PARTIES, POLITICS AND REGULATION: DO REPUBLICAN CONGRESSMEN REDUCE LOCAL ENFORCEMENT OF CLEAN AIR LAWS?

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Abstract

This paper studies the extent to which local Federal regulation responds to the preferences of local Congressional representatives. We use facility-level data over 1989-2005 to investigate the causal effect of a local U.S. Congressional Representative's party affiliation on the intensity of EPA enforcement of Clean Air laws in their local Congressional districts. Random assignment of electoral outcomes is obtained with a Regression Discontinuity design. In contrast to a popular view that regulation is driven by regulatory capture, we find that the individual Congressperson has a significant impact on rates of Clean Air Act inspection against local polluting facilities. New Republican (vs. Democratic) Representatives are estimated to significantly depress inspection rates in the first year after their election.

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Parties, Politics and Regulation:

Do Republican Congressmen Reduce Local Enforcement of Clean Air Laws?

1. Introduction

Federal administrative agencies make a variety of decisions that affect local firms and interests. A good example is the U.S. Environmental Protection Agency's choice of how often to inspect a local polluting facility for compliance with Federal pollution control laws. In this paper, we study how these enforcement decisions respond to the preferences of local Congressional representatives. Using data on enforcement of the Clean Air Act, we find a striking responsiveness of local EPA facility-level inspections to the party affiliation of the local Congressional representative (Democrat vs. Republican).

The nature and impact of political pressure on regulatory decision-making has been widely studied in economics and political science (Stigler, 1971; Weingast and Moran, 1983; Meier and O'Toole, 2006). Much of this literature studies the design of regulatory institutions, trying to understand when, why and to what extent legislative authority is delegated to the bureaucracy. We instead are interested in what influences bureaucratic decisions, given the powers that have been vested in regulators. Such decisions may be at the policy level, including rule making by regulatory agencies (e.g., Yackee and Yackee, 2006). However, much regulatory discretion is "on-the-ground," as in our case of environmental law enforcement.

The impact of political forces on local / on-the-ground enforcement of Federal regulation has been considered in a surprisingly small set of research papers. Scholz, Tombly and Headrick (1991) study the impact of local, State and Congressional political representatives on county-level OSHA enforcement in New York state from 1976-85; they find that more liberal (Democratic) Congressional representation is associated with more intensive local OSHA enforcement. Kleit, Pierce and Hill (1998) consider the impact of State legislators on Louisiana state enforcement of water pollution laws in 1993-4; they find that a local State legislator's membership on the Legislature's environmental oversight committees is positively related to the severity of plant-level penalties assigned in water-related enforcement actions. In his study on the enforcement of Federal water pollution laws, Helland (1998) considers how local Congressional representation on House and Senate environmental oversight committees affects plant-level inspections in the pulp and paper industry over 1989-93; he finds that committee membership is associated with reduced enforcement intensity while the committee member's environmental preference (as measured by a League of Conservation Voters ranking) favors greater enforcement intensity.[1]

While these papers all suggest that preferences of political representatives are correlated with local enforcement of Federal laws, these effects may be due to correlation between local constituent preferences and those of their political representatives as opposed to a *causal* link between the representatives and on-the-ground enforcement. That is, these papers do not address the potential endogeneity of the political variables, with unobservables (such as constituent preferences) potentially driving both attributes of the political representatives and enforcement outcomes. The purpose of this paper is to investigate *causal* effects of political representation, accounting for its potential endogeneity.

We gauge the preferences of the local Members of Congress using their party affiliation, Democrat vs. Republican. Substantial literature documents that party affiliation is highly correlated with policy preferences and voting behavior of Congressional representatives (Besley and Case, 2003; Lee, Moretti and Butler, 2004). Lee et al. (2004) find that not only does party

[1] There are much larger literatures on Congressional politics and overall environmental policy (e.g., Shipan and Lowry, 2001) and the "race to the bottom" in local environmental regulation (e.g., Konisky, 2007). We focus instead on how Congressmen affect "on the ground" / "street level" enforcement of Federal clean air laws.

affiliation drive Congressional voting behavior (so that voters *elect* policies), but that margins of victory have negligible effects on this behavior (so that voters do not *affect* policies per se). List and Sturm (2006) document a counterpoint to these results by showing that electoral incentives are important in driving politicians' choice of secondary policies – environmental spending in particular. However, their results do not suggest that party affiliation is unimportant as an indicator of policy preference. Indeed, Fredriksson, Wang and Mamun (2011) find that electoral incentives drive re-electable governors to the middle in determining natural resource spending, but that lame-duck governors exhibit significant party-related preferences for this spending. Overall, this research suggests that party affiliation is a good indicator of a Congressperson's policy preferences. Our question is: Do these preferences also play a role in driving bureaucratic decision-making in a Congressperson's district?

The central econometric challenge concerns the potential endogeneity of electoral outcomes, the issue that preoccupies the modern literature on effects of political parties (e.g., Lee, Moretti, and Butler, 2004; Lee, 2008; Ferreira and Gyourko, 2009; Fredriksson, Wang and Mamun, 2011; Pettersson-Lidbom, 2008). These papers exploit a Regression Discontinuity design to identify impacts of party affiliation on policy outcomes. We borrow this approach by focusing on regulatory outcomes in districts that had close Congressional elections. In close elections, random events (such as bad weather that is well known to favor Republican candidates) can tip an election in one direction or another, making the outcome randomly assigned. However, in our data (over 1989-2005), we find that the "close election" criterion is not sufficient to ensure random electoral outcomes (see Caughey and Sekhon, 2011, and Grimmer et al., 2011, for related critiques). Because incumbents win with extraordinarily high probability even in close elections, we focus on elections that are *both* close (with margins of victory less than 2.5 percent) and open (with no incumbent in the running). In what follows, we present detailed evidence on random assignment of electoral outcomes (Democrat vs. Republican) in the close-open data.

The close-open identification strategy focuses our study on arguably the least influential Members of Congress, those who have just been elected to open U.S. House seats. We find that the preferences of even these brand new Congressional representatives matter a great deal for local enforcement of Federal environmental laws.

Our empirical focus on Clean Air Act (CAA) enforcement is motivated by an extensive empirical literature on the determinants and effects of environmental enforcement activity (see Gray and Shimshack, 2011, for a recent survey). On one hand, governmental pressure for environmental performance – predominantly in the form of environmental inspections and the enforcement actions that can result from them – are consistently cited as the strongest influence on firm managers' choices of environmental strategies, including costly investments in staffing, audits, and internal operating protocols (Gray and Shimshack, 2011; Khanna and Anton, 2002). Enforcement can also ignite adverse public reaction in the media, by NGO's, and in financial markets.[2] As a result, CAA inspection rates are of consequence to local businesses. On the other hand, a well established result in the literature is the positive effect of environmental enforcement on pollution prevention (Magat and Viscusi, 1990; Gray and Shadbegian, 2007; Shimshack and Ward, 2008; Gray and Shimshack, 2011). As a result, CAA inspections are of consequence to the local environment. This dual importance of environmental inspections has fueled an empirical literature on what drives them. For example, environmental inspection rates

[2] Hamilton (1995) studies the impact of toxic release announcements on the media and stock prices. Gupta and Innes (2011) find that environmental inspections have a positive effect on the likelihood that a firm is targeted for an environment-related boycott or shareholder action. Innes and Sam (2008) document that firms participate in voluntary pollution reduction programs, and adopt effective but potentially costly environmental management programs, at least in part in order to obtain the regulatory benefit of a reduced inspection rate.

have been shown to respond to local economic conditions (Gray and Deily, 1991, 1996), reductions in a firm's pollutant releases (Decker, 2005; Helland, 1998), and a firm's participation in a voluntary pollution reduction program (Innes and Sam, 2008).

Estimating political impacts on CAA enforcement (our purpose) is therefore important in the narrow sense of understanding effects of pollution regulation, but potentially also in a broader sense of understanding the political economy of regulation. While we do not focus on the theory of on-the-ground regulation in this paper, our results have implications for crafting such a theory from three current schools of thought: (1) the "capture school" of Stigler (1971) and Peltzman (1976), and more recent common agency models of lobbying (Grossman and Helpman, 1994, 1996, 2002), in which special interests capture regulators with policies and decisions essentially up for sale; (2) the "minimal squawk" model of Leaver (2009), where regulators seek to avoid regulated firm "squawks" that can bring a regulator's mistakes to light; and (3) the theory of bureaucratic choice attributable to Niskanen (1971), where regulatory agencies bargain with legislators for larger budgets in exchange for regulatory decisions more tilted toward legislator preferences.[3] While empirical support for aspects of the first two theories has been found in State-level regulation of telecommunications (Figueiredo and Edwards, 2007) and public utilities (Leaver, 2009), neither admits a significant role for political preferences. At least with respect to Clean Air enforcement, our results thus suggest relevance of an adapted Niskanen (1971) model in which individual legislators can affect the decision-making of local regulators. Two properties of such an adaptation are suggested by our work: (1) individual Members of Congress have preferences over how local environmental enforcement is conducted (favoring more vigorous enforcement, for example, because the Congressman is more pro-

[3] Consistent with Niskanen's (1971) view, Coate (2002) identifies broad political impacts (of the party in power in the White House or in the Congress) on Federal Trade Commission merger policy.

environment, or less vigorous enforcement because the Congressman is more pro-business); and (2) individual Members of Congress, as opposed to *only* the most influential Members, are of consequence in the calculus of environmental regulators.

