, e.g., why public policies aimed at providing environmental amenities frequently deviate from their optimal designs (see, e.g., Babcock et al., 1997; Wu and Boggess, 1999; Wu et al., 2001).[4] In this paper, we take a step towards an improved empirical understanding of the motivations guiding politicians in the area of environmental policy. In particular, our objective is to empirically investigate the motivations exhibited by state governor's environmental spending choices.

Recent theoretical contributions guide our empirical work. Callander (2008) presents a model of electoral competition with heterogenously motivated (office *and* policy motivated) political candidates (both types have not previously appeared in the same model). In Callander's model, candidates commit to a policy position before an election, but the quality of implementation depends on the effort level exerted after winning office. Policy motivated candidates are assumed to care relatively more about policy outcomes (office motivated candidates may still put some relatively minor weight on policy), and if elected therefore exert

more effort in the policy implementation process. Voters weigh ideology against the quality of policy implementation, but cannot observe candidates' inherent type. Both types are fully strategic. Since voters endogenously prefer policy motivated candidates, office motivated candidates attempt to imitate their policy motivated opponents. Meanwhile, policy motivated candidates attempt to signal their type and separate from office motivated types. The policies that emerge as a result of political competition between heterogeneously motivated candidates may yield either convergence or divergence. That is, in an incomplete information environment the median voter may not support the candidate that promises the policy closest to her ideal point, and the median voter does not necessarily induce convergence. Callander's (2008) model suggests that politicians who are moving away from the median voter's preferred policy may not do so due to their wish to implement their own preferred policy, but rather to win votes.

Kartik and McAfee (2007) present a related model where some political candidates have unobservable "character" (policy preferences) and choose electoral platform and post election policies accordingly (see also Ansolobehere and Snyder, 2000; Groseclose, 2001; Aragones and Palfrey, 2002). Candidates with character are non-strategic and thus their platforms are set exogenously. In Kartik and McAfee (2007), strategic politicians without character imitate the policy positions of candidates with character, as voters are assumed to value character. This pushes policy choices away from the median voter's favored position. Non-centrist candidates may not actually have character but may be strategically mimicking opponents with desirable character. However, there will still be a tendency by strategic types to move towards the median voter.[5]

In this paper, we attempt to shed empirical light on the motivations guiding state politicians' environmental policy choices. Are state governors office or policy motivated, as discussed by Callander (2008)? Do they have character as modeled by Kartik and McAfee (2007)? Our empirical strategy focuses on actual environmental policymaking by Democratic and Republican governors in two different situations, i.e. our analysis proceeds in

two steps. First, we estimate the difference in environmental policy choice across incumbent Democratic and Republican state governors who are eligible for re-election. We assume that if politicians are policy motivated in the area of environmental policy, their policy preferences will coincide with their own party's ideology, a view supported by, e.g., Poole and Rosenthal (1984). These ideologies may, or may not, differ across political parties (our empirical analysis below suggests that they differ).[6] By comparing the environmental policy choice across re-electable governors from different parties we evaluate whether policies converge or diverge, i.e. whether pooling or separating equilibria emerge. On the one hand, if a majority of governors are sufficiently policy motivated (or, have character) in the area of environmental policy, this would lead to environmental policy divergence across governors of different party affiliations; a separating equilibria emerges. On the other hand, if all elected politicians are office motivated (or, lack character), or if office motivated candidates pool at policy motivated candidates' bliss points (alternatively, strategic politicians mimic those with character), this would lead to environmental policy convergence across governors of different party affiliations; a pooling equilibria emerges.

Second, we estimate the difference in environmental policy selected by governors who face a binding term limit (i.e., have lame duck status). By studying the environmental policy difference across lame duck governors, we aim to observe the actual difference in governors' ideal points conditional on party affiliation. Lame duck governors are generally assumed to set policy at their ideal points (Alesina, 1988; Besley and Case, 1995; List and Sturm, 2006).[7] Comparing environmental policy differences across re-electable and lame duck governors offers an opportunity to infer the motivations of governors in the area of environmental policy. For example, suppose re-electable governors exhibit no policy differences across parties, while lame duck governors exhibit significant policy differences. This would indicate that they have heterogeneous environmental policy preferences that are dominated by the office motive as long as they are eligible for re-election.

The environmental policy measure in focus is the growth rate of state environmental

expenditures during the 1970 to 2000 period (the categories are: fish and game; forests and parks; other natural resources). This measure has the advantage of being available for a relatively long time period, it is largely determined by decisions at the state level rather than the federal level (see, e.g., Brown, 2001), and is likely to be relatively unaffected by considerations related to interstate capital competition.[8]

As the winning candidates from different parties can be different in many (observable and unobservable) dimensions, failure to control for these differences sufficiently may preclude us from making meaningful inferences from the simple comparisons of the policy outcome across governors from different parties (e.g., simple OLS approach). In our estimations, we utilize both parametric and non-parametric regression-discontinuity designs (RDD) to circumvent this problem. These two RDD approaches exploit the fact that the party affiliation of the winning candidate is a deterministic function of vote share obtained by Democratic candidates. In other words, if we know the vote share of the Democratic party, we are certain about the party affiliation of the winning candidate. As a result, there is no confounding variable other than vote share (see Angrist and Pischke (2009) for further discussion). Intuitively, RDD approaches identify the causal effects of governor party affiliation by comparing policy outcomes of treatment and control groups with similar vote shares, i.e. states where the Democratic candidate barely won the election (the winning candidate is a Democrat) and states where Democratic candidates barely lost the election (the winning candidate is a Republican). Conditioning on similar vote shares, the RDD approaches mimic a random assignment of the party affiliation of the governor (Lee et al., 2004; Lee, 2008; Warren, 2008), thereby addressing the endogeneity problem associated with the incumbent's party affiliation. Utilizing these empirical techniques in our setting assures that the results are not due to the preferences of the constituency, but rather the objective and motivation of the winning candidate.

Our empirical results help shed novel empirical light on the motivations of politicians in the area of environmental policy. Competition between re-electable Democratic and Repub-

lican governors results in pooling equilibria; we find no significant differences in the growth rates of environmental spending in the 1970-2000 time period across governors from these two political parties. Turning to our estimates of lame duck governors' policy differences, however, we find a significant policy gap across political parties. Our results imply that Democratic lame duck governors set approximately 4-9% points lower growth rates in environmental spending than do their Republican counterparts. This suggests that the ideal growth rates are significantly different across parties, lending support to our assumption that political ideologies and preferences for environmental spending differ across parties.

In sum, we find that policy motivation and character do not appear to be important attributes of governors winning elections. Governors who win repeated elections (and thus end up as lame ducks) appear primarily office motivated in the area of environmental policy. However, they do have some degree of weak policy motivation (heterogeneous across political party). Our findings are robust to several modifications of the data set, including the exclusion of Southern states (where party differences historically have been close to indistinguishable) and the exclusion of states classified as "Green states" with high environmental group membership ratios.

