Controlling the Regulators: How Party Control of Government Shapes Environmental Regulation in the 21st Century

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Abstract

In an increasingly polarized political environment, the consequences of state elections for policy have increased in magnitude. But most knowledge about state policy responsiveness to the parties' programs comes from studies of legislative outputs, which make up only a fraction of the work of government. How does party control of government influence policy implementation, enforcement, and bureaucratic outputs? This question is particularly important in the environmental policy area, wherein scholars have highlighted political influence over administrative policy at the federal level and states have a great deal of leeway within a national regulatory framework. I use the case of the Clean Air Act's (CAA) Title V permitting program to assess the parties' influence over state-level regulatory enforcement. I apply a regression discontinuity design to assess whether Republican control of state houses and governors' mansions causes a change in the number of enforcement actions state agencies report between 2000 and 2017. The findings suggest that governors and state legislatures influence enforcement, albeit through different channels. The paper provides new evidence of political influence over administrative policy, and for partisan influence in environmental politics.

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1 Introduction

In February 2014, Duke Energy spilled 39,000 tons of coal ash–a toxic waste produced at coal-burning power plants–into the Dan River in Eden, North Carolina. The accident polluted the river for 70 miles downstream and, for many, drew attention to a deal that Duke had come to with the North Carolina Department of Environment and Natural Resources (DENR) several months prior. The company had been under fire from environmental groups for several years because of poor management of coal ash at sites around the state. Probably to preempt a lawsuit from the Southern Environmental Law Center, the DENR finally took action against the company for violating the federal Clean Water Act (Semuels 2017). The resulting settlement imposed a small fine on Duke and did not require the company to remove coal ash from leaky and unlined disposal ponds. Many argue that the accident could have been prevented if DENR had imposed stronger requirements for the company to clean up its coal ash management practices (Editorial Board 2015). Reinforcing this view, scholars have found that strong regulatory enforcement improves compliance with environmental regulations and deters future violations (Gray and Shimshack 2011, Shimshack 2014, Keohane et al. 2009).

For many, the accident also typified a broader culture of regulatory laxity, capitulation to industry interests, and abuse of the discretion that states are afforded in enforcing the federal pollution control laws. Between 2011 and 2014 the DENR cut staff by two-thirds due to budget cuts imposed by the legislature. The governor increased from 24 to 179 the number of staff at the agency that were exempt from civil service protections. This increased by a factor of seven the number of staff at the agency who served at the discretion of the governor or of his appointed agency director. And that director made a number of other changes that loosened the regulatory enforcement regime, including directing agency staff to issue toothless warning

letters to potential violators, rather than more meaningful notices of violation (Gabriel 2014). These changes illustrate that state environmental agencies have discretion in implementing the federal pollution control laws and that agency choices are subject to political influence. They may also indicate that enforcement has become partisan, since a Republican governor and a Republican-controlled state legislature oversaw the changes.

Political science theory predicts that the parties might influence regulatory enforcement in a direction that is consistent with the North Carolina example, but evidence to support this claim is thin. Scholars have returned mixed conclusions regarding the implications of partisanship or ideology for environmental policy (Ringquist 1993a, 1994, Medler 1989, Ka and Teske 2002, Potoski and Woods 2002, Yi and Feiock 2014, Nicholson-Crotty and Carley 2018, Ka and Teske 2002, Daley and Garand 2005, Bromley-Trujillo et al. 2016, Konisky and Woods 2012). More generally, most knowledge about the parties' influence on state policy comes from studies of legislative outputs (e.g., Caughey et al. (2017), Kousser (2002), Chen (2007), Yates and Fording (2005), Reed (2006)), but these make up only a fraction of the work of government. At the federal level, scholars have assessed political influence over bureaucratic activities (eg, Wood (1991), Ackerman and Hassler (1981), Layzer (2012), Cook and Polsky (2005)), but there is no consensus about how governors and state legislatures influence policy implementation, enforcement, and enactments emitted by administrative agencies rather than legislatures. This gap is particularly notable in the environmental policy area, where states have a great deal of leeway within a national regulatory framework. The present study addresses these gaps by asking whether party control of governors' offices and state legislatures influences regulatory enforcement. The results elucidate whether the changes that occurred in North Carolina are symptomatic of broader differences in regulatory enforcement under Republican and Democratic-controlled state governments.

I use the case of the Clean Air Act (CAA), which regulates stationary and mobile sources of air pollution, to assess political influence over regulatory enforcement. I apply a regression discontinuity (RD) design to assess whether electing a Democratic or a Republican governor

or state-legislative majority causes a change in the stringency with which state environmental agencies enforce the CAA. I also assess whether political influence varies according to the two branches' mechanisms of political control. The results suggest that both gubernatorial and state-legislative elections have consequences for environmental enforcement, and that influence from the executive and legislative branches differs according to their distinct mechanisms of political influence.

The paper proceeds in six sections. I first summarize empirical findings and highlight unanswered questions concerning the link between party control of government and state policy (Section 2). Next, I develop theoretical expectations for how the elected branches of state government influence administrative policy, and why we might expect to observe a partisan direction of influence (Section 3). I then introduce the case, methods, and data I use in the analysis (Section 4). I present and interpret results (Section 5) from the RD analyses and conclude (Section 6) with a discussion of the implications of these findings and the questions they raise for future research.

2 Background: State policy responsiveness and administrative policy

There is a growing body of evidence that partisanship affects elected officials' voting decisions, and that partisan control of government institutions has meaningful consequences for policy. Some studies conclude that there is no link (e.g.,Hofferbert (1966), Konisky (2007)), an ambiguous link (e.g., Alt and Lowry (1994)), or, counterintuitively, a negative relationship (e.g., Erikson et al. (1989), Lax and Phillips (2012), Erikson et al. (1993), Fredriksson et al. (2011)) between Democratic control of government and the liberalism of state policies. Increasingly, scholars have identified a link between party control of government and policy outputs (Caughey et al. 2017, Kousser 2002, Chen 2007, Yates and Fording 2005, Reed 2006), although this link is contingent on the policy area investigated, state institutional features

(Besley and Case 2003), and whether the parties split control of the governorship and legislature (Alt and Lowry 2000). Findings of a robust link between Democratic (Republican) control of elected institutions and the liberalism (conservativism) of state policies are consistent with theoretical expectations (Bawn et al. 2012, Layman et al. 2010) and empirical evidence of increasingly divergent partisan position-taking by legislators (Fowler and Hall 2015, Fowler et al. 2016, Poole and Rosenthal 1984, 2001, McCarty et al. 2006, Clinton 2006, Shor and McCarty 2011). Nonetheless, even if we accept recent findings that party control of government institutions influences policy outputs, the bulk of political science scholarship on the topic draws its conclusions from studies of legislative enactments. This body of evidence does not illuminate how the elected branches of government affect policy implementation, enforcement, or policy enactments that do not emit from the legislature.

3 Theoretical framework: Polarization, nationalization, and political influence over agency activities

3.1 Partisan Influence Over Regulatory Enforcement

Why and how do elected officials influence administrative policy, and why might the direction of influence differ between the two major American political parties? I define administrative policy as policy implementation, enforcement, or other policy actions that do not emit from the legislature but instead from administrative agencies. A theory of political influence over administrative policy in general, and regulatory enforcement in particular, begins with the observation that today's polarized, nationalized parties exhibit stronger and more consistent influence on state policy than they did in the early- and mid-twentieth century. A clear and growing gap has emerged between the preferences and attitudes of Democratic and Republican party leaders (Poole and Rosenthal 1984, 2001, McCarty et al. 2006, Clinton et al. 2004) and mass partisans (Levendusky 2009, Abramowitz 2010, Fiorina and Abrams

2008), including at the state level (Shor and McCarty 2011, Caughey and Warshaw 2017). Likewise, the state parties have become increasingly consistent with the national parties in their policy views (Hopkins 2018) and distinct from each other in their policy programs (Caughey et al. 2017). Overall, whereas historically the state parties might have responded to local and state-level concerns in distinctive (within the party) or similar (across the parties) ways, today's nationalized and polarized parties are likely to promote similar (within the party) but divergent (across the parties) policy programs around the country.

The second component of the theory concerns variation in the extent to which the parties seek to influence administrative policy, according to the organization of conflict around an issue. Political influence over bureaucratic behavior becomes a greater concern under conditions of higher conflict between political principals and between agents and principals (Waterman et al. 2004, Huber and Shipan 2002). Thus, if the parties' positions diverge on an issue, and if one of the parties' policy position diverges from the relevant administrative agency's mandate, political principals are likely to seek to influence agency actions. Nationalization and polarization imply a high degree of goal conflict between the parties about environmental regulation (Shipan and Lowry 2001, Kim and Urpelainen 2017), and between the increasingly anti-regulatory Republican party and the regulatory mandates of environmental management agencies. This conflict leads to the expectation that Republicans would seek to reduce enforcement activity, whereas Democrats would seek to increase it. I test this expectation by assessing whether, in states that narrowly elect Republican governors or state legislative majorities, regulatory enforcement action decreases relative to states that narrowly elect Democratic governors and state legislative majorities.

3.2   Mechanisms of Political Influence

While nationalization and polarization lead to the expectation of partisan conflict over regulatory enforcement, a satisfying theory of political control requires an understanding of the mechanisms that would enable partisan influence. Administrative policy occurs in the

executive branch of government, and the governor does not suffer from collective action problems to the extent that the legislature does. Thus, the executive's institutional position and structure suggest that governors are likely to exert stronger influence than legislatures. Still, both the executive and the legislature enjoy mechanisms of political influence over the bureaucracy. Thus, the third component of the theory addresses how partisan political principals from the different branches of government influence agency behavior.

The legislative and executive branches influence the bureaucracy through different channels. The executive primarily influences agency behavior through staffing. Either through their appointment powers or by rearranging an agency's personnel, executives influence the bureaucracy by deciding who works there (Moe 1985, Lewis 2010, Howell and Lewis 2002). Thus, in a context of polarized, nationalized parties, governors might influence regulatory agency behavior by nominating co-partisans to agency director positions, oversight boards, or other appointed staff offices.

The party in control of the legislature might affect agency activities through oversight, low-profile policy challenges, or the state budget. The budget represents the most direct avenue of influence. Legislatures use appropriations both to send signals to an agency and to hamstring or expand its capacity to pursue long-running cases or investigations (McCubbins et al. 1989). Additionally, the budget mechanism provides the legislature a means by which to send signals and adjust resources available to the attorney general, who is partially or wholly responsible for bringing enforcement actions in many states.

The executive and legislature negotiate to set the budget though. Some evidence suggests that governors effectively use the budgetary process to advance their policy agendas (Kousser and Phillips 2012), but executive influence in budgeting appears limited to reducing–rather than increasing–agency budgets relative to the legislature's desired appropriation (Kiewiet and McCubbins 1988, Wilson 1989). Also, the evidence for governors' success in budget negotiations is drawn from high-profile agenda items mentioned in governors' state of the state addresses (Kousser and Phillips 2012). It is unclear how these conclusions translate to

policy changes that both branches are likely to avoid publicizing.

Legislatures might also influence administrative policy through direct and indirect oversight from legislative committees with jurisdiction over a particular agency. Direct oversight mechanisms include "police patrol" activities like agency rule review, ad hoc studies or reports, field observations, hearings, and legislative sanctions (McCubbins and Schwartz 1984). These mechanisms are costly and therefore likely to occur more often as the relative political consequences of an agency's work increase. But more often, legislatures use indirect, "fire alarm" oversight mechanisms (McCubbins and Schwartz 1984, Wilson 1989, Potoski and Woods 2001). These include procedures, analysis requirements, and rules that are embedded into agencies' operations. Some requirements enable citizens and groups to challenge agency actions or raise infractions to the relevant legislative committee's attention (McCubbins and Schwartz 1984, McCubbins et al. 1987). Others define the evidence and frameworks agencies must use to make decisions. They thereby speed, slow, or change the direction of agency policy-making without the direct interference of the legislature in specific decisions (Potoski and Woods 2001, McCubbins et al. 1989). Of course, the consistency and direction of partisan influence on regulatory enforcement depends on the interests of the constituencies that an oversight committee's work affects and the extent to which interested constituencies are sorted between the parties. Polarization and nationalization suggest consistent partisan preferences and, thus, consistency in the direction of partisan influence on regulatory enforcement through committee oversight.

While oversight mechanisms are embedded into agency structures through enabling statutes (McCubbins et al. 1989, Huber and Shipan 2002), the legislature can use low-profile tactics such as riders on must-pass bills to redirect agency policy-making or change the rules and procedures that serve as oversight mechanisms (Layzer 2012, p. 21). In an example relevant to the case explored in this paper, Congress used appropriations riders to blunt the Reagan administration's efforts to roll back Clean Air Act implementation throughout the 1980s (Bryner 1995). Low-profile tactics influence policy without attracting the attention of other

political principals or groups empowered to raise fire alarms. This is an important feature for environmental regulatory enforcement, since many groups are empowered to raise fire alarms and the states operate within a federally enforceable framework. Overall, legislatures can influence agency activities by adjusting resources through the budget, using existing oversight mechanisms, or changing the rules and procedures that serve as oversight mechanisms for the legislature.