Both implications are potentially important for understanding the political / regulatory process, at least in the environmental context. The first suggests policy divergence across parties – versus Downsian (1957) policy convergence – along the environmental enforcement dimension. Generally speaking, policy divergence is a highly debated phenomenon in the political science literature (e.g., see Shipan and Lowry, 2001). The latter, however, focuses on issues broadly in the public eye, whereas we study regulatory enforcement decisions that are one-step removed from politicians and potentially, therefore, even more subject to suasion from lobbying activity.

On the second implication, why might regulatory authorities respond to a local Congressman's preferences in making their enforcement decisions? One possible answer comes from a Niskanen (1972) type model that is described in our on-line Appendix. The model implicitly captures an on-going and repeated relationship between the EPA (the environmental regulator) and the Congress (which authorizes EPA spending), treated as a bargaining interaction. The EPA proposes an overall budget and an inspection rate for each Congressman's district in an environment where individual Congressmen want the EPA to know their inspection preferences (in order to elicit EPA accommodations with these preferences on the inspection margin) and do not want to convey their precise preferences over the EPA's overall budget. The latter motivates an EPA interest in accommodating each and every Congressman on inspection choices, rather than only the few Congressmen on the margin between a "yes" and "no" vote on the budget proposal. In this setting, the EPA wants to raise the likelihood of each individual

Congressman's support for greater EPA funding by implicitly offering the Congressman a local environmental inspection rate closer to his or her most-preferred inspection target. This "bargain" raises the Congressman's incentive for a positive vote by raising the price of reversion to a status quo budget (and EPA-preferred inspection rates) in the event that the EPA budget proposal is defeated.

2. Preliminary Evidence

2.1 Summary Comparisons

We begin by presenting some preliminary evidence that local Congressional representatives' party affiliations may indeed be relevant to local environmental inspections. Figure 1 graphs average facility-level inspections and enforcement actions under the Clean Air Act (CAA), over our study period, for facilities in two types of areas. The first has all Democratic Congressional representatives (the two Senators and one Representative), and the second has all Republican Congressional representatives. The Figure reveals a consistent pattern of higher enforcement scrutiny in areas represented by three Democrats vs. three Republicans. Perhaps this could be explained by higher average facility-level pollution in the Democratic areas, which would motivate more enforcement attention to those facilities. However, this explanation is belied by Figure 2, which shows that the same sets of facilities have higher average toxic pollution levels (measured by toxicity-weighted releases of CAA-regulated chemicals reported in the Toxic Release Inventory) in the all-Republican areas than in the all-Democratic areas.

Table 1 presents a second set of comparisons. Consider a change in Congressional representation from all-Republican (3R) to two Republicans and one Democrat. Does the addition of a Democratic representative elevate environmental enforcement and lower pollution? The first panel compares average (per-facility) enforcement effort and weighted toxic CAA-regulated releases before and after the electoral changes in these districts. The statistics suggest that the addition of a Democrat is indeed associated with an increase in enforcement and a reduction in pollution. However, consider second a change in Congressional representation from all-Democrat (3D) to two Democrats and one Republican. Does the addition of a Republican representative reduce environmental enforcement and raise pollution? The second panel of Table 1 gives a somewhat mixed picture of the answer. Enforcement falls after one year under the new Congressional regime, but rises after two years; neither effect is statistically significant.

Both sets of evidence provide a preliminary clue (albeit only suggestive) that party affiliation of local Congressional representatives – and associated preferences of these representatives over local enforcement outcomes – may be relevant to local CAA enforcement intensity. A more careful study of these potential impacts follows.

2.2 A Preliminary Empirical Analysis: Data

We first construct a comprehensive unbalanced facility-level panel dataset over the years 1989-2005. The time period includes both Republican Presidential administrations (from 1989-1992 and 2001-2005) and Democratic Presidential administrations (from 1993-2000), as well as periods of both Republican majorities in the House of Representatives (1995-2005) and Democratic Congressional majorities (1989-1994). The panel includes all facility-year observations for which we have complete data. Restricted versions of this dataset, associated with the Regression Discontinuity approach, are discussed in detail in Section 3.

Dependent Variable. Our endogenous variable measures the extent of a facility's regulatory scrutiny under the Clean Air Act (CAA). We use either the number of times a facility was inspected for CAA compliance in a given year or a zero-one variable for whether or not a facility was inspected under the CAA. The EPA's Air Facility System (AFS) dataset provides each regulated facility's yearly numbers of inspections and enforcement actions, as well as the facility's zip code, county, state, and primary SIC code for the industry. Zip codes are used to tie facilities to Congressional districts and counties.[4]

Political (Explanatory) Variables. Using the U.S. Congressional Biography and the Wikipedia website, we collected political data on the party affiliation of U.S. Representatives from each Congressional district and U.S. Senators from each State from the 101st US Congress (1989-90) to the 109th (2005-06). Data on electoral vote margins in Congressional elections was obtained from the Clerk of the House.[5] From data collected by Gary Jacobson, we determined open seats (elections in which no incumbent was running).[6]

The central political variable of interest is a zero-one dummy variable indicating whether a facility's local Congressional Representative is Republican (one) or Democrat (zero). Effects of Senatorial representation are measured by a second zero-one dummy variable indicating whether the facility is in a State with at least one Republican U.S. Senator (one) or not (zero).[7]

Other Explanatory Variables. A key determinant of inspection frequency and likelihood is a facility's environmental performance in the past year (Gray and Shimshack, 2011). To measure environmental performance, we use a facility's toxicity-weighted lagged releases of TRI-reported CAA-regulated air pollutants as reported to the EPA's Toxic Release Inventory

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[4] We use information from the Missouri Census Data Center to match by zip code. A small number of facilities are located in zip codes that fall in more than one Congressional district. To enable coherent definition of our political variables, we omit these facilities from our data.

[5] Congressional election data is available from: http://clerk.house.gov/member_info/electionInfo/index.html

[6] The Jacobson data (out of U.C. San Diego) is widely used in political science research (see, for example, Jenkins and Monroe, 2012; Carson, et al., 2010).

[7] We considered a variety of other indicators for Senatorial party affiliation, including affiliation of junior and senior Senators, respectively; qualitative conclusions and model performance are similar to those with the simple indicator reported here.

(TRI). The TRI was begun by the EPA in 1988 under the Emergency Planning and Community Right to Know Act (EPCRA); the EPCRA requires all facilities of a minimal size to report any releases of a large set of named chemicals to the TRI. For consistency, we aggregate releases of the 170 toxic chemicals that are regulated under the CAA and reported under the TRI throughout our study period.[8] Many facilities appearing in the AFS dataset never appear in the TRI dataset, whether because they are not required to report under the EPCRA or because they have no TRI chemical releases. When including lagged toxic releases in our model, we therefore restrict attention to facilities that are common to both AFS and TRI datasets. We also restrict attention to facilities that are located in one of the 50 States of the U.S. In the merged (AFS-TRI) dataset, we have data on 17,635 facilities with an average number of year-observations per facility of 8.6. The total number of facilities in the AFS dataset is 84,101 and the average number of year-observations per facility is 7.3.[9]

While higher levels of lagged pollutant releases can promote more inspection scrutiny, so too can lagged enforcement actions that require follow up inspections (e.g., see Innes and Sam, 2008). Using the AFS data, we construct a lagged dummy variable for whether or not a facility was subject to a CAA enforcement action in the prior year.

Other socio-economic and demographic variables can be important determinants of environmental inspections. For example, Gray and Deily (1996) document the importance of economic pressures, with larger employers in high unemployment areas subject to less enforcement scrutiny. Per capita incomes can affect local preferences for environmental

[8] Toxicity weights can be found at www.epa.gov/oppt/rsei/pubs/toxwght97.pdf. Weighted releases are constructed as follows. Let m (=170) be the number of CAA regulated chemicals reported in the TRI; let x_i be a facility's release of chemical i (less than or equal to m) in a given year; and let w_i be chemical i's toxicity weight. The toxicity weighted average release by the facility is then given by $R_i = \sum_{i=1}^{m} w_i x_i \Big/ \sum_{i=1}^{m} w_i$.

[9] Due to the large number of facilities in our dataset, it is essentially impossible to tie our EPA data to firm-level financial data available from Compustat.

regulation and oversight, as well as local pressure for favorable environmental conduct by local facilities. More dense local populations can impact the sensitivity of the local public to local facilities' environmental performance. All of these forces can alter incentives for government inspections. To capture these effects, we construct annual county-level per capita income, unemployment rate and population density over our study period using data from the Bureau of Labor Statistics.

Strict environmental liability statutes (vs. weaker negligence statutes) can elevate incentives for favorable facility-level environmental performance and thereby reduce the need for enforcement oversight. We therefore include an annual indicator for whether or not a State has a strict environmental liability statute (using data from the Environmental Law Institute).