We believe our results complement the related empirical literature. Levitt (1996) finds that ideology is the overwhelming influence on U.S. senators' voting behavior, while voter preferences and the national "party line" are of minor importance in their utility functions. The literature on reputation building shows that policy is frequently distorted by politicians seeking to attract voters (Besley and Case, 1995, 2003; List and Sturm, 2006). Besley and Case (1995) found that Democratic term limited governors set significantly higher per capita total state taxes and state expenditures than other governors, while Besley and Case (2003) (extending their previous data set) find a significant effect only for state expenditures. List and Sturm (2006) find that re-electable governors cater to constituents with strong (pro- or anti-) environmental interests as long as they are eligible for re-election, but reverse this pattern upon gaining lame duck status. List and Sturm (2006) do not directly study inter-party

political competition. Cremer et al. (2008) build on Roemer (2001) where two parties each have militant (policy motivated) and opportunistic (office motivated) factions, and study the consequences for environmental policy outcomes. They calibrate a model using U.S. household data on income and preferences. When the militants of each party dominate, the equilibrium involves large national emission tax increases, whereas when the opportunists dominate subsidies are offered by both parties. The model also produces election probabilities, where the Democratic party is heavily favored in equilibria where the tax rate is raised sharply. However, Democrats and Republicans have fairly similar election probabilities in equilibria which entail subsidies. Since U.S. gasoline taxes are relatively low, the U.S. has not ratified the Kyoto Protocol, and the national party in power switches frequently, Cremer et al. (2008) interpret their results as indication that at the national level U.S. environmental policy has been guided mostly by the opportunistic factions of the Democratic and Republican parties. Our results constitute more direct evidence that U.S. environmental policies are guided by office motivated (opportunistic) politicians at various levels of government.

The remainder of the paper is organized as follows. Section 2 discusses the data and empirical methodologies used. Section 3 reports the main empirical results, as well as a robustness analysis. Section 4 provides a brief conclusion.

2. Data and Empirical Methodology

Based on the above discussion of the theoretical literature, our empirical analysis seeks to determine whether U.S. governors are primarily office or policy motivated in the area of environmental policy by estimating the: (i) environmental policy differences across re-electable Democratic and Republican governors; (ii) environmental policy differences across Democratic and Republican governors facing binding term limits. In order to identify the *causal* effects of party affiliation on environmental policies, we employ two different regression-discontinuity designs (RDD). As mentioned above, these RDD approaches identify the causal effects of party affiliation by comparing the policy outcomes of the treatment and control

groups with similar vote shares. Conditioning on similar vote shares, these RDD approaches mimic a random assignment of the party affiliation of the governor (Lee et al., 2004; Lee, 2008; Warren, 2008), thereby addressing the endogeneity problem associated with the incumbent's party affiliation. RDD can be used as randomized experiment because even though governors can influence vote shares, they cannot do so *precisely*. Intuitively, even though some governors are more likely to win than others, *every* governor has approximately the same chance of being just above or below the threshold. This implies that the local random assignment property of the RDD holds (see Lee (2008) and Lee and Lemieux (2010) for detailed derivations).

In practice, both parametric and nonparametric RDD approaches are employed. These two approaches differ in the means that they control for election vote shares: the parametric approach includes a flexible function of vote share in the regression estimation, whereas the nonparametric approach utilizes a sub-sample of states with close elections. These two approaches are equivalent asymptotically. Now we turn to more formal discussions of these approaches.

2.1. Empirical Methodology: Parametric Approach

To begin, let Y_1 and Y_0 denote two environmental policy variables to be compared. In particular, Y_1 (Y_0) represent *potential* environmental policies if a governor is a Democrat (Republican), such that

$$\begin{aligned} Y_1 &= \beta + \delta + u_1 \\ Y_0 &= \beta + u_0 \end{aligned} \tag{1}$$

The difference in environmental policies between Democratic and Republican governors is captured by δ. However, we never observe both Y_1 and Y_0 for the same governor as the governor cannot be a Democrat and Republican at the same time. Notice, however, that the observed policy for governor i could be expressed as $Y_i = Y_1 D_i + (1 - D_i)Y_0$, where D_i is the

party membership of the governor in state i, $D_i = 1$ if Democratic, $D_i = 0$ if Republican. Thus, we could write the observed outcome as a regression model,

$$\begin{aligned} Y_i &= Y_1 D_i + (1 - D_i) Y_0 \\ &= D_i(\beta + \delta + u_1) + (1 - D_i)(\beta + u_0) \\ &= \beta + \delta D_i + u_i \end{aligned} \tag{2}$$

where $u_i = u_0 + D_i(u_1 - u_0)$. Equation (2) is a common regression model. If $\mathbb{E}[u|D] = 0$, we can consistently estimate the effects of party ideology on environmental policies, δ, by Ordinary Least Squares (OLS). However, this assumption implies that gubernatorial party affiliation is uncorrelated with any other determinants of environmental policies. Such an assumption may be too restrictive and potentially fail to hold.

Although we can alleviate the bias by controlling for those (observable) variables that are commonly regarded as the determinants of environmental policies, it may still not be sufficient to identify the causal effect of candidate motivations based on party affiliation. As a result, recent studies of the party effects on policy outcomes have utilized the regression discontinuity design (RDD) method (Lee et al., 2004). This method exploits the fact that given the majority rule, the election outcome is a deterministic function of vote margin ("sharp" RDD),

$$D_i = I(m_i > 0) \tag{3}$$

where m_i is vote margin. Note that in the sharp RDD,

$$\mathbb{E}[u|D, m] = \mathbb{E}[u|m] = f(m) \tag{4}$$

Equation (4) implies that we can include and explicitly model the conditional expectation

$\mathbb{E}[u|D, m]$ in (2). Equation (2) becomes

$$\begin{aligned} Y_i &= \beta + \delta D_i + u_i \\ &= \beta + \delta D_i + \mathbb{E}[u_i|D_i, m = m_i] + \epsilon_i \\ &= \beta + \delta D_i + f(m_i) + \epsilon_i \end{aligned} \tag{5}$$

where β is a constant, δ is the coefficient of interest (the candidate party affiliation effect), $f(.)$ is a flexible function of m, and $\epsilon_i = u_i - \mathbb{E}[u_i|D, m = m_i]$ is the error term. By construction, the error term, ϵ_i, is no longer correlated with the party affiliation, D_i, and thus, explicitly controlling for $f(m_i)$ in the model, δ can be consistently estimated via OLS.[9,10] However, the main issue in the estimation is selecting the correct functional form of $f(.)$. While over-specification of $f(.)$ leads to consistent yet less precise estimates, under-specification produces inconsistent but more efficient estimates (Warren, 2008). Following the literature, the parametric specifications with quartic polynomials are preferred throughout the paper, as quartic polynomials are generally very flexible in the approximation of an underlying nonlinear function (see, e.g., Lee et al., 2004). Below, we nevertheless experiment with the functional form of $f(.)$ and utilize a less model-dependent nonparametric approach to assess robustness.

Several additional estimation issues warrant further discussion. First, in order to fully utilize the panel structure of our data, we include both state and time fixed effects in Equation (5). Moreover, as Hoxby (2000) and Pettersson-Lidbom (2008) note, the inclusion of these fixed effects allows us to use only within-state variations to identify the party affiliation effects; this is desirable since it is "more powerful and less subject to bias" (Hoxby, 2000, p.1253). Second, inclusion of additional covariates is in general not necessary in the RDD estimation. However, if the additional covariates are orthogonal to the treatment (conditional on the control functions), inclusion of these variables will not dramatically alter the estimates, and could potentially increase their precision. Thus, below we also add additional controls

(from List and Sturm, 2006), which also serves as a robustness check for the randomness of the RDD estimation (Pettersson-Lidbom, 2008).

