# The Selective Enforcement of Government Regulation: Battleground States and the EPA*


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The Electoral College creates incentives for politicians and regulators to direct policy favors to those states that represent the median voter (so-called "battleground" or "swing" states). We examine whether regulators treat battleground states favorably by examining whether the EPA is less likely to find a regulated facility in violation of the Clean Water Act (an economically important statute) if it is in a battleground state. We find that violation rates for these facilities are significantly lower than those in non-battleground states. Selective enforcement is widespread and spans nearly thirty years. Identification is obtained by the analysis of the violation rates of similar facilities located along the border between battleground and non-battleground states. This selective enforcement appears to primarily result from lax EPA oversight of state-level regulators in battleground states.



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## 1. Introduction

The vigorous and uniform enforcement of economically important regulation can be politically costly to politicians and other political actors, including the regulators themselves. This creates incentives that can lead to selective enforcement. While the impact of government regulation has been widely studied in economics, including whether it benefits or harms social welfare, an equally important question is whether or not regulation are is uniformly enforced.[1]

This study examines a setting in which both politicians and regulators have clear incentives to selectively enforce regulation at the state level. Specifically, we identify an exogenous and time-varying source of state-level political importance: whether or not a state is a "battleground" or "swing" state, the median voter in presidential elections. We also identify a set of economically important regulations that apply uniformly across all 50 US states: namely those related to the Clean Water Act of 1972 (CWA). We are then able to examine whether the CWA is as vigorously enforced in battleground states as non-battleground states.

Anecdotal evidence suggests that federal political favors are directed towards battleground states, including the selective enforcement of regulation to ease the economic or political costs associated with regulatory compliance. Examples of this include a 2018 regulatory decision by Interior Secretary Ryan Zinke to rescind a ban on offshore oil drilling that was imposed by the prior presidential administration. This decision was opposed by several states, including New Jersey, Delaware, Maryland, Virginia, North Carolina, South Carolina, California, Oregon, Washington, and Florida. However, only one state was granted an exception to this decision: the state of Florida, a critically important battleground state.[2]

[1] This literature includes Bertrand and Kramarz (2002), who find that product and entry regulation increases retailer concentration and slows employment growth, Botero, Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2004), who find that higher levels of labor regulation result in lower labor force participation and higher unemployment, and Atkeson, Hellwig, and Ordoñez (2015), who present a model which produces a level of entry fees that optimizes public welfare. Regarding the impact of regulation on environment, Grossman and Krueger (1995) find that economic growth is not correlated with environmental deterioration for developed economies (which, we add, typically feature strict environmental regulation). Shleifer (2005) notes that while American and European societies today are wealthier relative to previous periods, whether a corresponding increase in regulation has, on aggregate, been a help or a hindrance is not obvious.

[2] *New York Times*, "Trump Administration Drops Florida From Offshore Drilling Plan," January 9, 2018, *New York Times*, "Florida Is Exempted From Coastal Drilling. Other States Ask, 'Why Not Us?'," January 10, 2018, *Washington Post*, "Excluding Florida from a new offshore drilling plan sure looks partisan" January 10, 2018.

This study attempts to show that the favorable regulatory treatment of battleground states is systemic. Consistent with this, we find that facilities located in battleground states are significantly less likely to be found in violation of the CWA compared to facilities located in non-battleground states. This result is robust to a number of empirical tests and spans a sample period of nearly thirty years.

The political incentives that can result in some states being treated differently than others are related to the way that presidential elections are structured. In the United States, the outcome of presidential elections is determined by the Electoral College system, in which the candidate who receives a plurality of votes within a state typically receives all of that state's votes in the Electoral College. States that do not reliably support a particular party are frequently referred to as "battleground" or "swing" states, and these states often determine the outcome of presidential elections. The winner-take-all nature of the Electoral College system provides a clear set of incentives for presidents and presidential candidates to direct both policy favors and campaign resources toward battleground states. Further, because of the importance of the office of president, these incentives also affect the behavior of allied politicians and political parties, as well as other interest groups (including regulators). We argue that battleground state status represents a time-varying, exogenous shock to the political importance of voters in a given state which affects the willingness of politicians and regulators to vigorously enforce economically important regulation.

To determine whether or not regulation is selectively enforced in battleground states, we examine the violation rates of the CWA, an economically important statute.[3] The Environmental Protection Agency (EPA) is the primary regulator tasked with enforcing the CWA, and, among other things, it requires facilities that discharge water directly into surface waters of the United States to obtain a permit to do so. Data on these permits, and any related permit violations, are available from the EPA. Critical to our study, permit data

[3] Compliance with the CWA is costly. According to data from the Enforcement and Compliance History Online (ECHO) system as provided by the EPA, federal compliance costs associated with direct enforcement actions relating to the CWA were $12.4 billion for 2014, averaging $18.3 million per case. However, these numbers only include costs that result from federal enforcement cases, and does not include penalties and cost recovery amounts required by state-level regulators (who are the primary enforcers of the CWA). Neither does it include compliance costs for facilities that did not violate the CWA, or, if they did, were not subjected to enforcement actions. Thus, the compliance costs associated with direct federal enforcement actions represent only a fraction of overall compliance costs and can be viewed as a lower bound. A number of studies have attempted to estimate the total compliance costs associated with the CWA, though precise estimates of these costs are contentious. For example, Johnson (2001) finds that the compliance costs associated with the CWA were $93.1 billion in 2001.

includes the latitude and longitude of each regulated facility. This locational data, along with the changing battleground state status for states in our sample, allows us to obtain identification by examining the violation rates of similar facilities located along the border between battleground and non-battleground states. This border facility dataset includes 11,398 unique facilities with a total of 123,514 facility-year observations between 1976 and 2014. We find that violation rates are sharply discontinuous at the border: unconditionally, border facilities located in battleground and non-battleground states experience violation rates of 10.9% and 22.8% per year, respectively. The difference of 11.9 percentage points represents a 52.2% decrease in violation rates and is both economically and statistically significant.

We argue that this difference arises from a decrease in regulator enforcement intensity caused by the political importance of voters in battleground states. Violation rates are, of course, also affected by the compliance intensity of facility operators. Critical to our identification strategy, we argue that it is unlikely that an omitted variable causes a both an improvement to the ability of facility operators to comply with the CWA and a state being a battleground state in the Electoral College. While state-level environmental attitudes and economic conditions may plausibly affect both the compliance intensity of facility operators as well as the preference of voters for a given presidential candidate or political party, battleground states represent the *median* voter in the Electoral College; that is, voters in battleground states are relatively indifferent between candidates or parties. It is not immediately obvious as to what kind of factor would simultaneously result in both the voters of a state being "on the fence" with regards to presidential candidates and facility operators being more proficient at complying with the CWA, relative to their counterparts in Republican- and Democrat-leaning states. We argue that this is particularly true for our border sample where the close geographic proximity between facilities in battleground and non-battleground means that local voter demographics and economic trends are likely to be similar. Thus, we argue that the difference in violation rates between facilities results from a difference in the enforcement intensity of environmental regulators, and not the compliance intensity of facility operators.

Using a difference-in-difference approach, we also examine the violation rates of facilities located in states that change battleground status. Specifically, we examine the change in violation rates of non-battleground states that become battleground states after a presidential election. We find that prior to the election, non-battleground states experienced higher violation rates relative to neighboring battleground states. However, after the election, violation rates for these "new" battleground states drop, converging to the levels experienced by neighboring battleground states. We similarly examine "new" non-battleground states and find that after an election, violation rates for these states converge toward those of neighboring non-battleground states.

Next, we examine the correlation between violation rates and battleground state status using multivariate panel regressions. When facility fixed effects are included, the coefficient estimates on battleground state dummy variables represent difference-in-difference estimators and measure within-facility changes in violation rates that result from changes to the battleground status of the state in which the facility is located. Within our border sample, we find that battleground state status is associated with a 5.9 percentage point reduction in violation rates.

In order to classify battleground states, we use a method similar to that of Gerber, Huber, Dowling, Doherty, and Schwartzberg (2009). However, prior research has used a number of different methods of classifying battleground states. While there is substantial overlap between our set of battleground states and the battleground state classifications made by previous work, given the importance of this variable in the analysis, it is essential for the results to be robust to alternative definitions. We find that results are similar if we use the battleground states as determined by Shaw (1999, 2006) (who relies on interviews with campaign operators), Strömberg (2008) (who develops an empirical model), and Real Clear Politics (which relies on polls conducted immediately prior to an election). Namely, facilities located in battleground states are less likely to be found in violation of the CWA by regulators. That our results are robust to alternative specifications of battleground states helps address two potential problems: first, the possibility that the result is driven by chance due to the specific definition that we used. Second, the possibility that the variable used is somehow capturing something other than the electoral importance of the state.

