Smokestacks and the Swamp*

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Abstract

We examine the causal effect of politicians' partisan ideologies on firms' industrial pollution decisions. Using a regression discontinuity design involving close U.S. congressional elections, we show that plants increase pollution and invest less in abatement following close Republican wins. We also find evidence of reallocation: firms shift emissions away from areas represented by Democrats. However, costs rise and M/B ratios decline for firms whose representation becomes more Democratic, suggesting that politicians' ideological demands can be privately costly. Pollution-related illnesses spike around plants in Republican districts, suggesting that firms' pass-through of politicians' ideologies can have real consequences for local communities.

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1 Introduction

How do politicians' beliefs and preferences affect the firms they represent? A growing literature in political economy has shown that politicians' personal ideologies can significantly affect their legislative voting behavior and economic outcomes (Alesina, 1988; Lee, Moretti, and Butler, 2004; List and Sturm, 2006; Ferreira and Gyourko, 2009).[1] However, the literature linking politicians and firms has ignored politicians' ideologies and has largely focused on political access and political power as determinants of firm outcomes. As such, little is known about whether politicians push their own personal ideologies on constituent firms, a topic we examine in the context of firms' industrial pollution decisions and financial performance.

Anecdotal evidence suggests that politicians are capable of pushing their personal ideologies on even the largest of corporations. For example, Amazon Inc. cited the opposition of local politicians such as Rep. Alexandria Ocasio-Cortez (D–NY) in February 2019 when announcing its decision to cancel its planned $2.5 billion second headquarters location in Queens, NY – the largest economic development project in New York state history – despite strong support from local voters, New York City Mayor Bill de Blasio (D), and Governor Andrew Cuomo (D).[2] Even though a majority of her constituents supported Amazon's proposal, Rep. Ocasio-Cortez was triumphant following Amazon's cancellation, tweeting that "[t]oday was the day a group of dedicated, everyday New Yorkers and their neighbors defeated Amazon's corporate greed, its worker exploitation and the power of the richest man in the world."[3] Hence, Amazon's cancellation decision was seemingly a function of the personal ideologies of a few politicians rather than the preferences of the constituents these politicians were elected to represent.

Do firms such as Amazon routinely bend to the ideological wills of their local political representatives? Motivated by this question, we estimate the effects of politicians' partisan personal ideologies

[1] Alesina (1988), Lee, Moretti, and Butler (2004), and List and Sturm (2006) (among others) show that, as a result of their individual preferences or their party affiliation, candidates may have different private utility than their constituents, and that this in turn can result in different policy outcomes. We refer to such differences as candidates'/parties' "ideologies." We assume this definition carries over to differences in beliefs as well.

[2] A large majority of voters in Rep. Ocasio-Cortez's district supported Amazon's proposal (Vielkind, Jimmy, "Most in Ocasio-Cortez's District Opposed Her on Amazon Deal, Poll Finds," *Wall Street Journal*, April 10, 2019). Overall, more than two-thirds of New York state voters supported Amazon's proposed plan, with voters singling out Rep. Ocasio-Cortez as the "biggest villain' in the three-month debacle (https://ny.curbed.com/2019/3/18/18271134/amazon-new-york-voters-losing-hq2-bad-siena-poll).

[3] She later tweeted that she was "[w]aiting on the haters to apologize after we were proven right on Amazon and saved the public billions."

on firm decision-making and financial performance by examining the industrial pollution decisions of constituent firms following close U.S. House of Representatives elections. We focus on industrial pollution because, while firm decisions such as leverage and investment are usually non-partisan, industrial pollution is a major source of partisan friction that generates large ideological variation across politicians. For example, Figure 1 shows a 60-point divide between House Democrats' and House Republicans' median environmental voting records, a divide which has grown noticeably over time. In addition, unlike Compustat financial variables, emissions data and an index of production are available at the facility (and chemical) level for a large, representative sample of publicly- and privately-owned plants, providing a greater window into how ideology might affect firm decision-making. Finally, given the expanding effects of environmental factors on economic growth, firms' pollution decisions are becoming increasingly important in their own right.

The main challenge in estimating the causal effect of politicians' ideologies on firm outcomes is empirical: it is difficult to separate the effects of politicians' ideologies from effects such as shifting voter ideologies or changing economic conditions.[4] To overcome this challenge, we focus on the outcomes of close U.S. House of Representatives elections using a regression discontinuity design (RDD or RD design). Our RD design compares facility-level emissions across districts where a Democratic candidate barely won or barely lost their elections. As in Lee, Moretti, and Butler (2004), Ferreira and Gyourko (2009), and Akey (2015), the key identifying assumption is that districts where Democrats win close elections are similar along other dimensions to districts where Republicans win close elections. We also complement our RD tests with difference-in-differences tests that exploit various cross-sectional features of our data. We focus on Congressional election outcomes (versus gubernatorial or local election outcomes) because House members have no executive authority over their districts and federal law applies equally to all plants. These characteristics allow us better isolate ideologically-driven actions from actions caused by changes in the legislative or economic environment.[5]

[4]Identifying the effects of politicians' ideologies on firm outcomes is also difficult because legislation generally affects all firms at once and reflects the ideologies of many politicians; using speeches or other forms of political voice is complicated by the lack of a measurable connection or time frame between the speech and firms' actions; and measuring firms' responses to politicians' ideological beliefs is challenging because this information is difficult to disentangle from firm-level financial statements and requires detailed information on firms' business decisions.

[5]Congress has no executive authority under Article II of the U.S. Constitution. In contrast, state governors have significant control over their states' legislative agendas and generally possess unilateral executive authority over state agencies, making it more difficult to distinguish ideologically-driven actions from those imposed for other reasons, such as changing economic conditions. Another reason to focus on Congressional districts is that doing so allows us to exploit within-state variation, thus accounting for the governor's own ideology and the general ideology/regulatory posture of state agencies.

Our primary hypothesis is that, all else equal, closely-elected Democrats will be more ideologically predisposed to pay attention to the monitoring and enforcement of existing federal environmental laws within their districts.[6] As a result, when a Democrat is elected, state and federal regulators may either implicitly or explicitly face incentives to strengthen environmental oversight within the politician's district. Hence, we argue that the monitoring and enforcement of existing federal industrial pollution regulations will be stronger in districts represented by Democrats than Republicans. This argument is consistent with Glaeser and Shleifer (2001), who argue that the under- and over-enforcement of regulations can occur for purely political reasons, and with a growing literature documenting political interference in the regulatory process (albeit not for partisan purposes).[7]

How should firms respond to these changes in regulators' monitoring and enforcement postures? The answer depends on firms' objectives and existing emissions profiles. Firms with a low probability of exceeding permitted emissions levels may be largely unaffected by expected increases in monitoring and enforcement following a close Democrat win. However, for firms with a higher likelihood of exceeding permitted emissions levels, increases in the probability of inspections and enforcement may lead to meaningful increases in the marginal cost of maintaining high pollution levels. As such, high-pollution firms may find it optimal (from a net present value perspective) to invest in pollution-reduction activities that were previously deemed to be too expensive.[8] This logic gives rise to our main prediction: that firms' relative pollution levels will fall and pollution abatement activities will increase following the close election of a Democrat versus a Republican.

We present five main results. First, our regression discontinuity tests provide strong evidence that winning candidates' political party affiliations affect subsequent emissions in their districts. At the threshold, we find that emissions increase by approximately 20% when a facility is represented by a closely-elected Republican relative to a closely-elected Democrat. This result holds across a variety of RD bandwidth and estimation approaches recommended by the literature (Calonico, Cattaneo, and Titiunik, 2014; Cattaneo, Idrobo, and Titiunik, 2019). These discontinuities also exist for districts

[6]This assumption mirrors that of Di Giuli and Kostovetsky (2014), who state that "[t]he Democratic Party platform places more emphasis on CSR-related issues such as environmental protection [and others]."

[7]See, e.g., Dinç (2005), Benmelech and Moskowitz (2010), Fisman and Wang (2015), Mehta, Srinivasan, and Zhao (2020), Mehta and Zhao (2020), and Akey, Heimer, and Lewellen (2021).

[8]A profit-maximizing firm would invest in a pollution-reduction project if and only if the marginal cost of investing is smaller than the marginal expected value loss from not investing (for example, the probability of an inspection multiplied by the expected loss given inspection due to pecuniary penalties, lost reputation, and other factors). This analysis assumes a Friedman (1970)-style firm objective function; if firms care about objectives other than just profit maximization or shareholder value, they may naturally respond differently (Hart and Zingales, 2017, 2022).

represented by governors of both parties. As expected, the discontinuities go in opposite directions for seats that flip from Democrat to Republican and Republican to Democrat, respectively. We also exploit *within-party* variation in environmental ideology and show that our results are stronger for more ideological candidates, supporting differences in ideology as the key driver of our results.

Our main results survive a battery of robustness tests. We show that no discontinuities exist across a variety of district-level covariates (including voters' views on the environment), supporting the main identifying assumption behind our RD design.[9] McCrary (2008) tests also confirm no manipulation of the assignment variable. As further robustness, we follow Lowes and Montero (2021) and perform an RD test on the residuals from regressing pollution on state $\times$ chemical $\times$ year and firm $\times$ chemical $\times$ year fixed effects. These tests confirm that our results hold even after removing all variation across state-year and firm-year pairs, respectively. Placebo tests and standard error restrictions further confirm the overall robustness of our findings.

Second, we present evidence that firms reallocate pollution between facilities based on the party affiliation of the politicians representing each facility in Congress. We begin by showing that, even within the same firm-chemical-year, facility-level pollution declines following a Democratic election victory. We then augment this result with Giroud and Mueller (2019)-style tests where we examine how a facility's pollution depends on the political party representation of the firm's *other* facilities. We are able to include district $\times$ chemical $\times$ year fixed effects in these tests, allowing us to compare establishments located in the same district (and year), producing the same chemical, which are exposed to the same local economic conditions (and the same politician), but which vary in the share of Democrats who represent the parent firms' *other* establishments. We find that a win by a local Republican is associated with relatively larger increases in pollution (but not production) and relative decreases in post-production recycling and treatment when a firm's other plants are represented by Democrats, supporting reallocation. These findings suggest that firms manage their environmental footprints as rigorously as they manage other parts of their production footprints, and complement Buntaine, Greenstone, He, Liu, Wang, and Zhang (2021) by showing that firms effectively "turn up" their pollution reduction efforts at some plants when a closely-elected Democrat takes office.

[9] See, e.g., Lee, Moretti, and Butler (2004), Ferreira and Gyourko (2009), Do, Lee, Nguyen, and Nguyen (2012), and Akey (2015) for other examples of papers using similar RD experiments. Questions have been raised in the political science literature about whether close U.S. House elections are "random" enough to satisfy the identifying assumptions behind RD (see, e.g., Snyder, 2005 and Caughey and Sekhon, 2011), but such concerns have largely been found to be invalid (Eggers, Fowler, Hainmueller, Hall, and Snyder Jr., 2015, de la Cuesta and Imai, 2016, and this paper).

Third, despite firms' best efforts at reallocating pollution, we provide evidence suggesting that such reallocation is imperfect. We begin by rolling up emissions at the firm level and showing that total firm-level emissions fall following a close Democratic victory. This suggests that firms are unable to fully offset emissions declines in newly-Democratic districts with emissions increases elsewhere. Using data from Compustat, we then confirm that these emissions reductions are costly to the firm: firm-level COGS increases by approximately 4% and market-to-book levels decline by approximately the same amount when the share of a firm's facilities represented by a closely-elected Democrat goes from zero to one. Hence, while multi-plant firms are able to offset a sizable proportion of the costly emissions reductions they undertake in districts closely won by Democrats, these adjustments are nonetheless imperfect and result in significantly higher firm-level costs and a lower M/B ratio.

Fourth, we find that the pass-through of political ideologies through firms' pollution decisions appears to have a significant impact on the health of local communities. We split Congressional districts into smaller areas (3-digit Zip codes) and sort areas into those with high or low numbers of plants per year. We then examine whether the difference between respiratory-related illnesses in high-plant versus low-plant areas increases when the local representative is a Republican. We find that the incidence of respiratory illnesses increases by 7–8% after a district switches from Democrat to Republican in areas with a high numbers of plants (but no difference in areas with fewer plants, and no difference for non-respiratory illnesses). Payments for respiratory-related hospital visits also increase by 7–13% in high-plant areas within districts represented by a Republican versus a Democrat. We also find evidence that the reallocation of pollution also leads to shifts in the distribution of health costs across districts containing different facilities of the same firm.

The human costs of these effects are large. Our estimates imply that each Democrat-to-Republican House transition is associated with 67 additional hospital visits costing around $628,000 per year. These estimates imply that, if all 221 current Democratic representatives were replaced with Republicans in the next election cycle, we would expect an additional 29,614 hospital visits costing more than $277 million during the following cycle due to increased industrial pollution at constituent firms.

Finally, we examine the mechanisms through which the partisan ideological beliefs of U.S. representatives may affect constituent firms' pollution decisions. One advantage of our setting is that federal laws are consistent across states and House members have no executive authority in their districts, making it highly implausible that the effects we document are a result of federal rulemaking or the threat of future action. This leaves regulatory interference by politicians as a plausible potential mechanism. In particular, we hypothesize that Democratic representatives (and their staffs) may pay more attention to the monitoring and enforcement of environmental regulations relative to Republican representatives, and may lean on regulators to increase inspections and enforcement activities at plants in their districts.[10] If this is true, firms would be subject to different environmental monitoring and enforcement regimes based on the political party of their U.S. representative, leading firms to potentially re-optimize their pollution decisions.

Consistent with this hypothesis, we find that inspection propensities are higher and enforcement actions are more common at facilities represented by closely-elected Democrats, even though pollution at these facilities is lower on average than at similar facilities represented by closely-elected Republicans. Magnitudes are large: conditional on receiving at least one EPA inspection, inspections increase by approximately 20% after a Democrat wins a close election. We also find results along the extensive margin: firms are approximately 7% more likely to be inspected for the first time after a Democratic win. These findings complement Innes and Mitra (2015), who also show that inspections rise following Democratic victories in close Congressional elections.[11]

In addition to increased inspection frequencies, we find that enforcement actions rise following close-election wins by Democratic candidates. However, the increase in enforcement actions consists primarily of informal enforcement actions such as cease-and-desist letters that carry no pecuniary penalties. We find that such informal enforcement actions rise by 46%. In contrast, while formal enforcement actions also rise, once we condition on the increase in inspections, we find economically small changes in formal enforcement actions and monetary fines. Given the potential costs (both in fines and in reputational losses) associated with formal enforcement actions, these results provide evidence – albeit speculative – that a material fraction of firms are over-polluting under Republican representatives but reduce pollution under Democratic representatives to a level that does not trigger formal enforcement action (supporting the arguments in Blundell, Gowrisankaran, and Langer,

[10]In most U.S. states, state and local regulators are the primary enforcers of federal environmental regulations, working in conjunction with EPA regulators. Our hypothesis is that representatives (and their staffs) are leaning on the EPA and these regulators, many of whom likely work and reside in the representative's district. For anecdotal evidence on U.S. representatives leaning on the EPA and state environmental regulators, see, e.g., https://tinyurl.com/veaseyletter and https://tinyurl.com/watersletter.

[11]Like our paper, Innes and Mitra (2015) perform regression discontinuity tests involving close Congressional elections. However, Innes and Mitra (2015)'s RD tests only examine inspections; they do not examine the other pieces of our causal chain (e.g. pollution, production, and enforcement), nor do they examine reallocation, firm outcomes, or health effects.

2020). Consistent with this argument, we find that the reduction in pollution following a close Democratic win is larger for firms with high ex-ante pollution levels, who are arguably at the most risk of being formally sanctioned in the event of a violation.

Our empirical setting allows us to rule out a number of competing explanations. For example, changes in pollution may simply be a byproduct of changes in production levels. However, we find no increases in plant production at the RD threshold, thus ruling out production-based explanations for our results. We find instead that pollution declines because firms increase investment in abatement technologies and in post-production recycling and treatment following close Democrat wins. Another potential explanation is that our results could be driven by state-level factors such as gubernatorial elections or changes in states' regulatory posture. Most environmental regulation and enforcement falls to states under the EPA's "primacy" concept, so changes in state law or state agencies' enforcement posture can have a large impact on environmental enforcement outcomes. However, we show that our main effect persists even with state $\times$ chemical $\times$ year fixed effects, which should account for any state-level shifts in environmental regulations or governance. Another concern is that our results might simply reflect a firm's own political ideologies (Di Giuli and Kostovetsky, 2014; Hutton, Jiang, and Kumar, 2014; Fos, Kempf, and Tsoutsoura, 2021). However, this explanation would require the firm's own political ideology to change in lockstep with the party affiliation of their local representatives, and is not compatible with our within-firm results and enforcement results. While other factors could still contribute to our findings, we believe that the most likely explanation for our findings, given the evidence available, stems from differences in politicians' ideologies.

Our paper contributes to a number of areas of the literature. First, while other papers have studied the causal effects of political parties and political ideologies on economic outcomes (see, e.g., Alesina, 1988; Lee, Moretti, and Butler, 2004; List and Sturm, 2006; Ferreira and Gyourko, 2009), our paper is one of the first (to our knowledge) to link the partisan ideology of individual politicians directly to firm behavior and community health outcomes.[12] Our paper also contributes to the literature on political polarization by examining how *politicians'* polarization affects firms, as opposed to studying the effects of politically polarized economic agents within firms (see, e.g., Di Giuli and Kostovetsky, 2014; Hutton, Jiang, and Kumar, 2014; Kempf and Tsoutsoura, 2021; Fos, Kempf, and Tsoutsoura,

[12]In related work, Artes, Richter, and Timmons (2019) and Denes, Fisman, Schulz, and Vig (2019) show that changes in firms' political representation due to redistricting can also affect firm financial performance and investment decisions, though largely through a nonpartisan lens.

2021; Engelberg, Guzman, Lu, and Mullins, 2021).

Turning to firms' environmental policies and practices, a growing literature has shown that financial constraints (Cohn and Deryugina, 2018; Bartram, Hou, and Kim, 2021; Xu and Kim, 2021), limited liability (Akey and Appel, 2021), environmental activism by institutional holders (Akey and Appel, 2019; Naaraayanan, Sachdeva, and Sharma, 2020), the listing status of firms (Shive and Forster, 2020), CEO hometown favoritism (Li, Xu, and Zhu, 2021), and supplier networks (Schiller, 2018) can have a significant impact on firms' environmental policies. We add to this literature by showing that actors outside of a firm's own ecosystem – in particular, U.S. Representatives – can play a major role in determining firms' environmental policies, emissions, and the health of surrounding communities.

Finally, by showing that political ideology explicitly affects environmental inspections and enforcement, our paper contributes to the large economics literature on the enforcement of environmental regulations (see, e.g., Greenstone, 2002; Greenstone, List, and Syverson, 2012; Walker, 2013; Innes and Mitra, 2015; He, Wang, and Zhang, 2020; Buntaine, Greenstone, He, Liu, Wang, and Zhang, 2021) and on the political economy of regulatory enforcement more generally. In the environmental economics sphere, our paper contributes to the literature on the political economy of pollution (see, e.g., Konisky and Woods, 2010; Beland and Boucher, 2015; Monogan III, Konisky, and Woods, 2017; Lipscomb and Mobarak, 2017; Heitz, Wang, and Wang, 2020; Lueck, Pastrana, and Torrens, 2021) and complements the literature on the relationship between political ideologies and firm pollution (see, e.g., Helland and Whitford, 2003; Neumayer, 2003; Fredriksson, Neumayer, Damania, and Gates, 2005), which has primarily studied cross-country settings. Relative to these papers, we use a sharper identification strategy, exploit within-country variation in political ideologies, and specifically examine plant-level, firm-level, and community-level effects.