2.2. Empirical Methodology: Non-parametric Approach

Although a flexible function with higher order terms is likely to approximate the underlying function $f(.)$ reasonably well, the control function approach is model dependent. Therefore, we in addition employ a local linear regression approach (a non-parametric approach) to assess the robustness of our parametric results. Note that Equation (2) implies

$$\lim_{m\downarrow 0}\mathbb{E}[Y|m] - \lim_{m\uparrow 0}\mathbb{E}[Y|m] = \delta + \lim_{m\downarrow 0}\mathbb{E}[u|m] - \lim_{m\uparrow 0}\mathbb{E}[u|m] \tag{6}$$

Equation (6) implies that the difference between the conditional means (averages) of Y of those individuals just below and above the cutoff point (i.e., only *close* elections are relevant) would give us a consistent estimate of δ, provided that the observations are similar around the cutoff point (i.e. $\lim_{m\downarrow 0}\mathbb{E}[u|m] - \lim_{m\uparrow 0}\mathbb{E}[u|m] = 0$). The local linear regression approach is employed here. Guided by Imbens and Lemieux (2008), we utilize a simple rectangular kernel for local linear regression estimation. Notice that the local linear regression approach with a simple rectangular kernel is equivalent to fitting linear regression functions to the observations falling into the interval around the cut-off point (that is, all observations falling outside the interval are excluded), with the radius being the bandwidth h. That is, the effect is estimated by solving:

$$\min_{\beta,\delta,\alpha_1,\alpha_2} \sum 1\{-h \leq m_i \leq h\}(Y_i - \beta - \delta D_i - \alpha_1 m_i - \alpha_2 m_i D_i)^2 \tag{7}$$

The practical question is the choice of bandwidth, h. The bandwidth h defines "close" elections, and thus the sample utilized in estimation. For example, the bandwidth $h = 2$ means that we utilize only the sample of those states where the Democratic candidate barely lost with a vote share equal or greater than 48 percent, as well as those where the Democratic candidate barely won with vote share equal or less than 52 percent. The trade-off here is

similar to that in the parametric approach – a trade-off between efficiency and consistency. As seen from (6), as the interval (i.e. bandwidth h) increases, we increase the precision of our estimates by utilizing more observations in the analysis, but introduce larger bias by including more observations which may be quite different. On the other hand, as we restrict the sample to a smaller neighborhood, we find more consistent but potentially insignificant results due to the decreasing sample size. Indeed, a drawback is that the nonparametric approach is quite data-demanding. Thus, we experiment with several bandwidths in order to assess the robustness of our results. Moreover, as Lee and Lemieux (2010) note, even for nonparametric RDD estimation using only data around the cut-off point, there could still be bias. Thus, nonparametric estimation is a complement rather than a substitute for parametric estimation.

2.3. Validity of the Regression Discontinuity Design

Assuming that the RDD works, we have thus far motivated the RDD approach from the potential outcome framework and discussed several important practical issues. But how do we know that the RDD is appropriate in our context? Note that an important assumption behind the RDD is that $\mathbb{E}[u|m] = f(m)$ is a continuous function in m, see Equations (4) and (6). While it is impossible to test this assumption since u is unobserved, it is possible to test whether $\mathbb{E}[u|m]$ is continuous at the threshold (Lee and Lemieux, 2010). A discontinuity at the threshold (0 in our case) would indicate a failure of this assumption. Lee and Lemieux (2010) summarizes two tests proposed in the literature. The first test is to examine whether or not there exists a discontinuity in the baseline covariates at the threshold. This test is similar to tests that seek to demonstrate whether or not individuals in the treatment and control groups are similar so as to assess the validity of randomized experiments (Pettersson-Lidbom, 2008; Lee and Lemieux, 2010). The second test, proposed by McCrary (2008), is to test whether the marginal density of m is continuous – the log density test. A word of caution is warranted concerning these tests. Both of these tests are *not* a sufficient and necessary condition for identification. For example, for the first test, we still cannot test

the balancing property of the unobserved characteristics at the threshold. For the second test, it is valid under auxiliary assumptions (McCrary, 2008). That said, they are useful indications of the plausibility of the underlying continuity/smoothness assumption, and we shall conduct both tests below to assess the validity of the RDD.

2.4. Data

The majority of the 1970 - 2000 data comes from List and Sturm (2006) and the remaining part from Ansolabehere and Snyder (2006). We thus provide limited details here. The main variable of interest is the growth rate in environmental spending per capita. Following List and Sturm (2006), we define environmental spending per capita as the sum of three expenditure categories: (i) "fish and game," (ii) "forests and parks," and (iii) "other natural resources". List and Sturm (2006, page 1261) argue that these three spending categories "'record very similar types of expenditure, which are likely to be close substitutes for voters both in favor of and opposed to environmental policy."' One advantage of this measure for our purposes is that it depends to a large extent on state level political decisions (see Brown (2001)), not federal regulations. Although the federal government has partial influence over environmental policy, the states have considerable control over environmental expenditures, as discussed by List and Sturm (2006). Another advantage is the relatively long panel, with enough observations of lame duck period behavior.[11] Moreover, governors appear unlikely to engage in interstate capital-competition using environmental spending.

A dummy variable indicating lame duck governor status was constructed by Besley and Case (1995) and Besley and Case (2003), and was updated by List and Sturm (2006). This dummy takes a value of one when a governor faces a binding term limit and cannot seek re-election, and zero otherwise.[12] We include the control variables from List and Sturm (2006): state personal income per capita; the percentage of the population with membership in major environmental organizations (Sierra Club, Greenpeace and Friends of the Earth); state population; percentage of the population over age 65, and the percentage of the population aged 5-17. We choose these variables mainly for two reasons. First, we want to include

the same set of variables permitting the comparisons of results with the prior literature; these variables are the same as in List and Sturm (2006) and commonly included variables in related studies (e.g., Pettersson-Lidbom, 2008). Second, as discussed above, inclusion of observable variables is not necessary in the RDD estimation, but helps increase the precision of the estimates. The party affiliation and vote data come from Ansolabehere and Snyder (2006). Vote margins are constructed as the Democratic candidate's percentage of the vote share (minus 50 percent) that go to the top two candidates in the most recent gubernatorial election. We only consider votes that go to the top two candidates, and only elections with Democratic and Republican top candidates, i.e. we ignore an election if a third party candidate wins or comes in second. The vote margins in the most recent gubernatorial election are used.[13]

Table I presents summary statistics. A comparison of the growth rates of environmental expenditure per capita for Republican and Democratic governors, respectively, indicates a difference of 0.5 percentage points (3.3 percent for Republicans versus 3.8 percent for Democrats). While the growth rate is higher in states with Democratic governors, a further scrutiny of the summary statistics indicates that states with Republican governors are different from those with Democratic governors in multiple dimensions. For example, Democratic governors are more likely to be elected in states with a larger share of young persons in the population, but less likely to be elected in states with higher per capita personal income and with an older population (over age 65). This indicates that observed differences in environmental policy between Republican and Democratic governors may simply be due to differences in observable determinants of environmental policies that are correlated with governor election outcomes. Moreover, given the differences in these observable characteristics, it is possible that states with Democratic governors differ in some unobservable dimensions from states with Republican governors. This preliminary scrutiny of the data suggests that it is important to take potential endogeneity problems into account, and thus that our empirical approach is particularly relevant in this context.