We also conduct a falsification test to estimate the probability that our method for assigning battleground status would yield the results described above. Specifically, we run simulations in which we randomly assign battleground status to various states for each election year and repeat portions of our analysis. We find that it is extremely unlikely that our findings arise by chance associated with the method used to classify battleground states. We also examine the possibility that facility operators self-select their location along the border based upon a state's battleground state status. While violation rates are sharply discontinuous at the border between battleground and non-battleground states, the estimated population density is not. This result is inconsistent with facility operators changing their location based on battleground state status.

We also examine whether this battleground state effect varies for states that are long-term battleground states, and find that the battleground state effect is larger for these states. In addition, we find that the battleground state effect varies little depending upon the party of the sitting president and that the effect is constant over a four-year presidential election cycle (e.g., the effect is not concentrated in the election year).

Finally, we look at whether the EPA or state regulators drive this effect. While the EPA is ultimately responsible for the nationwide administration of the NPDES program, it allows states to request the authority to administer the NPDES program within their borders (albeit with EPA oversight). The EPA has delegated much of the day-to-day administration of the program to state-level environmental regulators in those states that have done so, with the EPA administering the program directly in states that have not. We find that the battleground state effect is almost entirely driven by states to which the EPA has delegated NPDES permitting and enforcement authority. When the NPDES program is administered directly by the EPA, we do not see a significant difference in violation rates between battleground and non-battleground states (though this result is based on a relatively small sample of states to which the EPA has not delegated parts of the NPDES program). Thus, it appears that, while the EPA does not treat battleground states differently when administering the NPDES program directly, the EPA is more lax in its oversight of the NPDES program when it is run by state-level regulators, and that this is particularly true for battleground states.

This study finds that the Electoral College system creates incentives that result in widespread selective enforcement of government regulation. We suggest at least three non-mutually exclusive supply-side channels by which this may occur: politicians asserting control over regulatory agencies, risk-averse regulators seeking to avoid conflict with politicians, and attempts by regulators to affect electoral outcomes (mechanisms consistent with the theoretical work of Peltzman (1976)). We argue that battleground state status represents an exogenous shock to the political importance of regulated facilities, and that our results support a causal relationship between battleground state status and favorable regulatory treatment.

We add to prior research that has found that regulation is not uniformly enforced by a large number of government agencies, including the Federal Trade Commission (Faith, Leavens, and Tollison (1982) and Weingast and Moran (1983)), the Internal Revenue Service (Hunter and Nelson (1995) and Young, Reksulak, and Shughart (2001)), the EPA (Mixon (1995)), the Nuclear Regulatory Commission (Gordon and Hafer (2005)), and the Securities and Exchange Commission (Correia (2014) and Heese (2015)). The mechanisms whereby this occurs include both demand-side channels (e.g., regulatory capture by industry; see Stigler (1971)) as well as supply-side channels (e.g., agency control by politicians; see Peltzman (1976)).

Politicians have incentive to selectively enforce regulation to improve the economic well-being of the median voter, thereby improving their chances of re-election. A large literature finds that the economic concerns of voters are a significant predictor of whether or not they choose to re-elect incumbent politicians (see Lewis-Beck and Stegmaier (2000) and Nadeau, Lewis-Beck, and Bélanger (2013) for surveys). Much of this literature examines fiscal policy as a means to deliver policy favors (see, for example, Hibbing (1984), Hoover and Pecorino (2005), and Larcinese, Rizzo, and Testa (2006); for a literature review, see Besley and Case (2003)). Prior work has also examined the mechanisms by which selective legal enforcement can increase political support (see Holland (2015) and Holland (2016)). To this literature we add another channel whereby policy favors can be delivered: the regulatory process. We argue that selectively enforcing regulation is an attractive mechanism whereby politicians can direct policy favors to key constituents since it is less observable and therefore less politically costly than many of the alternatives.

Prior literature also examines how politicians can impact the behavior of regulators. Our study contributes to the literature on the ways that regulatory enforcement is subject to political influence (including the theoretical work of Stigler (1971), Peltzman (1976), Snyder (1990), Grossman and Helpman (1994), and Shleifer and Vishny (1994)) and the control of government bureaucracy (see McCubbins, Noll, and Weingast (1987, 1989)).

Whether the legislative or executive branch exerts more influence over agency decisions is an open research question (see, for example, Percival (1991), Weingast and Moran (1983), and Wood and Waterman (1991)). To this literature, we add the observation that regulatory enforcement by the EPA is sensitive to the electoral priorities of the executive branch, which is consistent with the president exercising meaningful political control over government agencies.

Further, we contribute to the literature on the ways that political incentives affect enforcement behavior due to the preferences of the regulators themselves. For example, Agarwal, Lucca, Seru, and Trebbi (2014) find that state banking regulators are more lax than federal regulators, which is consistent with state regulators being more responsive to local economic concerns.

Finally, a large literature examines whether regulation is helpful or harmful, and what determines regulatory changes. Among other things, this literature finds that regulation can affect industry concentration as well as economic growth and employment (see Jayaratne and Strahan (1996), Bertrand and Kramarz (2002), Botero, Djankov, La Porta, Lopez-de-Silanes, and Shleifer (2004), and Atkeson, Hellwig, and Ordoñez (2015)) and that regulation is more likely to be relaxed when potential regulatory winners are stronger than potential losers (Kroszner and Strahan (2014)). This study does not contribute to those that examine the efficacy of government regulation. However, we observe that whether or not regulation is present is only part of the question; it is also important to consider whether regulation is uniformly enforced.

## 2. The Importance of Battleground States

Battleground states are important to both politicians and regulators. Politicians have incentives to treat battleground states favorably as it improves their electoral prospects or legislative priorities. Regulators also have incentives to treat battleground states differently, either to avoid conflict with politicians or, if a regulatory agency has political preferences, to improve the electoral prospects of its favored candidate or party.

In the United States, the outcomes of presidential elections are determined by the Electoral College system and not by popular vote. Under the Electoral College system, voters in each state select "electors" who have pledged to vote for a particular presidential candidate, and these electors ultimately vote to elect the president and vice president. The Electoral College allocates a number of electors equal to each state's number of congressional representatives (which is based on state population), with the District of Columbia receiving votes equal to the least populous state. There are currently 538 electors in the Electoral College corresponding to 435 members of the U.S. House of Representatives, 100 members of the U.S. Senate, and three electors allocated to the District of Columbia. In order for a presidential candidate to be elected to office, he or she must receive at least 270 votes in the Electoral College system. With the exception of Maine and Nebraska, the system is winner-take-all at the state level in that the presidential candidate receiving the majority of the popular vote receives all of that state's electors. While electors are not required by law to vote for the candidate to whom they are pledged, the electors themselves are selected by political parties and instances of an elector voting in a way inconsistent with his or her pledge have been rare.

Because of the winner-take-all nature of the Electoral College, states that do not reliably support either political party are usually the median voters in presidential elections and often determine their outcomes. These electorally competitive states are frequently referred to as "battleground" or "swing" states. The political incentives to appeal to these states can affect the behavior of both politicians and regulators.

### 2.1. Politicians and Battleground States

Why do politicians care about battleground states? Politicians wish to get elected and then re-elected, and this requires that they win a majority (or in some cases, a plurality) of votes. This usually requires that politicians appeal to the median voter, whose political alignment is less likely to be fixed. In the Electoral College, the median voter is a battleground state. Consistent with the political incentive to focus on median voters, prior research has found that presidential candidates direct more campaign resources toward battleground states.[4] Moreover, because of the political importance of the presidential office, the political

[4] For example, Strömberg (2008) argues that the Electoral College creates incentives for candidates to target campaign resources to battleground states. Hill and McKee (2005) find that battleground states receive significantly greater advertising expenditures from presidential campaigns and more frequent candidate visits. Doherty (2007) finds presidential travel targets battleground states and states with a large number of electoral votes, particularly during election years.

incentive to appeal to battleground states affects not only the presidential candidates themselves, but also their network of allied politicians, political parties, and interest groups.

These incentives influence not only political campaigns, but also governance. In general, incumbent politicians can improve their or their party's chances of re-election by directing policy favors to median voters. A large literature examines the kinds of policy favors that politicians direct toward key constituents. These include the targeted purchase of goods and services, transfer payments, and preferential tax treatment (see, for example, Lizzeri and Persico (2001)). Because the median voter in presidential elections is a state, we expect policy favors to be directed toward battleground states.

Appealing to the median voter also can be politically costly if it alienates other voters (see Shleifer and Vishny (1994)). We argue that this is particularly true when the median voter is a state and not a demographic (e.g., increasing social security benefits to seniors is less costly than increasing social security benefits to seniors in Nebraska). This limits the ability of politicians to explicitly direct policy favors toward battleground states through normal channels, which are often publicly observable (e.g., legislation). Regulation often creates winners and losers, and to the extent that regulatory enforcement imposes economic costs, the selective reduction of regulatory burdens can present a tangible economic benefit to states. We argue that the selective enforcement of government regulation represents an appealing mechanism through which politicians can deliver policy favors toward battleground states in order to maximize their political support while minimizing political costs (see Peltzman (1976)).