2 Data

2.1 EPA Emissions and Compliance Data

Our main data source for emissions is the Toxics Release Inventory (TRI) database produced by the U.S. Environmental Protection Agency (EPA). Most U.S. facilities that release toxic chemicals into the air, water, or certain land repositories are required to report their annual emissions releases to the EPA. The TRI database contains emissions data for approximately 770 chemicals spanning 33 categories. Facilities (plants) are required to report the annual number of pounds released for each chemical covered by the TRI program, as well as other information including plant coordinates and information about the plant's owners. The database is organized at the facility-chemical-year level. The TRI database also includes information about a plant's production ratio, which measures the annual percentage change in the quantity of output for each production process that contributes to the plant's emissions.

We obtain information on facilities' waste management activities from the EPA. The EPA's waste management hierarchy consists of five components, namely source reduction, recycling, energy recovery, treatment, and disposal. By eliminating pollution at the top of the production process, source reduction (also known as abatement) is the waste management activity most preferred by the EPA, with disposal being the EPA's least preferred activity. We obtain facility-chemical-year level data on abatement investment from the EPA's Pollution Prevention (P2) database, which is a companion database to TRI, and data on recycling, energy recovery, and treatment from TRI (following Li, Xu, and Zhu, 2021). While TRI and P2 are self-reported, there is limited evidence of misreporting and the EPA performs regular data audits. As such, the literature has concluded that misreporting is not a material issue (see, e.g., Greenstone, 2003 or Akey and Appel, 2019 for more about TRI data quality).

We also obtain federal environmental compliance data from the EPA's Enforcement and Compliance History Online (ECHO) data set. To determine whether a plant is in compliance with federal laws, EPA staff and state regulators conduct regular inspections that involve interviews, records reviews, and plant visits. Violations discovered by regulators can lead to either formal or informal enforcement actions. The ECHO data set contains information on whether a given enforcement action is formal or informal, the agency that initiated the action, and any penalties imposed on the facility.[13] Informal enforcement activities generally include warning letters or Notices of Violation, while formal enforcement activities may result in Administrative Compliance Orders (or state equivalent actions) and referrals to state attorneys general or the Department of Justice. Most inspections and enforcement actions in our sample are related to the Clean Air Act (CAA) program and the National Pollutant Discharge Elimination System (NPDES) under the Clean Water Act (CWA) program.

[13] ECHO is a collection of data sets covering compliance activities for various programs including the Clean Air Act (CAA), the National Pollutant Elimination Discharge System (NPDES), NPDES Biosolids, and the Integrated Compliance Information System for Federal Civil Enforcement Case Data (ICIS FE&C). We combine all of the data sets available in ECHO to construct our final data set.

It is important to note that most pollution permits specify complicated pollution limits by day, hour, or even minute that often change based on seasonal factors, production factors, or time-varying location-specific factors such as temperature, fish spawning patterns, and the arrival or presence of weather systems. As such, EPA and state regulatory staff cannot typically tell if a firm is in violation of its pollution permits by viewing its periodic emissions quantities (i.e. without performing an on-site inspection).[14] In addition, even if regulators *could* immediately determine whether a plant has exceeded its permitted level of pollution, firms would not face regulatory consequences unless regulators pursued enforcement actions, an outcome that EPA and state inspectors have applied unevenly across regions and time.[15]

2.2 District and Elections Data

To construct our main data set, we merge the TRI and ECHO data with Congressional boundary definitions from Lewis, DeVine, Pitcher, and Martis (2013) and Congressional district election results from the MIT Election Data + Science Lab. We then construct an indicator variable, *Democrat Win*, that equals one if a plant is located in a Congressional district that was won by a Democratic Party candidate in its most recent election, and equals zero otherwise.

2.3 Health Data

Our main source of data for health-related variables is the Center for Medicare and Medicaid Services (CMS), which is part of the U.S. Department of Health and Human Services. CMS provides a data set containing quantity and average price (payment) data related to many common medical procedures performed at more than 4,000 inpatient hospitals starting from 2011 for all Medicare and Medicaid recipients. We obtain this data for 622 Diagnosis Related Groups (DRGs), which we aggregate at the Major Diagnostic Category (MDC) level. There are 25 mutually-exclusive MDCs in the CMS

[14] For example, "EPA's enforcement program depends heavily upon inspections by regional or state enforcement staff as the primary means of detecting violations and evaluating overall facility compliance. Thus, the quality and the content of the agency's and states' inspections, and the number of inspections undertaken to ensure adequate coverage, are important indicators of the enforcement program's effectiveness." Source: https://www.govinfo.gov/content/pkg/GAOREPORTS-GAO-06-840T/html/GAOREPORTS-GAO-06-840T.htm.

[15] For example, "[GAO] found variations in regional [enforcement] actions reflected in the (1) number of inspections EPA and state enforcement personnel conducted at facilities discharging pollutants within a region, (2) number and type of enforcement actions taken, and (3) the size of the penalties assessed and the criteria used in determining the penalties assessed." Ibid.

taxonomy, with each MDC covering a broad diagnostic area such as the eyes, the respiratory system, the digestive system, or the skin.

Consistent with the literature on pollution and local health outcomes (see, e.g., Schwartz, 1996 and Hoek, Krishnan, Beelen, Peters, Ostro, Brunekreef, and Kaufman, 2013), our tests focus on MDC 4, which captures issues with the respiratory system. We also construct data for a "placebo" group of MDCs (18–22) which covers illnesses such as infectious diseases and mental health issues that are unlikely to be caused by pollution from local plants. Some tests also utilize health outcome and payments data spanning all MDC codes in the CMS data set.

To create our main data set, we merge our CMS data set with our emissions, compliance, and Congressional district data set at the Zip code level. To ensure that we accurately map health outcomes to Congressional districts, we drop all Zip codes spanning two or more Congressional districts. Since most Zip codes do not contain a hospital and most people travel outside of their Zip code to receive hospital care, we aggregate all health and plant data at the three-digit Zip code-district level. This level of aggregation corresponds roughly to a small metropolitan area or county, though large and mid-sized metropolitan areas often have multiple three-digit Zip codes.

2.4 Other Data Sources

We also use data from a number of other sources. We obtain political ideology scores from VoteView and the League of Conservation Voters, data on public environmental opinion from Yale Climate Opinion Maps, and data on state and federal environmental budgets from the Environmental Council of the States (ECOS). Data on campaign contributions to Congressional candidates are sourced from the Federal Election Commission. We also obtain firm accounting and financial performance data from Compustat, which we hand-match with the TRI data based on the name of the firm.

2.5 Summary Statistics

Table 1 reports summary statistics for our main variables on emissions, compliance, elections, and health. Our sample consists of 37,369 distinct plants during the period 1991-2016. Emission variables are defined at the plant-chemical-year level, while compliance-related variables are defined at the plant-year level. The average facility in our sample releases 30,846 pounds per chemical per year, experiences 0.8 inspections per year, and is subject to 0.15 enforcement actions per year, including both formal and informal actions. Our full sample contains 5,304 U.S. House elections with an average margin of victory for Democratic candidates of 2.7%, while our close election sample (margin of victory less than or equal to 5%) consists of 387 elections with an average Democrat win margin of 0.2%. Only 1 in 15 elections in our sample is considered close, and the probability of repeat close elections in a district is only 1.3%. Table 1 also shows that there are on average 54 respiratory-related hospital visits per Zip3-district-year costing around $485,000 in total.

3 Empirical Framework

Identifying the causal effect of politicians' ideologies on firm outcomes is challenging for a number of reasons. First, it is difficult to identify which actions a politician takes due to their own ideology versus actions taken in response to voters' ideologies or electoral considerations. Second, it is difficult to find settings in which politicians' ideologies can be directly traced to measurable firm outcomes. Third, politicians are not elected randomly, and there are many potential omitted variables (both within and outside of districts) that could be associated with both a politician's election victory and firm outcomes in the politician's district. Fourth, firm decisions could themselves affect the politician's ideology or election results (reverse causality). Finally, attributes of a politician other than ideology (for example, seniority) may directly affect firm outcomes as well.

To overcome these challenges, our main tests combine a regression discontinuity (RD) design around close elections with detailed plant-chemical-time level microdata on firms' pollution and production decisions. Close-election RD designs are common in the political economy literature (see, e.g., Lee, Moretti, and Butler, 2004; Ferreira and Gyourko, 2009; Akey, 2015) because elections (and particularly two-party elections) are well-suited for RD tests: there is a clear vote share at which point a candidate is declared the winner, and treatment – particularly right around the threshold – often depends on plausibly exogenous factors such as the weather, traffic, or other relatively minor factors that can affect voter turnout at the margins. In addition, plant-level microdata allows us to examine measurable firm outcomes and to separate environmental pollution decisions from those involving production and other firm decision variables.

We focus on close U.S. House of Representatives elections because these elections offer a number of empirical advantages relative to other election types. First, unlike the U.S. Senate, each House district has a single representative, making it easier to isolate actions related to the politician's ideology. Second, federal environment laws affect all plants in our sample, reducing the possibility that firms are responding to legislation caused by state or local changes in economic conditions or voter preferences.[16] Third, while executive-branch officials such as the President and state governors can direct government agencies to alter rules and regulations for electoral reasons (such as a campaign promise to reduce regulation), members of Congress have no authority over government agencies under Article II of the U.S. Constitution and thus have limited ability to benefit electorally from changes in regulations or enforcement in their districts. Finally, analyzing Congressional districts allows us to exploit within-state variation, thus accounting for the governor's ideology and the regulatory and enforcement posture of state agencies.

Our main tests use a candidate's political party as a proxy for their ideology regarding the environment. To implement our close-election RD design, we define *Win Margin* as the difference in the vote share of the Democratic candidate minus the vote share of the Republican candidate. We are interested in whether there is a discontinuity in pollution when *Win Margin* = 0. To examine this question, we first estimate the local linear RD equation:

$$Y_{ic(jd)t} = \beta \text{Democrat Win}_{dt} + \theta f(\text{Win Margin}_{dt}) + \delta \text{Democrat Win}_{dt} \times f(\text{Win Margin}_{dt}) + \mu_c + \varepsilon_{ict} \,, \quad (1)$$

where the main dependent variable is (log) pollution of chemical c at establishment i owned by firm j in Congressional district d at time t, Democrat Win_{dt} is an indicator for a Democrat winning the most recent election, $f(\text{Win Margin}_{dt})$ are polynomials of different order of the variable Win Margin_{dt}, and μ_c is a chemical fixed effect. The term Democrat $\text{Win}_{dt} \times f(\text{Win Margin}_{dt})$ allows the estimation of β to be identified when the win margin is equal to zero. In line with Akey (2015), we restrict the sample to elections with an absolute vote margin less than 5%.

Local linear RD regressions face a well-known trade-off between sample bandwidth size around the RD threshold and bias in the estimates of the RD coefficients. As such, we also report the results of a non-parametric RD estimation procedure that attempts to give relatively more weight to observations around the cutoff without sacrificing precision (see, e.g., Calonico, Cattaneo, and Titiunik, 2014; Lowes and Montero, 2021). This procedure allows the econometrician to specify a weighting

[16]In addition, passing an act of Congress is difficult and individual House members have little impact on legislative outcomes, reducing the possibility that our tests are picking up federal legislative actions targeting specific districts.

method for each observation in the sample (i.e., a kernel) and a (possibly non-linear) functional form for the relationship between the outcome variable and the running variable on each side of the cutoff. The procedure jointly estimates the RD parameter of interest, its optimal bias-corrected standard error, and the optimal sample bandwidth around the cutoff (Calonico, Cattaneo, and Titiunik, 2014). The exact specification for these tests is:

$$Y_{ic(jd)t} = \beta \text{Democrat Win}_{dt} + \theta g(\text{Win Margin}_{dt}) + \varepsilon_{ict} \ , \tag{2}$$

where $g(\text{Win Margin}_{dt})$ is now the RD polynomial. Our baseline specification is a local polynomial of order one in the vote margin estimated separately on each side of the zero margin cutoff. We use a triangular weighting kernel and calculate the optimal bandwidth by using the MSE-minimizing procedure suggested by Cattaneo, Idrobo, and Titiunik (2019). We also estimate the regression specifications with different polynomials and kernel definitions.

Following most papers in the RD literature, we do not impose strict fixed effects structures in our RD tests. However, as a robustness check, we also perform a Lowes and Montero (2021)-style "residualized" RD test where we replace $Y_{ic(jd)t}$ in equations 1 and 2 with the residuals from a first-stage regression in which we orthogonalize the Y variable with respect to either state $\times$ chemical $\times$ year or firm $\times$ chemical $\times$ year fixed effects. While this test removes nearly all variation in our main Y variable (pollution), it allows us to account for any within-state-time or within-firm-time variation caused by (for example) changes in the governor, changes in the state's environmental regulation posture, supply or demand shocks, changes in firm management, shocks to firm investment opportunities, changes in the financing environment, and so on, that might be somehow correlated with the close election outcomes in our sample.

In some cases it is not possible to use RD estimation techniques due to the data structure or the specific hypothesis being tested. In those cases, we estimate panel regressions of the form:

$$Y_{ic(jd)t} = \beta \text{Democrat Win}_{dt} + \mu_{FE} + \varepsilon_{ict} \ , \tag{3}$$

where, thanks to the granularity of our data, we are able to employ a variety of stringent fixed effects including year fixed effects, establishment fixed effects, establishment $\times$ chemical fixed effects, district fixed effects, firm $\times$ chemical $\times$ year fixed effects, state $\times$ year fixed effects, and state $\times$ year $\times$

chemical fixed effects, depending on the test. These multidimensional fixed effects allow us to isolate very specific sources of variation, such as (for example) variation in pollution for a given chemical across plants owned by the same firm at the same point in time.

4 Results

4.1 Main Result: Pollution

Figure 2 plots pollution around the RD threshold (zero percent win margin). We first rank district-year observations in our sample by their Democrat win margin in the most recent election cycle. We restrict the sample to a narrow ±5% Democrat win margin window, and we construct 18 equally-spaced bins on either side of the zero win margin cutoff.[17] In Figure 2, we report the average (log) emissions in each Democrat win margin bin, as well as the fitted values and 95% confidence intervals of a local polynomial regression on each side of the cutoff. Figure 2 shows a clear, discontinuous, statistically significant drop in emissions for plants located in districts that are won by closely-elected Democrats. This drop in emissions is roughly constant away from the zero win cutoff, suggesting that our result is not driven by outlying observations just above or below the zero cutoff.

In Table 2, we formally test for a discontinuity using the RD specifications listed in equations (1)–(2). The unit of observation in these (and most other) tests is a plant-chemical-year. Our first set of tests restricts the sample to the narrow 5% window around the zero margin cutoff. We then regress emissions on the *Democrat Win* dummy variable (column (1)) as well as on an interaction term between this indicator and the Democrat win margin (columns (2) and (3)). These local OLS specifications confirm the visual evidence from Figure 2: plants located in districts that are just-won by Democrats have on average 21.3% to 39.7% lower emissions than plants located in districts that are just-won by Republicans. Columns (4)-(7) report results using non-parametric local polynomial RD specifications (Calonico, Cattaneo, and Titiunik, 2014; Lowes and Montero, 2021). Each column contains a different combination of polynomial functional form (linear versus quadratic) and kernel weighting method (triangular versus Epanechnikov). As in the first three columns, we find that Democratic close election wins are associated with an average 35% reduction in plant-level emissions.

[17] In Appendix Figure A1, we plot the same figure using a linear polynomial fit over the ±5% Democrat win margin window and find similar results.

The magnitudes documented in Table 2 are large. The 35.5% reduction in emissions implied by the non-parametric specification in column (4) translates into a reduction in firm-level emissions of approximately 10,000 pounds relative to the sample mean of 30,846 pounds per plant-chemical-year, and approximately 130 pounds relative to the sample median of 369 pounds per plant-chemical-year. These results are quantitatively similar when we restrict the sample to a narrow 5% window. Overall, the results of Table 2 and Figure 2 provide strong empirical evidence that district-level affiliation to the Democratic Party is associated with economically large reductions in toxic emissions.

4.2 Robustness

4.2.1 Covariate Balance

Figure A2 shows that there are no discontinuities related to district-level GDP growth, the district-level unemployment rate, or district-level credit growth around the zero percent win margin.[18] Hence, districts in our close-election sample seem similar along observable economic dimensions, supporting the key identifying assumption behind our RD experiment.

We also provide evidence that districts on both sides of the cutoff share similar views about the environment. Figure A3 uses data from the 2020 Yale Climate Opinion Maps to show that residents of districts just won by Democrats in the 2018 election share similar views about the environment as residents of districts just lost by Democrats in 2018. While this test represents a single cross-section, the results support the idea that there are no significant differences in environmental viewpoints between districts just won or just lost by Democrats. Figure A4 also shows that the close elections in our sample are spread out across 48 of the 50 states and show very little correlation across regions.[19]

4.2.2 Residualized RD

We next follow Lowes and Montero (2021) and perform our baseline RD tests on residualized versions of our main emissions variable. We first regress emissions on state $\times$ chemical $\times$ year fixed

[18] We include employment because politicians' ideologies might affect employment at local plants, with possible effects on emissions. We include credit growth because politicians' partisan ideologies may lead to changes in local firms' credit conditions, which in turn might indirectly affect their abatement decisions (Xu and Kim, 2021). We measure credit growth using the (log) number of mortgage originations from HMDA and the number of consumer and small-business loan originations made under the Community Reinvestment Act.

[19] For example, the seven states with the largest number of close elections per Congressional district in our sample are Connecticut, Kansas, Maine, Nebraska, Nevada, New Hampshire, and Washington. These states have vastly different economic, social, and demographic profiles. There were no close elections in Mississippi or Vermont.

effects. These stringent fixed effects absorb all variation in pollution caused by time-varying state-level factors such as state-level supply or demand shocks, changes in the governor or legislature, state law changes, changes in the funding or posture of state agencies, and other similar factors. We then perform our baseline RD tests on this residualized outcome variable. This test allows us to rule out all confounding stories that rely on variation at the state-chemical-time level. Results are reported in Table A1 of the appendix. Columns (1) and (2) show that our main results survive this stringent robustness test: we still find economically and statistically significant reductions in pollution when a Democrat just wins an election. While magnitudes are lower than in Table 2, they are still quite sizable; for example, we find pollution reductions of approximately 3% to 14.5%.

In columns (3) and (4), we repeat the same exercise after first regressing plant emissions on firm $\times$ chemical $\times$ year fixed effects. These fixed effects absorb all variation in pollution caused by firm-level factors such as supply or demand shocks, shocks to investment opportunities, financing shocks, and shocks to the firm's environmental posture, among others. We find 3.4% to 5.2% reductions in emissions in districts closely-won by a Democrat representative, relative to contemporaneous emissions *by the same firm* in districts closely-won by a Republican representative. Collectively, these tests – while removing potentially useful variation – provide further evidence that confounding factors are highly unlikely to explain our results.

4.2.3 Governors and State Regulatory Agencies

We also examine whether politicians' impact on pollution differs depending on the political party of their governor. Beland and Boucher (2015) find that firm pollution is lower under Democratic governors, raising questions about whether our findings are simply capturing a governor effect (though our tests in columns (1) and (2) of Table A1 should rule out these concerns). More broadly, since most EPA laws are enforced by states, and since governors generally appoint the heads of state agencies (including those responsible for environmental enforcement), it is natural to think that the magnitudes of our effect might depend on the political party of the governor.[20]

Table A2 show that our main results are large and statistically significant regardless of the political

[20]In particular, consistent with Beland and Boucher (2015), we hypothesize that the effects would be larger when a Democrat is governor, since a Democratic governor will be more likely to appoint agency heads who care about the enforcement of environmental regulations, and since agencies under a Democratic governor might be more receptive to input and requests from members of Congress that relate to the enforcement of environmental regulations.

party of the governor. Consistent with Beland and Boucher (2015), magnitudes are slightly larger for Democratic governors, but the magnitudes and the statistical significance of these differences are small. When combined with the results in Table A1, the results in Table A2 provide even more evidence that state-level factors are not responsible for our results.