3. Empirical Results

3.1. Governors and Environmental Policy

We now turn to our empirical results. Prior to presenting our main results, we first provide a graphical analysis of state environmental policy. Figure 1 shows the relationship between the vote margin and the growth rate in average per capita environmental spending (within 1 percent intervals of the vote margin), separately for re-electable (Panel (a)) and lame duck (Panel (b)) governors. The thick lines are fitted values from fourth order polynomial regressions, and the thin lines represent 95 percent confidence intervals. Following the literature (e.g. Lee et al., 2004), the data points in the graphs are the averages of the growth rate of state environmental spending within each bin. For a given bandwidth h, we construct bins $(b_k, b_{k+1}]$, where $b_k = 0 - (K_0 - k + 1) \cdot h$; K_0 is the total number of bins *to the left* of the cut-off point 0, and $k = 1, \ldots, K$ (the total number of bins). Plotting the averages instead of the raw data makes the graph clearer and the interpretation more straightforward. However, the identified jump is the same. See the survey articles by Imbens and Lemieux (2008) and Lee and Lemieux (2010) for more details. As discussed before, the effect of party affiliation is identified by the discrete jump of a flexible function (here, a quartic function) of vote margin at the cut-off point, zero. A negative (positive) vote margin implies that a Republican (Democratic) governor is elected. As we can see from Figure (1), the jump (measured by the difference between two functions) at the cut-off point is small and positive for the sample of re-electable governors. That the confidence intervals for both fitted lines overlap considerably indicates that such a difference is likely to be statistically insignificant. In contrast, for the sample of lame duck governors the jump is large and negative. That the confidence intervals for both fitted lines do not overlap indicates that such a difference is likely to be statistically significant. Figure 1 thus illustrates a small (statistically insignificant) positive effect of party affiliation on the growth rate in environmental spending per capita for re-electable governors, but a large (statistically significant) negative effect for lame duck governors. This provides some initial evidence that governors behave differently when

they are lame ducks. As noted in Lee and Lemieux (2010), graphical presentation may easily be manipulated and thus mask the true effects. In Panels (c) and (d) we further provide a set of graphs using the bandwidth chosen by the cross-validation method proposed by Ludwig and Miller (2007) to minimize the bias in presentation. The results are similar.

Table II displays three sets of RDD results for governors eligible to run for re-election. For completeness, we also include the result of OLS estimation in Column (1). Panels A.1 and A.2 are the parametric results without and with covariates included in the estimations, respectively. Columns (2)-(5) report the RDD results using different polynomials for the control function. Panel B presents the nonparametric results using four different bandwidths, h. The coefficients reveal the effect on the growth rate of environmental spending of having a re-electable Democratic governor in office rather than a Republican. The results in Panel A.1 reveal a positive but statistically insignificant effect of winning governor party affiliation on environmental policy; the coefficient is roughly zero.

As discussed above, the inclusion of additional covariates is not necessary in RDD estimation. However, suppose the covariates – those orthogonal to or uncorrelated with the party affiliation around the cut-off points – are important determinants of environmental policy Y_i. Then, including these variables may reduce the residual variance, thereby improving the precision of estimates. In addition, inclusion of these additional variables should not dramatically alter the RDD estimates, and thus serve as an additional robustness check (Pettersson-Lidbom, 2008; Imbens and Lemieux, 2008). Therefore, we re-estimate the RDD models augmented by all additional controls discussed in the data section above. The results are presented in Panel A.2. We again find that the difference in the growth rate of environmental spending under re-electable Democratic and Republican governors is statistically insignificant across all specifications. Moreover, the results reported in Panels A.1 and A.2 display a high degree of similarity, indicating that RDD is a valid design.

We recognize that the RDD results above using the control function approach are model dependent. Therefore, we further assess the robustness of our parametric results by utilizing

a non-parametric RDD approach. Panel B in Table II displays the results. The estimates are consistent with the results presented in Panels A.1 and A.2, regardless of bandwidths chosen. As the bandwidth declines (yielding smaller bias) in the nonparametric estimation, the coefficient magnitudes generally become larger.[14] The inter-party difference does not attain statistical significance at conventional levels.[15] In sum, the insignificant results presented in Table II suggest that re-electable governors tend to reach a pooling equilibrium in the environmental policy dimension. Unless all governors have identical environmental policy preferences (not supported by our estimation results below), this suggests that a non-negligible share of governors are office motivated in regards to environmental policy.

We now turn to the results emerging from using the sample of lame-duck governors only, presented in Table III. The columns are analogous to those in Table II. While the OLS results in Panels A.1 and A.2 are insignificant, the RDD estimates in these panels are negative and statistically significant across all specifications. The estimates are economically significant, varying from -4.6 percent to -8.2 percent. The results suggest that the growth rate of environmental expenditures is slower when a Democratic lame duck governor is in office relative to when a Republican lame duck is seated.[16] Adjusting for covariates matters very little, as indicated in Panel A.2 in Table III. In particular, the point estimates remain negative, statistically and economically significant, ranging from -4.1 percent to -7.8 percent.[17]

Next, Panel B in Table III presents the results using nonparametric RDD estimation. Again, we find negative and statistically significant impacts of Democratic governor party affiliation on environmental spending. The results are robust to the choice of bandwidths. Although the precision decreases slightly in the case $h = 2$, this is likely due to smaller sample size (the sample size decreases as the bandwidth decreases).[18]

The results in Table III suggest that governors have some degree of (heterogeneous) policy motivation in regards to environmental policy. Their policy preferences differ across political party, as revealed by their lame duck behavior.[19] Nevertheless, the differences across the

regular and lame duck periods suggest that winning governors (those staying in office until the lame duck period) have a substantial degree of office motivation in the environmental policy area, as electoral competition induces pooling equilibria (policy convergence) (Table 2). Below, we report the results of a robustness analysis of the findings in Tables II and III.

3.2. Initial Robustness Analysis

Several issues merit further discussion. First, a governor in her last term may not view her current position as the end of her political career, as she could potentially run for higher office. If this were the case, office motivated lame duck governors would have an electoral incentive to moderate their policy choice. While such intentions are hard to measure precisely, we take this possibility into account by excluding the 12 governors in our sample who later ran for Congress (three of these excluded governors stayed in office long enough to face a binding term limit). The parametric result for lame ducks remains negative and statistically significant (results available upon request). The result should be viewed as a lower bound estimate (in terms of magnitude) of the policy difference among lame ducks. It is consistent with Besley and Case (1995) who provide evidence that lame duck governors care less about their reputation than other governors, i.e. they are on average not influenced by considerations of future re-election prospects.