While regulatory agencies are intended to function independent of political concerns, they are still subject to political control. In particular, the president typically appoints (and can remove) senior agency leadership. In addition, the president has the power to issue executive orders that can help or harm a regulatory agency, and can affect agency funding through coordination with, and influence over, the legislative branch. Thus, incumbent presidents (and other federal politicians) not only have an incentive to reduce the enforcement of costly regulations in battleground states, but the means to affect the behavior of regulators.

### 2.2. Regulators and Battleground States

Regulatory agencies exercise tremendous flexibility in interpreting and enforcing existing laws, and previous work has shown that the incentives affecting different categories of regulators impact their enforcement activity. For example, Agarwal, Lucca, Seru, and Trebbi (2014) find that state regulators are more lenient in enforcement of federal banking laws than their federal counterparts (the authors attribute this to, among other things, state regulators being more responsive to local economic concerns). We argue that regulators have incentives to treat battleground states differently from non-battleground states, even absent explicit direction from politicians, for at least two reasons.

First, understanding the importance of battleground states to federal politicians, a risk-averse regulator may choose to more lightly enforce regulations in these states to avoid a potential conflict with these same politicians. We note that, even if a given set of federal politicians wishes to govern in an impartial manner, as long as a risk-averse regulators do not perceive this with certainty, they may still choose to more lightly enforce regulations in battleground states.

Second, while ostensibly non-partisan, regulators may favor a particular presidential candidate or party for a number of reasons. These include preferences about political ideology (e.g., one candidate is more philosophically supportive of the agency's mission than the other), budgetary concerns (e.g., if a presidential candidate promises to either cut or bolster funding to the agency), or the career-related concerns of agency employees (e.g., the careers of legal staff are often advanced by work on new and significant regulatory rules, the prospect of which can be affected by candidates who promise more or less government regulation). These preferences can affect enforcement: regulators may choose to selectively enforce regulation in battleground states if they feel that, by doing so, they can improve the electoral prospects of their preferred presidential candidate.

## 3. Data and Identification Strategy

The data in our analysis comes from three primary sources. First, state-level data on presidential elections is gathered from David Leip's Atlas of U.S. Presidential Elections.[5]

[5] This data can be found at http://www.uselectionatlas.org.

Second, we use data from the EPA's National Pollutant Discharge Elimination System, which was created to facilitate compliance with the CWA. This data includes violations of the CWA and other information related to permits issued to facilities to discharge water.[6] Finally, we gather state-level data on per-capita income and unemployment rates from the Federal Reserve Economic Data (FRED) database.

### 3.1. Classification of Battleground States

Our study identifies battleground states based on the outcome of each presidential election between 1976 and 2012. Prior research has used a number of methods to classify a state as a battleground state in a given election year.[7] In this study, we use an approach similar to Gerber, Huber, Dowling, Doherty, and Schwartzberg (2009) (we also show that our results are similar when we use other specifications). Specifically, and for each election, we sort each state by the percentage of votes given to Democrats. We then cumulatively sum the Electoral College votes by state and subtract 270 (the minimum number of Electoral College votes a candidate requires to win election). We then take the absolute value of this difference and classify a state as a battleground state if this value is less than or equal to 75. Thus, a battleground state is among the "middle" 150 Electoral College votes out of the 538 possible.

We illustrate this methodology in Table I using the results of the 2012 presidential election. Gerber et al. (2009) argues that this approach yields results similar to the classification schemes of other studies which use different methods. However, since it does not rely interviews with campaign operatives, it has the advantage of being applicable to every presidential election for which we have state-level voting data. Battleground states that are classified using this method are listed in Table II for all presidential elections between 1976 and 2012, inclusive.

For the purposes of our analysis, a facility is determined to reside in a battleground state if the state was classified as such based on the outcome of the prior election cycle.

[6] This data can be found at https://echo.epa.gov/tools/data-downloads.

[7] For example, Shaw (1999, 2006) relies on interviews with consultants for presidential candidates during past elections in order to classify battleground states, which he defines as states viewed as pivotal by the campaign of either candidate. Hill and McKee (2005) use Shaw's approach, but classify a state as a battleground state only if both campaigns view it as such.

For example, if Ohio was determined to be a battleground state based on the results of the 1988 election, then a facility in Ohio is considered to be located in a battleground state from at least 1989 through 1992. It is not unreasonable to expect that political actors have some ability to predict which states will be decisive in upcoming elections, particularly as the election draws near. If we assume a level of foresight on the part of political actors (e.g., politicians and regulators) we could, say, classify Ohio as a battleground state for the years 1987 through 1990 based on the outcome of the 1988 election. Almost all of our analysis holds if we assume that battleground state status can be predicted two years in advance, and this is also true if we assume it can be predicted four years in advance, a result that is not surprising if only because battleground state status displays a high level of serial correlation (as seen in Table II).

### 3.2. The Clean Water Act and EPA Regulatory Enforcement

The Environmental Protection Agency (EPA) is the primary federal environmental regulator in the United States, and its statutory mandates includes the administration and enforcement of most federal environmental laws. While the EPA is primarily staffed by career civil servants, its senior leadership (including the head administrator, deputy administrator, assistant administrators, general counsel, and chief financial officer) is appointed by the president.

The EPA is responsible for the administration of the CWA, a broad and economically important statute. To facilitate enforcement of some of the law's provisions, the EPA has created the National Pollutant Discharge Elimination System (NPDES), which is used to limit pollutants discharged into surface waters. This system requires point sources (and certain non-point sources) that discharge water directly into surface waters through discrete conveyances (e.g., pipes and man-made ditches) to obtain a permit to do so, and submit to regular inspections. Point sources include industrial, government, and certain agricultural sites and facilities (e.g., manufacturing facilities, mining operations, hotels and real estate development, national parks, military installations, municipal and federal government facilities, etc.). Certain non-point sources may also be required to obtain an NPDES permit; these include stormwater discharges from industrial sites, municipal storm drains, and urban areas.

While the EPA is ultimately responsible for administering the NPDES program, the agency largely delegates its permitting, monitoring, and, to a lesser extent, enforcement functions to state regulators. These delegations are contingent on the state implementing and enforcing the NPDES program consistent with the EPA's interpretation of the law, and the EPA oversees state efforts. Currently, the EPA has authorized 46 states to administer the NPDES program within their borders.[8]

### 3.2.1. NPDES Facility Data

We collect data on NPDES permits and associated facilities from the EPA's website. The data includes both the postal address and latitude and longitude of the facility, as well as the dates for which each permit was active. In approximately 2.45% of the sample, the postal address of the facility does not agree with its latitude and longitude. In order to be included in our analysis, we require that the latitude and longitude place the facility within 50 miles of its zip code. The resulting sample includes 288,490 unique facilities between 1976 and 2014 (with 2,746,253 facility-year observations). The geographical location of facilities can be seen in Figure 1 for the presidential election years of 1976, 1988, 2000, and 2012, with the battleground states (as determined by the votes in these years) shaded in gray.

Facilities are assigned an SIC code as well as a facility type code. Of the 288,490 facilities in our sample, 127,467 have a valid SIC code and 189,097 have a valid facility type code (e.g., whether the facility is owned by a corporation, the federal government, a school district, etc.) Table III lists the distribution of facilities within the 49 Fama and French industry classifications in our sample (Panel A). The construction industry and the "other" category are the most strongly represented, accounting for 20.28% and 20.45% of the total, respectively.[9] Industries whose representation exceeds 4% of the sample include agriculture (5.84%), construction materials (6.25%), non-metallic and metal mining (4.36%), utilities (4.70%), personal services (4.01%), transportation (4.47%), wholesale (4.37%), and real

[8] The states of Idaho, Massachusetts, New Hampshire, and New Mexico are not currently authorized to issue NPDES permits themselves; the EPA administers the NPDES program directly in these states.

[9] While the "other" category does not feature prominently in many commonly used financial databases (such as CRSP or Compustat), this category includes sewage and irrigation functions that are more strongly represented among the facilities in the NPDES sample.

estate (4.33%). Panel B of Table III displays the distribution of facilities by facility type. At 70.41% of the total, privately owned facilities are the largest category of facility type.

### 3.2.2. NPDES Violation Data

The NPDES data includes permit violations as recorded for facilities in the sample starting in 1976. We include violations through 2014, the last full year for which we have data. We aggregate this violation data by year and combine it with the facility data on NPDES facilities to create an annual panel dataset. (The facility data includes the dates between which a permit was active.) The regulatory authority that grants the NPDES permit (usually a state environmental agency) is responsible for setting effluent limits for each facility, for conducting regular inspections of each facility, and for creating and enforcing compliance and permit schedules, all within EPA guidelines.