4.2.4 McCrary Test

Another concern is that the distribution of election margins may not be continuous at the RD threshold, suggesting that the assignment variable could potentially be manipulated. Figure A5 presents the results of a McCrary (2008) density test. The figure shows that the distribution of the assignment variable is smooth across the threshold, confirming that it is unlikely that the assignment variable (election outcomes) was systematically manipulated.

4.2.5 Additional Robustness

We also report the results of six additional robustness tests. First, Table A3 shows that our local OLS specification in Table 2 produces statistically-similar results when we cluster our standard errors at the facility level or using 97 distinct vote bins around the zero vote margin (Lee and Card, 2008).[21] Second, we present the results of two placebo tests in Figure A6 that show that our results are not spuriously caused by sample selection or other issues. Third, in Table A4, we show that our results are almost identical when we run Poisson regressions on the level of emissions instead of OLS regressions and non-parametric regressions on the natural logarithm of the level of emissions, reducing concerns that our results may be driven by the distribution of emissions (Cohn, Liu, and Wardlaw, 2021). Fourth, Table A5 confirms that the results in Table 2 hold after excluding power plants (NAICS two-digit code of 22) from our sample. Since power plant "production" is largely determined by economic activity in the region, this test provides additional evidence that differences in economic activity across districts are unlikely to explain our findings. Fifth, Table A6 shows that district-level emissions growth rates do not predict the outcomes of subsequent elections, suggesting that Democratic close-election victories are not driven by pre-election changes in emissions levels. Finally, Figure A7 shows that our results are robust to dropping individual U.S. states from our sample, confirming that our

[21] The Lee and Card (2008) clustering is motivated by the presence of small mass points in the distribution of the outcome variable around the zero win margin cutoff.

results are not being driven by one or more outlier states.

4.3 Production, Abatement, and Post-Production Recycling Activities

We now dig deeper into the pollution and production decisions made by firms following the close election of a Democratic representative. There are at least three non-mutually exclusive ways in which firms can reduce pollution at a plant. First, the firm can simply reduce production at the plant. This would keep the number of units of pollution per unit of production constant, but would lead to lower overall pollution. Second, the firm could invest in new abatement technologies to reduce the emissions occurring during the production process. Third, the firm could increase its post-production treatment and recycling activity.

We begin by utilizing the TRI's production index, which is available at the facility-chemical-year level, to examine whether pollution *per unit of production* falls after a plant is represented by a closely-elected Democrat. Since output is only available as an annual growth rate, we follow Akey and Appel (2019, 2021) and construct a contemporaneous measure of plant emissions relative to production for each plant-chemical-year as

$$\begin{aligned} \log\left(\text{Cumulative Emissions/Production}\right)_{ijt} &= \log\left(\prod_{\tau=2}^{t} \frac{1}{\text{Prod. Growth}_{ij\tau}} \times \frac{\text{Emissions}_{ij\tau}}{\text{Emissions}_{ij\tau-1}}\right), \\ &= \log\left(\frac{\text{Emissions}_{ijt}}{\text{Production}_{ijt}}\right) - K_{ij}, \end{aligned} \tag{4}$$

where Emissions_{ijt} are the emissions of chemical j by plant i in year t, $\text{Prod. Growth}_{ijt}$ is the ratio of year t's output and year $t-1$'s output associated with the production of chemical j in plant i (directly available from the EPA data), and K_{ij} is a plant-chemical constant.

In Table 3, we show that plant emissions in districts with close Democrat wins decrease even relative to production. In the first column of the table, we show that emissions decrease by around 9.3% relative to production when the district where the plant is located is just-won by a Democrat. In column (2), we confirm that this result holds economically and statistically using a non-parametric specification and a flexible RD bandwidth choice.[22] This result confirms that our effects are coming from channels related specifically to emissions and not channels related to general economic activity (such as taxes, spending, and non-environment related regulation and enforcement).

[22] In Table A7, we also document no effects on plant-level production when a district is just-won by a Democrat politi-

How can pollution go up or down if production remains unchanged? In Table 4, we examine whether firms invest in new abatement technologies and/or change their post-production recycling behavior in order to reduce pollution following a close Democrat win. Columns (1)–(2) of Table 4 show that firms indeed increase their investment in abatement activities following close Democrat wins. The magnitudes are large: the unconditional average number of abatement activities at the plant-chemical-year level is 0.06, so our estimates imply a 25% increase in abatement activities following a Democrat win.

In Columns (3)-(4), we examine whether firms reduce emissions through their post-production treatment, recycling, and energy recovery activities. The dependent variable in this panel is the post-production activity ratio, which is the sum of emissions reduced through treatment, recycling, and energy recovery activities divided by the total gross waste of a plant.[23] Columns (3)-(4) show that firms increase their post-production emission reduction activities after a Democrat win. Since the unconditional mean of the post-production activity ratio is 0.5, our estimates imply a 5.8% increase in the post-production ratio following a Democrat win. Collectively, these results suggest that firms increase both their production abatement and their post-production recycling activities following a Democrat win.[24] This helps to explain why we observe sizable reductions in pollution even though production remains unchanged.

4.4 Reallocation

Given the results in Tables 2 and A1, a natural question is whether firms with plants in multiple Congressional districts reallocate pollution to other plants that are represented by Republicans following the close election of a Democrat. All else equal, a firm with plants in areas represented by

cian. Similar to (4), we compute cumulative plant-level production related to chemical j as

$$\log\left(\text{Cumulative Production}\right)_{ijt} = \log\left(\prod_{\tau=2}^{t} \text{Prod. Growth}_{ij\tau}\right) = \log\left(\text{Production}_{ijt}\right) - K'_{ij}, \tag{5}$$

with K'_{jt} another plant-chemical-specific constant. Hence, while plant-level emissions clearly decrease following a close Democratic election, production at the same factories does not decrease.

[23] We define total gross waste as the sum of total actual releases after post-production activities and the emissions that are reduced through treatment, recycling, and energy recovery activities. The TRI emissions data that we use in our main tests excludes emissions reduced through post-production activities.

[24] In addition to investing in abatement technologies and post-production emission reduction activities, firms may also reduce pollution by "turning up" their existing abatement devices, as shown by Buntaine, Greenstone, He, Liu, Wang, and Zhang (2021) using detailed electricity consumption data from China. While our data does not allow us to isolate this channel, it would be consistent with (and complementary to) the results documented in Table 4.

both Democrats and Republicans may prefer for pollution to occur at the Republican plants, since less abatement investment and post-production recycling might be needed at these plants.

We examine this question in two ways. First, we perform OLS regressions that include firm $\times$ year or firm $\times$ chemical $\times$ year fixed effects, thereby forcing identification to come from comparing pollution outcomes across multiple plants in multiple districts producing the same chemical and owned by the same firm at the same point in time. We also include a variety of additional fixed effects such as state-time and district fixed effects in these tests. Given the limited number of close elections and multi-plant firms in our sample, it is not possible to meaningfully estimate these regressions in an RD setting with multiple layers of fixed effects. As such, we switch to a sample containing data from all elections (not just close elections) for these tests. We then perform panel regressions using the specification described in equation (3).

Table 5 presents the results of these tests. As before, the independent variable of interest is *Democrat Win*, which equals one if the district politician is a Democrat and equals zero otherwise. Column (1) confirms that our main RD results in Table 2 continue to hold within a broader sample of elections. Columns (2) and (3) further confirm that our results hold after including state $\times$ year (column (2)) and state $\times$ chemical $\times$ year (column (3)) fixed effects.

In columns (4)–(7) we add a firm $\times$ chemical $\times$ year fixed effect, thereby transforming the analysis into a purely within-firm-time analysis. Column (4) shows that within-firm-time plant-level emissions for the same chemical decline by approximately 4% following a Democratic victory, even after accounting for any systematic differences in emissions profiles among Congressional districts (as redefined each decade) using a Congressional district $\times$ chemical fixed effect.[25] This suggests that after a Democratic win, pollution falls at the focal plant relative to plants in other Congressional districts owned by the same firm at the same time. This result also helps to rule out Congressional redistricting as a driver of our results, since we observe similar results even after including district-decade fixed effects. Column (5) replaces the Congressional district $\times$ chemical fixed effect with an even more stringent facility $\times$ chemical fixed effect and finds similar results. Columns (6) and (7) include state $\times$ year and state $\times$ chemical $\times$ year fixed effects, respectively, thereby absorbing any

[25]Since most states' Congressional districts are redrawn every decade, there is no systematic continuity between districts in the time series. As such, we define Congressional district fixed effects at the district by decade level: the fixed effect corresponding to Maryland's 3rd Congressional district from 1993-2002 is different than the fixed effect corresponding to Maryland's 3rd Congressional district from 2003-2012.

variation caused by (say) changes in state laws, state politicians, or state economic conditions, even those that have differential effects on different chemicals. The results show that, even after removing nearly all variation in emissions, pollution still falls at the focal plant relative to other plants owned by the same firm producing the same chemical in the same year in the same state (but a different Congressional district). Collectively, the results in columns (4)–(7) are suggestive of reallocation: within the same firm, at the same time, within the same state, pollution decreases at plants newly represented by Democrats relative to other plants that are newly represented by Republicans.

Our second set of reallocation tests examines how shocks to the political representation of one plant affect pollution outcomes at *other* plants owned by the same firm. Specifically, we exploit differences in firms' plant networks by comparing pollution outcomes at two plants in the same district owned by different firms, where one of the firms has another plant in a different district whose political representation has recently changed. For example, suppose two firms both have plants in the focal district, and both firms' other plants are all represented by Republicans. Now suppose one firm's distant plant (not the focal plant) becomes newly represented by a Democrat. Our test examines pollution changes at that firm's *focal* plant, relative to the other firm's focal plant, as a result of the change in political representation at a distant plant.

To perform these tests, we follow Giroud and Mueller (2019) and construct, for each plant, a measure of the political ideology of the politicians representing a firm's *other* plants producing the same chemical at the same point in time. For each firm-facility-chemical-year observation, we define *Other Facilities' Democrat Share* as the fraction of other facilities (excluding the focal facility) owned by the same firm producing the same chemical at the same point in time that are located in districts represented by Democrats.[26] We also construct an indicator variable, *High Democrat Share*, that equals one when the *Other Facilities' Democrat Share* variable exceeds the median level, and equals zero otherwise. Finally, we include a variable, *Local Democrat*, that is analogous to the *Democrat Win* variable from Table 5 and captures pollution increases or decreases in the focal Congressional district. We then regress emissions on these variables.

Results are reported in Table 6. Columns (1)–(2) report results for the *Other Facilities' Democrat Share* variable, while columns (3)–(4) report results for the *High Democrat Share* variable. Columns

[26]This strategy also has parallels to the empirical strategy developed by Bertrand and Mullainathan (2003), though we use an average of other plants' representation by Democrats instead of isolating plants with owners headquartered in different locations.

(1) and (3) include chemical $\times$ year and facility $\times$ chemical fixed effects, while columns (2) and (4) include facility $\times$ chemical and district $\times$ chemical $\times$ year fixed effects (which absorb the *Local Democrat* variable). Columns (1) and (3) show that, while pollution falls when a facility is represented by a local Democrat, this effect is smaller when the firm's other facilities are located in districts represented by Democrats. Columns (2) and (4) show that, even after completely absorbing time-varying factors at the local district level (including the local representative), pollution is higher at the local facility by as much as 3–6% when the firm's other facilities are represented by Democrats.

Figure 3 depicts these results visually. Panel A plots pollution at a plant located in a given district as a function of the *Other Facilities' Democrat Share* variable. Panel A of Figure 3 shows that pollution in a given district is strongly increasing in the degree to which the same firms' other plants are represented by Democrats. This supports the idea that a given plant will pollute more if the firm's other plants are represented by Democrats, since the firm on average may attempt to reallocate more pollution to the focal plant.[27]

In Panel B of Figure 3, we attempt to better understand how firms reallocate pollution across plants by plotting post-production activities of a plant as a function of *Other Facilities' Democrat Share*. We focus on post-production activities because, unlike abatement investment, these activities are easier to "turn up" and "turn down" at different facilities (see, e.g., Buntaine, Greenstone, He, Liu, Wang, and Zhang, 2021). As in our previous tests, we normalize these activities by total gross emissions. Panel B shows that a firm's post-production activities are *decreasing* in the share of the firms' other plants represented by Democrats, suggesting that firms "turn down" (costly) post-production activities at the focal plant when a distant plant becomes newly represented by a Democrat.[28]

While reallocation tests are complicated to execute in an RD design, Figure A10 contains the results of RD tests as similar as possible in spirit to the tests in Table 6. Specifically, we examine pollution differences across facilities after first sorting facilities on *Other Facilities' Democrat Share*. Similar to columns (1) and (3) of Table 6, Figure A10 allows us to understand how the magnitude of the change in pollution caused by a Democrat win in the focal district is affected by the political

[27]Figure A8 shows that the pattern in Figure 3 holds when we look at pollution per unit of production rather than the level of pollution itself. The pattern in Figure 3 should also hold regardless of whether the focal plant is represented by a Democrat or Republican, but all else equal, pollution levels should be higher *conditional on Democrat Share* if the plant is represented by a Republican. Figure A9 confirms that this is indeed the case.

[28]In contrast, abatement investment is arguably less likely to be reallocated, as it is very costly to implement, longer-term in nature, and more likely to be irreversible.

representation of the firm's other plants.

To ease interpretation, the running variable in Figure A10 is the Republican win margin. The figure shows that the spike in emissions when a district is marginally won by a Republican representative is larger if the firm operates a larger share of its other plants in Democratic districts. This result is consistent with the idea that if the other plants owned by the firm are mostly located in Democrat (Republican) districts, the demand for pollution reallocation from these districts to the focal district is higher (lower), thereby leading to higher (lower) pollution in the focal district. This result provides yet another piece of evidence supporting the idea that firms reallocate pollution across plants due to the ideology of the politicians representing each plant.

4.5 Firm-Level Effects

We next examine whether politicians' beliefs and preferences can affect outcomes at the firm level. While plant-level emissions fall after a district becomes newly represented by a Democrat, firms also appear to attempt to reallocate pollution across plants. As such, it is unclear whether the net effect of politicians' ideologies on firms is meaningful, or whether firms can effectively side-step individual politicians' ideologies through the strategic reallocation of pollution across plants.

To examine this question, we begin by analyzing whether firms' COGS rise if they are represented by a closely-elected Democrat versus a closely-elected Republican.[29] Aggregating data across chemicals is challenging due to differences in the relative weight and toxicity of each chemical, so we aggregate our data to the firm-time level for each chemical. To capture the firm's exposure to political ideology, we construct a variable, *Democrat Share*, that equals the fraction of a firm's total plants operating in a Democratic district for a given chemical at a given point in time. We also construct a weighted version of *Democrat Share* that assigns higher weights to plants producing more of a given chemical, helping to ensure that our results are not driven by small or immaterial plants.[30] We then assess whether a firm's COGS increases when more of its plants are represented by Democrats.

[29] Abatement and recycling costs are accounted for as cost of goods sold (COGS) on firms' income statements if they can be imputed to specific production processes (as in the case of our chemicals), and as either other operating expenses or capital expenditures otherwise. See, e.g., page 314 of BP's 2019 environmental report (https://www.bp.com/content/dam/bp/business-sites/en/global/corporate/pdfs/investors/bp-annual-report-and-form-20f-2019.pdf).

[30] Individual plants have to report to TRI if the chemicals produced in these plants meet three size and toxicity criteria (https://www.epa.gov/toxics-release-inventory-tri-program/reporting-tri-facilities). Our weighted *Democrat Share* variable does not consider the impact of non-reporting plants on firm-level variables.

Our tests also include the equivalent of firm and time fixed effects – in our setting, these are firm $\times$ chemical and chemical $\times$ year fixed effects.

Table 7 presents the results of these tests. Columns (1) and (2) confirm that our main pollution result continues to hold at the firm-chemical-time level using the *Democrat Share* and weighted *Democrat Share* measures (which have similar magnitudes).[31] Columns (3) and (4) examine COGS. When more of a firm's plants are represented by Democrats (and hence, when firm pollution per unit of production falls), we indeed find that the firm's COGS is higher. The magnitudes are large: raising the Democrat share from zero to one would cause the firm's COGS to rise by approximately 4%. This finding suggests that reducing pollution has real costs for the firm.

We next look at firms' M/B ratios and Tobin's Q.[32] All else equal, increases in COGS should cause firm value to decline as well. Table 7 confirms this hypothesis: raising the Democrat share from zero to one would cause M/B ratios to fall by approximately 4% (columns (5) and (6)) and would cause Tobin's Q to decline by approximately 1% (columns (7) and (8)). Thus, complying with the ideologically-driven demands of local politicians can materially affect the value of constituent firms.

4.6 Real Effects on Public Health

We now examine whether differences in emissions induced by politicians' differing environmental ideologies can have real effects on local health outcomes. To do so, we merge health outcomes at the 3-digit Zip code (Zip3) level from CMS with our data on emissions and elections. We focus on the Zip3 level because this corresponds to the typical catchment area for most healthcare facilities. As described in Section 2, we focus on health outcomes related to respiratory diseases (CMS Major Diagnostic Category 4), since these health outcomes are more sensitive to emissions. We then compare how the number and cost of respiratory illnesses change when districts are represented by closely-elected Democrats versus closely-elected Republicans.

As in previous tests, we regress respiratory illness outcomes on the *Democrat Win* variable. How-

[31] Not every plant is required to report emissions to the EPA, so it is possible that there could be different results for non-reporting plants. However, classical measurement error in the *Democrat Share* variable would bias our estimates downward, and there is no reason to think that non-reporting plants would be systematically biased towards having higher or lower pollution when a Democrat is in office. In addition, the similar results across weighting schemes suggests that tiny plants that do not meet the TRI reporting requirements are unlikely to be biasing our results.

[32] The M/B ratio is defined as the market value of equity divided by the book value of equity. We use the definition provided on Ken French's website and restrict the sample to non-negative observations. We define Tobin's Q as total assets plus market equity minus book equity. Both definitions use data from Compustat.

ever, health outcomes in a district may change following a Democrat win due to factors other than changes in pollution (such as, say, a push to increase access to care). To isolate pollution-related health effects, we therefore focus on *within*-district outcomes: we compare areas within the same district that are plausibly more or less exposed to industrial pollution. Specifically, we interact the *Democrat Win* variable with an indicator variable called *High Number of Plants* that is equal to one if, within a Congressional district, the number of emitting plants in a given Zip3 is above the sample median in that year, and zero otherwise. Intuitively, high-plant areas should be more sensitive to changes in industrial pollution than low-plant areas. As such, the interaction between the Democrat Win indicator and the high number of plants indicator should capture the incremental change in respiratory health outcomes in emissions-sensitive (versus less-sensitive) areas when a district is won by a Democrat (versus a Republican). We also conduct a placebo test using diseases that are less likely to be caused by pollution exposure (CMS MDC categories 18 to 21), such as health problems related to infectious diseases, mental health, and drug issues).

Table 8 contains the results of these tests. Panel A focuses on respiratory health outcomes. In the first three columns of Panel A, our dependent variable is the number of clinical discharges related to respiratory diseases at the Zip3 level. The columns show two key findings. First, within a given district and year, high-plant areas unconditionally have 18.8%–32.5% more respiratory-related discharges than low-plant areas. Hence, high-emissions areas are generally associated with higher levels of respiratory illness, regardless of whether the district is represented by a Democrat or a Republican. However, columns (1)–(3) show that this baseline effect decreases by up to one-third when the district is won by a Democrat. In specification (3), where we exploit cross-sectional variation between Zip codes located in the same district in the same year, we find that areas with a high number of plants have 18.8% more discharges when the district representative is a Republican, but only 12.2% more discharges when the district representative is a Democrat, relative to areas in the same district with a low number of plants.[33] This translates into a difference of approximately 67 discharges per district per year, which is sizable: if all 435 Congressional seats were to flip from Democrat to Republican, this would result in an additional 58,290 hospital discharges per election cycle.[34]

[33]As described in Section 2, a single Zip3 area can span multiple Congressional districts, which allows us to identify Zip-district fixed effects. In Appendix Table A8, we also show that our results hold in the full CMS sample.