Based on this framework, the elections of Democratic and Republican governors and state-legislative majorities can have consequences for administrative policy through distinct mechanisms. State legislatures can expand, retract, or redirect resources available to agencies and to other state offices including the attorney general. Administrative activities vary in the resources they require, and this mechanism is most effective for influencing resource-intensive activities. State legislatures can also influence administrative policy through committee oversight and procedural changes that speed, slow, or change the direction of agency policy-making. These mechanisms are most likely to influence formalized enforcement activities that are subject to process constraints. Governors influence agency operations primarily through appointing friendly managers, re-organizing agency staffs, and adjusting agency priorities through their relationships with agency leadership. These theoretical predictions lead to the expectation that governors may exert strong direct influence on agency actions through their appointment powers, but they are constrained in their influence over resource-intensive activities or those subject to legislative oversight. I test this prediction by assessing heterogeneity in the effect that governors and state legislatures can achieve over different types of enforcement activities.

Prior research lends plausibility to this theory, but gaps remain in scholars' assessments of partisan influence over environmental policy in general and regulatory enforcement in particular. In one body of work, scholars have examined the mechanisms included in the theory presented here. Evidence suggests that appointment powers afford influence over regulatory decisions to governors (Wood and Waterman 1994, Koski 2007, Woods 2004), and

that appointed agency leaders have polarized along with the political parties at the national level (Devins and Lewis 2008). These studies lend credence to the theorized mechanism of gubernatorial influence: staffing. Evidence also suggests that indirect oversight mechanisms afford influence to the legislative committees that oversee an agency (Potoski and Woods 2001, Woods 2013, 2004). Much of this work relies on perceptual measures of influence rather than indicators of policy outputs. This suggests that some political influence is achieved through bureaucrats' anticipation of principals' reactions to agency decisions. But the survey-based evidence does not tell us whether political principals influence administrative policy outputs, or whether the direction of influence differs between the parties.

More broadly, state-level studies that assess partisan effects on environmental policy return mixed results. When scholars have included partisan control of government or "government ideology" (Berry et al. 1998) in predictive models, many have found that these influence environmental policy outputs (Yi and Feiock 2014, Lyon and Yin 2010, Huang et al. 2007, Carley and Miller 2012, Yi and Feiock 2012, Chandler 2009, Stoutenborough and Beverlin 2008, Vachon and Menz 2006, Nicholson-Crotty and Carley 2018). Others find null (Fisher 2006, Carley and Miller 2012, Ringquist 1993b, Daley and Garand 2005) or inconsistent (Ringquist 1994, Medler 1989, Ka and Teske 2002, Bromley-Trujillo et al. 2016) effects. Some of these studies operationalize partisan control as a dichotomous indicator of unified government (Huang et al. 2007, Bromley-Trujillo et al. 2016), but the theory developed here suggests that detecting the influence of partisan control merits a design that disaggregates control of the legislature and governor's office. Other studies rely on data from a time period preceding the rise in political polarization, when the direction of partisan influence may not have been consistent across the states (Ringquist 1993a, 1994, Konisky 2007, Medler 1989, Ka and Teske 2002). Some studies have applied causal designs to identify partisan differences in environmental spending (Fredriksson et al. 2011) or environmental outcomes (Beland and Boucher 2015). These results are suggestive, but leave the mechanisms of influence ambiguous. Overall, this research provides suggestive evidence that partisan politics

influences environmental policy, but it would stretch these findings to conclude that election outcomes cause a change in environmental policy outputs that is distinct from the enduring features of states' political, economic, and cultural contexts. This is a substantively important question. States play a crucial role in enforcing the federal pollution-control laws, and enforcement is critical to promoting compliance and strong environmental performance (Gray and Shadbegian 2007, Gray and Shimshack 2011, Shimshack 2014).

To bring clarity to this debate, the present study asks whether Republican or Democratic control of state government causes systematic changes in environmental regulation in the American states. More precisely, how does the election of a Republican governor or legislative majority affect enforcement compared with the counterfactual election of Democrats to the same institution in the same state? I also assess heterogeneity in the effect of the legislative and executive branches of government on enforcement activities, according to the resources and formal procedures associated with the activities that agencies undertake.

4 Case, design, and data for assessing political influence over regulatory enforcement

4.1 Case: The Clean Air Act

To examine political influence over regulatory enforcement, I examine state agencies' enforcement of the Clean Air Act (CAA). The CAA, passed in 1970, regulates the emissions of air pollutants from mobile and stationary sources. The law sets up a framework wherein states take primary responsibility for achieving national pollution-control standards established by the Environmental Protection (EPA). Specifically, states must develop State Implementation Plans (SIPs), which include air quality standards that meet or exceed federally established criteria. If states do not submit regulatory programs that are consistent with the national standards, or if EPA deems a state's implementation efforts inadequate, EPA enforces the

standards directly. Thus, within a nationally established framework, states have discretion in implementing and enforcing federal pollution-control standards. Cross-state comparability in policy outputs makes the CAA an ideal empirical setting for examining political influence over state bureaucracies.

The CAA also provides a theoretically relevant case because of the centrality of goal conflict to theories of political influence over the bureaucracy (Waterman et al. 2004, Huber and Shipan 2002). For some issues, such as education, states also enjoy broad policy leeway. But it is difficult to theoretically derive the direction of influence that the parties would exert or to operationalize policy indicators that are both comparable between states and more substantively meaningful than spending measures. Environmental regulation, by contrast, has become a hallmark of ideological conflict between the parties at the national level. Theoretically deriving the expected direction of partisan influence is straightforward, based on the observation that elite and mass partisans hold divergent preferences about environmental problems and policies (Kim and Urpelainen 2018, McCright et al. 2014), and that Republicans have trended away from support for environmental protection policies (Shipan and Lowry 2001, Kim and Urpelainen 2017). The study tests whether goal conflict between Republican political principals (governors and legislative majorities) and environmental regulatory agencies influences regulatory policy enforcement in the states.

I focus in particular on stationary sources regulated under the Title V permitting program. Title V was included in the 1990 CAA amendments to improve reporting, enforcement, and compliance with the law. Under the program, large sources of air pollution and some smaller sources are required to obtain permits that establish facility-level pollution control requirements. State and local agencies issue most Title V permits, assume responsibility for monitoring compliance and taking action against violators, and are required to report their enforcement actions to the EPA. Actions states might take include informal warnings, notices of violation that do not impose penalties but instead provide guidance and request action from the facility, administrative orders that may impose compliance orders or fines, and civil

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or criminal prosecutions. EPA groups these actions into "informal" and "formal" categories (United States Environmental Protection Agency 2017). Informal actions include warning letters and notices of violation, whereas formal actions include administrative orders and civil or criminal judicial proceedings. Two theoretically relevant features distinguish these kinds of actions: the resources they require and the actors involved in carrying them out. Environmental agencies can issue warning letters and notices of violation independently. In contrast, administrative orders and judicial proceedings require the involvement of lawyers, often from the Attorney General's office. Likewise, formal actions often unfold over several years and require tremendous financial resources. Warning letters and notices of violation can emerge from some amount of dialogue between a facility and enforcers during the monitoring process, but they are relatively quick to issue and require a lower investment of time and money.

Since the theory predicts that legislatures and governors influence administrative policy through different channels, their influence should be observed through different outputs. Specifically, the legislature's use of the budget and oversight mechanisms–including low-profile changes that alter the rules for administrative proceedings–are more likely to affect resource-intensive formal enforcement activities. This is due to the resources required, involvement of the attorney general (also subject to resource constraints controlled by the legislature), and oversight mechanisms associated with formal enforcement. The governor, on the other hand, is most likely to influence informal enforcement activities where the agency enjoys more discretion.

4.2   Design: Regression discontinuity and randomization inference

I use a regression-discontinuity (RD) approach to elucidate whether electing a Democratic or a Republican governor or state-legislative majority causes a change in enforcement activity in a state. The RD design allows researchers to identify a causal effect by focusing analysis within a narrow bandwidth spanning a treatment-assignment threshold (Lee and Lemieux

2010). In many political-science applications–including the present study–treatment is defined by electoral margins (the "running variable") and assigned according to the 50% vote share required for the Democratic or Republican candidate to win (Lee 2008). By focusing analysis on narrowly decided elections in states that are otherwise similar, the RD design allows the researcher to assess the causal effects of a party's electoral victory.

The approach rests on an assumption of continuity of potential outcomes at the treatment-assignment threshold (De la Cuesta and Imai 2016). If potential outcomes are continuous between units spanning the threshold, the RD design supports a claim that any discontinuity observed between units assigned to treatment and control is caused by the treatment. The continuity assumption is valid if there is no discontinuity in pretreatment covariates or placebo outcomes at the treatment-assignment threshold, within the bandwidth specified for the analysis. I use a series of robustness checks to test the validity of the continuity assumption underlying these analyses.[1] I find no significant discontinuities, which suggests that the continuity assumption is valid.[2]

For the analysis of gubernatorial control, I use a standard RD: treatment is defined as a Bernoulli random variable and assigned by electoral margins in gubernatorial elections. For the analysis of legislative control, I use the multidimensional RD approach developed by Feigenbaum et al. (2017). The main challenge for applying RD in a legislative context is that treatment–party control of the legislature–is determined not by one election but by many. Multidimensional RD combines these elections into a single running variable that measures the distance between a vector of district-level election outcomes and the electoral

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[1] I test for discontinuities using local linear regression, applying larger weights to observations near the treatment-assignment threshold (De la Cuesta and Imai 2016). I examine the effect of the lagged running variable (e.g., the previous election's vote margin in the gubernatorial RD) on the outcome variable; the current running variable on the lagged outcome variable; and the running variable on the lagged treatment (party of the governor or legislative majority) and running variables, the current running variable and treatment for the opposite branch of government, and public support for environmental protection. I derive estimates of public environmental concern using an adaptation of the dynamic group-level item response theory model developed in Caughey and Warshaw (2015). To test for lasting effects of party control of government, I also examine the effect of the lagged running variable and lagged treatment assignment on the outcome variable.

[2] The results of all placebo tests are included in Appendix Section A.1.

results that would deliver majority-party status. I construct the running variable by first determining the number of seats (m) the minority party lacks to achieve majority status. Second, I determine the distance to majority status in the minority's m closest elections.[3] Since treatment is defined as Republican victory, this distance is multiplied by -1 if the Republicans are in the minority. Once this variable has been constructed, the analysis proceeds as with simple RD, where treatment is assigned according to the 50% threshold.

A shortcoming of the regression discontinuity approach is that the design restricts analysis to the subset of the data that are concentrated near the treatment-assignment threshold. This restriction raises concern about applying large-sample statistical procedures in some applications (Cattaneo et al. 2015). Since gubernatorial elections only occur every four years, my analysis includes a small number of elections and an even smaller number of close elections. As a robustness check for the analysis of gubernatorial control, I complement the RD with randomization inference, a statistical technique designed for hypothesis testing with small samples.

In the randomization-inference framework, the researcher tests the sharp null hypothesis that there is no treatment effect for any unit. Under this sharp null, the potential outcomes for all units are known, since the potential outcome for each unit is the same under the observed randomization of treatment and any counterfactual treatment assignment. Since all potential outcomes are known under the null hypothesis, the researcher can derive the distribution of any test statistic and determine the p-value and confidence interval for an observed test statistic based on this distribution. The distribution of test statistics thus provides a reference distribution through which to determine the probability of observing the effect that was actually observed (Rosenbaum 2010, Ch. 2), and rejecting or failing to reject the null hypothesis. I test the sharp null hypothesis that there is no effect on enforcement following the election of a Republican governor. I provide additional details about randomization inference, including support for the identifying assumption of local

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[3]Following Feigenbaum et al. (2017), I use Euclidean distance, such that distance to majority status is determined by summing the squares of the margins of loss in the m closest elections.

randomization of treatment, in the Appendix.

4.3 Data: Measuring regulatory enforcement and party control of government

The treatment-assignment variables are derived from election returns. For the gubernatorial RD, I use election returns from 2000 to 2016 (CQ Press 2018) and define the running variable as Republican vote margin. State-years are assigned into treatment if the Republican candidate receives more than 50% of the two-party vote share. For the state-legislative RD, I use state legislative election returns from 2000 to 2016 (Klarner 2018). The running variable for the legislative analysis is the Euclidean distance between a vector of district-level election results and the results that would be required for the Republican party to win majority status (Feigenbaum et al. 2017). Consistent with prior applications of the multidimensional RD (e.g., Caughey et al. (2017)), I only examine the effect of electing a Republican majority to the lower legislative chamber since many state senators are not up for re-election every term. I do not include elections in multimember districts or Nebraska, since multimember districts pose design complications and the Nebraska legislature is nonpartisan.

The outcome variables reflect states' enforcement actions taken against major and synthetic minor facilities permitted under the CAA's Title V Program, between the years 2000 and 2017. I focus on this subset of enforcement actions to maximize consistency in reporting over time and between states. States are required to report enforcement actions taken against Title V-permitted major and synthetic minor emitters.[4] Also, the introduction of the Enforcement Compliance History Online (ECHO) database in 2002 substantially improved

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[4]While states are required to report enforcement actions taken against major and synthetic minor sources, federal reporting has been the source of contentious debate and some inconsistencies. In the present analysis, some caution is merited in interpreting the data: the absence of actions in the EPA database indicates at least that these actions were not reported to EPA and at most that they did not occur. By extension, it could be argued that any effect observed might reflect either state-level regulatory changes or symbolic opposition to federal overreach in reporting requirements. While either of these effects is interesting, it seems unlikely that states would under-report their enforcement actions on principle and risk the EPA entering to impose stricter enforcement. Nonetheless, replicating the analysis with data collected directly from the states would bolster confidence in the interpretation.

the consistency with which states reported their enforcement activities.[5] EPA modernized its CAA data collection system during the time period under study, and I use data from the modernized dataset, the Integrated Compliance Information System (ICIS-Air) (United States Environmental Protection Agency 2017).