Permit parameters can vary significantly between facilities, depending upon such factors as the characteristics of the surface water near the facility (e.g., its chemical composition, pH balance, temperature, etc.), legal definitions of what constitutes a pollutant, the state of compliance of the facility, etc. The EPA can overrule state environmental agencies if it disagrees with the terms of a permit. With regard to enforcement, the EPA can initiate its own enforcement proceedings, in addition to those initiated by the state regulator, in a process referred to as "overfiling." Overfiling is intended to promote the consistent enforcement of federal environmental law across states, and to protect against under-enforcement by state regulators.

In our dataset, violations of an NPDES permit are placed into four broad categories: effluent violations, single-event violations, compliance schedule violations, and permit schedule violations. Effluent violations involve the discharge of water that does not conform with the permit requirements for a given facility. NPDES facilities are required to regularly collect wastewater samples upon which they must conduct various chemical and/or biological tests, as appropriate to the requirements of the permit, to determine the presence and levels of various pollutants. They must then submit regular Discharge Monitoring Reports (DMRs), which contain the results of these tests, to the EPA and other permitting authorities. If discharged water does not conform to permit limits, an effluent violation may be generated through the automated review of these monitoring reports. The EPA can affect violation rates through at least two mechanisms: first, the EPA can overrule state regulators with regard to permit limits, and this can increase or decrease the probability of an eventual violation. Second, if a DMR for a given facility exceeds of one of the permit limits, the state can nevertheless report a "no violation" if additional supportive evidence suggests a violation is not warranted, or if there are mitigating circumstances. In these instances, the EPA can overrule the state regulatory authority and find the facility in violation.

Single event violations are one-time or long-term violations of an NPDES permit requirement as determined by regulators during compliance inspections. They can involve unauthorized wastewater bypass or discharge, pretreatment implementation violations, and any other violations observed during an inspection. As is the case with effluent violations, state regulators are primarily responsible for inspecting facilities and making violation determinations. The EPA can affect single event violation rates by sending its own regulators to inspect facilities, through the influence it exerts on state inspectors, and by overruling the findings of state inspectors.

Compliance and permit schedule violations are similar in that they both represent failures of facilities to meet timelines set by environmental regulators with regard to filing reports, performing equipment upgrades or other capital investments, or otherwise failing to meet an established deadline. Compliance schedules are typically imposed by regulators in response to an effluent single event violation as part of an effort to bring the facility back into legal compliance. Permit schedules are imposed by regulators as a condition of an NPDES permit. The EPA can affect compliance and permit schedule violation rates by overruling schedules approved by state regulators, as well as state-approved extensions or other modifications to the deadlines present in existing schedules.

Table IV contains summary statistics. Panel A displays statistics for the border sample. Included is the overall violation rate, as well as violation rates for the effluent, single event, compliance schedule, and permit schedule violations. We also show similar statistics for facilities located in non-battleground and battleground states. Panel B shows statistics for the full sample.

### 3.3. Identification Strategy

The geographical data of each regulated facility, along with the time-varying battleground state status, allow us to identify battleground state effects on violation rates. Specifically, we create a "border sample" of similar facilities located along the border between battleground states and non-battleground states. In order to be included in this sample, a facility in a battleground state must be located no more than 25 miles from a facility in a non-battleground state with the same 3-digit SIC code (and vice versa). Other than the battleground state status of the state in which they operate, the close geographical proximity of these facilities means that facilities in the treated group are matched to at least one similar facility in the untreated group (and vice versa). This matching enables us to control for unobserved heterogeneity such as time-varying industry effects, demographics, economic conditions, weather events, etc. Further, each matched set likely discharges to the same surface water and should therefore be subject to the same effluent limits. This is especially true in those common instances in which waterways form the border between states.

We argue that the battleground state status represents an exogenous shock to the political importance of facilities located in each state. We further argue that a reduction of violation rates within battleground states results from a reduced compliance intensity on the part of regulators. Lower violation rates can also result from increased compliance efforts on the part of facility operators, of course. However, a mechanism by which voters in a battleground state become relatively indifferent between political candidates while simultaneously improving the ability of its facility operators to comply with the CWA (relative to Republican- and Democrat-leaning states) is not readily apparent. By contrast, a number of mechanisms by which the political importance of a state might result in differential regulatory treatment by regulators and politicians is more immediately forthcoming.

The border sample includes 11,398 unique facilities (with a total of 123,514 facility-year observations), and this constitutes the main sample used in our analysis. The locations of facilities in the border sample for the presidential election years of 1976, 1988, 2000, and 2012 are shown in Figure 2.

## 4. Analysis: Do Facilities in Battleground States Experience Lower Violation Rates?

The question asked by this paper is straightforward: is regulation selectively enforced? To answer, we first examine the raw differences between violation rates of matched sets of facilities located along the border between battleground states and non-battleground states; these are reported in Table IV Panel A. By using matched sets, we are able to control for much time-varying unobserved heterogeneity. It can be seen that overall violation rate for facilities located in battleground states averages 10.9% per year, while the violation rate for facilities located in non-battleground states averages approximately 22.8% per year. This represents a reduction in violation rates for battleground states of 52.2% (or 11.9 percentage points), a result that is economically important and statistically significant as measured by a standard $t$-test. We also report results for the four specific subcategories of NPDES violations and find that they are similar.

Results for the full sample, shown in Panel B, are qualitatively similar to those of the border sample reported in Panel A. In this sample, facilities in battleground states experience an average violation rate of 7.8%, while those in non-battleground states experience violation rates of 13.2%. As before, the difference between these rates is statistically and economically significant, and results are similar for specific subcategories of NPDES violations.

It is apparent from these tables that violation rates, and the differences among them, are higher for the border sample relative to the full sample. This might partially owe to the fact that while most facilities in the full sample do not have a valid SIC code, we require facilities in the border sample to have one in order to facilitate the matching of similar facilities. To better enable us to compare the results in the border sample to those in the full sample, we break the full sample into "industrial" (those with a valid SIC code) and "non-industrial" (those without a valid SIC code) subsamples. Results for these subsamples are shown in Panels C and D of Table IV, respectively. It can be seen that industrial facilities do indeed have higher violation rates compared to non-industrial facilities. Further, the difference in violation rates between facilities located in battleground states and facilities located in non-battleground states also is higher for industrial facilities.

### 4.1. Violation Rates by Year

In Panel A of Figure 3, we show the average violation rates by year for battleground and non-battleground states for the border sample. In most years, facilities located in battleground states have substantially lower violation rates than those in non-battleground states (exempting only the 1988 and 1996 election cycles, for which violation rates were similar). This is consistent with facilities in those states receiving favorable regulatory treatment. Since the border sample is comprised of matched sets, we argue that the relationship between battleground state status and violation rates is likely causal.

Results for the full sample are shown in Panel B of Figure 3. Results are similar, though not as stark, to those in Panel A. We identify at least two reasons for this difference. First, the border sample is restricted to those facilities which have a valid SIC code. It can be seen in Panels C and D (which show results for facilities with and without a valid SIC code, respectively) that facilities with a valid SIC code generally have higher violation rates, and the difference in violation rates between battleground and non-battleground is larger. Second, facilities located along the borders of states are often located along major waterways. In fact, these waterways often serve as the border between states. We speculate that these facilities are subject to more stringent effluent limits relative to facilities not located near major surface waters, which results in their having a higher probability of violating their permits.

### 4.2. Difference-In-Difference Test for Border Facilities in States that Change Battleground State Status

What happens to the violation rates of facilities located in non-battleground states that become battleground states after an election (and vice versa)? To answer this question, we first examine the change in the violation rates of border facilities located in a non-battleground state that becomes a battleground state after a presidential election in year $t$ relative to facilities in neighboring battleground states. In Panel A of Figure 4 we show the change in violation rates for these "new" battleground states. For the four-year period prior to the election in time $t$, border facilities in these states experienced average violation rates that were 8.48 percentage points higher than neighboring facilities in battleground states. After the election, relative violation rates in these "new" battleground states were only

3.16 percentage points higher relative to those in neighboring battleground states. This represents a reduction of the relative violation rate of 62.7% (or 5.32 percentage points), a result that is economically and statistically significant (with a $t$-stat of $-5.80$). Thus, the violation rates in "new" battleground states converges to the violation rates of "old" battleground states.

We run a similar analysis for facilities in battleground states that become non-battleground states. Panel B shows that, during the four-year period prior to an election, facilities located in battleground states had an average violation rate that was 7.27 percentage points lower than facilities located in neighboring non-battleground states. However, after an election where these states changed battleground state status, the average violation rate was only 1.71 percentage points lower than the same set of facilities located in a neighboring non-battleground state. This represents a decrease in the average relative violation rate of 76.5% (or 5.56 percentage points), a result that is economically and statistically significant (with a $t$-stat of 9.87). Thus, the violation rates for facilities in the "new" non-battleground states converges to those of the "old" non-battleground states.

### 4.3. Do Facilities Choose to be Located in Battleground States?

Is the decision to locate a facility in a given state endogenous to whether that state is a battleground state? McCrary (2008) suggests examining the density of observations near the cutoff value of the forcing variable – in our case, a state border – to determine whether there is sorting around this threshold. If the decision to locate a facility is not exogenous to the battleground state status of a given locale, we might expect to see a population discontinuity along state borders which results from facility operators choosing their value of our forcing variable (i.e., they choose to locate in a battleground or non-battleground state).