[34]The difference in magnitudes is 6.6%, and on average there are 727 discharges per high-plant 3-digit Zip code per year and 1.4 high-plant Zip codes per district. Hence, the expected district-wide effect is 6.6% $\times$ 727 $\times$ 1.4 = 67 discharges.

In columns (4)-(6) of Panel A, we repeat the same experiment using payments for respiratory-related discharges as our outcome variable. Consistent with the results in columns (1)-(3), we find that the gap in respiratory-related medical expenses between high-plant and low-plant areas again shrinks considerably when the area is represented by a Democrat. For example, areas with a high number of plants have 18.9% higher respiratory-related payments when the district is represented by a Republican, but only 11.6% higher payments when the district is represented by a Democrat. The economic magnitude of this difference is again large: approximately $628,000 per district per year, which translates into a hypothetical cost of more than $546 million per election cycle if all Congressional seats were to flip from Democrat to Republican.

Panel B contains the results of placebo tests. These tests help to alleviate concerns that health outcomes broadly improve in areas represented by Democrats for reasons other than decreased pollution. If this were true, we would expect improvements in health outcomes across many categories of illnesses, and not only for respiratory diseases. However, Panel B shows that there are no incremental effects on our placebo health outcomes in pollution-sensitive areas when the district politician is a Democrat. These results suggest that our findings in Panel A are plausibly related to the politically-motivated changes in local pollution documented by our main tests.

4.6.1 Reallocation

We also examine whether there firms' reallocation of pollution across plants (Table 6) results in an effective reallocation of public health costs. As in Table 6, for each firm-plant-time triad, we compute the fraction of the firm's *other* plants that are represented by Democrats at that point in time. Since health outcomes are measured at the Zip3 level, we compute the average of this fraction across all plants within a given Zip3 area. We then assess whether respiratory illnesses in a given area are higher in cases where the plants in that area are owned by firms whose *other* plants are in areas represented by Democrats.

Figure 4 shows that adverse respiratory health effects are indeed stronger in Zip3 areas where plants are owned by firms whose other plants are primarily represented by Democrats. This suggests that firms' reallocation of pollution effectively leads to a reallocation of health care outcomes as well. As a placebo test, we also examine whether adverse health effects for non-respiratory diseases also increase in these areas. The answer is no, as shown in Figure A11: if anything, non-respiratory illnesses drop in these areas on a relative basis.

4.6.2 Firm-Level Effects

Finally, we roll up health outcomes at the firm level and assess whether aggregate respiratory discharges and payments decrease at the firm level following Democrat wins. Aggregating health outcomes is complicated by the fact that health outcomes cannot be traced to a specific firm or plant. In addition, unlike Table 8, we cannot feasibly compare high-plant versus low-plant areas since most firms do not own plants in both high-plant and low-plant areas within the same district. As such, it is not possible to calculate the precise firm-level health effects associated with changes in districts' Congressional representation.

However, it is possible to provide a very rough estimate of these effects. To partially overcome the challenges above, we aggregate all annual respiratory discharges and payments at the three-digit Zip code level and then assign these aggregate levels to each plant within the area. A firm's total and average annual respiratory discharges/payments are then defined as the sum (average) of all three-digit Zip discharges/payments associated with the firm's various plant locations. As in Table 7, we relate these firm-level health effects to *Democrat Share* and *Emissions-Weighted Democrat Share*, which capture the fraction of the firm's plants that are represented by Democrats.

Table A9 contains the results of these tests. Columns (1)–(4) examine total discharges and average discharges for all firms in the sample, while columns (5)–(8) examine total discharges and average discharges specifically at multi-plant firms. Columns (1)–(4) show that there is a noisy but sizable relationship between firm-level discharges and *Democrat Share*. For example, column (3) shows that moving the share of a firm's plants represented by Democrats from zero to one would be associated with a 4.4% decline in the firm's average discharges (though this result is not statistically significant). Columns (5)–(8) show that these effects are much larger at multi-plant firms: for example, column (7) shows that average discharges drop by more than 9% when *Democrat Share* is moved from zero to one. Collectively, these results – while speculative – indicate that even though firms do their best to reallocate pollution (and thus, effectively, illnesses), the net effects of Democratic victories on respiratory illnesses in their districts are material.

4.7 Mechanism: Increased Regulatory Oversight

Why is industrial pollution in a Congressional district a function of the district's representative's political ideology? One plausible channel is that politicians lean on local and/or federal regulators to increase or decrease environmental inspections and enforcement. As noted in the introduction, this channel is consistent with prior literature on political interference and with anecdotal evidence suggesting that U.S. House members regularly interact with state and federal environmental regulators on topics of interest to the representative. Given an expected increase in inspections and enforcement, plants that are at risk of over-polluting should reduce pollution provided that the cost of reducing pollution is lower than the expected pecuniary and non-pecuniary penalties from over-polluting.

A necessary condition for this hypothesis to be true is that firms are indeed over-polluting (or have a higher risk of over-polluting) prior to a Democrat winning a close election. While it is not possible to directly measure whether firms are over-polluting, the narrative above yields a simple prediction: all else equal, when a Democrat is closely elected, we would expect larger reductions in pollution at plants with high *prior* pollution, since these are the plants that were most likely to be over-polluting prior to the election. Figure A12 confirms that this is indeed the case: the drop in emissions is significantly larger at firms with above-median pollution (from the previous election cycle) within a given state-chemical-year triad. Hence, the largest drops in pollution following a close Democratic win occur at the plants with the highest level of pre-election pollution.

We next return to our RD setting to study the effect of close Democratic wins on EPA-related inspections and enforcement at the plant-year level. We first examine whether close wins by Democrats are associated with a higher probability of plants in their district being inspected. In the first two columns of Table 9, we show that close election victories by Democrats are associated with a 6.8%-7.8% increase in overall inspection volumes, which is significant. In columns (3) and (4), we examine the extensive margin. We find that close wins by Democrats are associated with a 2.2% increase in the likelihood of getting at least one inspection. This number is large, corresponding to a 6.7% increase relative to the unconditional probability of 32.64% of receiving at least one inspection for the average plant in our sample. We next examine the intensive margin. In columns (5) and (6), we find that close district wins by Democrats are associated with a 17.7% to 21.4% increase in inspections for plants that are already subject to inspections (i.e., plants with non-zero annual inspections). Figure 5 displays this pattern graphically. The magnitudes are again sizable: a 20% increase in inspections (based on our estimates) implies an extra 0.49 annual inspections for the average already-inspected firm (which has a sample mean of 2.44 inspections per year). Overall, the results of Table 9 provide strong evidence that Democrat district affiliation results in more EPA inspections, suggesting that Democrat representatives may induce environmental agencies to monitor firms' emissions more closely.

Does increased monitoring also result in stricter enforcement when a district is won by Democrats? In the absence of frictions, it is not clear that realized enforcement actions should change significantly: firms should optimally reduce pollution once the expected cost of over-polluting goes up. Hence, the expected effects of increased monitoring on realized enforcement penalties are unclear. Nonetheless, in the presence of frictions such as asymmetric information about inspection thoroughness, it seems reasonable to think that, even as firms are reducing pollution, there may still be a greater number of enforcement actions per inspection due to inspectors writing up firms for minor infractions that may not have been penalized under Republican representatives.

In Table 10, we study enforcement actions and pecuniary penalties at the plant-year level. In Panel A, we start from the extensive margin of enforcement. Columns (1) and (2) show that the probability of an enforcement action (either formal or informal) increases by around 6.4% when the district where the plant is located is just-won by a Democrat. Figure A13 presents this pattern graphically. Again, this estimated effect implies a large increase in enforcement actions, given that the unconditional probability of an enforcement following an inspection is equal to 21.98%. In other words, Democrat wins increase the probability of an enforcement by 29.11% following an inspection.

In columns (3)-(6) of Panel A, we show that the main effects from Columns (1) and (2) come primarily from an increase in *informal* enforcement (e.g., cease and desist letters) as opposed to formal enforcement (e.g., civil legal actions). Close Democrat wins are associated with a 7.7% increase in the probability of an informal enforcement action (an increase of 47.65% relative to the unconditional probability of 16.16% of an informal enforcement after an inspection), and only with a 2.7% increase in the probability of a formal action (an increase of 24.59% relative to the unconditional probability of 10.98% of a formal enforcement after an inspection). Consistent with these estimates, in the last two columns of Panel A we also confirm that the probability of a monetary penalty increases by around 2.2% following a close Democratic win.[35]

[35] EPA enforcement action milestone data for the period 1991-2016 reveals that the average and median duration of an EPA enforcement action are 1 year and 1 quarter, respectively, and that only 14% of the total enforcement actions last more than two years. These statistics mitigate possible concerns that our results may be driven by formal enforcement actions

The results in Panel A suggest that enforcement actions rise significantly following close Democratic wins. However, a large part of this effect may simply be caused by the fact that regulators are conducting more inspections in the first place. To disentangle higher enforcement from higher inspections, we examine the ratio of enforcement actions per inspection in Panel B of Table 10. We confirm that our enforcement results also hold on the intensive margin—enforcement actions per inspection increase by around 0.05 (a 27% increase relative to the sample mean) and informal enforcement actions per inspection increase by around 0.055 (a 52% increase relative to the sample mean) when a Democrat representative just-wins the district. However, formal enforcement actions per inspection and penalties per inspection do not experience statistically significant changes when a Democrat gains control of the local district. This is consistent with a channel in which, despite regulators feeling more empowered to punish firms for violating environmental regulations, firms respond to the increased probability of inspections and enforcement by reducing pollution to a level that does not trigger significant enforcement actions.

The EPA delegates most enforcement of federal environmental laws to state regulatory agencies.[36] Consistent with the idea that state regulators are the primary agencies responsible for enforcing EPA regulations, Table A10 shows that our results on inspections and enforcement are mostly driven by state (as opposed to federal) regulators, though the main results hold for federal regulators as well. This suggests that, to the extent that politicians are interfering with regulatory agencies, such political interference appears to mostly happen at the state rather than the federal level in our sample.

4.8 Additional Robustness

4.8.1 Within-party Variation

Our main hypothesis is that the ideology of politicians causes them to take actions to affect pollution in their home districts. By sorting politicians into groups based on their political party, our tests implicitly assume that political party memberships capture meaningful differences in the personal ideologies of politicians. Figure 1 provides evidence supporting this assumption: the figure shows that the amount of inter-party variation in LCV scores is many times larger than the amount of intra-party variation in representatives' environmental voting records. Nonetheless, it is useful to verify

concluded outside of the politician's current term.

[36]Generally speaking, if a state has more stringent environmental protection laws than the federal EPA laws, then the EPA usually delegates inspection and enforcement authority to the state. This is known as "delegation" or "primacy."

more systematically that politicians' actions are driven by their ideological views about the environment and not by some other factor that differs between Democrats and Republicans.

To do so, we exploit *within-party* ideological differences to see if, for example, firms pollute less in districts just won by Democrats with strongly pro-environment voting records relative to firms in other districts just won by Democrats with weaker environmental voting records. This test allows us to confirm that it is the ideology of the politician, rather than the politician's party, that is causing the changes in observed pollution levels at constituent firms.

We measure ideology in two ways. First, we obtain Member Ideology scores for each representative from the VoteView database. These scores are calculated based on politicians' voting records using the DW-NOMINATE methodology (detailed in the appendix to Poole and Rosenthal, 2001). Second, we utilize the same annual LCV environmental scorecards that we used to construct Figure 1. LCV reports a score ranging from 0 to 100 for each representative based on their voting record on environment-related bills, with 100 representing a perfect pro-environment voting record.

Results are reported in Figure 6. The figure shows that the reductions in firm pollution are much stronger in districts just won by "deep blue" (more ideological) Democrats than in districts just won by "light blue" (less ideological) Democrats. This result provides further evidence that the reductions in pollution documented in Table 2 are caused by differences in political ideology between Democrats and Republicans rather than other factors that happen to be correlated with political party membership.

4.8.2 Seat Pickups

If partisan ideology is a key driver of politicians' influence over emissions in their district, we would also expect to see strong effects on emissions when a district switches from being represented by a Democrat to being represented by a Republican (and vice versa; often known as a "pickup" for the winning party). To test this hypothesis, we start with all facilities that were represented by Republicans in the year prior to an election. We then break up these facilities into those that were represented by a Democrat after the election, and those that were represented by a Republican. We perform a similar exercise for facilities initially represented by a Democrat.

Table A11 confirms that seat pickups are associated with strong effects on local firms' pollution. Columns (1) and (2) show that, after a district moves from Republican to Democratic representation, relative emissions at facilities in that district decline by approximately 6%. Columns (3) and (4) show that, after a district moves from Democratic to Republican representation, relative emissions at facilities in that district rise by approximately 3%.[37] Figure 7 displays these patterns graphically. Collectively, these findings support the idea that partisan ideological differences are at least partially responsible for the stark changes in emissions in red versus blue districts during our sample period.

4.8.3 Reelection

We also show that changes in firms' emissions do not affect politicians' reelection probabilities. In Table A12, we use district-level emissions growth rates to predict whether an incumbent politician will be reelected. We define *Reelected* as equal to one if a sitting politician is reelected and equal to zero otherwise. We then interact the *Democrat Win* variable (from the previous election) with the district-level emissions growth rate during the politician's latest term (just prior to reelection). If Democratic politicians' preference for low emissions is a response to career considerations (such as shifts in voter preferences in their districts), then Democrats should be more likely to win reelection when emissions growth at constituent firms is low during their terms. However, the interaction term in all columns of Table A12 is statistically and economically insignificant, providing further support that career incentives and changes in voter preferences are unlikely to explain our results.

4.8.4 Political Power

All else equal, greater political power should translate into a greater ability to influence regulators. Hence, we might expect our results to be stronger for powerful politicians such as the chair or ranking member of House committees. In addition, all else equal, politicians with stronger ideological views should have a higher probability of influencing regulators. Thus, we might expect our results to be strongest for the *interaction* of politicians' ideologies and political power.

To examine this hypothesis, we perform a triple-difference analysis that interacts our main *Democrat Win* variable with two other variables. First, we construct a dummy variable that equals one if a member is a committee chair and equals zero otherwise. We then construct a second variable that captures the strength of House members' political ideologies. This variable takes the value of one

[37] The difference in magnitudes may relate to the irreversibility of some types of abatement investment: if a plant invests in new equipment under a Democrat, it may be very costly to fully remove it, and/or the investment may constitute a sunk cost (with low ongoing expenses), such that it is rational for firms to continue using the technology.

if a politician has a strong ideology (VoteView ideology score in the top quartile of the distribution) in either direction and takes the value of zero otherwise. We then interact these variables with each other and with our main *Democrat Win* variable.

The results are presented in Table A13. We find three results. First, there is still a reduction in pollution in districts represented by non-committee-chair, less-ideological Democrats. Second, this result is not stronger for districts represented by less-ideological *committee chairs*, suggesting that political power alone is not a major contributing factor to our results. Third, in columns (3)–(5), we find very strong within-firm effects coming from ideological Democratic committee chairs – the loading of -0.249 for the *Low Id. Score* $\times$ *Democrat Win* $\times$ *Committee Chair* variable in column (5) is more than 10 times larger than the loading on the main effect in the same column. This suggests that firms are potentially more likely to reallocate pollution when a plant is represented by a powerful committee chair with a strong ideology.

4.8.5 Political Connections

Finally, a natural concern is that our findings are simply capturing the effects of politically connected firms, who may alter their pollution behavior for reasons other than the politician's partisan ideology (such as, for example, to help the politician win election or reelection to office). This explanation seems unlikely for a number of reasons; for example, it is not clear why these effects would be concentrated amongst the most ideological representatives from both parties, and it is not clear why inspections and enforcement would change. Nonetheless, to rule out this explanation, we use campaign contribution data from the FEC to split firms each year into those who donated to the (winning) local representative in the most recent election cycle (connected firms), and those that did not (unconnected firms). We then restrict the sample to only include plants owned by unconnected firms. Figure A14 confirms that our main results are unchanged when we focus solely on plants owned by firms that are not connected to their local U.S. representative. This reinforces the central point that our main results appear to be capturing the effect of *political ideology* on regulatory and firm outcomes.

5 Conclusion

How do politicians' partisan beliefs translate into changes in firm behavior? In this paper, we provide causal evidence that politicians' ideology affects constituent firms' industrial pollution decisions. We focus on pollution decisions because of the availability of detailed, facility-level pollution and production data, and because politicians are strongly ideologically polarized on the issue of pollution.

Using a regression discontinuity design involving election outcomes in close U.S. congressional races, we find that plants pollute less when they are represented by a closely-elected Democrat than when they are represented by a closely-elected Republican. The decline in pollution stems from costly increases in abatement investment and post-production treatment activities, not from reduced production. We also show that multi-plant firms appear to reallocate pollution across plants after a change in one plant's political representation: using Giroud and Mueller (2019)-style tests, we show that a firm's individual plants pollute more and recycle less when more of the firm's *other* plants are represented by Democrats. Our results survive a battery of robustness tests and are stronger within party for more ideological representatives, confirming ideology as the main driver behind the wedge we observe in politicians' pollution postures.

Since we measure firms' compliance with federal environmental laws (that apply to all plants), our setting allows us to largely abstract from changes in legislation or rule-making. As such, we focus on politically-motivated regulatory interference as a plausible mechanism. Consistent with this mechanism, we show that facility inspections by environmental regulators increase by approximately 20% and informal enforcement actions rise significantly after a Democratic close election win.

Finally, we show that pollution differences caused by politicians' partisan ideologies can have meaningful impacts on the health of local communities. Hospital discharges and payments for respiratory diseases decrease around plants in Congressional districts represented by Democrats, whereas discharges and payments for illnesses plausibly unrelated to pollution do not change. In addition, we show that there is a reallocation of community health costs after a Democrat wins, with pollution – and health costs – being reallocated to other plants that are not represented by Democrats. Collectively, our results confirm that the Amazon dispute highlighted in the introduction is part of a larger trend: U.S. politicians routinely impart their partisan ideological beliefs on constituent firms, and these outcomes can have significant consequences for local communities.

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Figure 1

Environmental Polarization Over Time

This figure plots the median annual League of Conservation Voters (LCV) scores of Democrats and Republicans in the U.S. House of Representatives from 1991 to 2020. The LCV computes scores based on politicians' voting records on legislation related to the environment. A higher LCV score indicates that a politician voted in favor of a higher number of pro-environment law proposals. The data was retrieved from the LCV website.

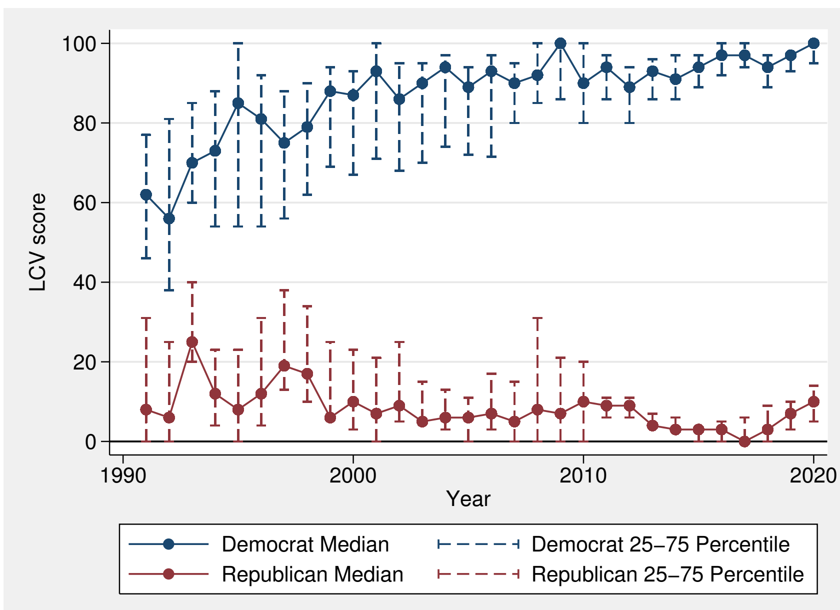

Figure 2

RD using Close Elections: Emissions

The figure plots the natural logarithm of facility-chemical-level toxic emissions in the two years following Congressional elections as a function of the vote share margin of a Democratic candidate in a congressional district. The vote share margin is the percentage by which a candidate won (lost) the election in a given district, and the sample uses elections won or lost by a margin of 5% or less. In the figure, we plot average emissions in the two years following a U.S. House of Representatives election for 36 bins of the election win margin distribution, as well as values and 95% confidence intervals from local polynomial regressions fitted on each side of the zero win margin threshold. The data on emissions is available at the facility-chemical-year level from the EPA's Toxics Release Inventory (TRI) database. The sample period is 1991-2016.