I aggregate facility-level enforcement data from ECHO to produce state-level, annual counts of formal, informal, and total enforcement actions taken by state and, where authorized, local environmental agencies. To control for differences in states' enforcement populations and annual shocks that affect enforcement in all states, I use the residualizing approach recommended by Lee and Lemieux (2010). I regress the logged count of actions on state and year fixed effects, and use the change in the residuals from this regression as the primary dependent variable.[6]

5   Results: Regulatory enforcement under Democratic and Republican administrations

## 5.1   National trends in CAA enforcement

Figure 1 shows national-level trends in states' enforcement efforts taken under the CAA since 2000, in states controlled by Democratic and Republican governors and state legislatures. The figure suggests two patterns of political influence. First, states controlled by Republican governors appear to conduct fewer enforcement actions than their Democratic counterparts, after accounting for between-state differences in the enforcement population. Second, the relationship between party control of government and changes in regulatory enforcement

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[5]Thanks to Mike Barrett, ECHO manager at EPA, for his insights about managing and interpreting data obtained through ECHO.

[6]I also run the models using simple logged counts of actions and the ratio of actions to manufacturing facilities. The results are substantively and statistically similar for the results estimated with the first-differenced counts, ratios, and the residualized, first-differenced counts. Robustness checks suggest better balance between treated and control groups for the results using the residualized, first-differenced variable. Thus, I use the residualized variables due to stronger support for the continuity assumption. I include results using first-differenced logged counts in Appendix Table A-14.

appears stronger for governors than for state legislatures. This is consistent with theoretical expectations that the executive is more easily able to influence administrative policy since it occurs in the executive branch. Nonetheless, this correlation does not provide evidence that the election of a Republican or a Democratic governor makes a meaningful difference in enforcement, all else equal. I turn to the regression discontinuity analysis to adjudicate this question.

Figure 1: National Trends in CAA Enforcement
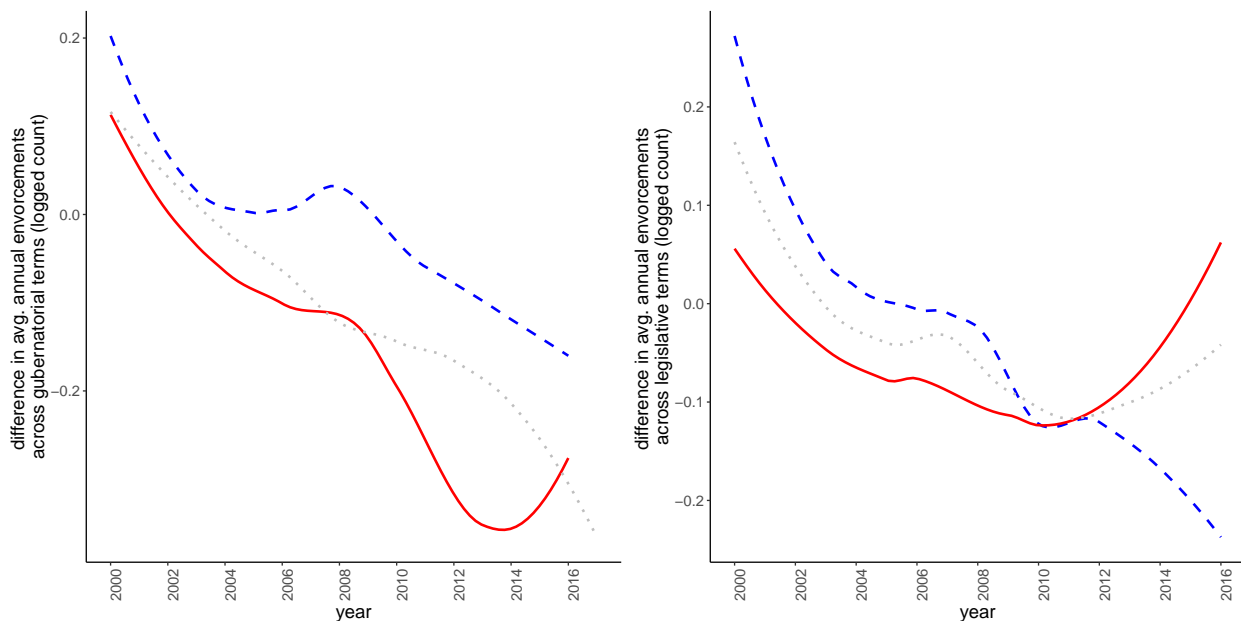
The figure shows enforcement activity in states headed by Democratic (dashed blue line) and Republican (solid red line) governors and state legislatures. The trends reflect the change in the natural log of annual actions taken during a gubernatorial or legislative term, compared with the election year.

5.2   Political Influence Over CAA Enforcement

I first assess the extent to which party control of governors' offices and state legislatures affects regulatory enforcement overall, by assessing discontinuities in total enforcement actions taken by narrowly elected Democratic and Republican governors and state legislatures. Figure 2 shows the effect of Republican control of governors' mansions and statehouses on enforcement actions taken by state agencies. I find evidence that the party of the gov-

ernor has a strong influence on total enforcement, whereas the effect of state legislatures is statistically indistinguishable from zero. This comports with prior research finding that appointment powers afford governors influence over the direction of agency policy (Wood and Waterman 1994, Koski 2007, Woods 2004). The analysis provides new evidence of a partisan difference in governors' influence: narrowly elected Republican governors reduce regulatory enforcement as compared with their Democratic counterparts. In contrast, the analysis suggests that legislatures, due to the challenges associated with collective action, are not as nimble as the executive in influencing policy.

Due to the small sample included in the bandwidth for the analysis of gubernatorial control, I use randomization inference to check the robustness of the large-sample result.[7] The results derived using randomization inference are broadly consistent with the large-sample RD results. While the point estimates are smaller for the difference in means test deployed using randomization inference, results from both models are substantively meaningful. A narrow Republican victory causes a 14%-42% reduction in enforcement, estimated using randomization inference and local linear regression-based RD, respectively. This amounts to between 6 and 18 fewer actions in the median state.

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[7]Appendix Figure A-6 shows the results of the randomization-based test and the large-sample RD.

Figure 2: Political Influence Over CAA Enforcement
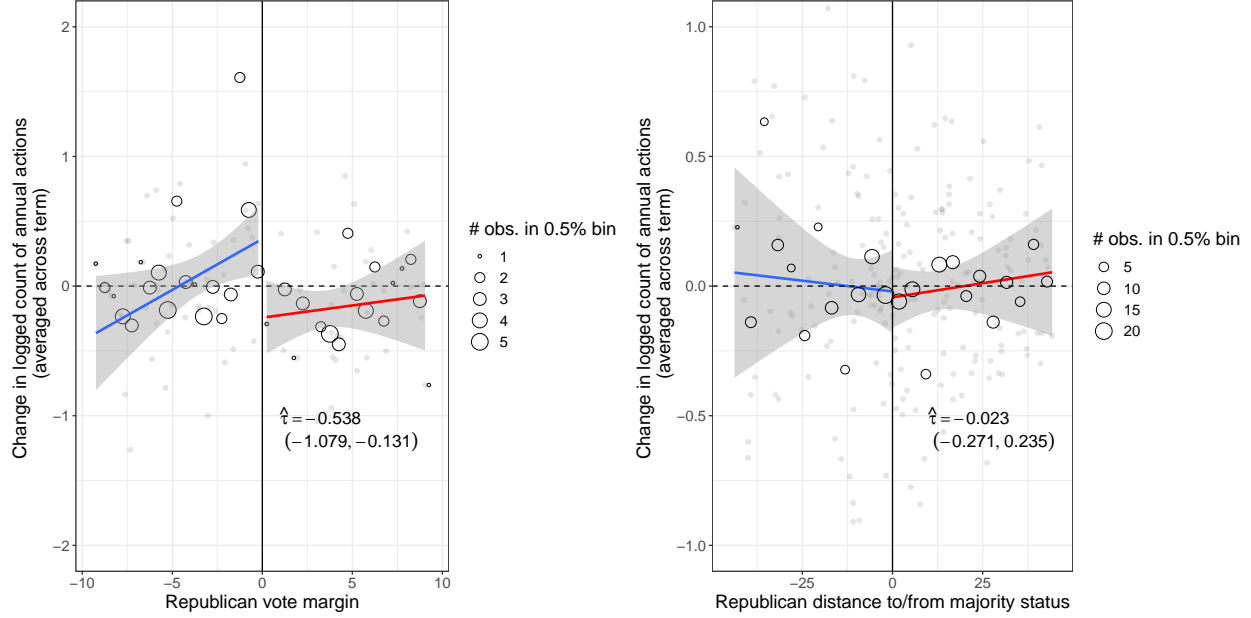
The figure shows the RD effect that narrowly elected Republican governors (left panel) and state legistative majorities (right panel) have on annual enforcement actions taken during each year of their terms, as compared with the year of their election. The effect was estimated with the rdrobust (Calonico et al. 2015) R package, using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and bias-corrected robust confidence intervals. The hollow points reflect local averages for each bin in the data, and they are sized to reflect the number of state-year observations in each bin. The dependent variable is the change in the residuals from a linear regression of the natural log of annual enforcement actions, averaged across the years of gubernatorial or legislative terms, on state and year fixed effects.

5.3   Heterogeneous Effects on Enforcement

These average effects may mask heterogeneity in the effects that legislatures and governors are able to exert through their differential mechanisms of political influence. Thus, I next assess whether governors and legislatures influence different types of agency activities. Figure 3 shows the results from RD models assessing the effect of governors and state legislatures on formal and informal enforcement actions.[8]

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[8]Results from all models are included in the Appendix. The figures and tables in the Appendix reflect results for each year of a term, in addition to the effect presented here, estimated using the average annual change in actions taken during gubernatorial terms and legislative sessions.

Figure 3: Governors' and Legislatures' Effects on Enforcement
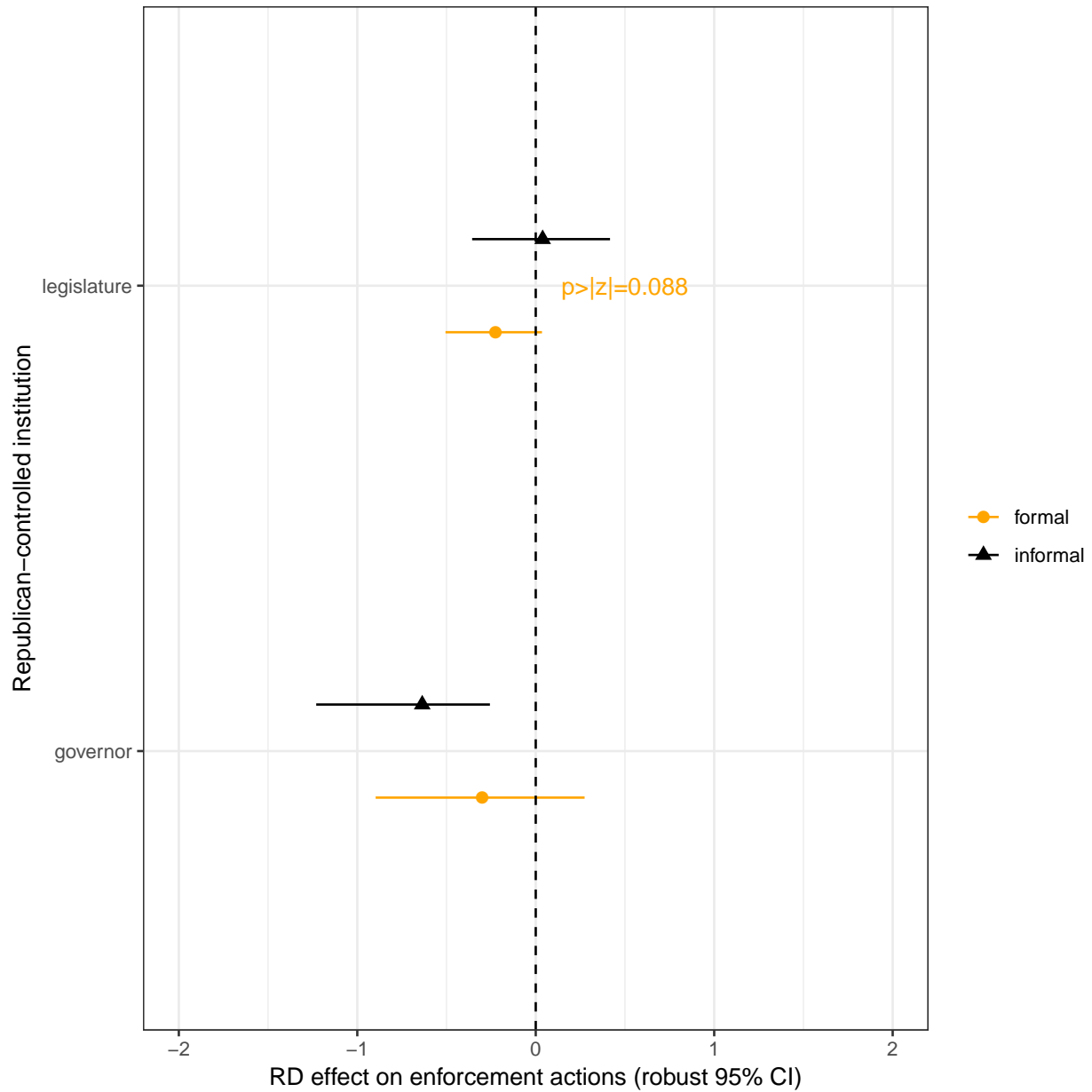
The plot shows the effect of Republican gubernatorial victories and state legislative majorities on formal and informal enforcement activities. The effects were estimated with the rdrobust (Calonico et al. 2015) R package, using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and bias-corrected robust confidence intervals. The dependent variable is the change in the residuals from a linear regression of the natural log of annual enforcement actions, averaged across the years of gubernatorial or legislative terms, on state and year fixed effects.