To examine whether facility operators along the border choose to locate in battleground or non-battleground states, we first plot average violation rates of facilities in battleground states as a function of their distance from the border of a non-battleground state (and vice versa). Results for the full sample are shown in Panel A of Figure 5. It can be seen that violation rates are sharply discontinuous at the border between battleground and non-battleground states. To examine any heaping-induced bias that may result from facility operators choosing to operate on one side of the border, we plot the number of facilities in the full sample as a function of their distance from the border in Panel B of Figure 5. It can be seen that there does not appear to be any population discontinuity along the border. We also run McCrary density tests by year, which indicates no sorting along the border (see McCrary (2008)). These results suggest that it is unlikely that facility operators choose a location based on battleground state status and that this is driving our result.

### 4.4. Multivariate Tests

Our method of creating matched sets of facilities located along the border between battleground and non-battleground states allows us to control for time-varying unobserved heterogeneity in our effort to understand battleground state effects on violation rates. We can account for more unobserved heterogeneity (such as unobserved state or facility effects) by using panel regressions. Results for these regressions are shown in Table V. We use three model specifications (separated into Panels A, B, and C) to examine the border facility subsample, the full sample, and the industrial and non-industrial subsamples. The dependent variable in all specifications is a dummy variable equal to one if the facility has experienced any kind of NPDES permit violation in a given year and zero otherwise. The independent variable of interest is a dummy variable that is equal to one if the facility is located in a battleground state as determined by the outcome of the most recent presidential election and zero otherwise. We use a large number of fixed effects and, as a consequence, estimate results with linear probability models. All standard error estimates are adjusted for two-way clustering by state and year to account for autocorrelation and cross sectional correlation in the panel (see Bertrand, Duflo, and Mullainathan (2004) and Petersen (2009)).

The first and most basic specification, shown in Panel A, includes 3-digit SIC industry-by-year fixed affects which absorb any time-varying industry effects (e.g., the effect of oil prices on the transportation industry, or business-cycle effects on the demand for consumer goods).[10] State or facility fixed effects are not included in this specification since they may at least partially subsume our variable of interest. In the case of the border sample

[10] For an explanation of using high dimensional fixed effects to control for unobserved heterogeneity, see Gormley and Matsa (2014).

(column (1)), being located in a battleground state has the effect of significantly reducing the probability a facility will be found in violation of its NPDES permit by 13.1 percentage points. In the full sample (column (2)), this battleground state effect is −5.2 percentage points, though this result is not significant at the usual confidence intervals. When the full sample is split between industrial and non-industrial facilities (columns (3) and (4), respectively), the battleground state effect is −8.7 percentage points for the industrial facilities (a result that is statistically significant) and −1.8 percentage points for the non-industrial facilities (a result that is not statistically significant).

The second specification, the results of which are seen in Panel B of Table V, adds state fixed effects to the specification in Panel A. These absorb any time-invariant state-level omitted variables. The inclusion of state fixed effects means that the coefficient estimate on the battleground state dummy represents a difference-in-difference estimate of within-state changes in violation rates. Results are qualitatively similar to those in Panel A, though the magnitude is reduced: facilities in battleground states are less likely to be found in violation of the CWA.

Finally, in our third specification, we replace the state fixed effects in Panel B with facility fixed effects. The inclusion of both facility fixed effects and industry and facility type-by-year fixed effects means that the coefficient estimate on the battleground state dummy represents a difference-in-difference estimate of both within-facility and time varying within-industry changes that result from changes to a facility's battleground state status. We also include six state-level control variables to capture time-varying state-level effects. (We cannot include state-by-year fixed effects as this would completely subsume our variable of interest.) These include the natural log of a state's electoral votes, the state unemployment rate, each state's per-capita income, and the one-year change in each of these three variables. Each variable is measured at the beginning of the year. Results are shown in Panel C of Table V. In this specification, each sample shows a reduction of violation rates for facilities located in battleground states, though this result is not statistically significant in the case of non-industrial facilities (column(4)). The statistically significant effects range from a 3.8 percentage point reduction in violation rates in the full sample (column (2)) to 5.9 percentage point reduction for facilities in the border sample (column (1)). We also note that the economic magnitude of the battleground state effect, at −5.0

and −5.9 percentage points, is similar for both the industrial and border facility samples, respectively. Both of these samples include only those facilities with a valid SIC code.

Consequently, we argue that battleground state status represents an exogenous source of variation in the political importance of a facility. These results also support a causal relationship between the political importance of a state arising from the Electoral College system and policy favors directed to that state.

### 4.5. Other Battleground State Classification Methods

Prior research has used several different methods to classify battleground states. While there is substantial overlap between our set of battleground states and the battleground state classifications made by previous work, we investigate whether our results are robust to these alternative classifications. Specifically, we examine whether results are similar using the battleground state classifications of Shaw (1999, 2006), Strömberg (2008), and the website RealClearPolitics.com.

Shaw (1999) and Shaw (2006) use interviews with campaign staff to make determinations about battleground state status. Specifically, interviewees were asked whether a campaign viewed a state as being in one of five categories: Base Republican, Lean Republican, Battleground, Lean Democratic, and Base Democratic. This was accomplished for five presidential elections, from 1988 through 2004. In our application, we classified as a battleground state one that was viewed as such by at least one of the presidential campaigns. Strömberg (2008) presents an empirical model that attempts to characterize the probability that a state will be decisive in the Electoral College in a given election cycle. Based upon the output of the model, and obtained from Figure 1 in Strömberg (2008), states are sorted in descending order on the probability that they will decisively affect the election for the 2000 and 2004 election cycles. We define as battleground states those that were at the top of these lists such that the sum of their electoral votes did not exceed 150. Finally, we consider the battleground states classifications of RealClearPolitics.com, which are based on political polling immediately prior to the elections in 2004, 2008, and 2012.

As described in Section 3.1, for the purposes of our analysis, a state is considered to be a battleground state if it was classified as such based on the outcome of the previous election. For example, if the state in which a facility is located was determined to be a battleground state in, say, the 2000 election, then that facility is classified as residing in a battleground state over the subsequent four years (2001 through 2004).

Results of our analysis using alternative classifications of battleground states are shown in Table VI. We use panel regressions on the resulting border samples and whose specifications are similar to those in Panel A of Table V. (We do not include state- or facility-level fixed effects as they largely subsume our variable of interest given the shorter time periods represented in these subsamples.) To facilitate a comparison with our main results, for each of the time periods spanned by these alternative classifications we include results for the battleground state definitions used elsewhere in this analysis. In the first two columns we show the results for the Strömberg (2008) battleground classification over the 2001-2008 time period. In column (1) we show the results using our definition for battleground states and in column (2) we show the results using the Strömberg (2008) specification. It can be seen that the battleground state effects are similar with regard to both their economic magnitude and statistical significance.

Similarly, we show the results of alternative specifications for Shaw (1999, 2006) in columns (3) and (4) and the results for the Real Clear Politics classifications in columns (5) and (6). It can be seen that our results are similar in each respect to alternative classifications of battleground states. Thus, our results do not appear to be driven by our method of classifying battleground states.

### 4.6. Falsification Tests - Battleground State Simulations

Using border samples, we conduct falsification tests to asses the probability that our method for assigning battleground states would yield the results outlined in the preceding sections. To accomplish this, we run simulations wherein we randomly assign battleground state status to various states and examine the resulting difference in violation rates between border facilities.

We randomize the assignment of battleground states in two ways. First, for each election year we assign each state a random number and sort in ascending order. We then classify as battleground states the maximum number of states with the lowest random numbers such that their total electoral votes do not exceed 150. We then run 10,000 simulations wherein we assign randomly assign battleground states over our sample period and examine the resulting violation rates in simulated battleground and non-battleground states. In Panel

A of Figure 6 we plot a normalized histogram of the difference between violation rates for border facilities in battleground and non-battleground states. Of the 10,000 simulations, none was lower than the observed difference of $-11.9\%$. We also run panel regressions as specified in Panel C of Table V and plot the normalized histogram of coefficient estimates on the battleground state dummy in Panel B of Figure 6. In this case it can be seen that, of the 10,000 simulations, only 0.55% produced coefficient estimates that were lower than the observed battleground state effect of $-5.9$ percentage points.