Second, the analysis of re-electable governors so far includes *all* governors eligible for re-election. However, governors who hold on to power long enough to become lame ducks may be different from those governors who lose power before winning a lame duck term. If voters care about the effort exerted in the policy implementation phase as assumed by Callander (2008), time in office may make a difference. Voters will have had a greater opportunity to evaluate a more seasoned governor's performance and make inferences about the probability of the governor being policy motivated. This should raise the governor's re-election prospects. That is, we may expect that governors re-elected to a lame duck term have some degree of policy motivation. On the other hand, Bernhardt et al. (2004) argue that to re-elect

an incumbent to a last lame duck term, the median voter must believe the incumbent to be relatively moderate, and therefore the median voter sets a stricter condition for re-election to a lame duck period. This would suggest that re-electable governors who eventually become lame ducks should not exhibit significantly different policies. We therefore restrict the sample to governors who remain in office sufficiently long to eventually become lame ducks. We denote this group of re-electable governors *Future-lame-duck* governors. We then repeat the analysis in Table II using only the sample of Future-lame-duck governors. In the interest of brevity, Table IV provides only the main parametric (the forth-order polynomials) and the non-parametric (a bandwidth $h = 2$) estimation results (remaining results available upon request). The results turn out to be invariant to the specification or approach used. First, while the point estimates are consistently positive, they remain statistically insignificant at conventional levels (except using a first-order polynomial control function, not reported in Table IV. Second, the estimated policy differences across parties are larger than those using the pooled sample of all re-electable governors reported in Table II. For example, the point estimate in Table IV, Column (1), indicates a 9.7 percent positive difference between Democratic and Republican governors. This is almost four times larger than the estimate obtained using the pooled sample (2.6 percent). Nevertheless, the insignificant results indicate that those governors who eventually end up winning a lame duck term are primarily office motivated, but with some degree of policy preferences. This is consistent with Callander (2008), who finds that this type of politician has an electoral advantage.

Third, while the RDD approach fares well in terms of our informal test above (i.e., the inclusion of extra control variables), we now formally assess the validity of this approach. As discussed above, there are two sets of tests commonly used in the literature (Lee and Lemieux, 2010). The first test is to examine the underlying similarity of the states where the sample observations have been collected. We cannot test this assumption for all state characteristics, in particular not for unobservable characteristics. We are, however, able to check whether or not the observable characteristics are similar around the cut-off point.

Following the literature (e.g. Pettersson-Lidbom, 2008; Lee and Lemieux, 2010), we therefore run a set of regressions using as independent variables the party affiliation dummy and the control function with quartic polynomial terms. We do so separately for each observable variable discussed in the data section. If indeed the RDD approach is valid and mimics a randomized experiment, we would expect that the party dummy has no association with the dependent variables in these regressions; the party affiliation dummy should not be significantly different from zero. The results of the specification tests are reported in Panel A, Table V. The first column presents the results for the sample of re-electable governors and the second column reports the findings for the lame-duck sample.

Contrary to the significant differences between Democratic and Republican governors in Table I, the specification tests in Table V reveal no significant differences in our sample between states led by Democratic and Republican governors. For two variables, (i) Percentage Population over age 65, and (ii) Percentage Population aged 5-17, the coefficients are roughly zero. The well balanced state characteristics confirm the *randomness* of the governors' party affiliations around the cut-off point; that is, the RDD is indeed as good as a random experiment. These results indicate the plausibility of the underlying identification assumption regarding smoothness (McCrary and Royer, 2011).

The second set of tests, the log density test proposed in McCrary (2008), examines whether the marginal density of m is continuous at the cut-off point. The results are reported in Panel B, Table V. The tests fail to reject the null of no discontinuity in the marginal density of m; none of the log discontinuity estimates are statistically significant at conventional levels, again confirming the plausibility of the RDD.

3.3. Further Robustness Analysis

So far, our analysis has assumed that candidate party affiliation effects are equivalent across states.[20] In this section, we investigate whether our results are driven by any group of states. Specifically, we consider whether Southern states or "Green" states (with a high density of members in environmental lobby groups) drive our results.

First, as noted by Lee et al. (2004, p.844) the literature generally finds that in the South, Democratic and Republican politicians "are ideologically closer than they are in the North.". If governors from Southern states are more homogenous in terms of policy preferences, the degree of policy motivation and divergence among all remaining governors may be underestimated. Thus, we would expect that the estimated differences among both re-eleactable and lame duck governors in the non-South may be larger than our earlier results for the full sample, assuming governors in the non-South have some degree of policy preferences. However, the results for re-electable governors from the non-South should remain unchanged, if the office motive dominates among governors from the non-South. We follow the division between Southern and non-Southern states as defined by the U.S. Census Bureau.

Second, List and Sturm (2006) find that governors' political behavior is influenced by the share of the population with an intense interest in environmental policy issues. Following List and Sturm (2006), we define Green states as the eight states with the largest membership in environmental groups measured as a percentage of the state population. We define Brown states as non-Green states. The Green states are California, Colorado, Connecticut, Maine, Massachusetts, New Hampshire, Oregon, and Vermont. We repeat our earlier analysis excluding these eight states. We have no a priori information regarding differences in policy preferences across Democratic and Republican governors in Green states versus Brown states. Thus, we cannot predict how our results will be altered by the exclusion of these states.

The results excluding Southern and Green states are reported in Panels A and B in Table VI, respectively. Since non-parametric approaches are data-demanding, we report only the main parametric results with quartic polynomial terms and additional covariates. In the sample using re-electable governors only we find no significant differences, consistent with our earlier results. The estimates in Table VI of 0.03 (non-Southern states) and 0.017 (Brown states), respectively, are similar to the 0.025 obtained utilizing the full sample in Table II (Panel A.2, Column 4). However, in the non-South sample the estimated effect

of a Democratic versus a Republican lame duck equals -13.6% (larger than when using the pooled sample in Table III) and is again statistically significant. Thus, including the South masks a somewhat greater difference in governor policy preferences in the rest of the nation. However, the office motivation still appears to dominate in the Non-South. In Brown states, Democratic lame ducks have a significantly lower growth rate of environmental spending than their Republican counterparts. Thus, the estimates in Table VI are similar to our baseline estimates. Overall, the results of this analysis suggest that our results are robust and not driven by these two different groups of states.

4. Conclusion

In this paper, we utilize parametric and nonparametric regression discontinuity approaches to assess the motivations of politicians who win the office of state governor. Our focus is state environmental spending. While there are no significant differences across political parties for governors eligible for re-election, lame duck governors set significantly different environmental policies. We find that governors set environmental policies consistent with them being primarily office motivated, but they also exhibit some degree of heterogeneous policy motivation across political parties.

Notes

[1]See "The Mind Behind the Muscles" by R. Lacayo, J. Ressner, and A. Leuker, *Time Magazine*, August 18, 2003.

[2]For example, Downs (1957) assumes that candidates are office motivated, while Wittman (1977, 1983) assumes they are policy motivated.

[3]Persson and Tabellini (2000, p.485) argue that "The tension between opportunism and partisanship reflects our imperfect understanding of ideology in politics." (Persson and Tabellini (2000) refer to policy and office motivated candidates as partisan and opportunistic, respectively).

[4]For studies of majority voting on environmental policy issues, see, e.g., Oates and Schwab (1988), McAusland (2003), Siqueira (2003), Cremer et al. (2004, 2008), and Roelfsema (2007).

[5]While policy motivation in Callander (2008) is similar to character in Kartik and McAfee (2007), there are some differences. In Callander (2008), candidates have preferences over the final policy outcome and not over campaign platforms, while in Kartik and McAfee (2007) candidates with character have direct preferences over platforms. In Callander (2008) a candidate's advantage from having policy motivation or character appears endogenously among voters.

[6]See, e.g., Dunlap et al. (2001) for evidence that a larger fraction of Democrats than Republicans supported increased spending for environmental protection during the 1973-1998 period. However, Buttel and Flinn (1978), e.g., find that Democrats and Republicans did not differ greatly in their concerns for the environment.