The results shown in Figure 3 suggest that the lack of an overall effect for state legislatures masks an effect that the legislature is able to achieve over formal enforcement activities. This result is significant at the 90% confidence level, but not the 95% confidence level. It is substantively meaningful in the context of total cross-sectional and over-time variation in enforcement. The effect is approximately one-third of a standard deviation in the dependent variable, and implies a 20% reduction in formal actions taken. The median state would bring three fewer judicial cases or administrative orders under a narrowly elected Republican-controlled state legislature.

The results also suggest that Republican governors exert stronger influence than legislatures, but that governors' influence is most readily observed in informal warning letters and notices of violation. The effect of a Republican gubernatorial victory on formal punitive actions is statistically indistinguishable from zero, whereas the effect on informal actions is robust across the governor's term with the exception of year three.[9] While these differences in effects are not statistically significant, they are suggestive of heterogeneous effects.

Together, these results provide support for the theory that the legislature and governor affect enforcement through distinct channels. Governors exert the strongest influence on regulatory enforcement in terms of substantive magnitude. Their influence is primarily observed in the frequency with which agencies issue warning letters and notices of violation rather than administrative and civil judicial actions. This provides support for the theory that governors influence enforcement by appointing politically friendly agency managers who may adjust agency priorities and the frequency with which the agency takes relatively low-cost signaling actions against polluters. But governors are hamstrung in their ability to influence resource-intensive activities. Instead, through its oversight powers and appropriations authority, the party in control of the state legislature can constrain the resources at an agency's disposal for bringing lawsuits and adjust the rules and procedures that govern formal administrative actions. Notably, the budget mechanism also allows the legislature to

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[9]Appendix Figure A-5 shows the results for all years of governors' terms. Appendix Figure A-3 shows the results estimated using various bandwidths.

change the resources available to the attorney general's office, which is partly or fully responsible for bringing formal enforcement actions. The involvement of the attorney general in these cases may introduce a political check on the governor's power, particularly if the attorney general is of the opposite party.

6   Conclusion: Implications and future research

I have used a regression discontinuity setup to examine whether state legislatures and governors influence enforcement of the Clean Air Act, one of the nation's landmark pollution-control laws. I also develop a theory to explain political influence over state administrative policy, which could be further developed through testing in other issue areas or temporal settings. The findings advance knowledge of state policy responsiveness by identifying the direction and magnitude of the consequences of partisan electoral victories for administrative–rather than legislative–policy outputs. I also find suggestive evidence that governors and state legislatures influence enforcement through different channels. Governors–through their appointment powers and relationships with agency directors–can substantially redirect agency enforcement effort, but their effect is primarily seen in the frequency with which agencies send informal warning letters and notices of violation to Title V-permitted facilities. State legislatures–through their control of the resources available to agencies and attorneys general, committee oversight, and low-profile policy changes–influence the frequency with which agencies level administrative penalties and bring judicial cases.

The results from the analysis of gubernatorial influence suggest that enforcement may be one policy mechanism behind Beland and Boucher's (2015) finding that pollution decreases under narrowly elected Democratic governors. Informal actions generally precede administrative orders and judicial actions and make up the majority of enforcement actions taken by state agencies. Thus, even if governors' influence is primarily felt in informal activity, a shift in informal enforcement under a new governor represents meaningful changes in

states' enforcement regimes. Furthermore, although warning letters and notices of violation require relatively minimal response from the recipient, they send a meaningful signal that can promote compliance and deter future violations at the recipient facility and surrounding facilities (Gray and Shimshack 2011). These results show that elections have consequences for enforcement. Because of the importance of enforcement for promoting compliance and improving environmental performance, elections have consequences for environmental quality.

The analysis also provides evidence that state legislatures influence formal enforcement activity, which is consistent with theoretical and empirical work finding that legislatures influence the bureaucracy through budgeting (Ansolabehere and Snyder 2006, McCubbins et al. 1989, Bryner 1995), oversight (Potoski and Woods 2001, Woods 2013, 2004), or low-profile policy-change tactics (Hacker 2004, Layzer 2012). Extending the time frame to include more elections in the relatively narrow bandwidth supporting the continuity assumption would strengthen the claim that state legislative elections affect formal enforcement activity. Extending the time frame and sample for this analysis would require collecting data directly from state agencies, as the states reported their actions to EPA less consistently prior to the year 2000.

This analysis opens at least three opportunities for further inquiry into political influence over administrative policy, where theoretical development has been rich but empirical evidence is somewhat thin. First, theory suggests that political influence over the bureaucracy varies across policy areas, according to dimensions such as the level of conflict between principals and agents (Waterman et al. 2004, Huber and Shipan 2002), distributive concerns (Ringquist 1994), and information asymmetry (Waterman et al. 2004, Ringquist 1994). This analysis provides evidence for political influence when there is high conflict between political principals of different parties, and between partisan principals and their bureaucratic agents. Future research should compare political influence over bureaucratic behavior across policy areas that vary in goal conflict and other relevant dimensions. Second, future research

should investigate how these effects shrink, grow, counteract, or cumulate in eras with differing degrees of partisan polarization. Third, while these results are consistent with the theorized mechanisms of influence available to governors and state legislatures, qualitative process tracing in a sample of typical cases could further elucidate these mechanisms.

A Appendix

A.1 Robustness Checks for the Continuity Assumption: Large-sample RDD for Gubernatorial Elections

Figure A-1: Robustness Checks for the Continuity Assumption: RD for Gubernatorial Elections
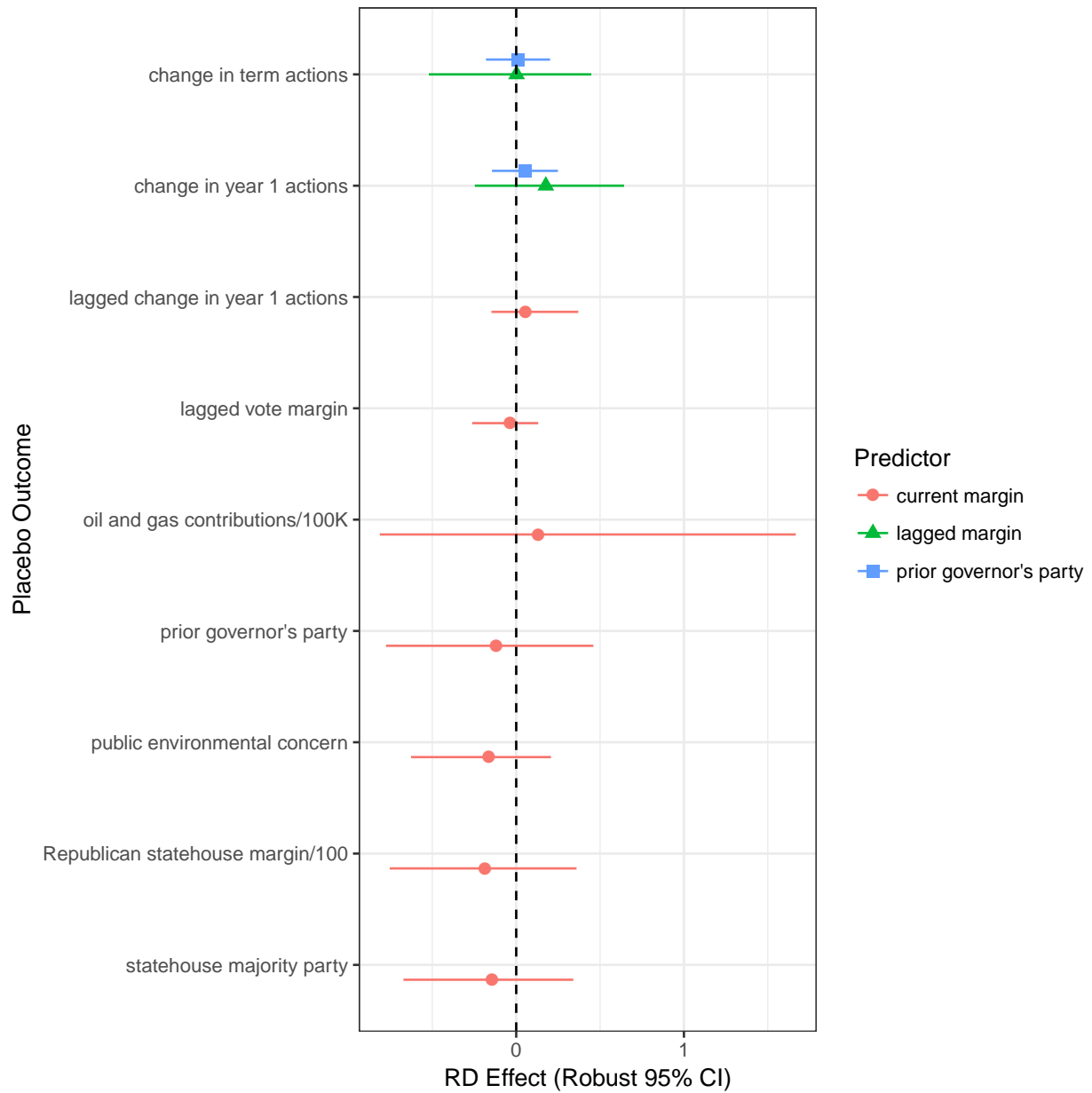
The plot shows 95% confidence intervals for placebo tests of several predictors on placebo outcomes. The estimates reveal no significant discontinuities in placebo outcomes at the treatment-assignment threshold.

Table A-1: Effect of current running variable on change in informal actions lagged by one, two, and three years

Lag (years)	Estimate	Pr >|z|	Eff. N	BW
1	0.056	0.399	77	0.089
	(-0.148, 0.37)			
2	0.043	0.98	88	0.112
	(-0.455, 0.467)			
3	0.264	0.449	66	0.107
	(-0.363, 0.82)			

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is the change in the residuals from a two-way fixed effect regression of the natural log of annual actions on state and year fixed effects.

Table A-2: Effect of lagged running variable on informal actions (first year and averaged across term)

Years since election	Estimate	Pr >|z|	Eff. N	BW
Avg. (1-4)	0.002	0.883	80	0.13
	(-0.521, 0.448)			
Year 1	0.177	0.383	83	0.138
	(-0.247, 0.643)			

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is the change in the residuals from a two-way fixed effect regression of the natural log of annual actions on state and year fixed effects.

Table A-3: Effect of lagged treatment (Republican victory) on informal actions (first year and averaged across gubernatorial term)

	Years since election:	
	Avg. (1-4)	1
Lagged treatment	0.011	0.052
	(0.097)	(0.100)
Constant	0.004	−0.022
	(0.066)	(0.068)
Observations	149	147
R^2	0.0001	0.002
Adjusted R^2	−0.007	−0.005
Note:	*p<0.1; **p<0.05; ***p<0.01	

The effects were estimated using OLS regression.

Table A-4: Effect of current running variable on lagged running variable and lagged treatment

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Lagged vote margin	-0.036	0.514	70	0.11
	(-0.263, 0.131)			
Lagged Republican victory	-0.119	0.616	69	0.105
	(-0.776, 0.46)			

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variables are the lagged running variable and lagged treatment.

Table A-5: RD estimates of effect of current vote margin on legislative vote margin or statehouse party majority

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Legislative vote margin (euclidean distance)	-18.639	0.488	101	0.123
	(-75.409, 35.973)			
Republican statehouse majority	-0.144	0.521	105	0.126
	(-0.672, 0.34)			

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variables are the running variable that assigns treatment and the treatment variablefor the state legislative analysis.

Table A-6: RD estimate of effect of gubernatorial vote margin on public environmental concern

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Public environmental concern	-0.164 (-0.627, 0.207)	0.324	111	0.126

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is state-level environmental concern, estimated using an adaptation of the dynamic group-level item-response theory model developed by Caughey and Warshaw (2015).