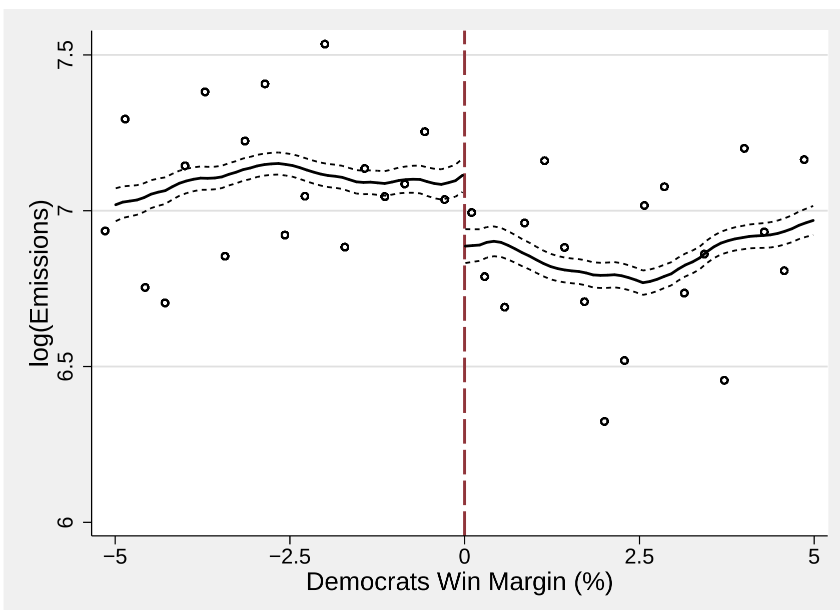

Figure 3

Within-Firm Reallocation of Toxic Emissions

This figure plots the relationship between a plant's emissions (Panel A) and abatement (Panel B) and the share of the other plants of the same firm that are represented by Democrats. In Panel A, we first remove any time-invariant differences in the total level of emissions for each chemical by regressing facility-level emissions on facility $\times$ chemical fixed effects. We drop single-plant firms from the sample, and we construct a plant-level variable representing the share of the same firm's *other plants* that are represented by Democrats. We call this plant-level variable "Other Facilities' Democrat Share." In the figure, we plot average emissions in 25 bins of the "Other Facilities' Democrat Share" distribution. In Panel B, we repeat the same exercise using a firm's residualized post-production activity ratio (the ratio of emissions reduced through post-production treatment, recycling, and energy recovery activities to the total gross waste of a plant) as the dependent variable. The total gross waste of a plant is the sum of total actual releases and the emissions that are reduced through treatment, recycling, and energy recovery activities. All the variables are defined as in Table 1, and the sample period is 1991-2016.

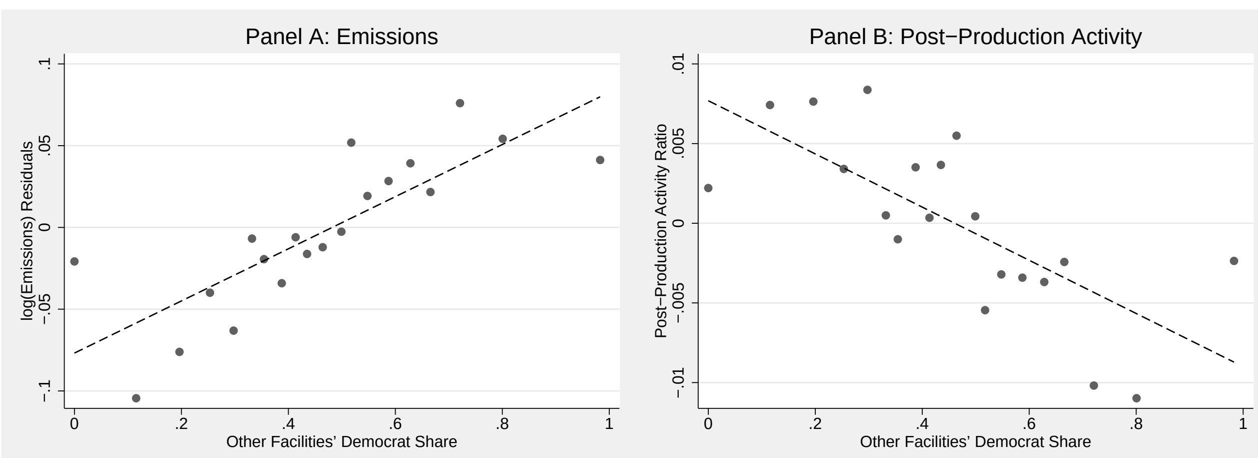

Figure 4

Reallocation Tests: Respiratory Diseases

This figure plots the relationship between the number of patient discharges related to respiratory diseases in an area, and the average value of "Other Facilities' Democrat Share" across plants located in that area. The procedure we follow to produce this figure is similar to that described in Figure 3, but the horizontal axes in this figure represent *average* values of "Other Facilities' Democrat Share" for all plants located in an area. Panel A includes Zip codes that contain a number of establishments higher than the median in our sample, and Panel B includes Zip codes with a number of establishments lower than the median. The data on patient discharges is described in Table 1. All variables are defined as in Table 1, and the sample period is 2011-2016.

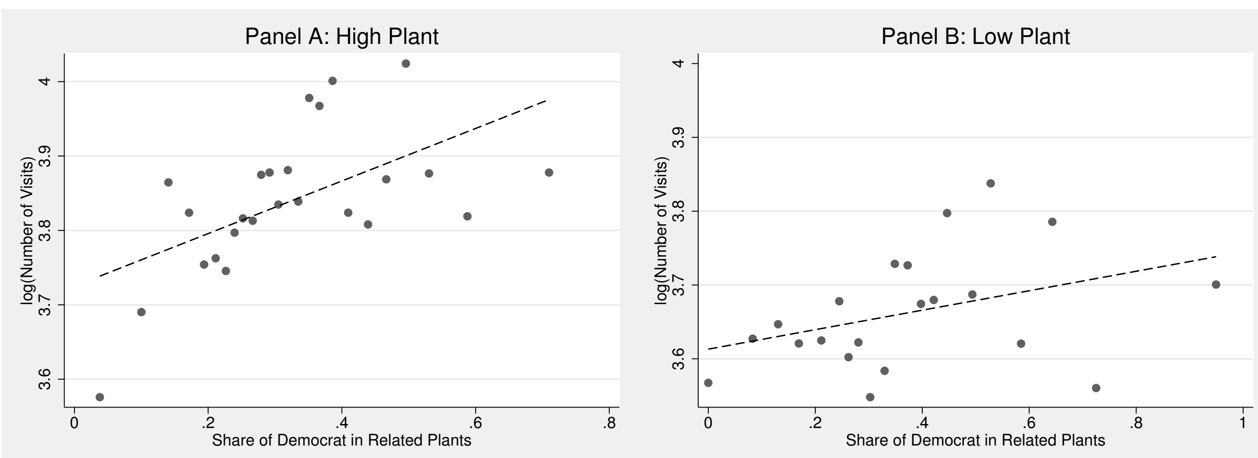

Figure 5

RD using Close Elections: Inspections

The figure plots the natural logarithm of facility inspections in the two years following Congressional elections as a function of the vote share margin of a Democratic candidate in a congressional district. Inspection data comes from the EPA's ECHO data set. The figure is identical to Figure 2 except for the outcome variable. All variables are defined as in Table 1, and the sample period is 1991-2016.

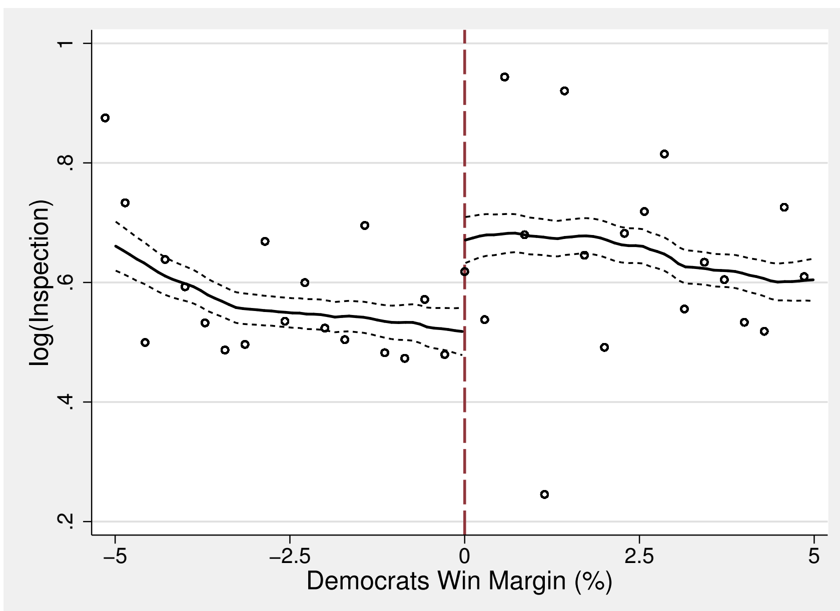

Figure 6

The Effects of Ideology

In this figure, we split the sample of close elections based on Democrats' ideology using two measures of ideological variation: ideology scores from the VoteView database of voting records, and environmental scorecards from the League of Conservation Voters (LCV). As in Figure 2, the figure plots the natural logarithm of toxic emissions at the facility-chemical-year level in the two years following Congressional elections as a function of the vote share margin of a Democratic candidate in the congressional district. In Panel A, our measure of ideology is the politician's ideology score from VoteView (updated annually). The deep blue line represents marginal wins by Democrats in the 25th percentile of the Democrat ideology distribution, while the light blue line represents marginal Democrat wins by all other Democrats. In Panel B, our measure of ideology is the politician's annual LCV score. The deep blue line represents marginal wins by Democrats in the 75th percentile of the Democrat LCV score distribution, while the light blue line represents marginal Democrat wins by all other Democrats. The LCV scores come from the LCV website. Democrat win margin is the percentage by which a Democrat candidate won or lost the election. The sample uses elections won or lost by a margin of 5% or less.

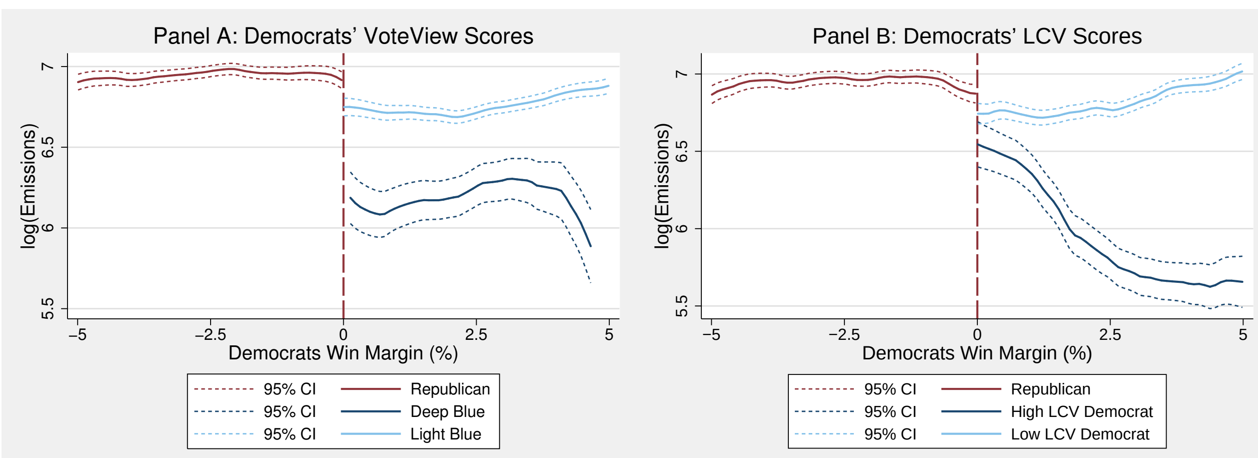

Figure 7

Examining "Switchers"

The figure shows the difference in emissions for facilities located in areas that switch from a Democratic to a Republican representative, and vice versa. In Panel A, we average emissions across all facilities represented by Democrats in a given election cycle, and then compute the average difference in emissions for districts that switch to a Republican representative in the next election cycle and those that keep a Democrat representative in the next election cycle. In Panel B, we average emissions across all facilities represented by Republicans in a given election cycle, and then compute the average difference in emissions for districts that switch to a Democrat representative in the next election cycle and those that keep a Republican representative in the next election cycle. In the figure, we report average differences as well as 95% confidence intervals around these averages, and we normalize emission differences to equal zero in the year of the (November) election. All the variables are defined as in Table 1, and the sample period is 1991-2016.

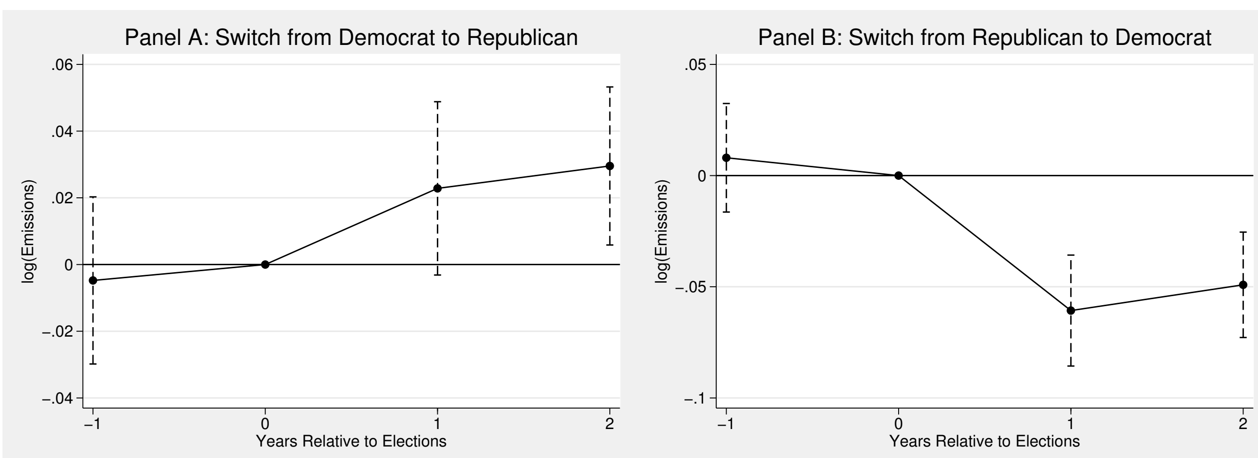

Table 1

Summary Statistics

This table presents summary statistics for the main variables in the paper. Emissions are annual toxic chemical releases (in pounds) at the facility-chemical-year level over the period 1991-2016, available on the U.S. Environmental Protection Agency's (EPA) website. Congressional election data are from the MIT Election Data and Science Lab. Abatement is the number of source reduction activites at the facility-chemical-year level over the period 1991-2016 reported in the EPA's Pollution Prevention (P2) database. The post-production reduction ratio is the ratio of emissions reduced through post-production treatment, recycling, and energy recovery activities to the total gross waste of a plant over the period 1991-2016, available from the EPA TRI database. Cost of goods sold, Market-Book ratio and Tobin's Q are from Compustat, which we hand-match with the TRI data based on the name of the firm. Health data, including the number of discharges and total payments (in millions of US dollars) for respiratory diseases and non-pollution related diseases are from the Center for Medicare & Medicaid Services (CMS) over the period 2011-2016. Environmental regulation compliance data including inspections, enforcement actions, and penalties, are from the EPA's Enforcement and Compliance History Online (ECHO) dataset over the period 1991-2016. All data is publicly-available.

	Mean	SD	p10	p50	p90	Facilities	Observations
Emissions	30845.79	177340.99	0.00	369.00	43486.00	37,369	1,784,978
Democrat Win Margin	2.67	36.88	-41.26	0.96	50.43	.	5,304
Democrat Win Margin (Close Elections)	0.19	2.96	-3.95	0.21	4.14	.	387
Source Reduction Abatement	0.26	0.77	0.00	0.00	1.00	36,262	1,589,601
Post-production Reduction Ratio	0.50	0.45	0.00	0.53	1.00	37,369	1,534,814
log(COGS)	6.83	1.82	4.50	6.83	9.10	.	19,578
Market-Book Ratio	3.12	3.19	0.94	2.25	5.70	.	15,991
Tobin's Q	1.67	0.79	0.99	1.43	2.65	.	16,814
Discharges(Respiratory)	54.45	58.33	13.00	34.00	121.00	.	60,352
Total Payment (Respiratory)	0.48	0.54	0.10	0.30	1.10	.	60,352
Discharges(Placebo)	82.25	138.20	12.00	32.00	205.00	.	28,282
Total Payment (Placebo)	1.06	1.76	0.09	0.43	2.71	.	28,282
Inspections	0.80	1.67	0.00	0.00	2.00	37,333	438,272
Enforcement	0.15	0.53	0.00	0.00	0.00	37,333	438,272
Formal Enforcement	0.06	0.30	0.00	0.00	0.00	37,333	438,272
Informal Enforcement	0.09	0.35	0.00	0.00	0.00	37,333	438,272
Penalty	277.89	1889.76	0.00	0.00	0.00	37,333	438,272

Table 2

Representatives' Political Ideologies and Toxic Emissions

In this table, we study the effect of marginal district wins by Democratic Party candidates on emissions by local plants. In column (1), we regress the natural logarithm of plant-level emissions on a dummy equal to one if the district where the plant is located was won by a Democrat in its most recent election, and equal to zero otherwise. In columns (2)-(3), we augment this specification with a linear interaction term between the dummy and Democrat margin votes in a local OLS regression framework. In columns (4)-(7), we use non-parametric local polynomial RD estimators (Calonico, Cattaneo, and Titiunik, 2014), experimenting with linear and quadratic polynomials and triangular and Epanechnikov kernels. In columns (1)-(3), we report standard errors clustered at the district-year level. In columns (4)-(7), we report robust bias-corrected standard errors as in Calonico, Cattaneo, and Titiunik (2014). R^2 values are unreported since they are not available for the nonparametric regressions. The dependent variable is defined as in Table 1. In columns (1)-(3), the sample includes all U.S. House of Representatives elections with an absolute vote margin of less than 5%. In columns (4)-(7), the sample includes all House elections. The sample period is 1991-2016.