[7]This is a common assumption in the existing theoretical and empirical literature. For example, using a model with imperfect information, Besley and Case (1995) argue that a politician sets policy according to her preferences when no longer eligible to stand for re-election. They provide empirical evidence that lame duck governors are less concerned with their reputation than other governors, as the former group do not react to natural disasters to the same extent. List and Sturm (2006) provide a model where a lame duck governor no longer caters to voters with environmental concerns, but set policy at her own ideal point. Alesina (1988) suggests that in a one-shot game (such as the lame duck period) campaign promises lack credibility and the winning politician sets policy at her bliss point, while in a repeated game the politician is able to credibly move towards the median voter (see also Bernhardt et al. (2004)).

[8]Brown (2001) reports on overall state environmental spending for year 1986 to 2000. In year 2000, e.g., the Environmental Protection Agency (EPA) provided only 24 percent of state environmental funds.

[9]This is the reason why the parametric RDD is also called the control function approach. The difference between the OLS model and the parametric RDD model is whether or not $f(m_i)$ is controlled for in the

estimation, and not the actual estimation technique. Both models are estimated via OLS. For example, the difference between the OLS specification and the linear RDD specification is that the vote margin variable itself, m_i, is not controlled for in the OLS estimation. The RDD approach is analogous to the Heckman selection approach in the sense that both approaches explicitly model the relationship between the unobservable error and the treatment (here, D). However, the majority rule provides additional information since the party affiliation of an elected governor (the treatment) is a deterministic function of the vote share only. Equation (4) resulted from this information. This allows us to explicitly model the relationship between unobservable errors and party affiliation using a (flexible) function of vote share only. This is done without making any distributional assumptions, contrary to the Heckman selection approach.

[10]In other words, we estimate the flexible function as one function with a jump at the cut-off point of zero. Note that while we have only one function in the model, this does not imply that the underlying functions on both sides are necessarily the same. Instead, we can consider it as an approximation of a more complex underlying function; a composite of the functions on both sides.

[11]See, e.g., (Levinson, 2001, p.131-158) (data spans 1977-1994, except 1987) and Konisky (2007) (data spans 1985-2000) for state level measures of environmental policy stringency.

[12]35 states had gubernatorial term limits in year 2000. Thus, our data exploits the differences across states in term limit legislation, as well as the changing election eligibility status of incumbent governors in states with term limits. See Table 1 in List and Sturm (2006) for an overview of term limit legislation across states during the 1970-2000 period, and Lopez (2003) for a survey of the effects of term limits.

[13]We focus only on races where a Democrat and a Republican are the top two candidates because our interest is in estimating the difference in environmental policy between the Democratic (the treatment group) and Republican (control group) political parties across gubernatorial elections. Vote margins are constructed as the candidate's percentage of the vote share (minus 50 percent) that belongs to the top two candidates. If we were to include candidates from other political parties, our treatment variable is not well defined. For example, if the top two candidates were a Democrat and a third-party candidate, then the Democratic candidate's losing the election (i.e., winning a vote share below the cut-off point) does not mean that the Republican candidate wins the election. Our treatment, D, is then not equal to 1, nor is it 0.

[14]The results for bandwidths between $h = 2$ and $h = 15$ are highly similar in both Tables 2 and 3, and we therefore omit these estimates (results available upon request).

[15]Recall that there is also a trade-off in the non-parametric approach. As we restrict the sample to a smaller neighborhood, we find more consistent but potentially less precise results due to the decreasing sample size. That is, the insignificant results may be attributed to the sample size. However, we note that the standard errors do not increase dramatically as the sample size decreases. Moreover, in comparing the

results using the electable governors and lame-duck governor samples below, we can also see that the sample size is less likely to be the reason for the insignificant results observed here.

[16]We note that in a study of strategic interactions among states, Konisky (2007) reports several models indicating that in states with Democratic governors, the number of plant environmental inspections were significantly lower. Konisky (2007) does not adjust for vote margin, however.

[17]We recognize that time lags involved with budget decisions may affect these estimates. We believe such lags are likely to lead to under-estimates of the policy divergence across lame ducks.

[18]Recall that a bandwidth $h = 2$ implies that we include only the states where the Democratic candidate barely lost (with vote share greater than 48 percent), and the states where the Democratic candidate barely won (with vote share less than 52 percent). A bandwidth of $h = 2$ results in a sample size of 48.

[19]We believe there are a number of possible reasons why our findings differ from those of Besley and Case (1995), who find that Democratic lame ducks set significantly higher per capita total state taxes and expenditures. These include: (i) By utilizing a regression-discontinuity design, we account for the endogeneity problem to a greater extent than does Besley and Case's study; (ii) The dependent variables are different; Besley and Case study more aggregate measures; (iii) We use the growth rate of expenditures rather than levels; and (iv) The time periods differ. We also note that Millimet et al. (2004) (who extend Besley and Case's data set to 1999) find that Republican lame ducks raise overall state taxes and spending per capita more than do Democratic lame ducks.

[20]Strictly speaking, even the estimated effects in RDD estimation using the full sample are not necessarily *average* treatment effects. As the identification mainly comes from the cut-off point, without stronger assumptions the estimated effects can be interpreted as *local* average treatment effects for a certain sub-population. For further discussion of this issue, see, e.g., Imbens and Lemieux (2008) and Lee and Lemieux (2010).

References

Alesina, A. 1988. "Credibility and Policy Convergence in a Two-party System with Rational Voters." *American Economic Review* 78:796–805.

Angrist, J.D. and J. Pischke. 2009. *Mostly Harmless Econometrics*. Princeton University Press.

Ansolabehere, S. and J.M. Snyder. 2006. "Party Control of State Government and the Distribution of Public Expenditures." *Scandinavian Journal of Economics* 108:547–569.

Ansolobehere, S. and J.M. Snyder. 2000. "Valance Politics and Equilibrium in Spatial Election Models." *Public Choice* 103:327–336.

Aragones, E. and T.R. Palfrey. 2002. "Mixed Equilibrium in a Downsian Model with a Favored Candidate." *Journal of Economic Theory* 103:131–161.

Babcock, B.A., P.G. Lakshminarayan, J. Wu, and D. Zilberman. 1997. "Targeting Tools for the Purchase of Environmental Amenities." *Land Economics* 73:325–339.

Bernhardt, D., S. Dubey, and E. Hughson. 2004. "Term Limits and Portk Barrel Politics." *Journal of Public Economics* 88:2383–2422.

Besley, T. and A. Case. 1995. "Does Electoral Accountability Affect Economic Policy Choices? Evidence from Gubernatorial Term Limits." *Quarterly Journal of Economics* 110:769–98.

Besley, T. and A. Case. 2003. "Political Institutions and Policy Choices: Evidence from the United States." *Journal of Economic Literature* 41:7–73.

Brown, R.S. 2001. "States Put Their Money Where Their Environment Is." *Mimeo, The Environmental Concil of the States,* `www.ecos.org`.

Buttel, F.M. and W.L. Flinn. 1978. "The Politics of Environmental Concern: The Impacts of Party Identification and Political Ideology on Environmental Attitudes." *Environment and Behavior* 10:17–36.

Callander, S. 2008. "Political Motivations." *Review of Economic Studies* 75:671–697.

Cremer, H., P. De Donder, and F. Gahvari. 2004. "Taxes, budgetary rule and majority voting." *Public Choice* 119:335–358.

Cremer, H., P. De Donder, and F. Gahvari. 2008. "Political Competition Within and Between Parties: An Application to Environmental Policy." *Journal of Public Economics* 92:532–547.