Our second method of randomizing battleground state assignments better replicates the battleground state dynamics in our sample (in particular, the cross-sectional and serial correlation in battleground state status, as well as the unconditional probability that a given state will be a battleground state). In an attempt to more realistically model voter dynamics and resulting battleground states, we also randomly assign battleground states by modeling the change in the percentage of votes to Democrats using an Ornstein-Uhlenbeck process:

$$dV_{i,t} = (\alpha_i - \beta_i V_{i,t})dt + \sigma_i dB_{i,t}. \tag{1}$$

We estimate the parameters separately for each state $i$ by regressing the change in the percentage of votes to Democrats between an election at time $t-4$ and time $t$ on a constant and the level of the vote at time $t$ in elections years spanning 1968 through 2012:

$$V_{i,t} - V_{i,t-4} = \alpha_i - \beta_{i,t} V_{i,t-4} + \epsilon_{i,t}. \tag{2}$$

We estimate the diffusion coefficient $\sigma_i$ as the standard deviation of the error terms for each state. Finally, we assume that the state-specific Brownian motions in equation (3) are correlated using the correlation matrix obtained from the error terms in equation (2).

An Ornstein-Uhlenbeck process has several attributes which makes it appealing in our context. First, model parameters imply a state-specific long-run average of votes to Democrats (equal to $\alpha_i/\beta_i$), and states that have consistently voted for Democrats in our sample will more likely vote for Democrats in our simulations (with the same being true for votes to Republicans). Importantly, it also means that states that are more often battleground states in our sample will be more likely to be battleground states in our simulations. Second, by using a mean-reverting process, and by allowing the Brownian motions to be correlated between states, we introduce both cross-sectional and serial correlation to the changes of battleground state status into our simulations, which better mirrors the characteristics of our actual sample.

Starting with the actual state-level votes for Democrats in the 1968 election, we run 10,000 simulations of votes for Democrats in subsequent election years. For each path, we use the simulated votes for Democrats to assign battleground states each election year using the method outlined in Section 3.1 and illustrated in Table I. We report our results in Figure 7. In Panel A we plot the unconditional probability that a state is a battleground state in our simulations against the unconditional probability that a state is a battleground state in our actual sample. It can be seen that, in general, states that have a high probability of being a battleground state in our sample also have a high probability of being a battleground state in our simulations.

In Panel B of Figure 7, we plot a normalized histogram of the raw difference between violation rates of facilities in battleground states and non-battleground states. In can be seen that, on average, this simulated difference is −4.4% (which stands in contrast to the average difference of 0.0% in Panel A of Figure 6). This results from the fact that battleground states in our sample are substantially more likely to be battleground states in our simulations. Nevertheless, only 0.35% of our simulations produced a battleground state effect lower than the observed effect of 11.9%. In Panel C we report the normalized histogram of the battleground state effect observed in our panel regressions and find that the coefficient estimates in our simulations are lower than the observed effect of −5.9 percentage points in only 2.6% of our simulations.

Taken together, the results of these simulations indicate that it is extremely unlikely that the battleground state effects observed in our sample were arrived at by chance associated with our method for assigning battleground states.

### 4.7. Multivariate Tests - With Interaction Terms

In this section, we examine whether the battleground state effect varies for long-term battleground states, the party affiliation of the president, and the year in the four-year presidential election cycle. Is the battleground state effect more or less pronounced for long-term battleground states? To examine this, we define a state as being a “long-term” battleground state if it was a battleground state in at least four election cycles (the median in our sample). We then create a dummy variable to indicate whether a battleground state is a long or short-term battleground state and interact it with the battleground state dummy variable previously defined. We repeat the multivariate analysis outlined in the preceding section and report the results in Panel A of Table VII. Model (1) adds industry-by-year fixed effects, model (2) adds state fixed effects, and model (3) includes the control variables from Table V, facility fixed effects, and industry-by-year fixed effects. In all three specifications, results are strongest economically for states that are long-term battleground states. Thus, the battleground state effect is larger for states that are more reliably battleground states.

We also examine whether the party of the president affects the battleground state effect. To examine whether or not this is the case, we create a dummy variable equal to one if the president is a Democrat and zero otherwise. We similarly create a Republican dummy variable. We then interact these presidential party variables with the battleground state dummy variable. We report the results in Panel B of Table VII. In each of the three models considered, it can be seen that the battleground state effect is similar regardless of whether a Democrat or Republican is president, and thus the result does not appear to arise from the party of the president.

Finally, we wish to examine whether the battleground state effect varies depending on the year of the four-year election cycle. It could be, for example, that violations in battleground states result from the increased scrutiny of these states during presidential election years, a pressure that is less likely to exist in other years. To do this, we create four dummy variables corresponding to the first, second, third, and fourth year of an election cycle (during which the presidential election is held in the fourth year), and interact these with the battleground state variable. Results are shown in Panel C of Table VII. It can be seen that the battleground state effect is relatively constant over the entire election cycle, and is in all cases statistically significant in each of the three models considered. The differences between coefficients are not significant in any of these cases. Thus, the battleground state effect does not appear to be driven by inter-election cycle effects.

### 4.8. The EPA and State Regulators

Enforcement of the CWA involves both federal and state regulators. While the EPA is ultimately responsible for the administration of the NPDES program, it has delegated the authority to issue permits, as well as monitoring and enforcement activities, to some (but not all) state regulators. The EPA runs the full NPDES program in states to which it has not delegated this authority. This raises a question: is the battleground state effect largely driven by state or federal regulators?

In Figure 8, we plot the violation rates of battleground and non-battleground states for both state-controlled and EPA-controlled NPDES programs in Panels A and B, respectively. Two observations are immediately apparent. First, and particularly later in the sample period, violation rates are substantially higher when the EPA is in control of the NPDES programs. By comparison, state regulators appear more reluctant to find facilities in violation of the CWA, a finding consistent with Agarwal, Lucca, Seru, and Trebbi (2014) who argue that state regulators are more forgiving than federal regulators.[11] Second, the battleground state effect appears to be more consistently pronounced when state regulators are in charge of the NPDES program (Panel A of Figure 8). When the EPA is in charge, there does not appear to be a consistent battleground state effect. This is consistent with states being less likely to vigorously enforce the CWA and the EPA being more lenient in its oversight of state regulators in battleground states.

We examine this further using panel regressions. Using our industrial sample, we repeat the analysis in Table V, but split the sample into two based on whether a facility is located in a state where the state or EPA is in control of the NPDES program. Results are shown in Table VIII. In the first specification (which includes industry-by-year fixed effects), it can be seen that facilities in battleground states experience significantly lower violation rates than those in non-battleground states within the state-run NPDES program sample (column (1)). However, this battleground state effect disappears for the EPA-run NPDES programs. The second specification adds state fixed effects, and results are similar (although the battleground state effect appears to reverse when the EPA runs the NPDES program in a state, and it is positive and significant). Our final specification adds facility fixed effects as well as control variables. Again, the battleground state effect is negative and significant within the subsample of state-run NPDES programs, and is insignificant for states whose NPDES programs are administered by the EPA. These results are similar

[11] See also "The EPA Must Improve Oversight of State Enforcement," a December 9, 2011 report by the Office of Inspector General at the EPA which examines EPA oversight of state-run NPDES programs.

to those in Figure 8: facilities in battleground states are less likely to be found in violation of the CWA, but only if they reside in states which administer their own NPDES program.

These results are consistent with the EPA being more lax with regards to its enforcement of the CWA in battleground states. However, this is accomplished primarily through diminished oversight of state environmental regulators in those battleground states to which it has delegated parts of the NPDES program. When the EPA directly administers the NPDES program, there does not appear to be looser enforcement of the CWA in battleground states.

Finally, we note that, at the beginning of our sample period, approximately 30 states were authorized to run the NPDES program in their state, while the EPA administered the program for the remaining 20 states. By the end of our sample period, the EPA administered the NPDES program in only four states, and had delegated much of the enforcement activity to the remaining 46. Panels C and D of Figure 8 show the number of states in the battleground and non-battleground state samples for the state-run and EPA-run NPDES programs. It can be seen that, for most of our sample period, our results for the EPA-run programs are driven by a small number of states (in many cases, only one or two battleground states). Thus we recommend some caution in interpreting the above results.

## 5. Conclusion

The literature on the impact of government regulation has largely focused on whether it benefits or harms social welfare. We examine an equally important and related question: is regulation uniformly enforced? In our study, we find that government actors selectively enforce economically important government regulation, and this selective enforcement is widespread, economically meaningful, and benefits politically important states. Our findings suggest that electoral politics provides incentives to both politicians and regulators to selectively enforce government regulation. Specifically, and consistent with differing levels of enforcement standards, we find that facilities located in battleground states experience lower violation rates of the Clean Water Act relative to facilities in non-battleground states. We achieve identification and control for unobserved heterogeneity by focusing our analysis on similar facilities located along the border of battleground and non-battleground states.

We argue that the battleground status of a state in which a facility is located represents an exogenous source of political importance to facility operators, and that the relationship between battleground state status and violation rates is causal. Our evidence is consistent with the idea that government regulators – in our case the EPA – selectively enforce regulation.