	Dep. Variable: log(Emissions)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democrat Win	-0.213**	-0.397**	-0.305***	-0.355***	-0.349***	-0.353***	-0.355***
	(0.08)	(0.16)	(0.12)	(0.03)	(0.03)	(0.04)	(0.04)
Mehtod	Local OLS	Local OLS	Local OLS	NP	NP	NP	NP
Polynomial Order	Zero	Linear	Linear	Linear	Linear	Quadratic	Quadratic
Kernel	–	–	–	Tri.	Epa.	Tri.	Epa.
Chemical FE	No	No	Yes	–	–	–	–
Observations	94,140	94,140	94,111	1,329,508	1,329,508	1,329,508	1,329,508

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 3

Emissions and Plant Production

In this table, we study the effect of marginal district wins by Democratic Party candidates on emissions per unit of production. The dependent variable is the natural logarithm of cumulative emissions per production at the plant-chemical level, as described in equation (4). In column (1), we regress this outcome variable on a dummy equal to one if the district where the plant is located is marginally won by a Democrat, together with a linear interaction term between the dummy and the Democrat vote margin in a local linear OLS framework. In column (2), we use the non-parametric local polynomial RD estimator of Calonico et al. (2014), specifying a linear polynomial and a triangular kernel. We report standard errors clustered at the district-year level for the local linear OLS regression and robust bias-corrected standard errors as in Calonico et al. (2014) for the non-parametric regression. In column (1), the sample includes close U.S. House of Representatives elections (with an absolute vote margin of less than 5%), while the sample includes all House elections in other columns. The sample period is 1991-2016.

	log(Cumulative Emissions/Production)	
	(1)	(2)
Democrat Win	-0.093*	-0.057***
	(0.06)	(0.02)
Method	Local OLS	NP
Polynomial Order	Linear	Linear
Kernel	–	Tri.
Chemical FE	Yes	–
Observations	84,306	1,178,073

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 4

Abatement and Post-Production Activity Tests

In this table, we study the effect of marginal district wins by Democratic Party candidates on source abatement investment and on post-production treatment, recycling, and energy recovery activities. In columns (1)-(2), the dependent variable is the natural logarithm of 1 + the total number of plant-level abatement activities for a specific chemical in a year. The dependent variable in columns (3)-(4) is the ratio of emissions reduced through post-production treatment, recycling, and energy recovery activities to the total gross waste of a plant—the sum of the plant's total actual releases and the emissions that are reduced through treatment, recycling, and energy recovery activities. The specifications mimic those in Table 3. The dependent variable is defined as in Table 1, and the sample contains all U.S. House of Representatives elections during the period 1991-2016.

	Log(1+Abatement)		Post-Production Reduction Ratio	
	(1)	(2)	(3)	(4)
Democrat Win	0.033*	0.018***	0.030**	0.025***
	(0.02)	(0.00)	(0.01)	(0.00)
Method	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.
Chemical FE	Yes	–	Yes	–
Observations	104,915	1,491,554	102,520	1,438,655

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 5

Political Ideology and Within-firm Emissions

This table uses OLS panel regressions to examine the relationship between a plant's toxic emissions and the political affiliation of the local representative. In this table, the dependent variable is the natural logarithm of emissions at the plant-chemical-year level in the two years following a U.S. House of Representatives election. Democrat Win is an indicator that takes the value of one if a candidate from the Democratic party won the last election in the district where the plant is located. Standard errors are clustered at the district-year level. The sample period is 1991-2016.

	Dep. Variable: log(Emissions)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democrat Win	-0.059***	-0.054***	-0.057***	-0.042***	-0.020**	-0.018*	-0.020*
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
District-Decade FE	Yes	No	No	No	No	No	No
Year FE	Yes	No	No	No	No	No	No
Firm × Chemical × Year FE	No	No	No	Yes	Yes	Yes	Yes
District-Decade × Chemical FE	No	No	No	Yes	No	No	No
Facility × Chemical FE	No	No	No	No	Yes	Yes	Yes
State × Year FE	No	Yes	No	No	No	Yes	No
State × Year × Chemical FE	No	No	Yes	No	No	No	Yes
R-Squared	0.076	0.051	0.462	0.850	0.929	0.929	0.938
Observations	1,329,508	1,329,508	1,279,458	790,904	782,632	782,632	739,229

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 6

Reallocation of Toxic Emissions

This table examines how a facility's pollution depends on the political party representation of the firm's other facilities. In columns (1) and (2), we regress annual plant-chemical-level toxic emissions on the share of plants owned by the same firm at the same time that are represented by a Democrat, which we refer to as "Other Facilities' Democrat Share," as in Figure 3. In columns (3) and (4), we regress emissions on an indicator for whether this share is above the median in our sample. Standard errors are clustered at the district-year level. The dependent variable is defined as in Table 1, and the sample contains all U.S. House of Representatives elections during the period 1991-2016.

	Dep. Variable: log(Emissions)			
	(1)	(2)	(3)	(4)
Other Facilities' Democrat Share	0.043***	0.063***		
	(0.01)	(0.01)		
Local Democrat	-0.026**		-0.025**	
	(0.01)		(0.01)	
High Other Facilities' Democrat Share			0.022***	0.027***
			(0.01)	(0.01)
District-Decade × Chemical FE	Yes	No	Yes	No
Chemical × Year FE	Yes	No	Yes	No
Facility × Chemical FE	Yes	Yes	Yes	Yes
District × Chemical × Year FE	No	Yes	No	Yes
R-Squared	0.905	0.922	0.905	0.922
Observations	1,123,775	897,686	1,123,775	897,686

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 7

Firm-Level Pollution and Financial Outcomes

In this table, we examine the firm-level financial effects of changes in environmental pollution. The unit of observation is a firm-chemical-year. We examine firms' costs of goods sold (COGS), market-to-book ratios (M/B) and Tobin's Q. COGS should include any changes in the cost of abatement, while the M/B ratio should capture the cumulative expected financial effects of a change in firms' pollution profiles. To obtain firm-level COGS, we manually match each firm name in the EPA-TRI dataset with firm names in Compustat. The variable Democrat share in columns (1), (3), (5) and (7) is the fraction of a firm's total plants operating in a Democrat district. In columns (2), (4), (6) and (8), we weigh each plant-chemical-year observation by its total emission levels, such that plant-chemical Democrat Share observations associated with higher emissions receive more weight. The dependent variable in columns (1)-(2) is the natural logarithm of firm-level emissions. The dependent variable in columns (3)-(4) is the natural logarithm of firm-level costs of goods sold (COGS). The dependent variable in columns (5)-(6) is the firm-level M/B ratio, censored at zero. The dependent variable in columns (7)-(8) is the firm-level Tobin's Q. Standard errors are clustered at the district-year level, and the sample period is 1991-2016.

	log(Emissions)		log(COGS)		Market-Book Ratio		Tobin's Q	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Share	-0.040*		0.048***		-0.132*		-0.022*	
	(0.02)		(0.01)		(0.07)		(0.01)	
Emissions-Weighted Democrat Share		-0.062***		0.037***		-0.139**		-0.020**
		(0.02)		(0.01)		(0.06)		(0.01)
Chemical-Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm Chemical FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.863	0.864	0.951	0.951	0.519	0.519	0.668	0.668
Observations	189,858	189,858	189,313	189,313	155,413	155,413	162,633	162,633

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 8

Real Effects: Local Health Outcomes

This table uses OLS regressions to examine the relationship between local health outcomes and political affiliations. The dependent variables are the natural logarithm of discharges and total payments for respiratory diseases (Panel A) and for pollution-unrelated diseases (Panel B). Democrat Win is an indicator that takes the value of one if a Democratic candidate won the last election in the district where the plant is located. High Num. Plants is an indicator that takes the value of one if the number of plants in the area is above median. ZIP is the three-digit Zip code. MDC is major diagnostic category code that divides all possible principal diagnoses into 25 mutually exclusive diagnosis areas. Standard errors are clustered at district-year level. The sample period is 2011-2016.

Panel A: Respiratory Diseases

	log(Number of Discharges)			log(Total Payments)		
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	0.014	0.007		0.101***	0.021	
	(0.02)	(0.02)		(0.02)	(0.02)	
High Num. Plants	0.325***	0.288***	0.188***	0.350***	0.301***	0.189***
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Democrat Win × High Num. Plants	-0.082***	-0.071**	-0.066**	-0.126***	-0.075**	-0.073**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
ZIP FE	Yes	Yes	No	Yes	Yes	No
District-Decade FE	No	Yes	No	No	Yes	No
Year FE	Yes	Yes	No	Yes	Yes	No
District × Year FE	No	No	Yes	No	No	Yes
ZIP × District-Decade FE	No	No	Yes	No	No	Yes
R-Squared	0.187	0.239	0.273	0.207	0.264	0.299
Observations	60,351	60,349	60,336	60,351	60,349	60,336

Panel B: Placebo, Pollution-Unrelated Diseases

	log(Number of Discharges)			log(Total Payments)		
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	0.023	-0.012		0.131***	-0.041	
	(0.02)	(0.04)		(0.03)	(0.04)	
High Num. Plants	0.212***	0.149***	0.112***	0.259***	0.167***	0.124***
	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)
Democrat Win × High Num. Plants	0.035	0.060*	0.004	-0.041	0.053	0.004
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.05)
ZIP FE	Yes	Yes	No	Yes	Yes	No
District-Decade FE	No	Yes	No	No	Yes	No
Year FE	Yes	Yes	No	Yes	Yes	No
District × Year FE	No	No	Yes	No	No	Yes
ZIP × District-Decade FE	No	No	Yes	No	No	Yes
MDC FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.216	0.249	0.275	0.431	0.469	0.493
Observations	28,276	28,273	28,227	28,276	28,273	28,227

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 9

Mechanism: Regulatory Inspections

In this table, we study the effect of marginal district wins by Democratic Party candidates on inspections by the EPA. The dependent variable in columns (1)-(2) is the natural logarithm of 1 + plant inspections in a given year. The dependent variable in columns (3)-(4) is an indicator variable that takes the value of one if a plant gets EPA inspection in a year, and zero otherwise, pinning down the extensive margin of inspections. The dependent variable in columns (5)-(6) is the natural logarithm of plant inspections, pinning down the intensive margin of inspections. In columns (1), (3) and (5), we estimate our regressions using a local OLS specification, while in columns (2), (4) and (6) we use a non-parametric local polynomial RD estimator (Calonico et al., 2014), specifying a linear polynomial and a triangular kernel. We report standard errors clustered at the district-year level for local linear OLS regressions, and robust bias-corrected standard errors as in Calonico et al. (2014) for non-parametric regressions. The sample contains all U.S. House of Representatives elections during the period 1991-2016. In columns (1), (3), and (5) we restrict the sample to elections with an absolute vote margin of less than 5% during the same period.

	log(1+Inspections)		Inspection Dummy		log(Inspections)	
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	0.078*	0.068***	0.029	0.022***	0.214***	0.177***
	(0.04)	(0.01)	(0.03)	(0.01)	(0.07)	(0.02)
Method	Local OLS	NP	Local OLS	NP	Local OLS	NP
Polynomial Order	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.	–	Tri.
Observations	30,773	414,343	30,773	414,343	9,419	132,990

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table 10

Mechanism: Regulatory Enforcement

In this table, we study the effect of marginal district wins by Democratic Party candidates on EPA enforcement and penalties. The dependent variables in Panel A are indicator variables equal to one if the plant gets an EPA enforcement, an informal enforcement, a formal enforcement, or a penalty, and equal to zero otherwise. The dependent variables in Panel B represent the number of enforcement actions, informal enforcement actions, formal enforcement actions, and penalties relative to inspections. The specifications in this table mimic those in Tables 3 and 9.

Panel A: Enforcement Dummies

	Enforcement		Informal Enf.		Formal Enf.		Penalty	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Win	0.064*	0.068***	0.080**	0.077***	0.003	0.027***	0.005	0.022***
	(0.03)	(0.01)	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Method	Local OLS	NP	Local OLS	NP	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.	–	Tri.	–	Tri.
Observations	9,419	132,990	9,419	132,990	9,419	132,990	9,419	132,990

Panel B: Enforcement per Inspection

	$\frac{\text{Enforcement}}{\text{Inspections}}$		$\frac{\text{Informal Enf.}}{\text{Inspections}}$		$\frac{\text{Formal Enf.}}{\text{Inspections}}$		$\frac{\text{Penalties}}{\text{Inspections}}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Win	0.050	0.055***	0.058**	0.055***	-0.005	0.009*	-47.802	28.719
	(0.04)	(0.01)	(0.03)	(0.01)	(0.02)	(0.00)	(61.44)	(23.97)
Method	Local OLS	NP	Local OLS	NP	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.	–	Tri.	–	Tri.
Observations	9,419	132,990	9,419	132,990	9,419	132,990	9,419	132,990

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Appendix: For Online Publication

Figure A1

Emissions RD using Close Elections: Linear Fit

This figure re-plots the main results from Figure 2 using linear fits on both sides of the margin of victory threshold. The figure is otherwise identical to 2.

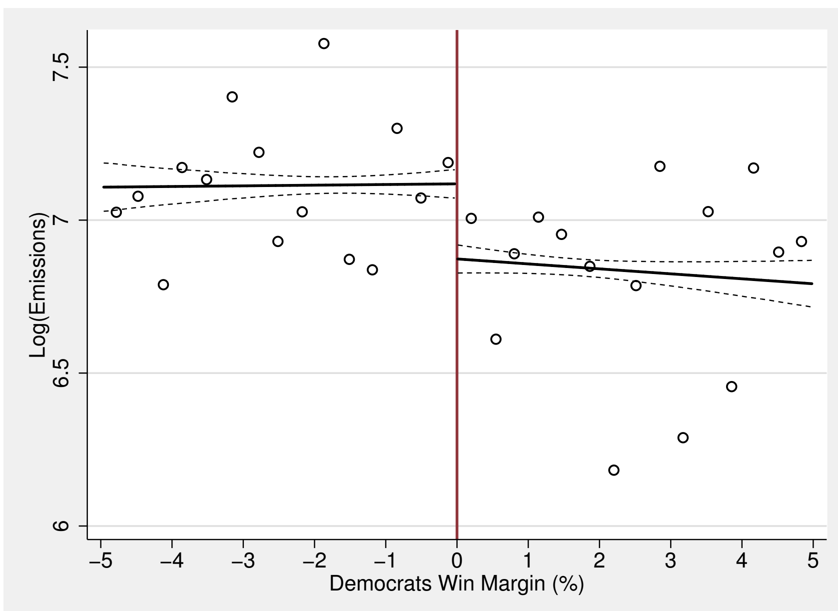

Figure A2

Covariate Balance Tests

The figures show the results of covariate balance tests in the close elections in our sample. Panels A to D respectively plot the natural logarithm of district-level GDP, the percentage unemployment rate, the natural logarithm of the number of CRA originations, and the natural logarithm of the number of HMDA originations in the two years following Congressional elections as functions of the vote share margin of a Democratic candidate in a congressional district. Democrat win margin is the percentage by which a Democrat candidate won or lost the election. The sample uses elections won or lost by a margin of 5% or less. The sample period is 1991-2016.

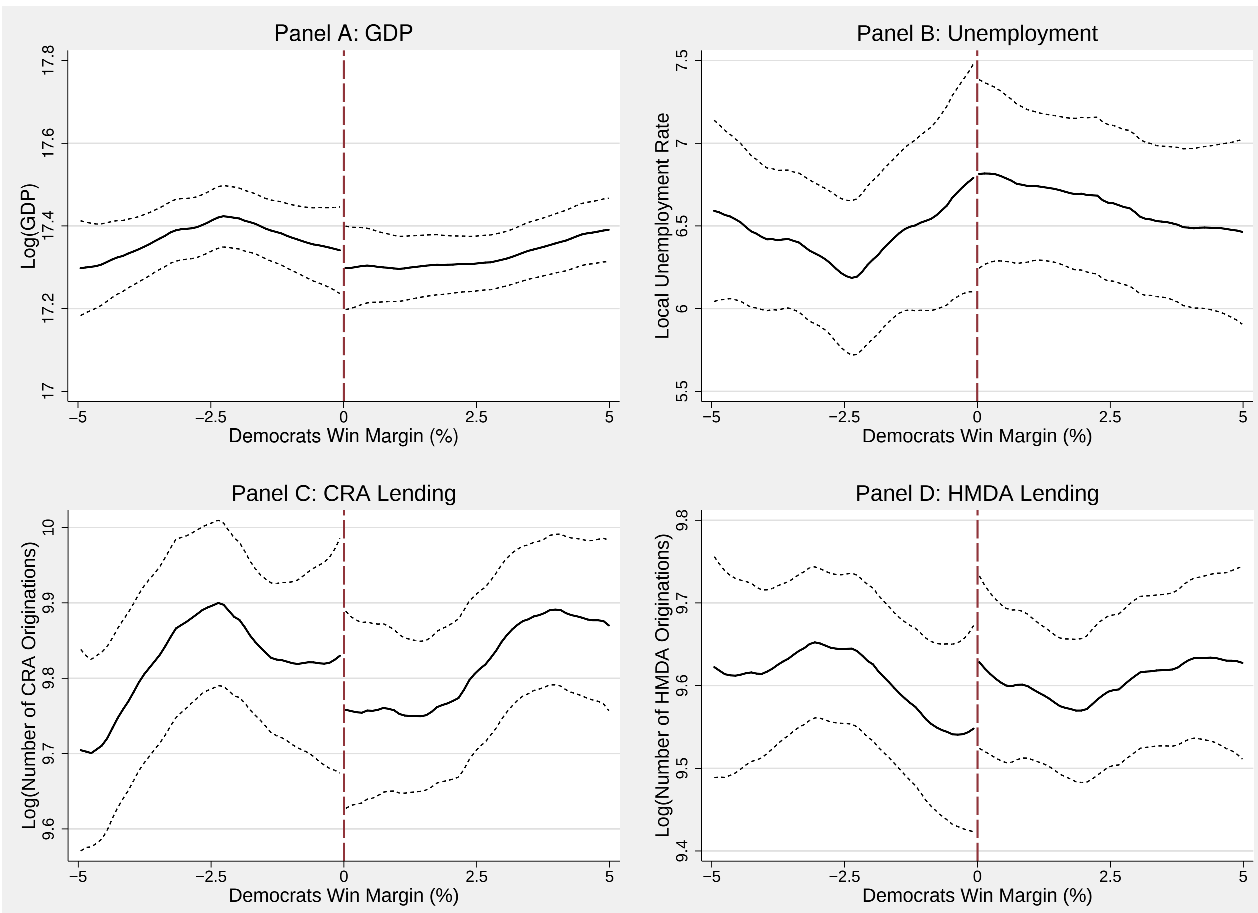

Figure A3

Opinion on the Environment

This figure reports congressional district residents' opinion on the environment in 2020 as a function of the vote share margin of the local Democratic candidate in the 2018 Congressional election. The vertical axis represents the percentage of local residents who think Congress should be doing more or much more to address environmental issues. The data is obtained from the Yale Climate Opinion Maps (YCOM) and is only available for 2020. Democrat win margin is the percentage by which a Democrat candidate won or lost the election. The sample uses elections won or lost by a margin of 5% or less.

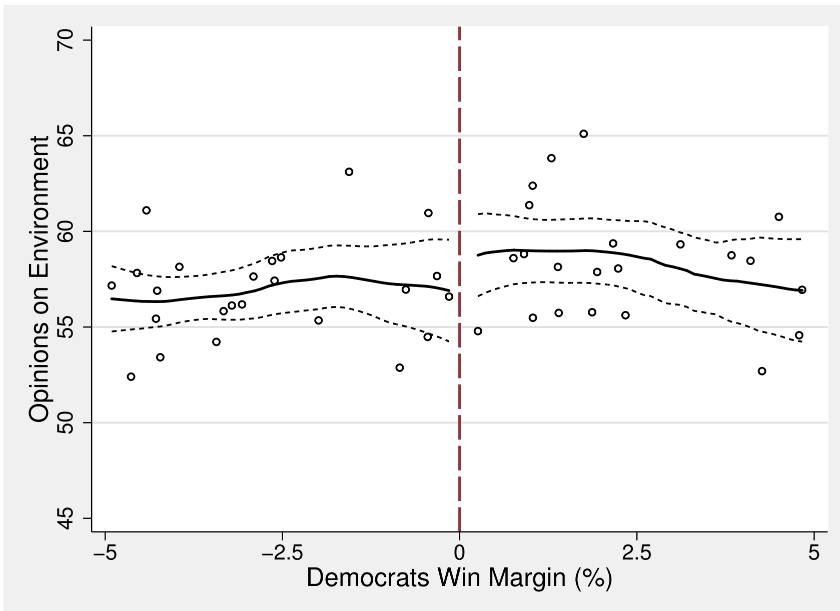

Figure A4

Close Elections Around the US

The figure plots the total number of close elections normalized by the number of districts in each US State from 1991 to 2016.