Finally, local environmental views of the public can also affect enforcement incentives. For example, more "environmentalist" constituencies may either promote government enforcement or substitute for it (the latter found in Innes and Sam, 2008, for example). We therefore include State-level per capita Sierra Club membership.

Table 2 presents variable definitions and summary statistics for the integrated data. The table reveals that sample TRI reporters are subject to a slightly greater number of CAA inspections and enforcement actions; statistics for other control variables are similar across the two datasets (AFS and AFS-TRI).

2.3 Preliminary Model and Results

Table 3 presents regression results for a preliminary model of facility-level inspection counts (our dependent variable) using the full AFS-TRI data and treating the local Congressional representative's party affiliation (our treatment) as exogenous. In Section 3 – where we present our main analysis – we consider a Regression Discontinuity approach that accounts for the likely endogeneity of local electoral outcomes. Table 3 presents (i) count panel models that account for individual (random) facility effects (model (1)) and cluster the errors at a State level to account for covariation across facilities within a State and across time for each facility (model (2), Bertrand, et al., 2004),[10] and (ii) linear models that account for individual (fixed and random) effects and again cluster the errors at a State level.[11]

In all of the Table 3 estimations, both the Republican House Member dummy and the dummy for at least one Republican Senator have negative coefficients. The Senatorial dummy is statistically significant in all models, although only at the ten percent level in the linear models. The House dummy is statistically significant in all but the linear fixed effects model. Estimated proportional marginal effects of the House Republican dummy range from negative 1.13 percent (in the linear fixed effects model) to negative 5.67 percent in the Negative Binomial. Estimated proportional marginal effects of the Senatorial Republican dummy are much larger and consistent across the models, ranging from negative 29.25 percent to negative 32.94 percent.

Consistent with expectations, lagged enforcement actions have a significant positive impact on inspection intensity. Per capita Sierra Club membership and per capita incomes both have significant negative effects on inspection counts, consistent with prior results in the literature (e.g., Innes and Sam, 2008) and with the conjecture that environmental constituencies serve as a substitute for environmental law enforcement. Strict liability statutes are also associated with lower inspections, but statistical significance of this effect vanishes when accounting for cross-observation correlation.

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[10] For the first count model, we use the Poisson random effects estimator. For the second, we use the Negative Binomial in order to avoid the equi-dispersion restriction imposed by the Poisson (but not the Negative Binomial or the Poisson random effects models); the restriction is rejected in statistical tests. A zero-inflated Poisson (random effects) estimation did not converge.

[11] Because our data represents a sample rather than the entire population of regulated facilities, there is an arguable preference for random (vs. fixed) facility effects (Nerlove, 1971; Greene, 2003). However, in the linear models, the Hausman test rejects random (vs. fixed) effects. When modeling the facility effects as random, we incorporate State and industry (two-digit SIC) fixed effects. In all cases, we incorporate time fixed effects.

3. Endogenous Electoral Outcomes and the Close-Open Data

The Issue. So far, we have assumed that Congressional party affiliation (as an indicator of the member's preferences) is exogenous. However, it is likely that unobservable variables drive both electoral outcomes (Democrat vs. Republican) and environmental regulation in a district. For example, pro-environment preferences of the public may favor both Democratic representation and greater regulatory scrutiny of environmental performance in a district. Conversely, higher inspections may spur businesses to promote Republican candidates and thereby tilt elections in their favor. Stated differently, one can conceive of any number of potential omitted variables in almost any specification of our Table 3 regressions; to the extent these variables are correlated with our key Congressional dummy, we may expect omitted variable / endogeneity bias.[12]

In view of this possibility, we seek to identify the effect of party affiliation by using a Regression Discontinuity (RD) design that yields an unbiased estimate of the treatment effect, regardless of the model specification. Following prior practice (e.g., Lee, et al., 2004: Ferreira and Gyourko, 2009; Fredriksson, et al., 2011; Pettersson-Lidbom, 2008), this is done by focusing on facilities and districts in which the electoral outcome (Democrat vs. Republican) is randomly assigned and therefore exogenous by construction. Sufficiently close elections presumably have this property, with outcomes determined by random events (such as weather) that tip the election in one direction or the other.

However, even in close elections, electoral outcomes are far from random in our dataset. Let us define a close election as one in which the margin of victory is less than 2.5 percentage points (so that Republican and Democratic vote shares are within 2.5 percent of those needed for victory). For these Congressional elections, over our 1989-2005 study period, incumbents won

[12] These might include firm compliance histories and/or preferences of other local politicians, for example.

83.1 percent of the elections in which they were running. Defining a close election more narrowly, as one in which the margin of victory is less than 1.5 percentage points, incumbents won 80.6 percent of the elections in which they were running. Clearly, in these elections, outcomes are not randomly assigned. Incumbent party affiliation is presumably driven by any unobservables that are the putative source of endogeneity; moreover, in our data, incumbency – even in close elections – largely determines electoral outcomes (see also Caughey and Sekhon, 2011, and Grimmer et al., 2011).

We therefore focus instead on elections that are both close and open, that is, in which there is not an incumbent running for office. We consider open-seat Congressional elections in which the margin of victory was less than 2.5 percent – 64 elections in our sample. In these elections, 51.6 percent (33) were won by the party holding the seat in the prior Congress. This is about as close to a coin flip as one could hope for! The proportion of elections that were won by Republicans is also almost identical in seats originally held by Democrats (57.1 percent of 28 elections) and seats originally held by Republicans (58.3 percent of 36 elections).

The RD Design. Regression Discontinuity (RD) designs take a variety of forms (see, for example, Fredriksson, et al. (2011) and Pettersson-Lidbom (2008), for excellent discussions). Our baseline approach is to focus only on close open data for which we have evidence that electoral outcomes are randomly assigned, estimating a model of the form,

$$Y_{it} = \alpha + \gamma C_{it} + \beta' X_{it} + \varepsilon_{it}, \tag{1}$$

where Y is an index function that determines environmental inspections, C is the Republican Congressional dummy (our treatment), X is an exogenous set of covariates (including State, time and industry effects), and ε is a random disturbance. We estimate binary (Probit) and count (Poisson) models of inspections that take the form of equation (1) (for respective index functions) and use the close (+/- 2.5 percent) open data. These baseline results are reported in Tables 5 and 6. While we include varying sets of covariates for the sake of precision, the validity of the estimation strategy hinges only on random assignment of the treatment, not on the covariate specification.

In defining the margins of "closeness," the analyst faces a tradeoff (as one widens the band) between efficiency and potential specification error / bias. A variety of robustness checks are therefore performed on the baseline regressions. First, we consider models with more and less parsimonious sets of controls (see Tables 5 and 6). Second, we consider narrower definitions of close, with the close open data defined by no more than 2 percent and 1 percent margins of victory (Table 7). Third, with larger margins, there is a concern that the margins themselves (rather than the electoral outcome) may be driving results. Following Imbens and Lemieux (2008) and Fredriksson, et al. (2011), we estimate models of the treatment effect using local linear regressions that add two regressors to the equation (1) model: v_{it} (vote margin) and $v_{it}*C_{it}$ (vote margin times the treatment dummy).[13] We implement the local linear models using both the (+/-) 2.5 percent and (+/-) 2 percent margin data, as reported in Table 8.

Fourth and finally, many RD analyses employ the control function approach that exploits all available data and estimates the treatment effect by controlling for a flexible functional form in the running variable, in our case the relative vote share (see, for example, Fredrikkson, et al., 2011; Petterson-Lidbom, 2008):

(2) $$Y_{it} = \alpha + f(s_{it}) + \gamma\, C_{it} + \beta' X_{it} + \varepsilon_{it},$$

[13] The vote margin is defined as the difference between the Republican vote share and the Democratic vote share.

s_{it} is the relative Republican vote share and f() is a polynomial in vote share of order three, four, or five.[14] In all cases, we limit attention to open seats (for which we have random assignment at the margin), but do not restrict the data by vote margin. The benefit of this approach is efficiency (due to an expanded dataset); the cost is potential specification error in the flexible functional form. A final check is the Ferreira and Gyourko (2009) control function model that adds to equation (1) a right-side cubic polynomial in the vote margin, $f(v_{it})$, and the interactions, $f(v_{it})*C_{it}$.[15] The control function checks are reported in Table 9.

Before turning to the regressions, we discuss the close open (+/- 2.5 percent) data, its relationship to the overall sample, and balancing checks. This is followed with a preliminary graphical presentation of the regression discontinuity, obtained by fitting fifth order polynomials in vote margin on either side of the discontinuity (v_{it}=0). Finally comes our main evidence, the regression tables. In all figures and regressions, we focus on regulatory outcomes in the year immediately after each open seat election.

The Close Open Data. Table 4 gives summary statistics for the close-open (2.5 percent) data. There are only two noticeable differences between the close-open and full samples (comparing Tables 2 and 4). First, average population density for the close open sample is lower than for the full sample. Second, the close open sample draws more from the Northeast and less from the South than the full sample. Other indicators are similar in magnitude. In both samples, facilities are inspected, on average, approximately once per year with an average frequency of approximately 60 percent. Unemployment rates average slightly more than five percent over the

[14] The relative Republican vote share s is the ratio between the raw Republican vote share and the sum of the Republican and Democratic vote shares. The Congressional dummy C measures the jump at s=50, with our data limited to open seat districts in which the Republican and Democrat were the top two vote getters.