Downs, A. 1957. "An Economic Theory of Political Action in a Democracy." *Journal of Political Economy* 65:135–50.

Dunlap, R.E., C. Xiao, and A.M. McCright. 2001. "Politics and Environment in America: Partisan and Ideological Cleavages in Public Support for Environmentalism." *Environmental politics* 10:23–48.

Groseclose, T. 2001. "A Model of Candidate Location when One Candidate has a Valance Advantage." *American Journal of Political Science* 45:862–886.

Hoxby, C. 2000. "The Effects of Class Size on Student Achievements: New Evidence from Population Variation." *Quarterly Journal of Economics* 115:1239–85.

Imbens, G. and T. Lemieux. 2008. "Regression Discontinuity Designs: A Guide to Practice." *Journal of Econometrics* 142:615–635.

Kartik, N. and R.P. McAfee. 2007. "Signaling Character in Electoral Competition." *American Economic Review* 97:852–870.

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. 2008. "Randomized Experiments from Non-random Selection in U.S. 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, D.S., E. Moretti, and M.J. Butler. 2004. "Do Voters Affect or Elect Policies? Evidence from the U.S. House." *Quarterly Journal of Economics* 119:807–859.

Levinson, A. 2001. "An Industry Adjusted Index of State Environmental Compliance Costs." In *Behavioral and Distributional Effects of Environmental Policy*, edited by C. Carraro and G.E. Metcalf, pp. 131–158. Chicago: University of Chicago Press.

Levitt, S.D. 1996. "How Do Senators Vote? Disentangling the Role of Voter Preferences, Party Affiliation, and Senator Ideology." *American Economic Review* 86:425–41.

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

Lopez, E.J. 2003. "Term Limits: Causes and Consequences." *Public Choice* 114:1–56.

Ludwig, J. and D.L. Miller. 2007. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *Quarterly Journal of Economics* 122:159–208.

McAusland, C. 2003. "Voting for pollution policy: the importance of income inequality and openness to trade." *Journal of International Economics* 61:425–451.

McCrary, J. 2008. "Manipulation of the Running Variable in the Regression Discontinuity Design: A Density Test." *Journal of Econometrics* 142:698–714.

McCrary, J. and H. Royer. 2011. "The Effect of Female Education on Fertility and Infant Health: Evidence from School Entry Policies Using Exact Date of Birth." *American Economic Review* 101:158–95.

Millimet, D.L., D.M. Sturm, and J.A. List. 2004. "The Economic Consequences of Electoral Accountability Revisited." *Mimeo, Southern Methodist University* .

Oates, W.E. and R.M. Schwab. 1988. "Economic competition among jurisdictions: efficiency enhancing or distortion inducing." *Journal of Public Economics* 35:335–354.

Persson, T. and G. Tabellini. 2000. *Political Economics*. Cambridge, MA: MIT Press.

Pettersson-Lidbom, P. 2008. "Do Parties Matter for Economic Outcomes? A Regression-Discontinuity Approach." *Journal of the European Economic Association* 6:1037–56.

Poole, K.T. and H. Rosenthal. 1984. "The Polarization of American Politics." *Journal of Politics* 46:1061–79.

Roelfsema, H. 2007. "Strategic delegation of environmental policy making." *Journal of Environmental Economics and Management* 53:270–275.

Roemer, J.E. 2001. *Political Competition: Theory and Applications*. Cambridge, MA: Harvard University Press.

Siqueira, K. 2003. "International externalities, strategic interaction, and domestic politics." *Journal of Environmental Economics and Management* 45:674–691.

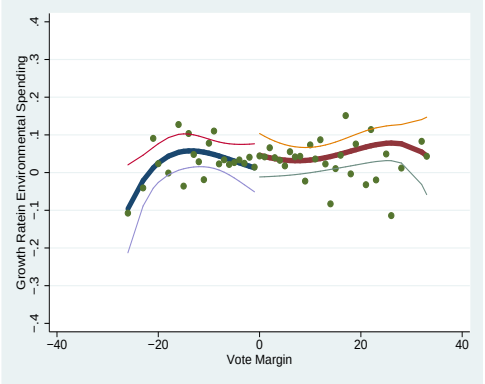
Warren, P. 2008. "State Parties and State Policies: A Double Discontinuity Approach." *mimeo, Clemson University* .

Wittman, D. 1977. "Candidates with Policy Preferences: A Dynamic Model." *Journal of Economic Theory* 14:180–89.

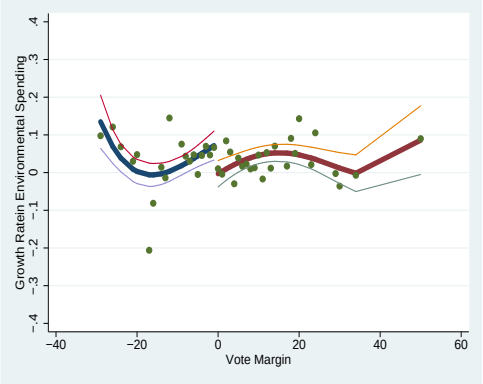
Wittman, D. 1983. "Candidate Motivation: A Synthesis of Alternatives." *American Political Science Review* 77:142–57.

Wu, J. and W.G. Boggess. 1999. "The Optimal Allocation of Conservation Funds." *Journal of Environmental Economics and Management* 38:302–321.

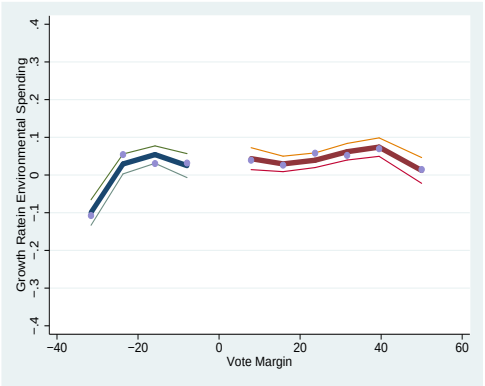
Wu, J., D. Zilberman, and B.A. Babcock. 2001. "Environmental and Distributional Impacts of Conservation Targeting Strategies." *Journal of Environmental Economics and Management* 41:333–50.



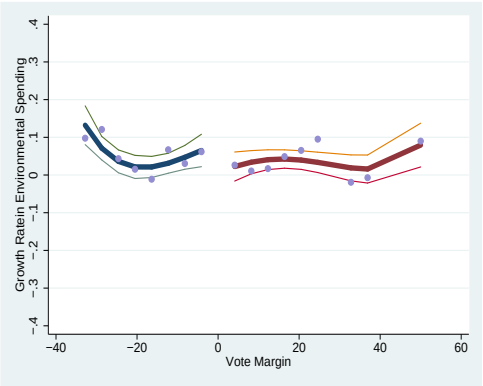
(a) Re-electable Governors: $h = 1$



(b) Lame Duck Governors: $h = 1$



(c) Re-electable Governors: $h^* = 7.9$



(d) Lame Duck Governors: $h^* = 4.1$

Figure 1: Estimates of Democratic Governors on Environmental Policy

Note: The thick lines are fitted values from fourth order polynomial regressions, and the thin lines represent 95 percent confidence intervals. h is the bandwidth; h^* is the bandwidth chosen by the cross-validation method (Ludwig and Miller (2007)). Negative (positive) vote margins imply that Republican (Democratic) governors are elected. The data points in the graphs are the averages of the growth rate of state environmental spending within each bin; see text for details.