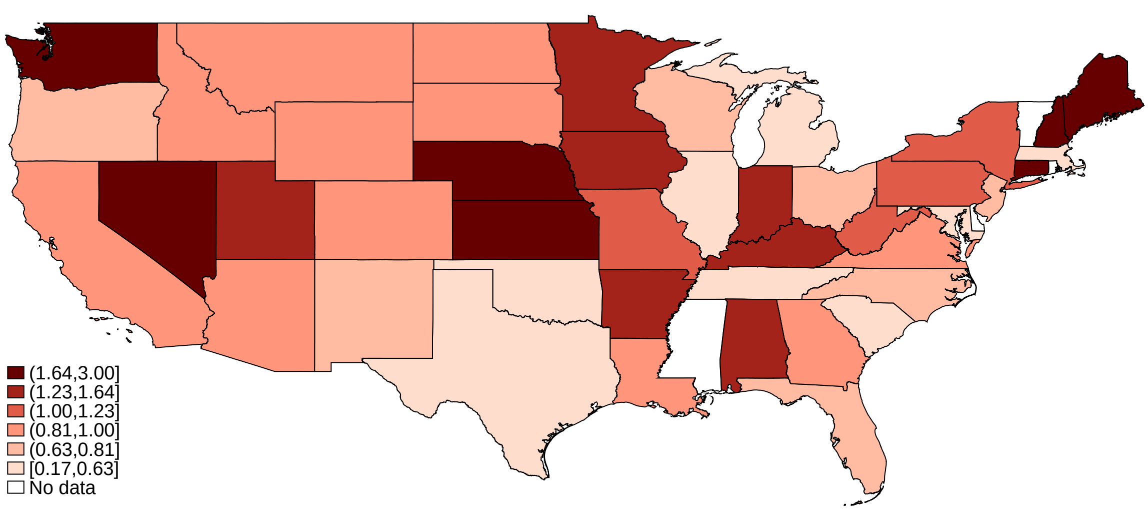

Figure A5

McCrary Test

The figures plots the results of a McCrary (2008) density test of the null hypothesis that the distribution of close election does not feature discontinuities around the zero Democrat margin vote cutoff. The sample includes the universe of US congressional elections from 1991 to 2016.

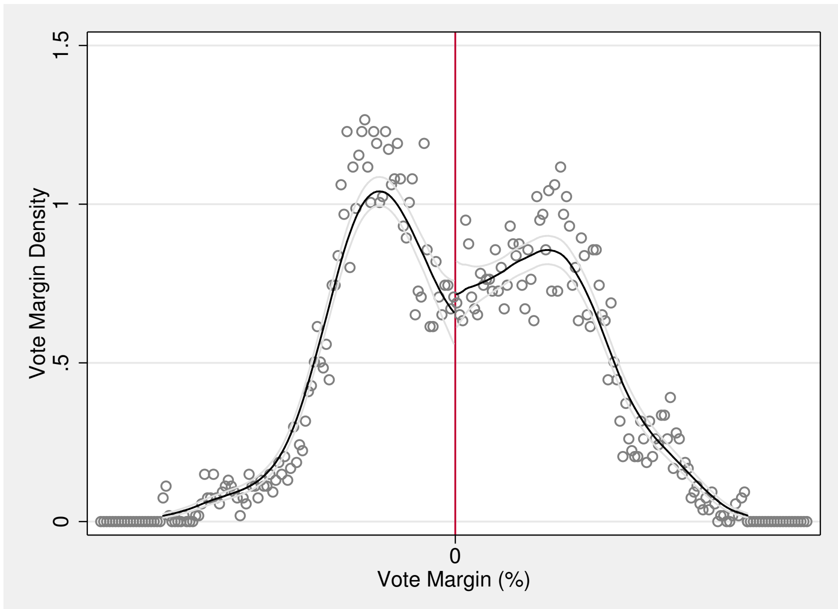

Figure A6

Placebo Tests

Both figures show placebo tests for the nonparametric specifications in Table 2, column (4). Panel A shows the distribution of coefficients from estimating 10,000 specifications where the Democrat margin of victory is uniformly randomly assigned across districts in our sample and all other data is left unchanged. Panel B shows the distribution of coefficients from estimating 10,000 specifications where the political party is uniformly randomly assigned across district politicians in our sample and all other data is left unchanged. In panels, we report our actual coefficient from Table 2, column (4), for comparison.

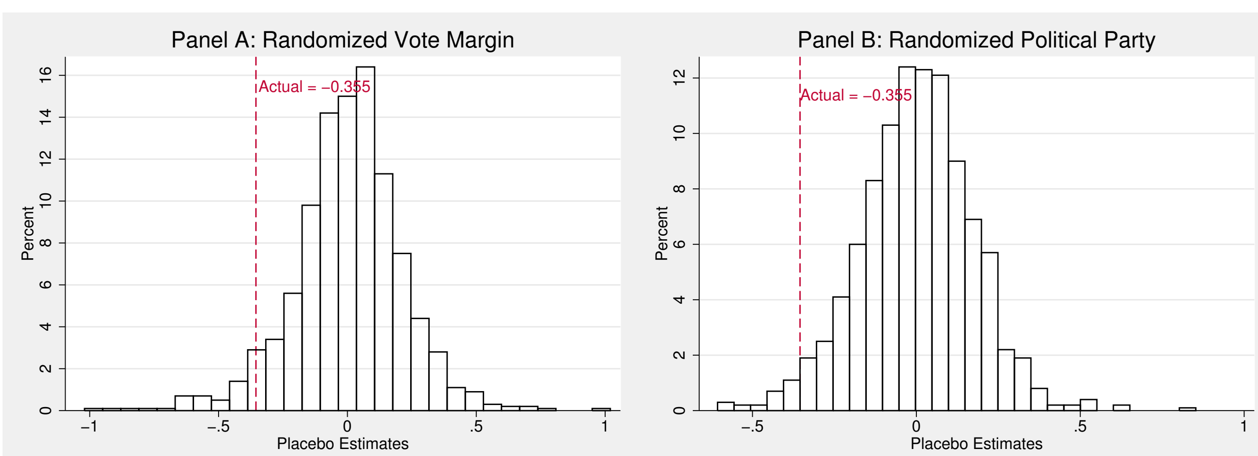

Figure A7

Robustness: Removing Individual States from the Sample

This figure shows the cross-sectional distribution of parameter estimates for the local OLS and nonparametric specifications in Table 2, columns (3) and (4), when excluding one US State at a time from the sample. In the figure, we also report the actual estimated coefficients from Table 2, column (3) and (4) for comparison.

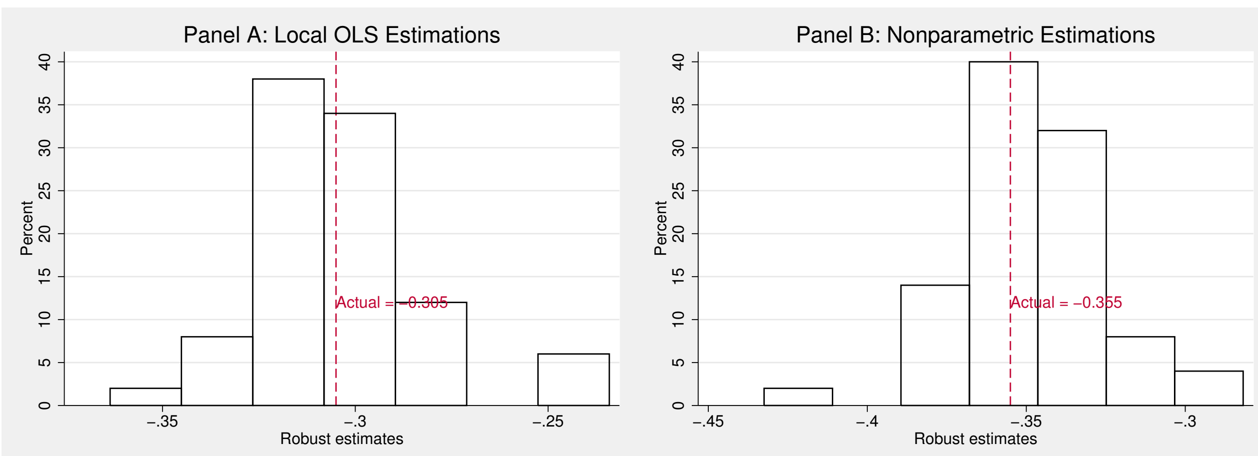

Figure A8

Reallocation of Pollution Per Unit of Production

This figure is identical to Figure 3, but instead of plotting pollution as the outcome variable, we plot pollution per unit of production. All other details are identical to Figure 3.

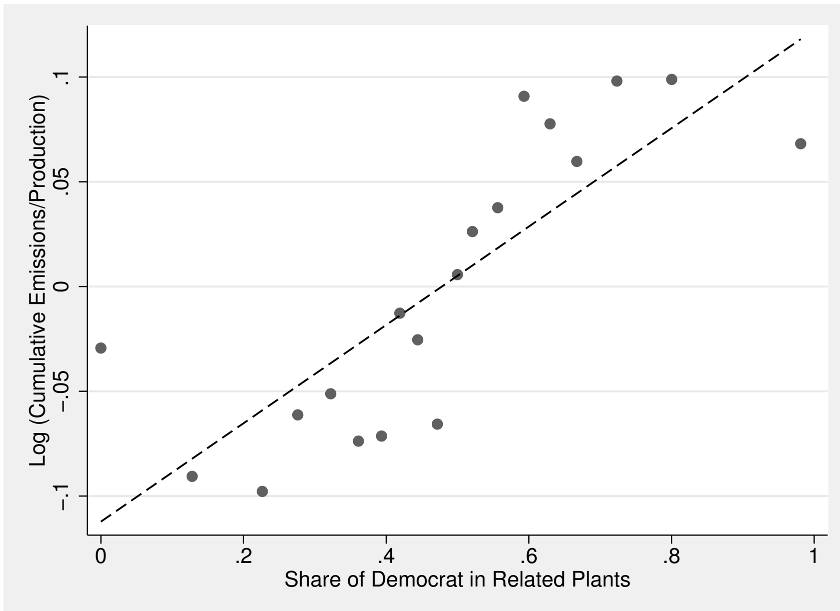

Figure A9

Political Affiliation and Reallocation

In this figure, we split the sample in Figure 3 into observations where the focal plant is represented by a Democrat (blue) versus a Republican (red). All other details are identical to Figure 3.

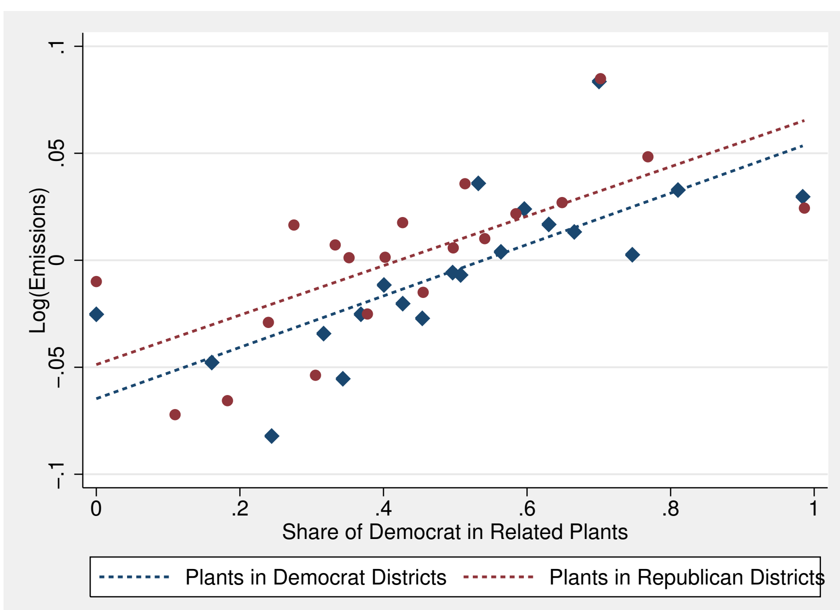

Figure A10

Reallocation of Toxic Emissions: RD Evidence

In this figure, we repeat the same exercise as in Figure 2, but we split the sample into plants that have above- and below-median values of the "Other Facilities' Democrat Share" variable. This variable captures, for each plant-chemical-year triad, the share of the plant owner's *other* plants that are represented by Democrats. For ease of interpretation, we report our results as a function of the Republican win margin. As in Figure 3, to produce this figure we only keep firms that have plants in more than one district. All the variables are defined as in Table 1, and the sample period is 1991-2016.

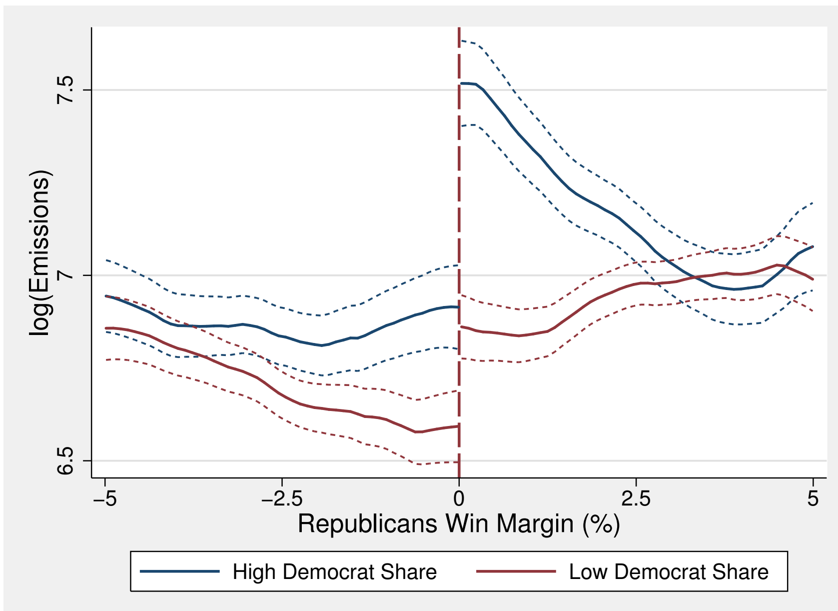

Figure A11

Reallocation Tests: Placebo on Non-Respiratory Diseases

This figure provides a placebo test on the results of Figure 4 to confirm that there is no evidence of reallocation of public health costs for non-respiratory diseases. The procedure we follow to produce this figure is identical that of Figure 4.

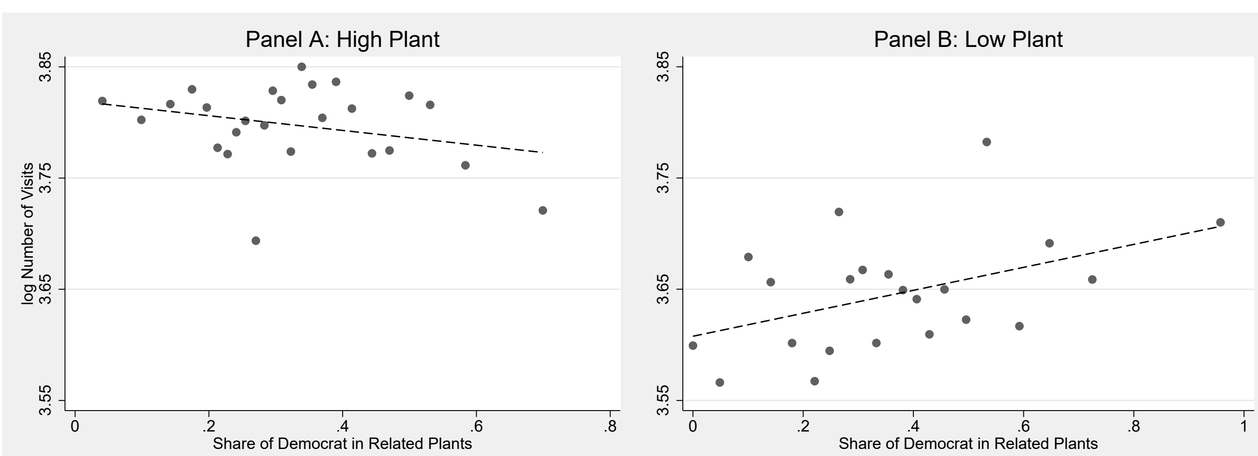

Figure A12

High Ex-ante Pollution Plants

In this figure, we split the sample from Figure 2 into plants that are *ex ante* high polluters and low polluters and plot the resulting emissions for plants in each group around close Congressional elections. Plants are sorted into high- or low-pollution groups in year t based on whether their emissions are above or below the median level of pollution at the state-chemical level in year $t-1$. All other details are identical to Figure 2.

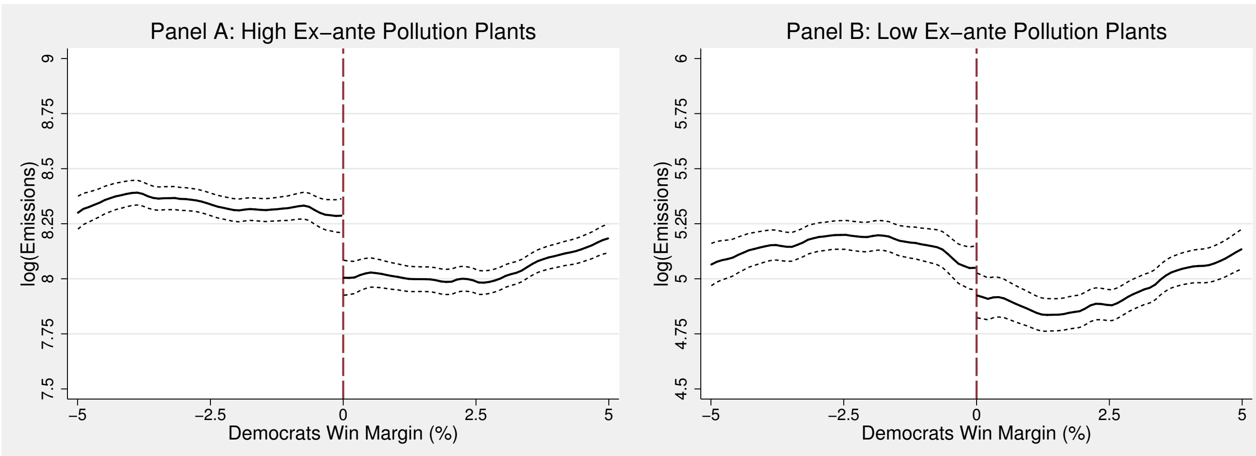

Figure A13

RD using Close Elections: Enforcement

The figure plots the natural logarithm of facility-level enforcement actions in the two years following Congressional elections as a function of the vote share margin of a Democratic candidate in a congressional district. The figure is identical to Figures 2 and 5 except for the outcome variable. All variables are defined as in Table 1, and the sample period is 1991-2016.