A.2 Robustness Checks for the Continuity Assumption: Legislative Elections

Figure A-2: Robustness Checks for the Continuity Assumption: RD for Legislative Elections
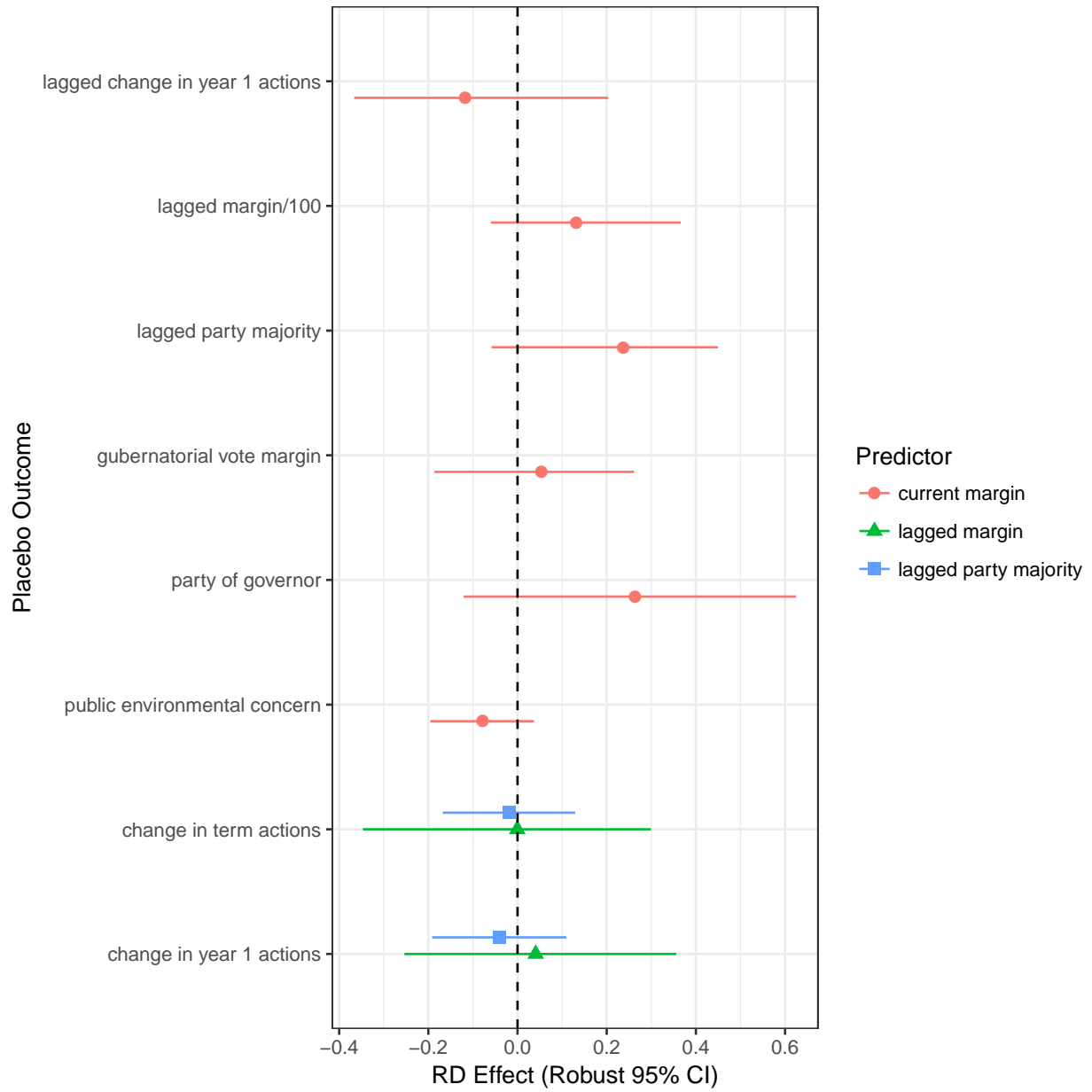
The plot shows 95% confidence intervals for placebo tests of several predictors on placebo outcomes. The estimates reveal no significant discontinuities in placebo outcomes at the treatment-assignment threshold.

Table A-7: Effect of current running variable on lagged outcome

Lag (years)	Estimate	Pr >|z|	Eff. N	BW
1	-0.117 (-0.366, 0.203)	0.574	196	54.481
2	0.051 (-0.308, 0.43)	0.745	212	65.324
3	0.061 (-0.313, 0.487)	0.67	177	60.398

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is the change in the residuals from a two-way fixed effect regression of the natural log of annual actions on state and year fixed effects.

Table A-8: Effect of lagged running variable on current outcome variable (first year and averaged across term)

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Avg. (1-2 or 1-4)	-0.001 (-0.347, 0.299)	0.885	181	43.619
Year 1	0.041 (-0.254, 0.356)	0.742	213	66.942

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is the change in the residuals from a two-way fixed effect regression of the natural log of annual actions on state and year fixed effects.

Table A-9: OLS Effect of lagged treatment (Republican majority/minority status) on current outcome variable (first year and averaged across gubernatorial term)

	Years since election	
	Avg.	1
Lagged Republican majority status	−0.019	−0.041
	(0.075)	(0.077)
Constant	0.006	0.016
	(0.055)	(0.056)
Observations	288	286
R^2	0.0002	0.001
Adjusted R^2	−0.003	−0.003

Note:	*p<0.1; **p<0.05; ***p<0.01

The effects were estimated using OLS regression.

Table A-10: RD estimates of effect of current running variable on lagged running variable and lagged treatment

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Lagged distance to majority	13.161	0.159	171	40.403
	(-5.972, 36.586)			
Lagged Republican majority status	0.237	0.131	198	54.248
	(-0.058, 0.449)			

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variables are the lagged running variable and lagged treatment.

Table A-11: RD estimates of effect of current vote margin on gubernatorial vote margin or party of governor

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Gubernatorial vote margin	0.053 (-0.186, 0.261)	0.743	122	65.893
Party of governor	0.264 (-0.121, 0.625)	0.186	117	60.286

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variables are the vote margin and the treatment variable for the gubernatorial analysis.

Table A-12: RD estimates of effect of running variable on public environmental concern

Outcome variable	Estimate	Pr >|z|	Eff. N	BW
Environmental concern	-0.078 (-0.196, 0.037)	0.18	253	72.306

The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The outcome variable is state-level environmental concern, estimated using an adaptation of the dynamic group-level item-response theory model developed by Caughey and Warshaw (2015).

A.3   Results

Figure A-3: Point estimates and confidence intervals at various bandwidths
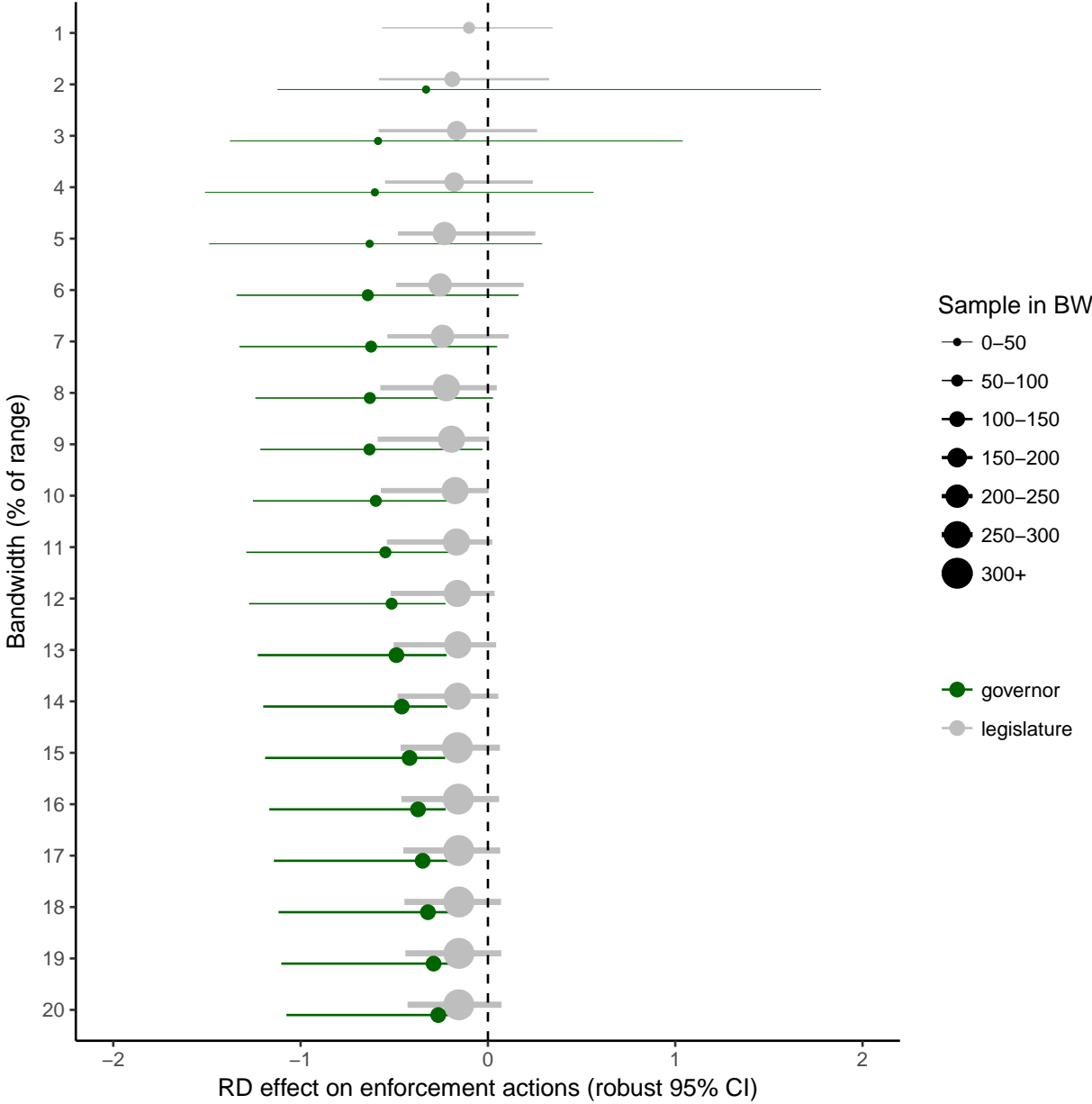
The figure shows the point estimates and bias-corrected robust 95%confidence intervals for the large-sample RD estimates of the influence of Republican governors and state legislatures on informal and formal enforcement actions, respectively.

A.4 Results: Governors

Figure A-4: Governors' Influence Over Informal Enforcement
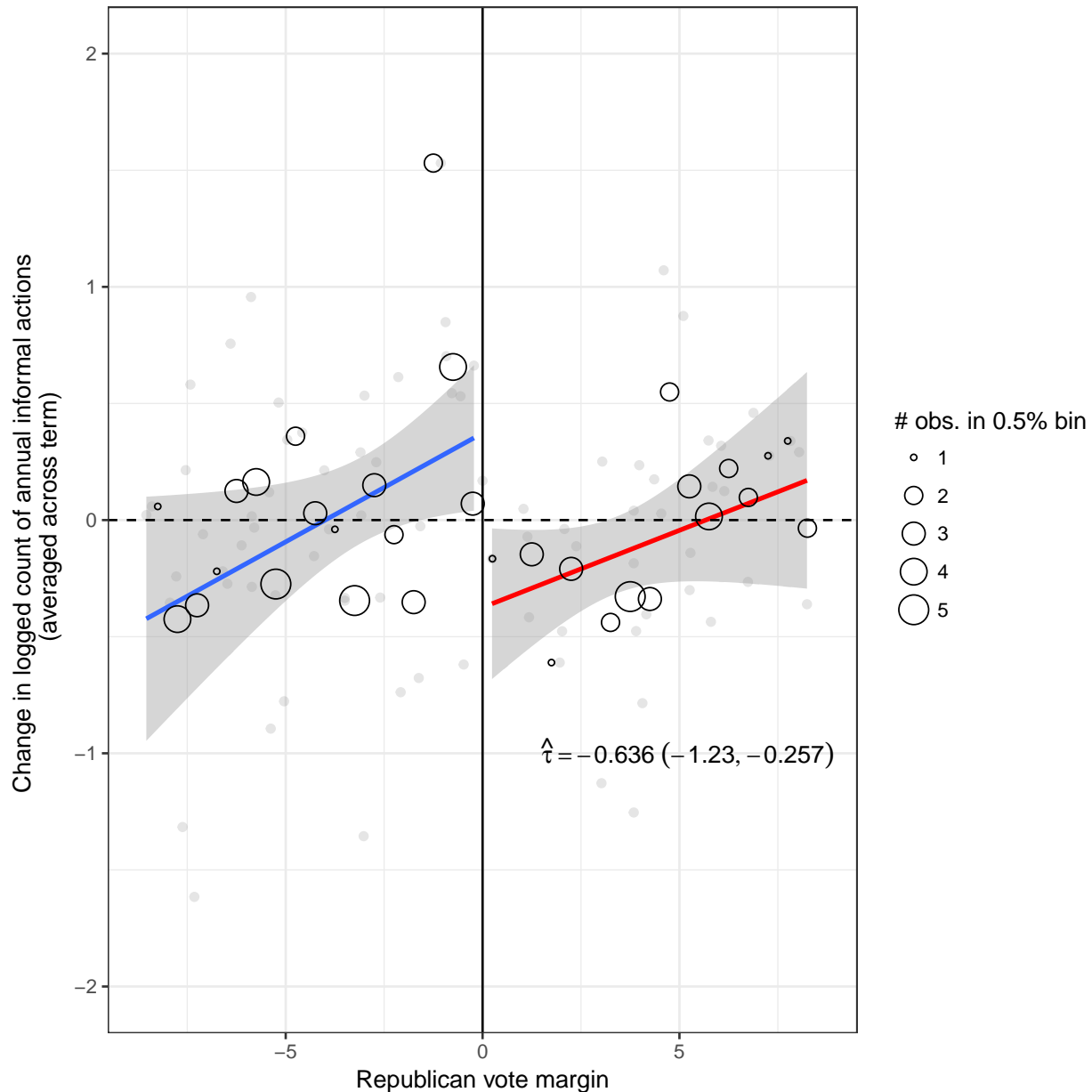
The figure shows the RD effect that narrowly elected Republican governors have on the change in annual informal enforcement actions taken during each year of their term, as compared with the year of their election. The effect was estimated with the rdrobust (Calonico et al. 2015) R package, using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and bias-corrected robust confidence intervals. The hollow points reflect local averages for each bin in the data, and they are sized the reflect the number of state-year observations in each bin. The dependent variables are the residuals from two-way fixed-effects regressions of the logged change in annual enforcement actions taken one, two, three, and four years after an election, and averaged across gubernatorial terms, on state and year fixed effects.