[15] When including treatment interactions (such as f(v)*C), the coefficient on the Congressional dummy (γ) measures the treatment effect at the jump only when the running variable equals zero at the jump. Vote margins, rather than vote shares, must therefore be used for running variable controls in these models.

study period. Per capita incomes are approximately $25,000 per year. Facilities are represented in the House of Representatives by Republicans in roughly 55 percent of the cases, with a slightly higher Republican representation in the close open data than in the full sample. Facilities have at least one Republican Senator in roughly 70 percent of the cases.

Random Assignment. Even in the close open dataset, we find evidence that State-level unobservables are correlated with both electoral and regulatory outcomes. Table 4 presents summary statistics for the overall close (+/- 2.5%) open dataset, broken down by observations in Democrat-won and Republican-won districts. If we have pure random assignment of the electoral outcome, there should be no correlation between the Democrat vs. Republican outcome and prior regulatory outcomes or other district attributes. Measuring the district attributes (Sierra Club membership, unemployment, per capita incomes, etc.) with district / election level observations, we find no evidence of correlation. We also consider local political circumstances, including the State's relative Republican vote share in the most recent Presidential election (as a proportion of the two-party vote), and the party affiliation of the State's governor, the State legislature, and the prior Congressman; none of these indicators is correlated with the electoral outcome of interest. Nor is there evidence of correlation for lagged releases or lagged enforcement actions. However, lagged CAA inspections – which are naturally measured at the facility level – are significantly different between the Democrat and Republican won districts. The Republican-won areas have significantly higher levels of lagged environmental inspection. This correlation persists with finer definitions of "close" (1.5 percent margins, for example).

We expect to see significant State effects on environmental regulation for a variety of reasons, most importantly due to the EPA's pervasive (but selective) delegation of regulatory enforcement to State authorities. In view of this expectation, we examine whether we have random assignment once we net out State effects. We do this by constructing deviations of the inspection lags from corresponding State-averages taken from our entire AFS dataset (of 84,101 facilities and 617,546 observations). Table 4 reveals no significant difference in the pre-election inspection deviations (net of State averages) for the Democrat and Republican won districts.

In summary, once we control for State effects, we have evidence of random assignment of electoral outcomes in the close open data. In what follows, we therefore control for State effects when evaluating the impact of Congressional electoral outcomes on regulation.

RD Graphs. We begin with a standard graphical depiction of the treatment effect. We present fitted values for CAA facility-level inspections (Figure 3) and lagged inspections (Figure 4) and associated 95 percent confidence intervals for varying vote margins, using estimated fifth order polynomials on both sides of the discontinuity (v_{it}=0), controlling for State effects.[16] Figure 4 provides a visual falsification check, confirming that *pre*-election inspections do not exhibit a significant jump at the winner (v_{it}=0) threshold. In contrast, Figure 3 reveals a significant negative jump in *post*-election inspections as one moves from a Democrat victory (v_{it}<0) to a Republican victory (v_{it}>0). The estimated effect of a Republican Congressman – the jump in Figure 3 – is to reduce the average number of inspections by .22 (roughly 22 percent of the overall sample mean from the open seat data). This estimate is larger than the simple difference at the bottom of Table 4, but strikingly similar to the analog from our main close open regressions below (Table 6). The figures also present inspection averages, purged of State effects, for various vote margin bins (for example, margins between -2 to 0 percent, 0 to 2, - 4 to -2, 2 to 4, and so on); these "actuals" roughly track the fitted polynomials.

As stressed by Pettersson-Lidbom (2008) and others, the RD approach permits a causal interpretation of electoral outcomes – and their effects on environmental enforcement – only at

[16] In Figures 3 and 4, State effects are evaluated at sample means.

the discontinuity between Democrat and Republican won elections. That is, Figures 3 and 4, as well as the control functions estimations (Table 9), reveal causal effects at the zero-vote-margin (50 percent share) discontinuity and not of vote margins / shares themselves. The shapes of the fitted counts in Figure 3 are therefore irrelevant for inference.

Evidence from the Close Open Data. Using the close open (+/- 2.5 percent) data, our main (benchmark) RD evidence is drawn from equation (1) models that control for a variety of key determinants of inspection. These controls permit much more precision in measurement of the Congressional party effect vis-à-vis the simple differences presented in Table 4, but again we stress that the absence of other possible controls does not compromise the causal interpretation of our treatment (the logic of random assignment). Table 5 reports results from Probit regressions of facility-level inspection outcomes (zero if no inspections, one if positive inspections) on the electoral outcome in the close-open elections, State dummies, and assorted covariates in different models. Table 6 reports analogous regressions using count measures of facility-level inspections.[17] In the tables, more complete models (adding year and industry effects) are presented as one moves from left to right. The most parsimonious models are the least precise, and our most preferred models are on the far right, including time effects, industry effects, and the full range of covariates available to us. In all models, errors are clustered at the Congressional district level (Bertrand, et al., 2004). For each model variant, we present estimations both with and without lagged inspections ("dynamic" and "non-dynamic," respectively). In the dynamic models, the lags are almost always significant, but are potentially endogenous. To correct for this potential endogeneity, we employ a non-linear instrumental

[17] Because our inspection counts contain a large number of zero's (40.1 percent) and predominantly values less than five (98.1 percent), we account for the count structure of the data using a Poisson model.

variable approach that, following standard practice (e.g., Greene, 2003), uses lagged exogenous data to identify the lagged inspection regressor.[18]

Consistent in all of these regressions is the negative effect of Republican Congressional affiliation on environmental inspection intensity. In the binary (Probit) models of Table 5, Republican representation is estimated to reduce the average inspection probability by between 9 and 12.2 percent, which translates into a proportional reduction of approximately 20 to 27 percent. All of these estimated impacts are statistically significant.

In the count models of Tables 6, qualitative results are similar. All models estimate negative effects of Republican representation, effects that are statistically significant in all but the least precise non-dynamic (left-most) model. Estimated magnitudes of effect jump significantly when controlling for time and industry. In the more parsimonious (left-side) models, the estimated effect of a Republican Congressman is to reduce inspection counts by 20 to 21 percent. In the more complete (right-side) models of Table 6, the corresponding estimated effect is to reduce inspection counts by between 35 and 41 percent. In both statistical and economic terms, these estimated effects are significant.

Robustness Checks. Tables 7 to 9 report a number of robustness checks on our main estimations. Table 7 presents results from our most precise models (the last two columns of Tables 5 and 6) using finer definitions of "close" to construct our close-open dataset. Recall that we have defined close elections as those with a margin of victory less than 2.5 percent. In Table 7, we present our preferred models using close elections that have a margin of victory less than

[18] The dynamic models are estimated by two-stage-residual-inclusion (2SRI), following Terza, et al. (2008). The latter authors show that the 2SRI approach, unlike other two-stage methods, yields consistent parameter estimates in general non-linear models. In the binary (Probit) models, the first stage is also a Probit estimation. In the count models of Table 6, the lagged inspection regressor is the log of one plus the lagged inspection count (reflecting the exponential functional form of the Poisson, and following Hill, Rothschild and Cameron, 1998); the first stage therefore takes a Tobit form. However, in estimating the 2SRI count models of Table 6, we do not reject exogeneity of the lag at any reasonable statistical level (e.g., 35 percent or less) and therefore report uninstrumented dynamic regressions.

two and one percent, respectively. Table 8 presents estimates of the treatment effects in local linear regressions (where we add the vote margin and its interaction with the Republican Congressional dummy). Tables 7 and 8 indicate that the negative effect of Republican representation is robust to a finer definition of close elections and to controls for vote margin.

Perhaps of most interest are results from the control function approach (equation (2)), reported in Table 9. All reported regressions employ available data from all open seat districts in which the Republican and Democrat were the top two vote getters. We report outcomes using different flexible functions in the relative vote share (polymonials of orders three, four and five), different sets of controls (varying from "reduced" models that only include the treatment, state effects and vote share variables, to "full" models that include all other available controls), non-dynamic and dynamic, and Ferreira/Gyourko (2009) models that also include interactions between vote margin polynomials and the Congressional dummy.[19] In all models, the estimated effect of the Republican Congressional dummy is negative. In the Probit models, the estimated effect is statistically significant in almost all cases and takes on values similar to those estimated in our first close-open regressions (of Table 5). In the Poisson models, the "full" models yield similar estimates to those in the close-open regressions (of Table 6), but the other models yield estimates that are smaller in magnitude and generally not statistically significant.

Closing Remark. We close by noting that this analysis focuses on arguably the least powerful members of Congress, those just elected from open seats, with no Congressional seniority or experience. We nevertheless find that these representatives have a significant impact on local enforcement of Federal Clean Air laws in their districts. Republican representation in

[19] Between our "reduced" and "full" model is a "base" model that only excludes the lagged release variable. This model is advantageous because of the large increase in observations made possible when voiding the restriction that the facility-year be represented in the TRI. The corresponding disadvantage is the loss in precision from exclusion of an important control.

the close open seats leads to an estimated proportional reduction in the local probability of a government environmental inspection, for a given facility, of roughly 20 to 30 percent.