Table I: Summary Statistics

	Full Sample	Republican	Democrat
Growth Rate of Environmental Expenditure Per Capita (in 1982-1984 dollars)	0.036 (0.168)	0.033 (0.167)	0.038 (0.169)
State Personal Income Per Capita	13008.79 (2425.674)	13392.12 (2431.479)	12718.62 (2382.087)
State Population	4.966 (5.215)	5.44 (5.888)	4.608 (4.613)
Percentage Population over age 65	0.118 (0.02)	0.121 (0.019)	0.116 (0.021)
Percentage Population aged between 5 and 17	0.206 (0.028)	0.203 (0.026)	0.209 (0.028)
Membership in Environmental Organizations	0.843 (0.357)	0.861 (0.35)	0.83 (0.361)
No. of Observations	1388	598	790

[1] Standard deviations in parentheses. See the text for detailed variable definitions.

Table II: Estimates of Democratic Governor on Environmental Policy: Re-Electable Governors

	(1)	(2)	(3)	(4)	(5)
		Panel A. Parametric Results			
	OLS Result	Parametric Regression Discontinuity Results			
		Linear	Quadratic	Triple	Quartic
		Panel A.1: Without Additional Controls			
Democrat	0.006	0.007	0.004	0.014	0.026
	(0.009)	(0.014)	(0.014)	(0.016)	(0.017)
No. of Obs	1028	992	992	992	992
		Panel A.2: With Additional Controls			
Democrat	0.005	0.007	0.004	0.013	0.025
	(0.009)	(0.014)	(0.014)	(0.016)	(0.017)
No. of Obs	1028	992	992	992	992
		Panel B. Nonparametric Results			
		$h = 2$	$h = 15$	$h = 20$	$h = 25$
Democrat		0.011	0.02	0.011	0.008
		(0.044)	(0.016)	(0.015)	(0.015)
No. of Obs		204	850	944	971

[1] Notes: Robust standard errors (clustered at the state level) in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variable in all estimations: Growth rate in state environmental spending per capita. State and time fixed effects are included in all parametric estimation specifications (Panel A). The results in Panel A.2 are obtained from the parametric specifications with additional covariates included. These covariates are state personal income per capita; state population; percentage population over age 65; percentage population aged between 5 and 17; membership in environmental organizations.

[2] Column (1) reports the OLS results. Columns (2)-(5) in Panel A report the parametric RDD results using control function approach with different order of polynomial terms. Columns (2)-(5) in Panel B report the nonparametric RDD results using the local linear regression approach. Columns correspond to different choices of bandwidths, h. The difference in the number of observations between Column (1) and Column (2) is due to missing information on the vote margin for some re-electable governors; e.g., some governors did not gain office through an election but due to other reasons.

Table III: Estimates of Democratic Governor on Environmental Policy: Lame Duck Governors

	(1)	(2)	(3)	(4)	(5)
		Panel A. Parametric Results			
	OLS Result	Parametric Regression Discontinuity Results			
		Linear	Quadratic	Triple	Quartic
		Panel A.1: Without Additional Controls			
Democrat	-0.016	-0.051***	-0.046**	-0.05**	-0.082***
	(0.012)	(0.019)	(0.018)	(0.021)	(0.022)
No. of Obs	360	360	360	360	360
		Panel A.2: With Additional Controls			
Democrat	-0.018	-0.046**	-0.041**	-0.045**	-0.078***
	(0.012)	(0.019)	(0.018)	(0.021)	(0.024)
No. of Obs	360	360	360	360	360
		Panel B. Nonparametric Results			
		$h = 2$	$h = 15$	$h = 20$	$h = 25$
Democrat		-0.092*	-0.055**	-0.064***	-0.052**
		(0.046)	(0.026)	(0.023)	(0.021)
No. of Obs		48	282	301	337

[1] Notes: Robust standard errors (clustered at the state level) in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Dependent variable in all estimations: Growth rate in state environmental spending per capita. State and time fixed effects are included in all parametric estimation specifications (Panel A).

[2] The results in Panel A.2 are obtained from the parametric specifications with additional covariates included. These covariates are state personal income per capita; state population; percentage population over age 65; percentage population aged between 5 and 17; membership in environmental organizations.

[3] Column (1) reports the OLS results. Columns (2)-(5) in Panel A report the parametric RDD results using the control function approach with different orders of polynomial terms. Columns (2)-(5) in Panel B report the nonparametric RDD results using the local linear regression approach. Columns correspond to different choices of bandwidths h.

Table IV: Estimates of Democratic Governor on Environmental Policy: Re-Electable Governors (*Future-Lame-Duck Sample*)

	Parametric		Non-Parametric
	Quartic	Quartic	$h = 2$
	(1)	(2)	(3)
Democrat	0.097	0.095	0.112
	(0.086)	(0.086)	(0.103)
State Fixed Effects	Yes	Yes	
Time Fixed Effects	Yes	Yes	
Other Controls	No	Yes	
No. of Observations	278	278	58

[1] Notes: Robust standard errors in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[2] Columns (1) and (2) report the RDD results using the control function of quartic polynomials without and with covariates, respectively. Column (3) reports the RDD results using nonparametirc results with the bandwidth $h = 2$. The estimation utilizes only the sample of *Future-Lame-Duck* governors in the non-lame duck periods.

[3] Dependent variable in all estimations: Growth rate in state environmental spending per capita.

Table V: Specification Tests of Random Assignment Assumption of RDD

	Estimates	
	Re-Electable	Lame Duck
Panel A: Balancing Tests on Observed Covariates		
State Personal Income Per Capita	-431.236	685.283
	(514.002)	(1031.67)
State Population	-1.498	-1.172
	(1.365)	(1.508)
Percentage Population over age 65	-0.001	0.004
	(0.004)	(0.009)
Percentage Population aged between 5 and 17	0.006	-0.008
	(0.006)	(0.012)
Membership in Environmental Organizations	0.034	0.04
	(0.072)	(0.098)
Panel B: McCrary (2008) Log Density Tests		
Log Discontinuity Estimates	-0.174	-0.122
	(0.112)	(0.190)

[1] Standard errors in parentheses. All discontinuities are estimated with a regression of the variable on 4th order polynomial terms of margin variable and an indicator of democratic governor. The coefficients and standard errors in the table are those of the indicator variable.

Table VI: Heterogeneity Results: The Effect of Democratic Governor on Environmental Policy

	Re-electable	Lame Duck	Re-electable	Lame Duck
	(1)	(2)	(3)	(4)
	Non-Southern States		Brown States	
Democrat	0.03	-0.136**	0.017	-0.076***
	(0.02)	(0.056)	(0.019)	(0.026)
No. of Observations	740	163	832	330

[1] Notes: Robust standard errors (clustered at the state level) in brackets. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

[2] Dependent variable in all estimations: Growth rate in state environmental spending per capita.

[3] All the estimation is based on forth-order polynomials. In addition to state and time fixed effects, the following covariates are included in the estimation: state personal income per capita; state population; percentage population over age 65; percentage population aged between 5 and 17; membership in environmental organizations.

[4] Southern states are Alabama, Arkansas, Delaware, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, West Virginia.

[5] The Green states are California, Colorado, Connecticut, Maine, Massachusetts, New Hampshire, Oregon, and Vermont. Brown states are the non-green states.