Table A-13: Results Derived from Residualized, Logged Count

| Years After Election	Estimate	Pr >|z|	Eff. N	BW
Total Actions				
1	-0.566 (-1.131, -0.164)	0.009	87	0.094
2	-0.439 (-1.028, 0.045)	0.072	85	0.092
3	-0.458 (-1.091, 0.066)	0.083	83	0.098
4	-0.659 (-1.403, -0.123)	0.019	85	0.107
Avg.	-0.538 (-1.079, -0.131)	0.012	87	0.094
Informal Actions				
1	-0.747 (-1.475, -0.267)	0.005	80	0.087
2	-0.468 (-1.037, -0.045)	0.032	89	0.101
3	-0.302 (-1.032, 0.244)	0.226	86	0.113
4	-0.863 (-1.733, -0.334)	0.004	72	0.088
Avg.	-0.636 (-1.23, -0.257)	0.003	81	0.087
Formal Actions				
1	-0.162 (-0.756, 0.392)	0.535	107	0.141
2	-0.086 (-0.748, 0.639)	0.879	93	0.123
3	-0.486 (-1.285, 0.219)	0.165	85	0.118
4	-0.315 (-1.174, 0.705)	0.624	84	0.125
Avg.	-0.3 (-0.897, 0.274)	0.297	101	0.129

Effect of Republican victory on annual enforcement actions, from 1-4 years after an election. The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The dependent variable is the change in residuals from a fixed effects regression with state and year fixed effects on the natural log of annual enforcement actions.

Table A-14: Results Derived from Change in Logged Count of Enforcement Actions

Years After Election	Estimate	Pr >|z|	Eff. N	BW
Total Actions				
1	-0.575 (-1.128, -0.181)	0.007	91	0.096
2	-0.365 (-0.978, 0.189)	0.185	99	0.111
3	-0.328 (-1.295, 0.58)	0.455	102	0.119
4	-0.76 (-2.037, 0.201)	0.108	109	0.13
Avg.	-0.653 (-1.183, -0.243)	0.003	96	0.1
Informal Actions				
1	-0.715 (-1.419, -0.243)	0.006	81	0.088
2	-0.279 (-0.84, 0.185)	0.21	97	0.114
3	-0.106 (-1.113, 0.752)	0.705	107	0.136
4	-0.816 (-2.155, 0.184)	0.099	106	0.132
Avg.	-0.698 (-1.302, -0.3)	0.002	89	0.094
Formal Actions				
1	-0.195 (-0.798, 0.36)	0.459	104	0.132
2	-0.03 (-0.663, 0.699)	0.959	97	0.127
3	-0.223 (-1.269, 0.872)	0.717	92	0.122
4	-0.325 (-1.619, 0.975)	0.627	103	0.144
Avg.	-0.367 (-0.996, 0.177)	0.171	107	0.139

Effect of Republican victory on the change in annual informal enforcement actions, from 1-4 years after an election. The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The dependent variable is the change in the natural log of annual actions.

Figure A-5: RD Effect of Republican Control of Governorships on CAA Enforcement Actions
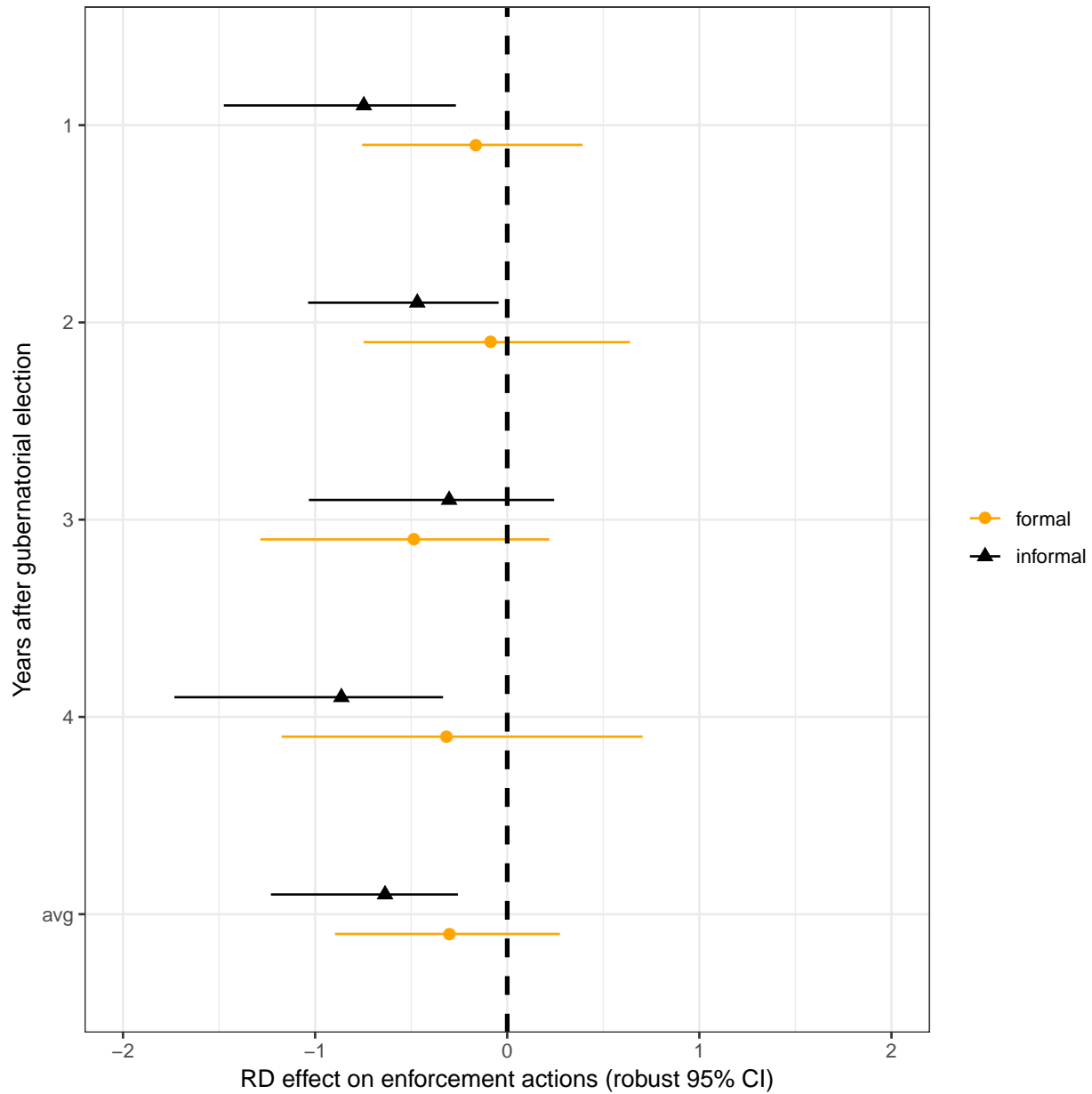
The plot shows 95% confidence intervals for the effect of Republican gubernatorial election victory on enforcement activity. The models were estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and robust confidence intervals. The dependent variable is the change in the natural log of annual enforcement actions, averaged across gubernatorial terms. The dependent variables are the residuals from two-way fixed-effects regressions of the logged change in annual enforcement actions taken one, two, three, and four years after an election, and averaged across gubernatorial terms, on state and year fixed effects.

Table A-15: Point estimates and confidence intervals at various bandwidths

Estimate	Pr >|z|	Eff. N	BW
-0.23	0.21	8	0.01
(-4.369, 0.96)			
-0.33	0.659	15	0.02
(-1.124, 1.778)			
-0.587	0.784	22	0.03
(-1.378, 1.04)			
-0.603	0.371	36	0.04
(-1.511, 0.564)			
-0.631	0.186	45	0.05
(-1.488, 0.29)			
-0.641	0.125	60	0.06
(-1.342, 0.163)			
-0.624	0.069	68	0.07
(-1.327, 0.05)			
-0.63	0.061	77	0.08
(-1.241, 0.027)			
-0.632	0.04	84	0.09
(-1.215, -0.028)			
-0.598	0.016	90	0.1
(-1.255, -0.131)			
-0.547	0.007	95	0.11
(-1.29, -0.21)			
-0.514	0.005	98	0.12
(-1.275, -0.226)			
-0.488	0.005	106	0.13
(-1.229, -0.221)			
-0.46	0.005	110	0.14
(-1.2, -0.216)			
-0.418	0.004	118	0.15
(-1.19, -0.228)			
-0.373	0.004	125	0.16
(-1.167, -0.226)			
-0.348	0.005	127	0.17
(-1.143, -0.207)			
-0.32	0.005	134	0.18
(-1.118, -0.195)			
-0.29	0.006	139	0.19
(-1.103, -0.189)			
-0.265	0.006	145	0.2
(-1.076, -0.176)			

The table shows RD estimates for the effect of Republican gubernatorial victories on informal enforcement, estimated using bandwidths from 0 to 0.2 of proportional vote margin. The dependent variable is the change in logged count of annual actions taken during each year of a governor's term.

A.5 Randomization-Based Analysis of Gubernatorial Influence

Since gubernatorial elections only occur every four years, my analysis includes a small number of elections and an even smaller number of close elections. As a robustness check for the analysis of gubernatorial control, I complement the RD with randomization inference, a statistical technique designed for hypothesis testing with small samples.

In the randomization-inference framework, the researcher tests the sharp null hypothesis that there is no treatment effect for any unit. Under this sharp null, the potential outcomes for all units are known, since the potential outcome for each unit is the same under the observed randomization of treatment and any counterfactual treatment assignment. Since all potential outcomes are known under the null hypothesis, the researcher can derive the distribution of any test statistic and determine the p-value and confidence interval for an observed test statistic based on this distribution. The distribution of test statistics thus provides a reference distribution through which to determine the probability of observing the effect that was actually observed (Rosenbaum 2010, Ch. 2), and rejecting or failing to reject the null hypothesis.

To derive the distribution of the test statistic (in this, case, a t-test of difference of means) and test the hypothesis, the researcher simulates all possible random assignments of treatment and determines the probability of observing the observed test statistic, if the null hypothesis is true. Under the additional local stable unit treatment value assumption, the test can be inverted to derive a confidence interval for the size of the effect (Ho and Imai 2006, Cattaneo et al. 2015, Rosenbaum 2010). I follow the approach developed by Cattaneo et al. (2015) for applying randomization inference to calculate a p-value and confidence interval in a regression discontinuity setup. I test the sharp null hypothesis that there is no effect on enforcement following the election of a Republican governor. Table A-16 and Figure A-8 reflect the results and confidence intervals for this difference in means test, estimated using the residualized and first-differenced dependent variable. Figure A-6 compares the estimates derived using randomization inference and the conventional large-sample RD

design. The stricter identification assumption required to apply randomization inference narrows the bandwidth within which the analysis is conducted. This increases the efficiency of the estimates, but also reduces their substantive magnitude. Still, overall the results from randomization inference support the conclusions drawn using the conventional RD.

Table A-16: P-values for Fisher's Exact Test of Effect of Republican Gubernatorial Control on CAA Enforcement

Outcome Variable	Year 1	Year 2	Year 3	Year 4	Average
Total actions (residualized, logged count)	0.0427	0.0977	0.1365	0.0706	0.0373
Total actions (logged count)	0.0421	0.1597	0.3923	0.19	0.0135

This table shows p-values for a difference of means test in a test of the sharp null hypothesis that there is no difference in mean outcomes between Republican and Democratic-led environmental agencies.