4. Conclusion

There are a number of different perspectives on how members of the U.S. Congress influence regulatory behavior. The "political control of the bureaucracy" literature generally stresses how more powerful members of Congress influence regulatory decisions about broad policy (such as how the airwaves are regulated, the level of safety and environmental standards for products and firms, or the imposition of trade sanctions). The influence is either exercised for policy purposes (the purposes for which Congressional officials are elected) or for the more illicit ends of doing the bidding for special interest groups in order to extract campaign contributions (the "capture school" of Stigler, 1971, and others). In either case, regulators respond to Congressional influence because Congress controls the agency's purse strings and other policy decisions of interest to the bureaucrats or their ultimate bosses in the Executive political hierarchy (Niskanen, 1971).

In this paper, we investigate the scope for the decentralized exercise of Congressional influence by studying how local regulatory decisions are affected by the local Congressional representatives. Here it is not powerful Members of Congress influencing broad policy, but local (and not particularly powerful) Members of Congress influencing local policy. For example, in a Niskanen-type model of legislative-regulator bargaining over budgets and enforcement, each individual Member of Congress can be important because the regulator seeks to increase the probability that each Member supports their next budget request. This process may encourage preferential agency enforcement decisions that curry favor with the individual Member.

Using party affiliation as an indicator for a Congressional representative's preferences (over environmental enforcement in our empirical example), we study the impact of these affiliations on local EPA enforcement of Clean Air Act laws in the representative's own constituency. We identify a Representative's party affiliation with a Regression Discontinuity design that focuses our analysis on districts that have had a close election for an open seat. This approach also focuses our analysis on arguably the least powerful Members of Congress, those with no seniority at all. Even for these Congressmen, we find statistical evidence that Republican affiliation of the local Congressman significantly dampens CAA inspection intensities for polluting facilities in the Congressman's district. This result provides evidence for a political-economic model in which: 1) Congressional representatives have discernable political preferences over local environmental enforcement in their districts, and 2) the local EPA regulators respond to these local Congressional preferences.

In contrast, our results are hard to square with a pure lobbying model of regulatory policy. On one hand, lobbying models seem particularly appropriate for regulatory decisions of the type that we study because politicians are once-removed and, therefore, presumably inoculated from any resulting political fallout. Moreover, our results do not rule out a role for lobbying per se, as party-specific Congressional preferences may be driven by the pursuit of campaign contributions from lobbying firms. However, because "capture school" models account for neither the preferences of politicians, nor the mechanism by which Members of Congress can influence local regulation, they cannot entirely explain our findings.

Politicians' distance from local regulatory decisions also suggests that they are unlikely to reflect secondary policy outcomes that single-issue voters would reward or penalize (List and Sturm, 2006). As a result, the setting we study is arguably particularly appropriate to identify underpinning preferences of the Congressional representatives. This said, our analysis does not identify the source of Congressional preferences per se. Do Republicans simply dislike inspections and Democrats like them? Or is there a separating political equilibrium in which anti-inspection (pro-business) contributors and supporters favor Republicans and pro-inspection (pro-environment) contributors and supporters favor Democrats, and influence is exercised in proportion to respective strengths of support? Whatever the mechanism for polarization, our results suggest that this mechanism is important not only to broad policy, but also to local regulatory enforcement.

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Figure 1: Facility Level Yearly Average Inspection plus Enforcement Actions in Politically Polar Areas

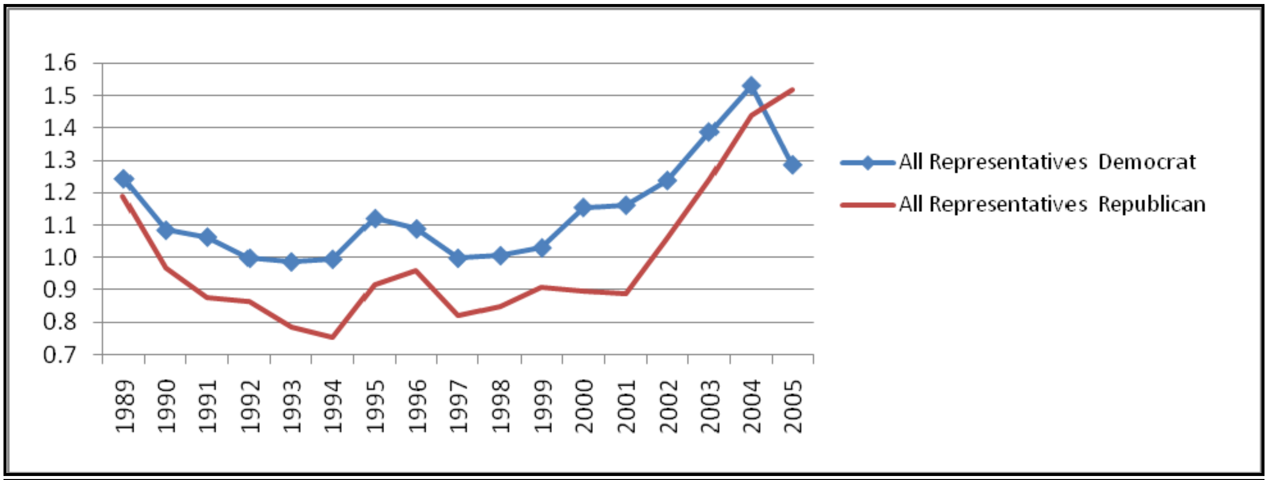


Figure 2: Facility Level Yearly Average of Total Toxicity Weighted Release in Politically Polar Areas