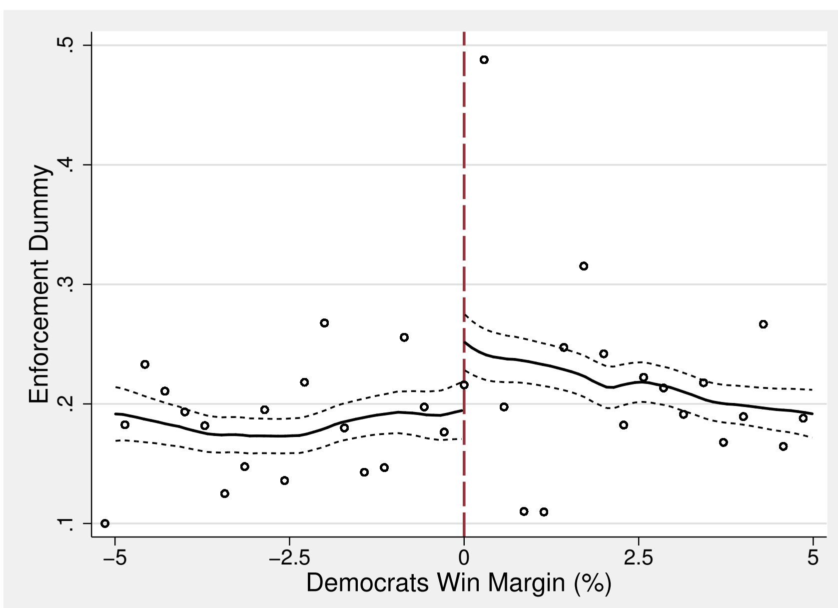

Figure A14

Politically-Unconnected Firms

This figure shows a similar pattern to Figure 2 in the sample of firms that are *not* politically connected to a local politician. The data on political connections comes from the the Federal Election Commission (FEC). We define a firm to be politically connected if it donated to the winning district politician over the last election cycle. The procedure we follow to construct this figure is identical to that of Figure 2.

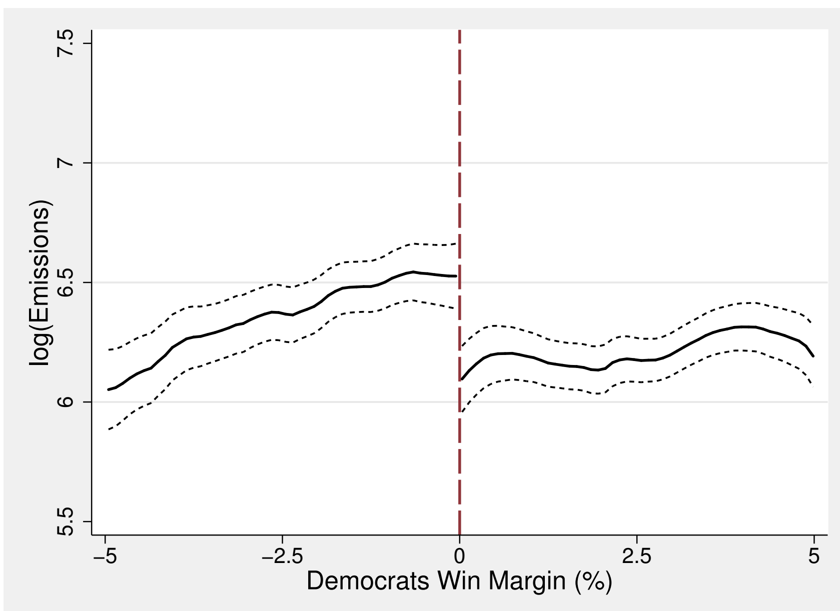

Table A1

RD Results: Residuals

This table presents the results of a robustness test to our main test in Table 2, where we orthogonalize the natural logarithm of plant-chemical-year emissions with respect to different combinations of fixed effects. The dependent variable in columns (1)–(2) is the residual of an OLS regression of the natural logarithm of plant-chemical-year emissions on state $\times$ year $\times$ chemical fixed effects. The dependent variable in columns (3)-(4) is the residual of an OLS regression of the natural logarithm of plant-chemical-year emissions on firm $\times$ year $\times$ chemical fixed effects. These specifications are otherwise identical to columns (2) and (4) in Table 2.

	Dep. Variable: log(Emissions) Residuals			
	(1)	(2)	(3)	(4)
Democrat Win	-0.145**	-0.031*	-0.034	-0.052***
	(0.07)	(0.02)	(0.07)	(0.02)
Method	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.
Chemical FE	Yes	–	Yes	–
Observations	90,555	1,281,479	57,320	811,995

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A2

RD Split on Governors' Political Parties

This table runs the same regressions as in Table 2, but splits the sample between close elections in states represented by Democrat and Republican governors. Governors' political affiliations are obtained from Congressional Quarterly (CQ) Press U.S. Political Stats.

Panel A: Democratic Governors

	Dep. Variable: log(Emissions)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democrat Win	-0.353***	-0.438*	-0.341*	-0.406***	-0.471***	-0.348***	-0.370***
	(0.13)	(0.26)	(0.19)	(0.05)	(0.04)	(0.07)	(0.08)
Method	Local OLS	Local OLS	Local OLS	NP	NP	NP	NP
Polynomial	Zero	Linear	Linear	Linear	Linear	Quadratic	Quadratic
Kernel	–	–	–	Tri.	Epa.	Tri.	Epa.
Chemical FE	No	No	Yes	–	–	–	–
Observations	45,446	45,446	45,404	551,241	551,241	551,241	551,241

Panel B: Republican Governors

	Dep. Variable: log(Emissions)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democrat Win	-0.089	-0.389**	-0.302**	-0.325***	-0.342***	-0.308***	-0.291***
	(0.11)	(0.19)	(0.14)	(0.05)	(0.05)	(0.05)	(0.05)
Method	Local OLS	Local OLS	Local OLS	NP	NP	NP	NP
Polynomial	Zero	Linear	Linear	Linear	Linear	Quadratic	Quadratic
Kernel	–	–	–	Tri.	Epa.	Tri.	Epa.
Chemical FE	No	No	Yes	–	–	–	–
Observations	48,694	48,694	48,666	778,267	778,267	778,267	778,267

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A3

Robustness: OLS Standard Error Clustering

In this table, we conduct robustness on the OLS results from Table 2 with additional clustering at the facility level and using 1% equally-spaced bins (Lee and Card, 2008). In columns (1) and (4), we regress the natural logarithm of plant-level emissions on a dummy equal to one if the district where the plant is located was won by a democrat in its most recent election, and equal to zero otherwise. In columns (2)-(3) and (5)-(6), we augment the specification with a linear interaction term between the dummy and democrat margin votes in a local OLS regression framework. The sample contains all U.S. House of Representatives elections with an absolute vote margin of less than 5% during the period 1991-2016.

	Dep. Variable: log(Emissions)					
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	-0.213***	-0.397***	-0.305***	-0.213*	-0.397*	-0.305*
	(0.06)	(0.12)	(0.11)	(0.12)	(0.23)	(0.17)
Method	Local OLS	Local OLS	Local OLS	Local OLS	Local OLS	Local OLS
Polynomial	Zero	Linear	Linear	Zero	Linear	Linear
Chemical FE	No	No	Yes	No	No	Yes
SE Clustering	Facility	Facility	Facility	L-C Bins	L-C Bins	L-C Bins
Observations	94,140	94,140	94,111	94,140	94,140	94,111

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A4

Robustness: Poisson Regressions

In this table, we conduct additional robustness on the OLS results from Table 2 using Poisson regression specifications. We regress the raw number of plant-level emissions on a dummy equal to one if the district where the plant is located was won by a democrat in its most recent election, and equal to zero otherwise. In columns (2)-(3), we augment the specification with a linear interaction term between the dummy and democrat margin votes. In column (3), we add chemical fixed effects. The sample contains all U.S. House of Representatives elections with an absolute vote margin of less than 5% during the period 1991-2016.

	Dep. Variable: Emissions (Pounds)		
	(1)	(2)	(3)
Democrat Win	-0.184***	-0.340***	-0.348***
	(0.07)	(0.13)	(0.12)
Method	Poisson	Poisson	Poisson
Polynomial	Zero	Linear	Linear
Chemical FE	No	No	Yes
Observations	118,698	118,698	118,698

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A5

Robustness: Excluding Power Plants

This table runs the same regressions as in Table 2 after excluding power plants from the sample. Power plants are defined by their NAICS code (22; Utilities industry).

	Dep. Variable: log(Emissions)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Democrat Win	-0.203**	-0.271*	-0.160	-0.308***	-0.319***	-0.348***	-0.289***
	(0.09)	(0.16)	(0.11)	(0.03)	(0.04)	(0.03)	(0.04)
Method	Local OLS	Local OLS	Local OLS	NP	NP	NP	NP
Polynomial	Zero	Linear	Linear	Linear	Linear	Quadratic	Quadratic
Kernel	–	–	–	Tri.	Epa.	Tri.	Epa.
Chemical FE	No	No	Yes	–	–	–	–
Observations	87,245	87,245	87,214	1,237,932	1,237,932	1,237,932	1,237,932

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A6

Robustness: Predicting Election Results

This table examines whether pre-election pollution growth rates can predict subsequent election results. The dependent variable is the *Democrat win* dummy variable used in previous tables. The independent variable is the district-level emissions growth rate from election cycle *t-2* to election cycle *t-1* (which we use to predict elections in cycle *t*). Columns (1) and (2) use logit and probit specifications, respectively, while columns (3) – (5) use OLS. Columns (1) to (4) restrict the sample to close elections, while column (5) includes all elections. The sample period is 1991-2016.

	Democrat Win				
	(1)	(2)	(3)	(4)	(5)
Pre-Election Emissions Growth	-0.090	-0.055	-0.022	-0.026	0.001
	(0.10)	(0.06)	(0.02)	(0.02)	(0.00)
Method	Logit	Probit	OLS	OLS	OLS
Sample	Close Elections	Close Elections	Close Elections	Close Elections	All Elections
Year FE	No	No	No	Yes	Yes
R-squared			0.002	0.011	0.007
Observations	610	610	610	610	9,318

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A7

Cumulative Plant Production

In this table, we study the effects of close Democratic Congressional victories on plant production. The dependent variable is the natural logarithm of cumulative production (the cumulative product of the production ratio at the plant-chemical level, as defined in Equation (5)). The production ratio is the ratio of the quantity of output that uses a specific chemical in any given year relative to the quantity of output that used the same chemical in the previous year. The specifications in the table mimic those in Table 3.

	log(Cumulative Production)	
	(1)	(2)
Democrat Win	0.022	0.007
	(0.02)	(0.01)
Method	Local OLS	NP
Polynomial	Linear	Linear
Kernel	–	Tri.
Observations	46,618	630,875

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A8

CMS Inpatient Data, Full Sample

This table uses OLS regressions to examine the relationship between local health outcomes and political affiliations. The dependent variable is the natural logarithm of discharges and total payments for all types of procedures. Democrat Win is an indicator that takes the value of one if a Democrat won the most recent Congressional election in the district of the plant. High Num. Plants is an indicator that takes the value of one if the number of plants in the area is above median. ZIP is the three-digit Zip code. MDC is major diagnostic category code that divides all possible principal diagnoses into 25 mutually exclusive diagnosis areas. Standard errors are clustered at district-year level, and the sample period is 2011-2016.

	log(Number of Discharges)			log(Total Payments)		
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	0.021	-0.007		0.113***	-0.000	
	(0.02)	(0.02)		(0.02)	(0.02)	
High Num. Plants	0.215***	0.183***	0.121***	0.240***	0.202***	0.127***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Democrat Win × High Num. Plants	-0.039*	-0.036*	-0.043*	-0.084***	-0.044*	-0.041*
	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
ZIP FE	Yes	Yes	No	Yes	Yes	No
District-Decade FE	No	Yes	No	No	Yes	No
Year FE	Yes	Yes	No	Yes	Yes	No
District × Year FE	No	No	Yes	No	No	Yes
ZIP × District-Decade FE	No	No	Yes	No	No	Yes
MDC FE	Yes	Yes	Yes	Yes	Yes	Yes
R-Squared	0.174	0.208	0.226	0.267	0.306	0.323
Observations	369,610	369,609	369,606	369,610	369,609	369,606

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A9

Firm-level Health Effects

This table uses OLS regressions to examine the firm-level, aggregate health effects of reduced pollution following Congressional election victories by Democrats. The dependent variable is the natural logarithm of total discharges and average discharges at the firm level. These quantities are computed by aggregating discharges in all three-digit Zip codes where the firm's plants are located. Democrat Win is an indicator that takes the value of one if a Democrat won the most recent Congressional election in the district of the plant. High Num. Plants is an indicator that takes the value of one if the number of plants in the area is above median. ZIP is the three-digit Zip code. MDC is major diagnostic category code that divides all possible principal diagnoses into 25 mutually exclusive diagnosis areas. Standard errors are clustered at district-year level, and the sample period is 2011-2016.

	log(Total Discharges)				log(Average Discharges)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Share	-0.019		-0.085		-0.044		-0.092*	
	(0.06)		(0.07)		(0.05)		(0.05)	
Emissions-Weighted Democrat Share		-0.050		-0.107**		-0.046		-0.082**
		(0.05)		(0.05)		(0.04)		(0.04)
Chemical $\times$ Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm $\times$ Chemical FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Full Sample	Yes	Yes	No	No	Yes	Yes	No	No
Multi-plant Firms	No	No	Yes	Yes	No	No	Yes	Yes
R-Squared	0.897	0.898	0.897	0.897	0.866	0.866	0.861	0.861
Observations	41,791	41,791	36,476	36,476	41,791	41,791	36,476	36,476

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A10

Inspections and Enforcement by State and Federal Regulators

In this table, we study the effect of marginal district wins by Democratic Party candidates on inspections and enforcements led by state and federal regulators. The dependent variable in columns (1)-(2) is the natural logarithm of total plant inspections in a year. The dependent variable in columns (3)-(4) is an indicator variable that takes the value of one if a plant gets an EPA inspection in that year, and zero otherwise. The dependent variable in columns (5)-(6) is an indicator variable that takes the value of one if a plant receives an EPA enforcement, and zero otherwise. The dependent variable in columns (7)-(8) is the number of enforcements per inspection. Columns (1), (3), (5), and (7) of both panels report results for our favorite linear OLS specifications. Columns (2), (4), (6), and (8) report results for non-parametric local polynomial RD estimators with linear polynomials and triangular kernels. We report standard errors clustered at the district-year level for local linear OLS regressions, and robust bias-corrected standard errors as in Calonico et al. (2014) for non-parametric regressions. The sample contains all U.S. House of Representatives elections during the period 1991-2016. In columns (1), (3), (5) and (7), we restrict the sample to elections with an absolute vote margin of less than 5% during the same period.

Panel A: State Regulators

	log(Inspections)		Insp. Dummy		Enf. Dummy		$\frac{\text{Enforcement}}{\text{Inspections}}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Win	0.071*	0.058***	0.030	0.021***	0.084**	0.077***	0.063**	0.058***
	(0.04)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)	(0.03)	(0.01)
Method	Local OLS	NP	Local OLS	NP	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.	–	Tri.	–	Tri.
Observations	30,773	414,343	30,773	414,343	9,419	132,990	9,419	132,990

Panel B: Federal Regulators

	log(Inspections)		Insp. Dummy		Enf. Dummy		$\frac{\text{Enforcement}}{\text{Inspections}}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Democrat Win	0.008	0.007***	0.012	0.010***	-0.014	-0.007	-0.010	-0.005
	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.01)	(0.01)	(0.00)
Method	Local OLS	NP	Local OLS	NP	Local OLS	NP	Local OLS	NP
Polynomial	Linear	Linear	Linear	Linear	Linear	Linear	Linear	Linear
Kernel	–	Tri.	–	Tri.	–	Tri.	–	Tri.
Observations	30,773	414,343	30,773	414,343	9,419	132,990	9,419	132,990

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A11

Emissions and Seat Pickups

This table splits our full sample into four groups around election periods. In group 1, we focus on cases where a Democrat held the seat before and after the election. In group 2, we focus on cases where a Democrat held the seat before the election but a Republican held the seat after the election. In group 3, we focus on cases where a Republican held the seat before and after the election. In group 4, we focus on cases where a Republican held the seat before the election and a Democrat held the seat after the election. In the first two columns of the table, we keep the sub-sample of groups 1 and 2, and we define "Switchers" as an indicator for plant-chemical observations in group 2. In column (1), we interact the "Switchers" indicator with a post-election indicator, and in column (2) we interact the "Switchers" indicator with individual year-level indicators, keeping the election year as a baseline. In the last two columns of the table, we keep the sub-sample of groups 3 and 4, and we define "Switchers" as an indicator for plant-chemical observations in group 4. In column (3), we interact the "Switchers" indicator with a post-election indicator, and in column (4) we interact the "Switchers" indicator with individual year-level indicators, keeping the election year as a baseline. The dependent variable is log(emissions), as in Table 2. Standard errors are clustered at the district-year level, and the sample period is 1991-2016.

	log(Emissions): R-D Switchers		log(Emissions): D-R Switchers	
	(1)	(2)	(3)	(4)
Switchers × Post Election	-0.059***		0.029***	
	(0.01)		(0.01)	
Switchers × Election Year -1		0.008		-0.005
		(0.01)		(0.01)
Switchers × Election Year +1		-0.061***		0.023*
		(0.01)		(0.01)
Switchers × Election Year +2		-0.049***		0.030**
		(0.01)		(0.01)
Low-Order Terms	Yes	Yes	Yes	Yes
District × Election Year FE	Yes	Yes	Yes	Yes
Facility × Chemical FE	Yes	Yes	Yes	Yes
R-squared	0.892	0.892	0.890	0.890
Observations	1,516,595	1,516,595	1,407,224	1,407,224

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A12

Emissions and Reelections

This table examines whether district-level pollution growth rates affects politicians' likelihood of being re-elected. We report results from regressing lagged district-level emissions growth rates on the *Democrat win* dummy variable used in previous tables. Columns (1) and (2) use logit and probit models, respectively, while columns (3) – (6) use OLS regressions with various fixed effects. The sample period is 1991-2016.

	Reelected					
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat Win	-0.022	-0.012	-0.004	-0.002	-0.148***	-0.155***
	(0.05)	(0.03)	(0.01)	(0.01)	(0.02)	(0.03)
Emissions Growth Rate	0.034	0.019	0.005	-0.000	0.000	-0.006
	(0.04)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Democrat Win × Emissions Growth Rate	0.007	0.005	0.001	0.001	0.001	0.001
	(0.05)	(0.03)	(0.01)	(0.01)	(0.01)	(0.01)
Method	Logit	Probit	OLS	OLS	OLS	OLS
Year FE	No	No	No	Yes	No	Yes
District-Decade FE	No	No	No	No	Yes	Yes
R-squared			0.000	0.053	0.251	0.301
Observations	10,229	10,229	10,229	10,229	10,229	10,229

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.

Table A13

Affiliation, Power, and Ideology

This table uses OLS panel regressions to examine the relationship between plant toxic emissions and politicians' party affiliations, their power, and their ideology. The dependent variable is the natural logarithm of emissions at the plant-chemical-year level. Democrat Win is an indicator equal to one if a candidate from the Democratic party won the last election in the district where the plant is located, and equal to zero otherwise. Committee Chair is an indicator equal to one if a candidate is the chair of a committee in Congress, and equal to zero otherwise. Ideological is an indicator equal to one if a candidate has an ideology score lower than the 25th percentile of the ideology distribution within the Democratic Party or higher than the 75th percentile of the distribution within the Republican Party, and equal to zero otherwise. Ideology scores are from VoteView. Standard errors are clustered at the district-year level. The sample period is 1991-2016.

	Dep. Variable: log(Emissions)				
	(1)	(2)	(3)	(4)	(5)
Democrat Win	-0.047**	-0.035**	-0.026**	-0.020*	-0.020*
	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Democrat × Chair	0.047	-0.002	0.039	0.017	0.016
	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Ideological × Democrat × Chair	0.032	-0.006	-0.143**	-0.168**	-0.222***
	(0.09)	(0.11)	(0.07)	(0.07)	(0.07)
Lower Order Terms	Yes	Yes	Yes	Yes	Yes
District-Decade FE	Yes	No	No	No	No
Year FE	Yes	No	No	No	No
Firm × Chemical × Year FE	No	Yes	Yes	Yes	Yes
District-Decade × Chemical FE	No	Yes	No	No	No
Facility × Chemical FE	No	No	Yes	Yes	Yes
State × Year FE	No	No	No	Yes	No
State × Year × Chemical FE	No	No	No	No	Yes
R-Squared	0.077	0.850	0.929	0.930	0.938
Observations	1,300,744	770,151	761,731	761,731	718,698

Note: Standard errors in parentheses. ***, **, and * respectively denote statistical significance at the 1%, 5%, and 10% levels.