Figure A-6: Randomization Inference and Large-Sample RD Estimates of Effect of Republican Control of Governorships on CAA Enforcement Actions
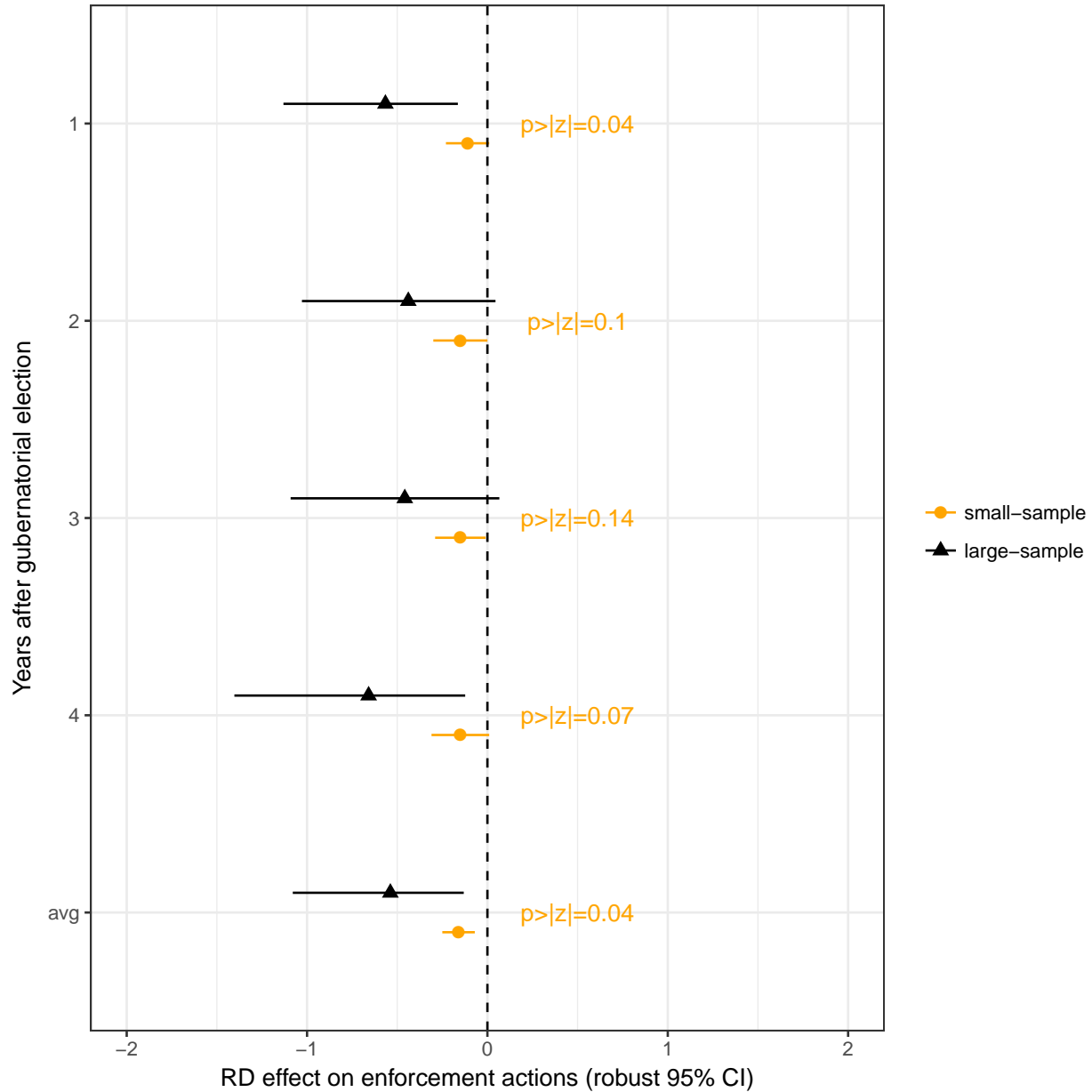

The plot shows the effect of Republican gubernatorial victories on CAA enforcement. The effect was estimated with the rdlocrand (Cattaneo et al. 2018) (small sample) and rdrobust (Calonico et al. 2015) (large sample) R packages. The large-sample model was estimated using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and robust confidence intervals. The small-sample confidence intervals correspond to a difference-in-means test. The dependent variable is the change in the residuals from a linear regression of the natural log of annual enforcement actions, averaged across the years of a governor's term, on state and year fixed effects and an intercept term.

Applying randomization inference in an RD design rests on the assumption of local randomization of treatment, which is more stringent than the continuity assumption (De la Cuesta and Imai 2016). When elections are very close, assignment into treatment (Republican victory) and control (Democratic victory) is quasi-random. Quasi-random election outcomes approximates random assignment to treatment in an experimental setting. Random assignment ensures that the distribution of potential outcomes is the same in treated and control groups. This balance assures the researcher that the difference in average outcomes between the two groups is caused by the treatment rather than systematic differences in background variables between the two groups.

To test this assumption and select the bandwidth for the randomization-based RD analysis, I use a procedure that is similar to the robustness checks used to examine the validity of the continuity assumption in large-sample RDs. I test for balance in the same pre-treatment covariates and placebo outcomes used to test the continuity assumption in the large-sample RD models. I then select the largest window within which I cannot reject the null hypothesis of no difference in means in pretreatment covariates and placebo outcomes between treatment and control groups. I use a confidence level of 85% for this test, under the logic that the greater danger is in committing Type II error in bandwidth selection (Cattaneo et al. 2015). Failure to reject the null hypothesis provides evidence that the local randomization assumption holds within a given window around the treatment-assignment threshold. To avoid mistakenly conducting the analysis within a bandwidth where there is imbalance across pretreatment covariates, I set a significance level that makes it more difficult to fail to reject the null hypothesis (Cattaneo et al. 2015). Figure A-7 shows the p-values associated with a series of difference-of-means tests for windows of varying widths across the treatment-assignment thresholds, along with the bandwidths used for the randomization-based RD. I use the widest bandwidth within which I can reasonably rule out imbalance on all of the pretreatment covariates and placebo outcomes, with $\alpha = 0.15$.

A.6 Bandwidth Selection for Randomization Inference

Figure A-7: Bandwidth Selection for Randomization Inference-based RD Analysis of Effect of Republican Gubernatorial Control on CAA Enforcement
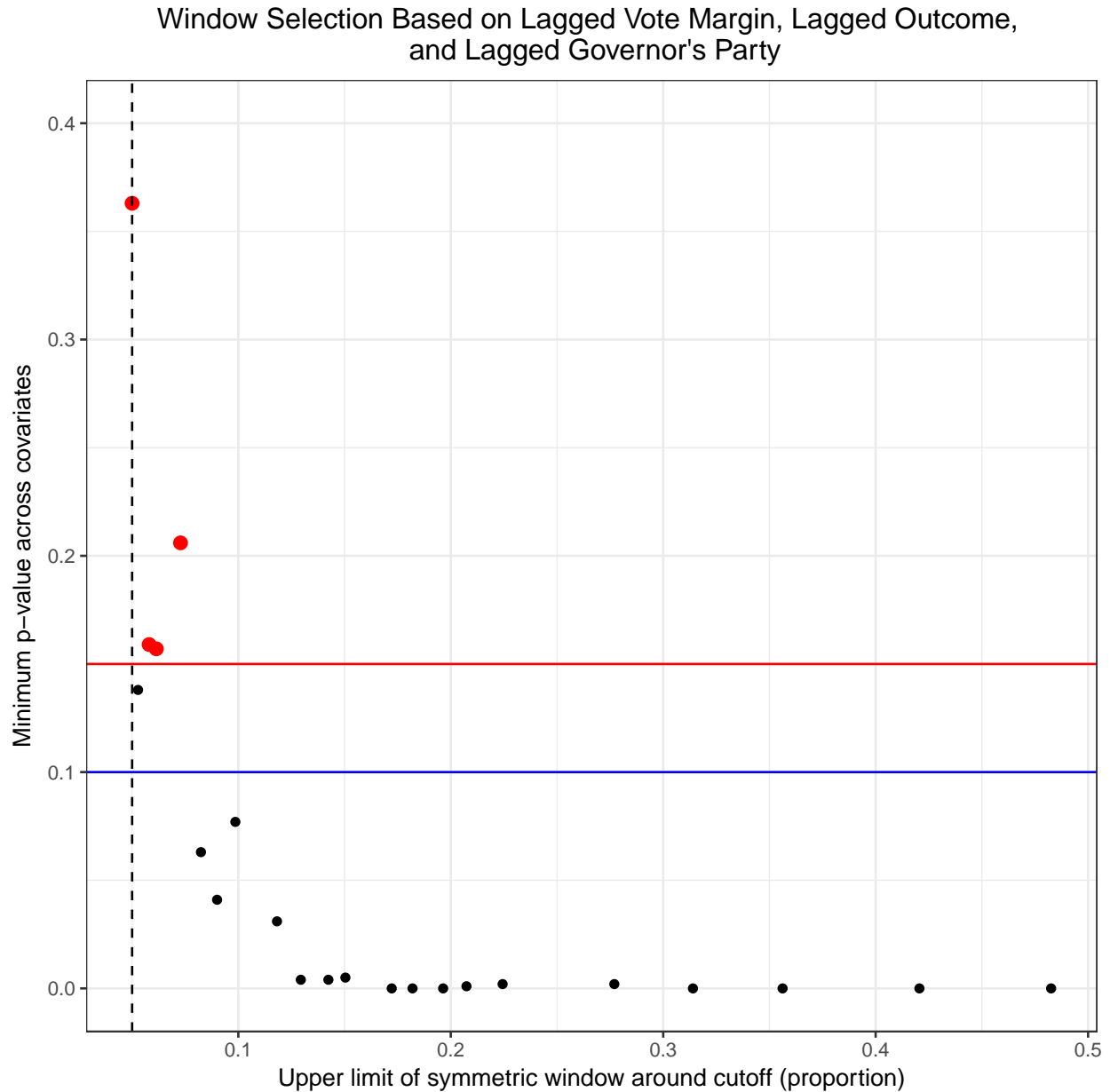
The plot shows the minimum p-value for difference of means tests used to verify the local-randomization assumption and choose a bandwidth for RD analysis using randomization inference. The groups are defined by the treatment-assignment threshold (50% vote share), and each point reflects the minimum p-value for a difference in means test between treated and control groups on the same covariates used to test the validity of local randomization in the large-sample RD. These are the previous election's margin, the outcome variable lagged by one year, and the prior governor's party.

Figure A-8: Randomization Inference: Effect of Republican Control of Governorships on CAA Enforcement Actions
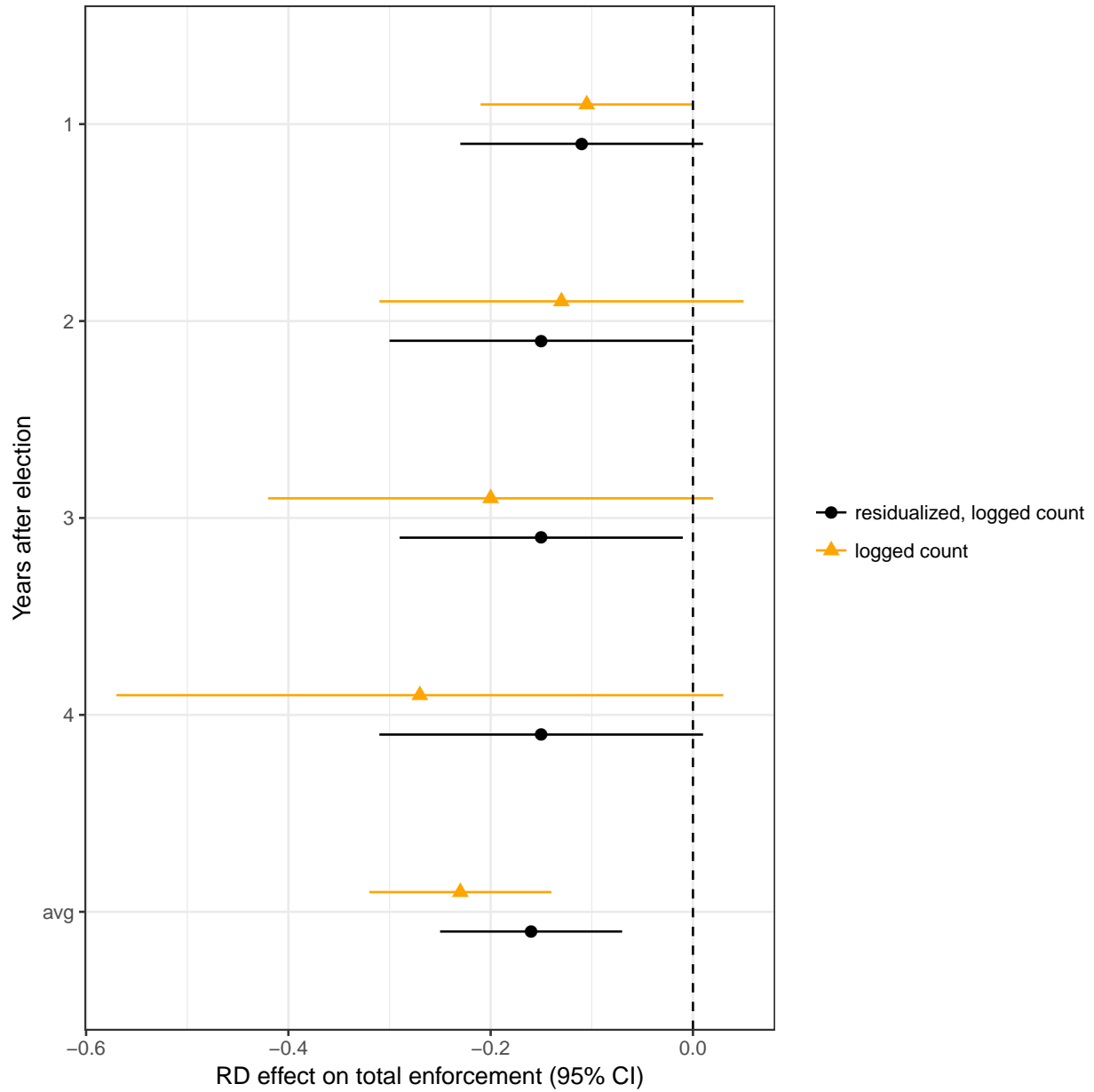
The plot shows 95% confidence intervals for the difference of means in enforcement activity between Republican and Democratic governors elected with +/- 5% vote margins. The confidence intervals were calculated using the rdlocrand package (Cattaneo et al. 2018). The dependent variable is the change in annual enforcement actions taken one, two, three, and four years after an election, and averaged across gubernatorial terms.