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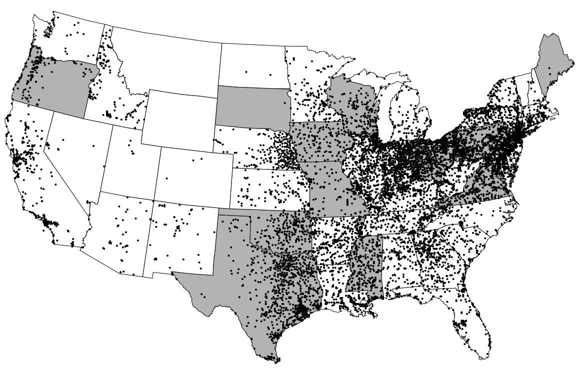
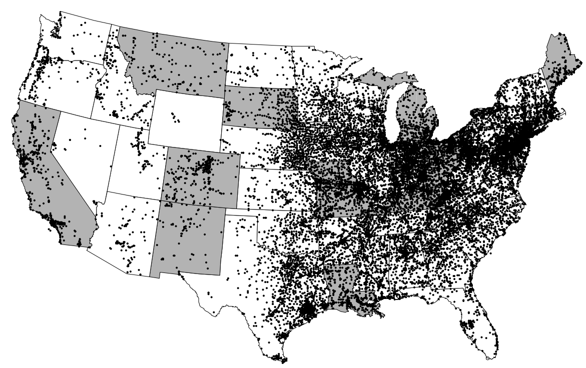
**Figure 1: NPDES Facilities**

This figure displays the location of all NPDES facilities. Panels A, B, C, and D show the location of facilities during the 1976, 1988, 2000, and 2012 election years, respectively. Battleground states for these election years are shaded in gray. This figure is discussed in Section 3.2.1.

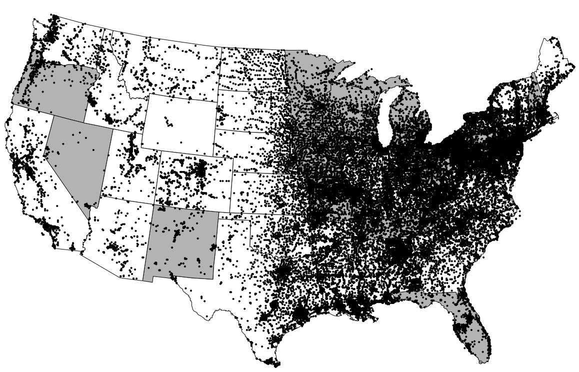
Panel A: Election Year 1976



Panel B: Election Year 1988



Panel C: Election Year 2000



Panel D: Election Year 2012

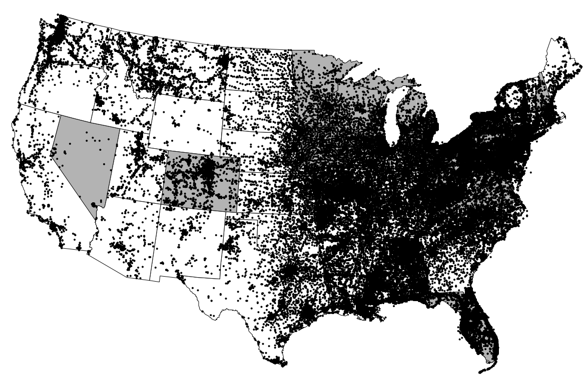


**Figure 2: NPDES Border Facilities**

This figure displays the location of NPDES facilities located in non-battleground states that are no more than 25 miles away from a facility in a battleground state which has the same 3-digit SIC code (and vice versa). Panels A, B, C, and D show the location of facilities during the 1976, 1988, 2000, and 2012 election years, respectively. Battleground states for these election years are shaded in gray. This figure is discussed in Section 3.2.1.

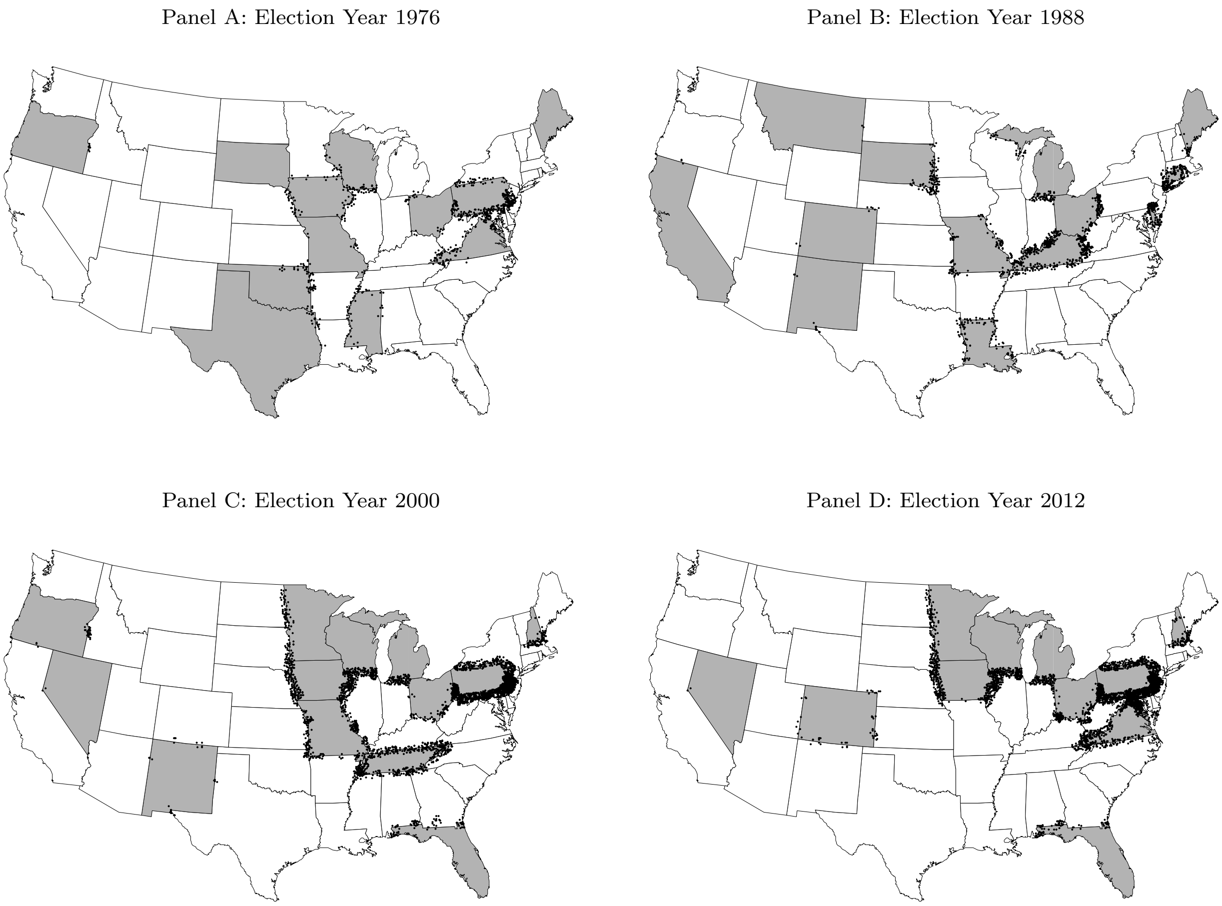


**Figure 3: Violation Rates for Battleground and Non-Battleground States (1976 – 2014)**

This figure displays the violation rates for NPDES facilities located either in non-battleground or battleground states. Panel A displays the rates for border facilities, or those facilities that are located in non-battleground states that are no more than 25 miles away from a facility located in a battleground state with the same 3-digit SIC code (and vice versa). Panel B displays violation rates for the full sample. Panels C and D display the rates for industrial and non-industrial facilities (those with valid SIC codes and those without, respectively). This figure is discussed in Section 4.1.