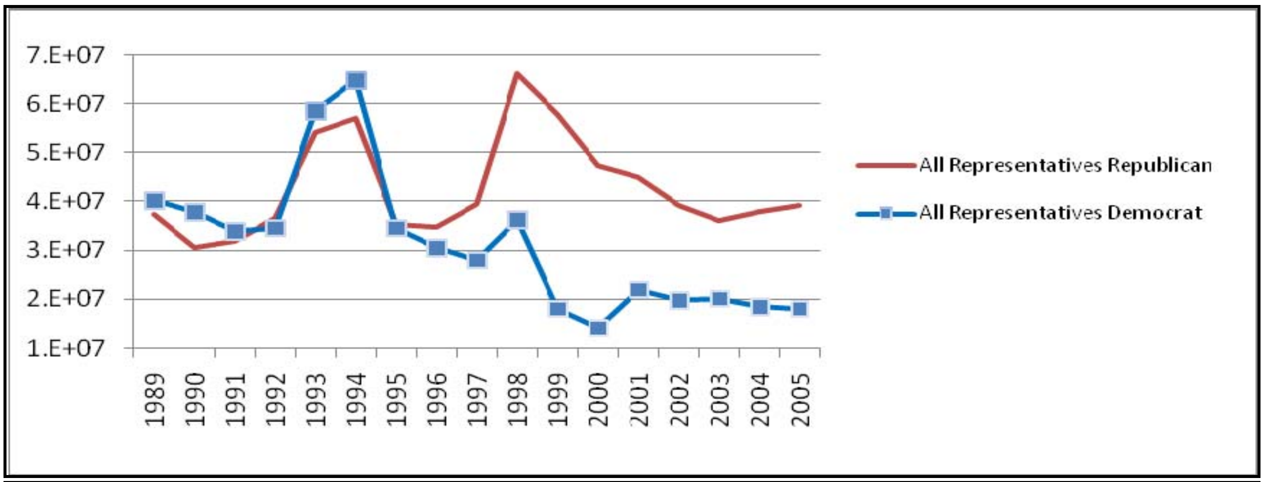


Figure 3: Post-Election Number of Inspections

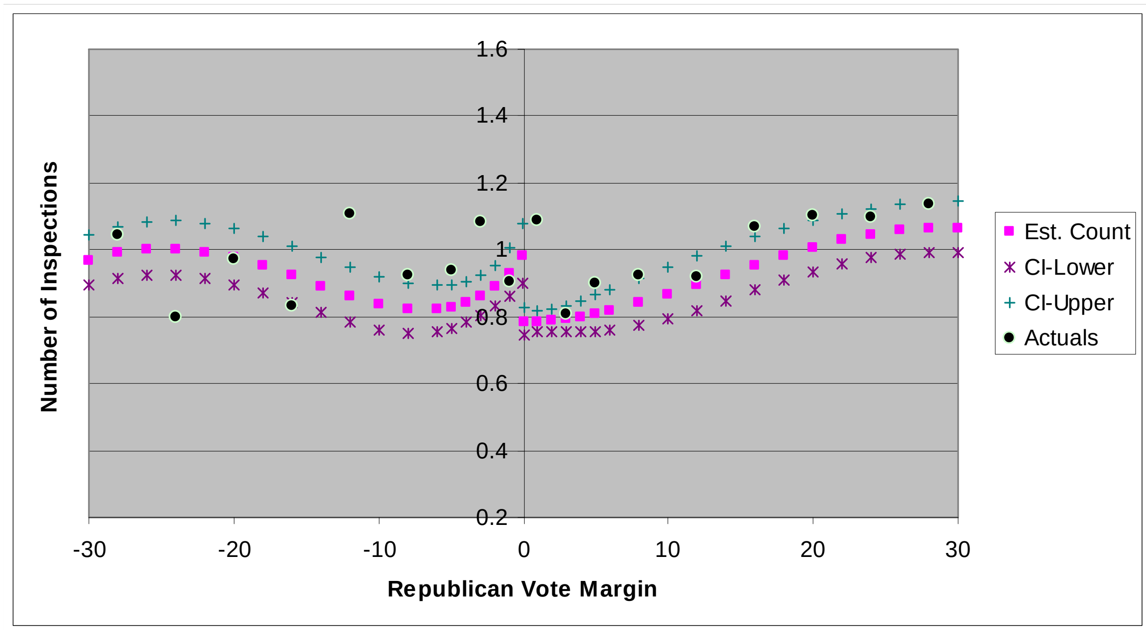
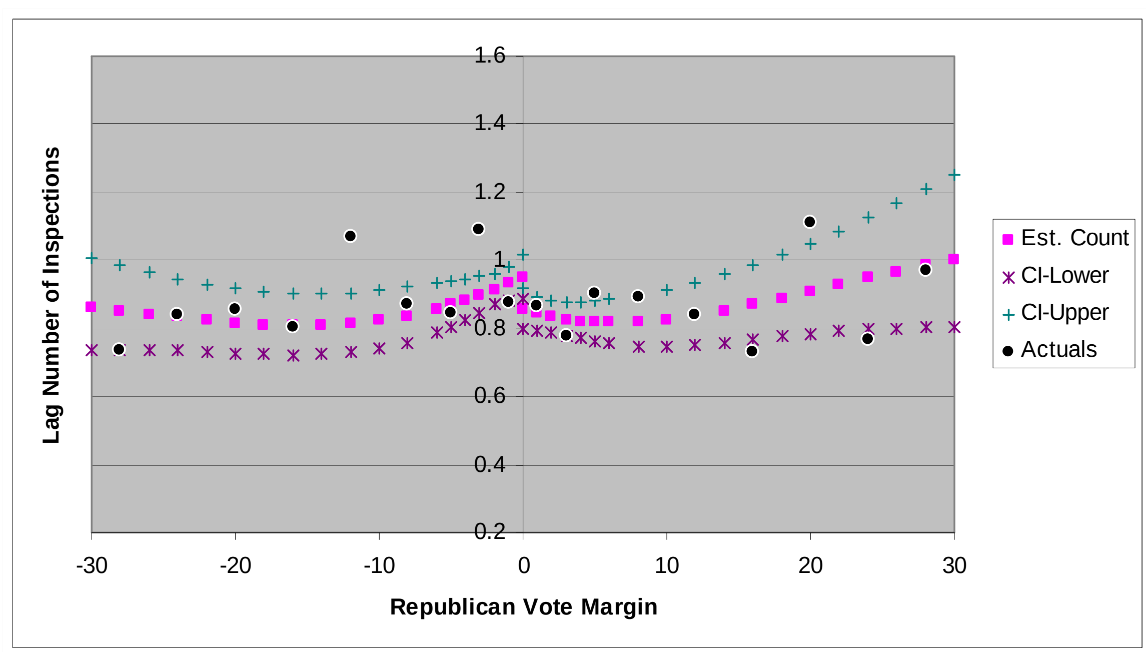


Figure 4: Pre-Election Number of Inspections



Notes: Figures 3 and 4 present fitted values from estimated fifth-order polynomials in vote margins on either side of the (v=0) cutoff, and associated 95 percent confidence intervals. A vote margin is the difference between Republican and Democratic vote shares in a U.S. House election. The estimations use data from all Congressional districts in which the past year's seat was open and the top vote getters were Democrat and Republican. State fixed effects are included and evaluated at sample means. "Actual" data averages (controlling for State effects) are presented for two to four percent vote margin bins (two percent bins close to the v=0 threshold, four percent bins for larger margins).

Table 1: Impact of a Change in Political Representation on Environmental Parameters

	Yearly Average Inspections plus Enforcement Actions	Yearly Average Toxicity Weighted Release
Areas Represented by 3 Republican Politicians	0.999	43,800,000
The Same Areas Represented by 2 Republican Politicians & 1 Democrat Politician After ***ONE*** Year	1.024	35,000,000
Percentage Change (z stat)	2.50% (7.15)***	–20.09% (-16.86)***
Number of Observations	57	
Areas Represented by 3 Republican Politicians	0.985	41,900,000
The Same Areas Represented by 2 Republican Politicians & 1 Democrat Politician After ***TWO*** Years	1.016	35,400,000
Percentage Change (z stat)	3.15% (4.86)***	–15.51% (-10.86)***
Number of Observations	107	
	Yearly Average Inspections plus Enforcement Actions	Yearly Average Toxicity Weighted Release
Areas Represented by 3 Democrat Politicians	1.337	34,800,000
The Same Areas Represented by 2 Democrat Politicians & 1 Republican Politician After ***ONE*** Year	1.275	40,300,000
Percentage Change (z stat)	–4.64% (0.87)	25.15% (8.00)***
Number of Observations	97	94
Areas Represented by 3 Democrat Politicians	1.139	34,800,000
The Same Areas Represented by 2 Democrat Politicians & 1 Republican Politician After ***TWO*** Years	1.293	36,600,000
Percentage Change (z stat)	13.52% (1.37)	5.17% (2.94)***
Number of Observations	183	177

Table 2: Variables and Summary Statistics

Variable Name	Definition	Source +	AFS Data Mean (sd) ++	AFS-TRI Data Mean (sd)++
Inspections	Number of inspections for a facility (annual)	AFS	0.986 (1.691)	1.023 (2.347)
Enforcement Actions	Number of enforcement actions for a facility (annual)	AFS	0.056 (0.407)	0.088 (0.573)
CAA-TRI Release +	Toxicity weighted CAA-regulated chemicals reported to TRI, by facility (annual)	TRI	26288 (188126)	26288 (188126)
Sierra	Sierra club membership per capita in facility's state (annual)	Sierra Club	0.002 (0.002)	0.002 (0.002)
Strict Liability	Binary = 1 if state has a strict liability statute (annual)	ELI	0.766 (0.423)	0.785 (0.411)
Unemployment	Annual unemployment rate in county where facility is located (annual)	BLS	5.380 (2.179)	5.512 (1.995)
Population Density	Population per square mile in county where facility is located (annual)	BLS	827 (1981)	916 (1882)
Per Capita Income	Real per capita income in county of facility (annual, 2000 US$)	BLS	25614 (6427)	25930 (6130)
Republican Congressman Dummy	Binary = 1 if facility represented by a Republican Congressman (annual)	US Cong Bio	0.535 (0.499)	0.513 (0.500)
At Least One Republican Senator	Binary = 1 if facility is represented by at least one Republican Senator (annual)	US Cong Bio	0.722 (0.448)	0.682 (0.466)
Northeast Dummy	Binary = 1 if facility is located in a Northeast state +++	AFS	0.159 (0.366)	0.180 (0.384)
Midwest Dummy	Binary = 1 if facility is located in a Midwest state +++	AFS	0.330 (0.470)	0.374 (0.484)
South Dummy	Binary = 1 if facility is located in a Southern state +++	AFS	0.422 (0.494)	0.382 (0.486)
West Dummy	Binary = 1 if facility is located in a Western state +++	AFS	0.088 (0.284)	0.064 (0.244)

+ CAA=Clean Air Act, AFS = Air Facility System (EPA), TRI = Toxic Release Inventory (EPA), ELI = Environmental Law Institute, BLS = Bureau of Labor Statistics, US Cong Bio = US Congressional Biography.
++ Summary statistics are for the AFS Dataset, which does not restrict the sample to TRI reporters. CAA-TRI release statistics use the AFS-TRI Dataset, which restricts the sample to TRI reporters (facilities that reported to the TRI one year or more during our sample period). Number of observations = 617,546 (AFS), 151,687 (AFS-TRI).
+++ Northeast states = CT, MA, ME, NH, NJ, NY, PA, RI, VT; Midwest states = IA, IL, IN, KS, MI, MN, MO, ND, NE, OH, SD, WI; Southern states = AL, AR, DE, FL, GA, KY, LA, MD, MS, NC, OK, SC, TN, TX; Western states = AK, AZ, CA, CO, HI, ID, MT, NM, NV, OR, UT, WA, WY.