A.7 Results: State Legislatures

Figure A-9: Effect of Republican Control of Governorships on CAA Enforcement Actions
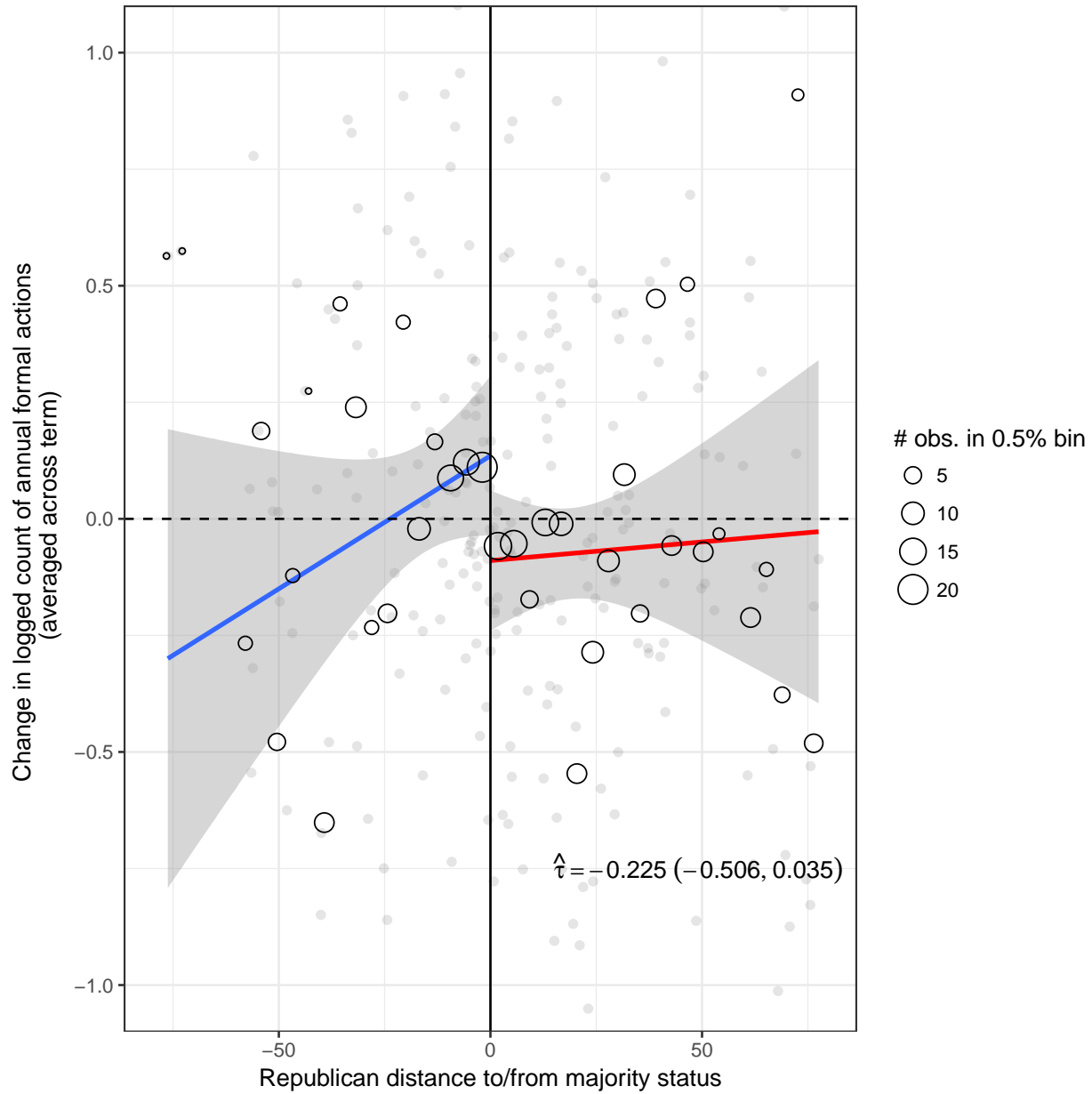
The plot shows the effect of Republican statehouse majorities on CAA enforcement. The effect was estimated with the rdrobust (Calonico et al. 2015) R package, using a triangular-kernel local linear estimator, MSE-optimal bandwidth, and robust confidence intervals. The estimate is significant at the 90% confidence level.

Table A-17: Results Derived from Residualized, Logged Count

| Years after election	Estimate	Pr >|z|	Eff. N	BW
Total Actions				
1	-0.058 (-0.304, 0.167)	0.567	233	57.705
2	-0.088 (-0.391, 0.206)	0.544	203	46.758
	(-0.456, 0.26)			
Avg.	-0.023 (-0.271, 0.235)	0.888	205	45.403
Informal Actions				
1	0.019 (-0.351, 0.381)	0.935	211	50.181
2	0.013 (-0.445, 0.45)	0.992	215	51.442
Avg.	0.039 (-0.356, 0.416)	0.879	208	48.425
Formal Actions				
1	-0.227 (-0.542, 0.051)	0.104	239	68.611
2	-0.224 (-0.543, 0.048)	0.101	226	60.731
Avg.	-0.225 (-0.506, 0.035)	0.088	253	78.636

Effect of Republican victory on annual enforcement actions, from 1-2 years after an election, and averaged across legislative sessions. The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The dependent variable is the change in residuals from a fixed effects regression with state and year fixed effects on the natural log of annual enforcement actions.

Table A-18: Results Derived from Change in Logged Count of Enforcement Actions

Years after election	Estimate	Pr >|z|	Eff. N	BW
Total Actions				
1	-0.047 (-0.294, 0.177)	0.626	233	58.724
2	-0.08 (-0.469, 0.332)	0.738	229	56.326
Avg.	-0.012 (-0.257, 0.238)	0.94	211	48.243
Informal Actions				
1	0.037 (-0.313, 0.389)	0.832	217	53.998
2	0.089 (-0.379, 0.62)	0.637	219	53.217
Avg.	0.054 (-0.323, 0.429)	0.783	213	49.929
Formal Actions				
1	-0.222 (-0.54, 0.07)	0.131	245	74.521
2	-0.218 (-0.606, 0.157)	0.248	237	68.056
Avg.	-0.239 (-0.537, 0.033)	0.083	252	76.653

Effect of Republican statehouse majority status on the change in annual informal enforcement actions, from 1-2 years after an election and averaged across the legislative term. The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The dependent variable is the change in the natural log of annual actions.

Figure A-10: RD Effect of Republican Control of State Legislatures on CAA Punitive Actions, Large-Sample RD
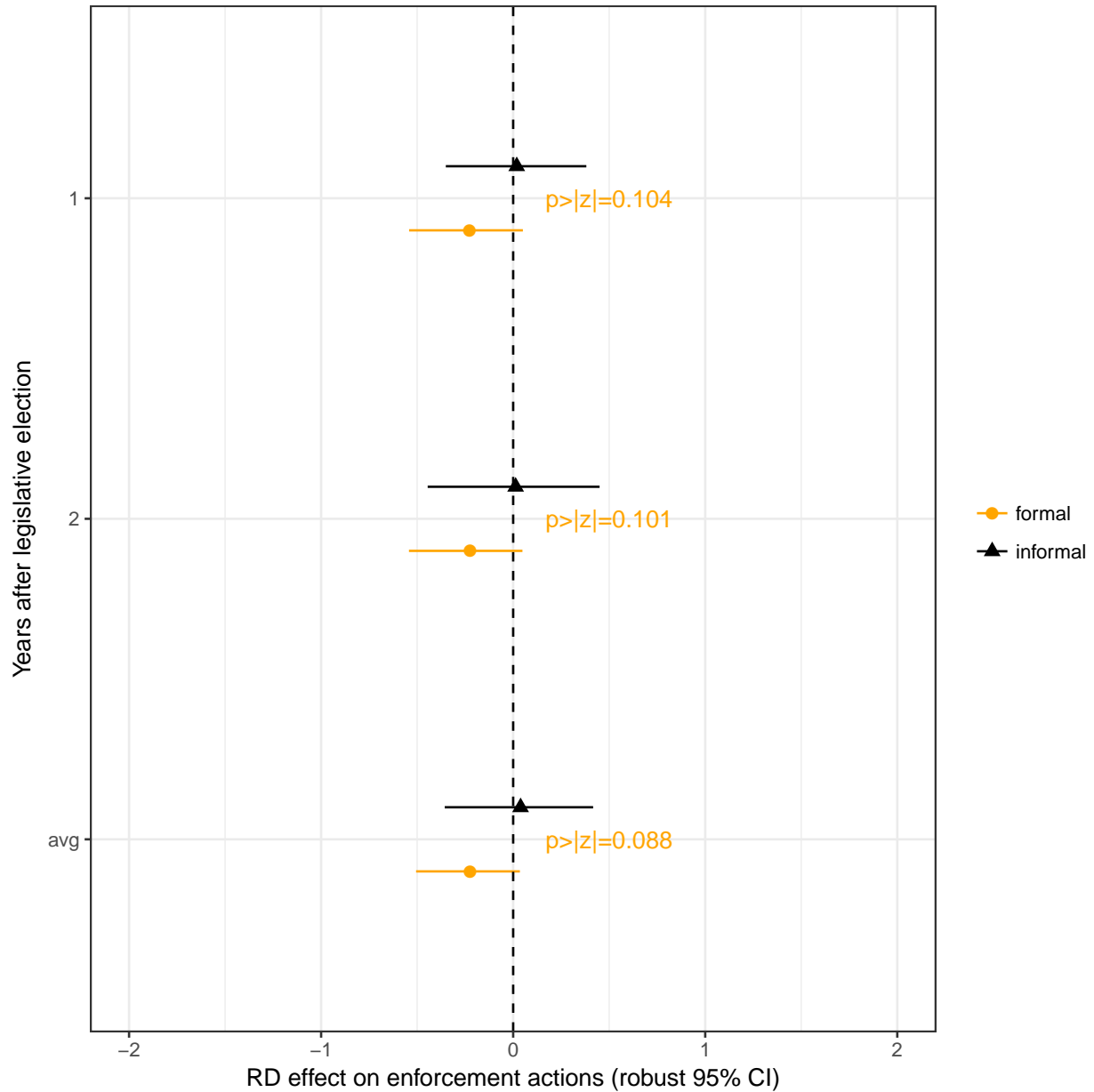
Effect of Republican legislative victory on the change in annual enforcement actions, in the first two years years after an election. The effect was estimated using the rdrobust package (Calonico et al. 2015), using a triangular-kernel local linear estimator, MSE-optimal bandwidths, and robust confidence intervals. The dependent variable is the change in residuals from a two-way fixed effect regression of the natural log of annual actions on state and year fixed effects.

Table A-19: Point estimates and confidence intervals at various bandwidths

Estimate	Pr >|z|	Eff. N	BW
-0.101	0.636	75	10
(-0.565, 0.345)			
-0.19	0.583	124	20
(-0.582, 0.327)			
-0.166	0.457	158	30
(-0.583, 0.262)			
-0.18	0.442	189	40
(-0.549, 0.24)			
-0.232	0.543	213	50
(-0.481, 0.253)			
-0.255	0.39	230	60
(-0.49, 0.191)			
-0.243	0.198	243	70
(-0.537, 0.111)			
-0.222	0.097	255	80
(-0.574, 0.047)			
-0.194	0.054	267	90
(-0.589, 0.006)			
-0.175	0.053	278	100
(-0.572, 0.003)			
-0.168	0.072	286	110
(-0.539, 0.024)			
-0.163	0.088	291	120
(-0.519, 0.036)			
-0.161	0.1	294	130
(-0.503, 0.044)			
-0.162	0.12	299	140
(-0.482, 0.055)			
-0.163	0.137	303	150
(-0.466, 0.064)			
-0.158	0.131	305	160
(-0.462, 0.06)			
-0.156	0.144	307	170
(-0.452, 0.066)			
-0.155	0.153	308	180
(-0.446, 0.07)			
-0.155	0.157	309	190
(-0.44, 0.071)			
-0.155	0.164	313	200
(-0.429, 0.073)			

The table shows RD estimates for the effect of Republican state legislative majorities on formal enforcement, estimated using bandwidths from 0 to 20% of the range of the running variable. The dependent variable is the change in residualized, logged count of annual actions taken during each year of a legislative term.

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