Table 3: Preliminary Regressions with Full Data

Dependent Variable: Number of Inspections	(1) Random Effects Poisson Regression	(2) Cross Section Negative Binomial	(3) Linear Fixed Effects	(4) Linear Random Effects
Independent Variables	Marginal Effect	Marginal Effect	Marginal Effect	Marginal Effect
Lagged Release	2.01×10^{-8} (1.22×10^{-8})*	1.82×10^{-7} (1.12×10^{-7})*	-9.52×10^{-9} (3.64×10^{-8})	4.42×10^{-8} (5.51×10^{-8})
Lagged Enforcement Dummy	0.084 (0.010)***	0.622 (0.038)***	0.060 (0.079)	0.329 (0.061)***
Sierra	–15.66 (2.66)***	–15.10 (7.466)**	–17.66 (5.858)***	–18.66 (8.047)**
Strict Liability	–0.170 (0.017)***	–0.088 (0.137)	–0.149 (0.137)	–0.121 (0.141)
Unemployment	–0.003 (0.003)	0.005 (0.014)	–0.011 (0.029)	–0.007 (0.023)
Population Density	5.70×10^{-6} (3.97×10^{-6})	-6.37×10^{-6} (12.6×10^{-6})	16.35×10^{-5} (36.36×10^{-5})	4.50×10^{-6} (10.90×10^{-6})
Per Capita Income	-12.40×10^{-6} (1.41×10^{-6})***	-9.37×10^{-6} (4.37×10^{-6})**	-43.90×10^{-6} (15.90×10^{-6})***	-15.30×10^{-6} (5.47×10^{-6})***
Republican Congressman Dummy	–0.047 (0.008)***	–0.058 (0.025)**	–0.012 (0.047)	–0.020 (0.011)*
At Least One Repub. Senator	–0.321 (0.010)***	–0.346 (0.177)**	–0.318 (0.188)*	–0.336 (0.199)*
State, Year, SIC dummies	YES	YES	YES	YES
Number of Facilities	17,635	17,635	17,635	17,635
Number of Observations	151,687	151,687	151,687	151,687

Notes: (i) Robust standard errors in parentheses, clustered by State in Models (2)-(4). (ii) ***, ** & * respectively indicate significance at 1%, 5% & 10% level.

Table 4: Summary Statistics for the Close-Open (+/- 2.5%) Data

Variable	Close Open Overall+ (1) Mean (Std. Dev.)	Democrat-Won (2) Mean (# of Obs)	Republican-Won (3) Mean (# of Obs)	Difference (4) Diff. = (2)–(3) (z statistic)
			Election−level	
Sierra	0.0020 (0.0033)	0.002 (27)	0.003 (37)	−0.001 (−0.721)
Strict Liability	0.734 (0.442)	0.815 (27)	0.730 (37)	0.0766 (0.801)
Unemployment	5.074 (2.038)	5.558 (27)	5.358 (37)	0.200 (0.438)
Population Density	371.878 (648.391)	459.921 (27)	693.223 (37)	−233.302 (−1.172)
Per Capita Income	24502.44 (5470.351)	25255.56 (27)	25882.29 (37)	−626.725 (−0.481)
At Least One Repub. Senator Dummy	0.695 (0.461)	0.704 (27)	0.649 (37)	0.055 (0.460)
Northeast Dummy	0.265 (0.441)	0.2222 (27)	0.2778 (37)	0.0054 (−0.511)
Midwest Dummy	0.325 (0.469)	0.2963 (27)	0.1944 (37)	0.1018 (0.931)
South Dummy	0.321 (0.467)	0.2222 (27)	0.3889 (37)	−0.1667 (1.472)
West Dummy	0.089 (0.284)	0.2593 (27)	0.1667 (37)	0.0926 (0.888)
Prior (Lag) Republican Congressman+	0.562 (0.496)	0.5555 (27)	0.5676 (37)	-0.0121 (-0.096)
Republican Governor+	0.531 (0.503)	0.4815 (27)	0.5676 (37)	-0.0861 (-0.683)
Republican State House Majority+	0.281 (0.453)	0.2963 (27)	0.2703 (37)	0.0260 (0.228)
Republican Presidential Vote Share (Most Recent)+	0.484 (0.062)	0.4843 (27)	0.4844 (37)	-0.0001 (-0.003)
			Facility−Level	
Lag Inspections	0.9908 (2.5517)	0.8094 (1873)	1.1337 (2378)	-0.3243 (-4.47)***
Lag Inspections (binary)	0.5526 (0.4973)	0.4944 (1873)	0.5984 (2378)	-0.1040 (-6.79)***
Lag Enforcement Actions	0.0393 (0.2606)	0.0379 (1873)	0.0404 (2378)	-0.0025 (-0.314)
Lag CAA-TRI Release	21548 (50506)	19572.5 (556)	23232.9 (652)	-3660.4 (-1.285)
			Deviations from State Averages	
Lag Inspections	0.9908 (2.5517)	−0.0124 (1873)	−0.0286 (2378)	0.0162 (0.226)
Lag Inspections (binary)	0.5526 (0.4973)	0.0061 (1873)	−0.0027 (2378)	0.0088 (0.585)
Inspections	0.9790 (1.3130)	0.0163 (2306)	−0.0923 (2934)	0.1087 (3.13)***
Inspections (binary)	0.5912 (0.4917)	0.0319 (2306)	−0.0382 (2934)	0.0701 (5.26)***

Notes: ***, ** & * indicate significance at 1%, 5% & 10% level. The Close Open data restricts the sample to observations for which the last Congressional election was open (no incumbent running) and close (margin of victory within +/- 2.5%). +Column (1) gives facility-level means (standard deviations) for the overall Close Open sample , except for the political variables (measured at the election level).

Table 5: Probit Model of Inspections (Binary) in Close-Open Districts

Independent Variables	Marginal Effect (Clustered Standard Error)		Marginal Effect (Clustered Standard Error)		Marginal Effect (Clustered Standard Error)	
	Non-Dynamic	Dynamic	Non-Dynamic	Dynamic	Non-Dynamic	Dynamic
Republican Congressman Dummy	**–0.100 (0.054)***	**–0.114 (0.031)*****	**–0.092 (0.052)***	**–0.122 (0.030)*****	**–0.090 (0.053)***	**–0.115 (0.033)*****
Lagged Inspections (binary)	NO	0.718 (0.020)***	NO	0.718 (0.019)***	NO	0.716 (0.017)***
Sierra	60.605 (34.777)*	59.345 (17.955)***	2.301 (6.872)	5.781 (4.152)	–3.000 (7.188)	0.836 (4.734)
Strict Liability	–0.214 (0.132)*	0.422 (0.042)***	0.212 (0.175)	–0.431 (0.067)***	–0.265 (0.172)	–0.443 (0.069)***
Lagged Average Release	5.72e-07 (2.51e-07)**	–3.86e-07 (3.02e-07)	6.51e-07 (2.66e-07)***	–2.23e-07 (2.74e-07)	5.48e-07 (3.04e-07)*	–1.89e-07 (2.94e-07)
Lagged Enforcement Dummy	0.134 (0.061)**	0.021 (0.062)	0.107 (0.058)*	0.017 (0.060)	0.104 (0.061)*	0.023 (0.060)
Unemployment	–0.006 (0.014)	0.027 (0.012)**	0.029 (0.011)***	0.008 (0.010)	0.028 (0.011)***	0.017 (0.010)*
Population Density	–4.76e-05 (4.55e-05)	6.49e-05 (3.05e-05)**	–4.92e-05 (3.91e-05)	6.96e-05 (2.36e-05)***	–4.30e-05 (3.86e-05)	5.77e-05 (2.36e-05)***
Per Capita Income	–1.95e-06 (6.58e-06)	8.11e-06 (3.79e-06)**	2.44e-06 (5.31e-06)	6.11e-06 (3.53e-06)*	4,21e-06 (4.95e-06)	9.31e-06 (3.20e-06)***
At Least One Republican Senator Dummy	–0.003 (0.125)	–0.108 (0.077)	–0.076 (0.144)	–0.280 (0.066)***	–0.122 (0.142)	–0.290 (0.072)***
First Stage Residual	NO	–1.336 (0.357)***	NO	–1.135 (0.189)***	NO	–0.996 (0.192)***
State Dummies	YES	YES	YES	YES	YES	YES
Year Dummies	NO	NO	YES	YES	YES	YES
SIC Dummies	NO	NO	NO	NO	YES	YES
R^2	0.13	0.26	0.18	0.29	0.21	0.32
Number of Observations	1333	1174	1333	1174	1319	1161

Dependent variable = facility-level zero-one inspection (one if at least one inspection conducted) the year after a close (within +/- 2.5 percent) election in an open Congressional district. Standard errors in parentheses, robust clustered (by Congressional district). Average marginal effects are reported. The inspection lag is treated as endogenous (using two stage residual inclusion, 2SRI) in the dynamic models, where test statistics for the null of exogeneity have p-values less than .001.

<u>Table 6: Poisson Model of Inspection Counts in Close-Open Districts</u>

Independent Variables	Coefficient (Clustered Standard Error)		Coefficient (Clustered Standard Error)		Coefficient (Clustered Standard Error)	
	Non-Dynamic	Dynamic	Non-Dynamic	Dynamic	Non-Dynamic	Dynamic
Republican Congressman Dummy	**–0.232 (0.209)**	**–0.224 (0.120)***	**–0.527 (0.250)****	**–0.427 (0.134)*****	**–0.516 (0.269)****	**–0.450 (0.144)*****
Lagged Inspections	NO	0.974 (0.099)***	NO	0.939 (0.098)***	NO	0.892 (0.098)***
Sierra	14.940 (8.860)*	9.988 (5.844)*	16.724 (10.951)	9.431 (7.559)	–2.958 (12.886)	-1.357 (8.080)
Strict Liability	0.282 (0.464)	-15.005 (0.910)***	0.278 (1.622)	0.0373 (1.210)	0.481 (1.611)	–15.664 (1.030)***
Lagged Average Release	1.33e-06 (7.03e-07)**	7.71e-07 (5.34e-07)	1.58e-06 (6.34e-07)***	8.96e-07 (5.50e-07)	1.17e-06 (4.57e-07)***	6.39e-07 (5.03e-07)
Lagged Enforcement Dummy	0.526 (0.170)***	0.144 (0.132)	0.429 (0.168)***	0.148 (0.134)	0.385 (0.161)***	0.167 (0.107)
Unemployment	–0.053 (0.048)	–0.037 (0.032)	0.051 (0.048)	0.016 (0.037)	0.035 (0.045)	0.013 (0.035)
Population Density	–20.04e-05 (17.03e-05)	5.29e-05 (12.91e-05)	–20.03e-05 (15.21e-05)	5.24e-05 (13.61e-05)	–21.03e-05 (16.30e-05)	3.65e-05 (14.62e-05)
Per Capita Income	–2.17e-05 (2.09e-05)	1.24e-05 (1.27e-05)	–5.07e-06 (17.80e-06)	1.87e-05 (1.42e-05)	–4.41e-06 (16.90e-06)	1.73e-05 (1.35e-05)
At Least One Republican Senator Dummy	–0.122 (0.321)	–0.187 (0.294)	–0.755 (0.585)	–0.802 (0.337)**	–0.851 (0.596)	–0.886 (0.357)**
State Dummies	YES	YES	YES	YES	YES	YES
Year Dummies	NO	NO	YES	YES	YES	YES
SIC Dummies	NO	NO	NO	NO	YES	YES
Log Likelihood	–1770.53	–1497.65	–1709.63	–1476.01	–1635.59	–1441.83
Number of Observations	1351	1310	1351	1310	1351	1310

Dependent variable = annual inspection count (facility-level) the year after a close (within +/- 2.5 percent) election in an open Congressional district. "Lagged inspections" = log of one plus lagged inspection count. Standard errors in parentheses, robust clustered (by Congressional district). The inspection lag is treated as exogenous in the dynamic models, where test statistics for the null of exogeneity have p-values of .576, .362, and .884.

Table 7. Models of Inspections with Finer Close Open Data (+/- 2% and +/- 1%)

(A) Probit Models of Inspections (Binary) Marginal Effect (Clustered Standard Error)				
	+/- 2 % Close Open Data		+/- 1% Close Open Data	
	Non-dynamic	Dynamic	Non-dynamic	Dynamic
Repub. Cong. Dummy	-0.618 (0.259)***	-0.225 (0.086)***	-0.218 (0.095)**	-0.391 (0.003)***
Lag Inspect	NO	0.703 (0.022)***	NO	0.553 (0.124)***
First Stage Residual	NO	-0.779 (0.191)***	NO	-0.263 (0.186)
No. of Obs.	820	713	392	332
(B) Poisson Models of Inspections (Count) Coefficient (Clustered Standard Error)				
	+/- 2 % Close Open Data		+/- 1% Close Open Data	
	Non-dynamic	Dynamic	Non-dynamic	Dynamic
Repub. Cong. Dummy	-2.309 (0.888)***	-1.290 (0.664)*	-2.025 (0.764)***	-1.905 (0.314)***
Log (1 + Lag Inspect)	NO	0.901 (0.109)***	NO	0.971 (0.167)***
1st St. Residual	NO	NO	NO	NO
p-val: 1st St. Resid.	N/A	N/A	N/A	0.424
No. of Obs.	853	817	403	376

Table 8. Local Linear Models of Inspections in Close Open Districts

(A) Probit Models of Inspections (Binary) Marginal Effect (Clustered Standard Error)				
	+/- 2.5 % Close Open Data		+/- 2% Close Open Data	
	Non-dynamic	Dynamic	Non-dynamic	Dynamic
Repub. Cong. Dummy	-0.188 (.2332)	-0.613 (.1184)***	-0.364 (.3959)	-0.869 (.1399)***
Lag Inspect	NO	0.986 (.0145)***	NO	0.960 (.0431)***
First Stage Residual	NO	-1.539 (.2796)***	NO	-1.207 (.3287)***
No. of Obs.	1319	1161	820	713
(B) Poisson Models of Inspections (Count) Coefficient (Clustered Standard Error)				
	+/- 2.5 % Close Open Data		+/- 2% Close Open Data	
	Non-dynamic	Dynamic	Non-dynamic	Dynamic
Repub. Cong. Dummy	-1.493 (.6684)**	-1.265 (.4677)***	-2.297 (1.3473)*	-2.066 (.8388)**
Log (1 + Lag Inspect)	NO	0.875 (.0962)***	NO	0.901 (.1098)***
1st St. Residual	NO	NO	NO	NO
p-val: 1st St. Resid.	N/A	0.211	N/A	0.678
No. of Obs.	1351	1310	853	817

Notes: Standard errors in parentheses, robust clustered at the Congressional District. ***,**,* denote significance at 1%, 5%, and 10%. All models include all controls; State, year and industry effects. Table 8 models include the Republican vote margin (v) and its interaction with the Congressional dummy (v*C). The dynamic Probit models include the lag of inspections (binary), treated as endogenous (using 2SRI). In the dynamic Poisson models, we treat the (log of one plus) inspection lag as exogenous because first stage residuals are not significant (and the Table 7B 2% 2SRI did not converge).

Table 9. Control Function Models of Inspections in Open Seat Districts

	Probit Models of Inspections (Binary) Marginal Effect (Clustered Std. Error)			Poisson Models of Inspections (Count) Coefficient (Clustered Std. Error)		
	Reduced Models +					
Polynomials in Vote Share	3	4	5	3	4	5
Repub. Cong, Dum. ME/Coeff	-0.081 (0.0424)*	-0.078 (0.0424)*	-0.092 (0.0487)*	-0.159 (0.0881)*	-0.149 (0.0935)	-0.080 (0.1002)
	Base Models ++					
Polynomials in Vote Share	3	4	5	3	4	5
Repub. Cong. Dum. ME/Coeff	-0.094 (0.0433)**	-0.093 (0.0433)**	-0.102 (0.0496)**	-0.154 (0.0949)	-0.152 (0.0982)	-0.078 (0.1061)
	Full Models +++					
Polynomials in Vote Share	3	4	5	3	4	5
Repub. Cong. Dum. ME/Coeff	-0.082 (0.0456)*	-0.081 (0.0452)*	-0.066 (0.0534)	-0.246 (0.1195)**	-0.248 (0.1181)**	-0.212 (0.1390)
	Dynamic Base Models ++++					
Polynomials in Vote Share	3	4	5	3	4	5
Repub. Cong. Dum. ME/Coeff	-0.103 (0.0406)**	-0.106 (0.0395)***	-0.112 (0.0474)**	-0.088 (0.0781)	-0.088 (0.0781)	-0.064 (0.0915)
First Stage Residual	No	No	No	Yes	Yes	Yes
p-value on test for exogeneity of lag	0.601	0.697	0.701	0.017	0.017	0.017
	Ferreira / Gyourko Models +++++					
Polynomials in Vote Share	3	3	3	3	3	3
Controls	Reduced	Base	Full	Reduced	Base	Full
Repub. Cong. Dum. ME/Coeff	-0.132 (0.0582)**	-0.141 (0.0591)***	-0.083 (0.0650)	-0.145 (0.1292)	-0.145 (0.1381)	-0.187 (0.1810)

Standard errors in parentheses, robust clustered at the Congressional District level. ***,**,* denote significance at the (two-sided) 1%, 5%, and 10% levels, respectively. In all models, data are for facilities in Congressional Districts that, in the prior year, had an open seat Congressional election in which the top two vote getters were a Republican and a Democrat.

+ Reduced Models (non-dynamic) include the Republican Congressman Dummy; State effects; and polynomials in the vote share, s=R/(D+R) where D=Democrat vote share, R=Republican vote share. No. of obs = 23525 for all models.
++ The Base Models (non-dynamic) include all controls except lagged release; State, year and industry effects; and polynomials in vote share. No. of obs = 20380 (20399) for the Probit (Poisson) models.
+++ The Full Models (non-dynamic) include all controls; State, year and industry effects; and polynomials in vote share. No. of obs = 5601 (5632) for the Probit (Poisson) models.
++++ The Dynamic Base Models include Base Model controls, the lagged inspection dummy (in the Probit) and lag of one plus lagged inspections (in the Poisson). In the Probit models, we fail to reject the null of exogeneity of the lag and treat the lag as exogenous. In the Poisson models, we reject the null of exogeneity of the lag and treat the lag as endogenous using 2SRI estimations (with lagged controls as instruments). No. of obs = 18354 (18379) in the Probit (Poisson) models.
+++++ The Ferreira/Gyourko models (non-dynamic) include Base Model controls; third order polynomials in vote margin (v=R-D) and its interactions with the Republican Congressman dummy (C): v, vC, v^2, v^2C, v^3, and v^3C. No. of obs = 23807/20627/5601 (23807/20644/5632) for the Reduced/Base/Full models of the Probit (Poisson).