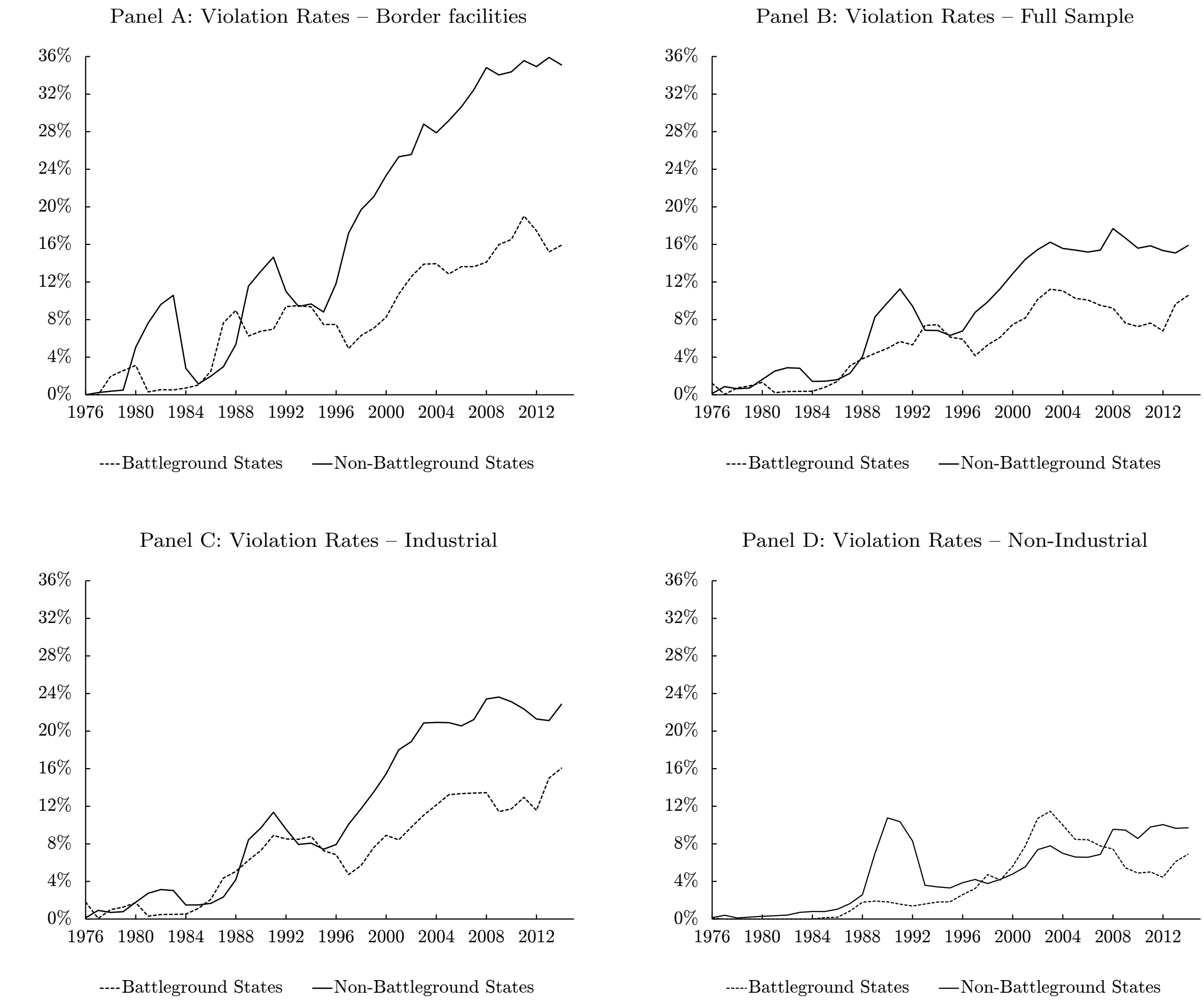


**Figure 4: Relative Violation Rates for Facilities in New Battleground and Non-Battleground States (1976 – 2014)**

This figure displays the change in relative violation rates for border facilities located in new battleground and non-battleground states. A "new" battleground state is defined as a state that was classified as a non-battleground state from time $t-3$ through time $t$ (where time $t$ is a presidential election year) but whose status changes to a battleground state for the time periods $t+1$ through $t+4$ as a result of the presidential election in time $t$. A new non-battleground state is similarly defined as a state that was a battleground state from $t-3$ through $t$ but was a non-battleground state from time $t+1$ through $t+4$. Panel A shows the change in violation rates for border facilities located in non-battleground states that become battleground states after time $t$ and are measured relative to matched border facilities that are located in battleground states. Panel B shows the change in violation rates for border facilities located in battleground states that become non-battleground states after time $t$ and are measured relative to matched border facilities that are located in non-battleground states. The difference-in-difference estimates for the average four-year change of relative violation rates before and after the presidential election at time $t$ are also shown with the $t-$stat in parentheses. This figure is discussed in Section 4.2.

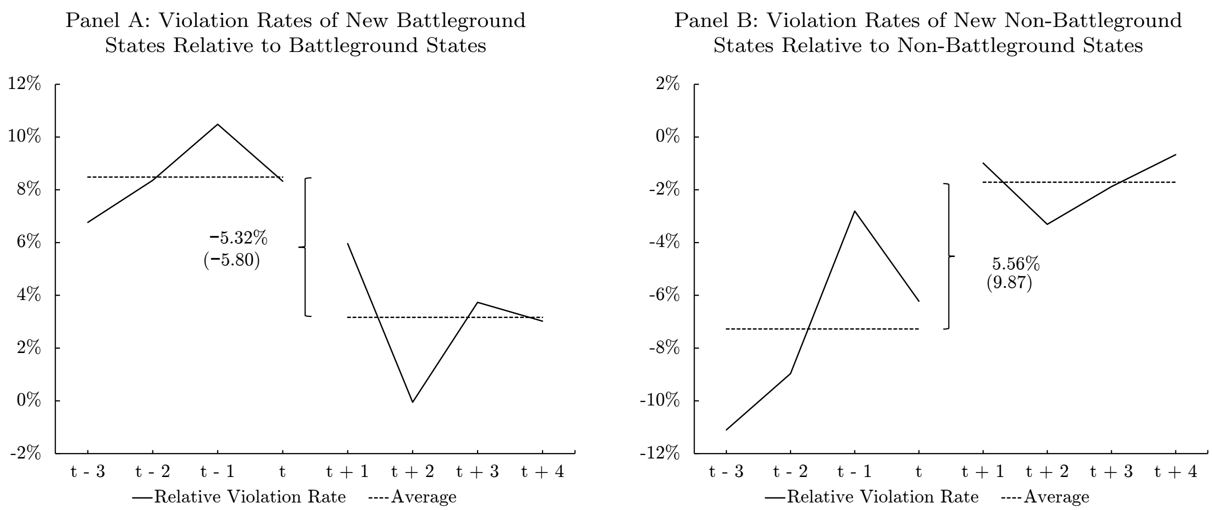


**Figure 5: Violation Rates Near the Borders Between Battleground and Non-Battleground States (1976 – 2014)**

This figure displays the average violation rates for facilities, as well as the number of facility-year observations, for facilities located near the border between battleground and non-battleground states. Panel A shows the average violation rates for facilities (on the $y$-axis) plotted against the distance from the border between battleground and non-battleground states (on the $x$-axis). Negative $x$-values indicate the distance from the border into battleground states, while positive values indicate the distance into non-battleground states. Panel B shows the number of facility-year observations (on the $y$-axis) plotted against the distance from the border into battleground and non-battleground states. McCrary density tests indicate no sorting along the border (see McCrary (2008)). This figure is discussed in Section 4.3.

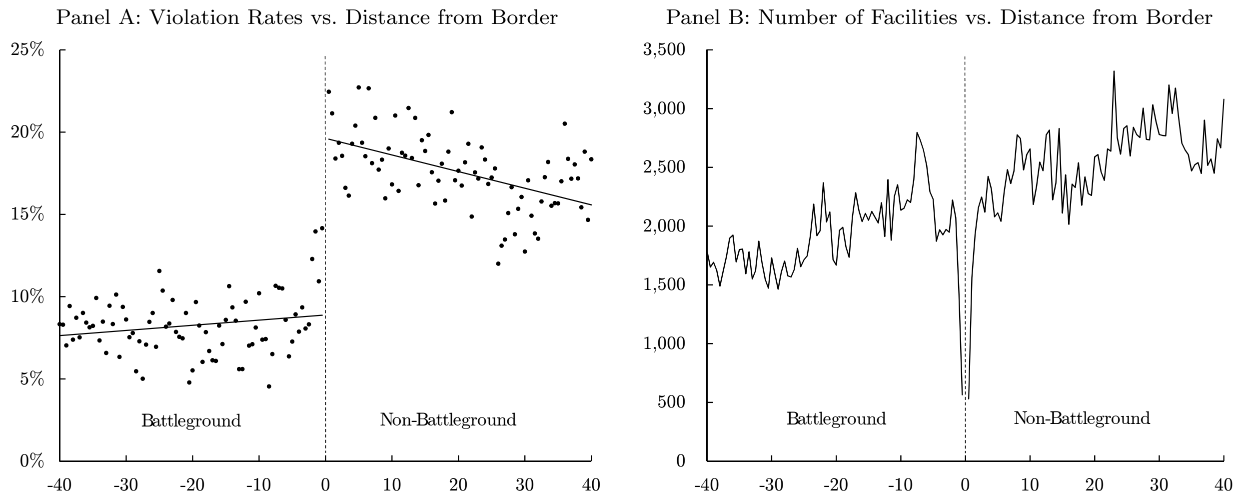


**Figure 6: Relative Violation Rates with Random Battleground States – Pure Random (1976 – 2014)**

This figure displays normalized histograms of the difference between NPDES violation rates for border facilities located in battleground and non-battleground states, and where battleground state status is randomly assigned. This was accomplished each election year by assigning each state a random number, sorting by that random number, and then assigning battleground status to the maximum number of states at the top of the list such that their total number of electoral votes did not exceed 150. We then combine this randomly generated battleground state listing with our panel dataset of NPDES border facilities and examine the resulting difference in violation rates. We repeat this exercise 10,000 times. Panel A shows the estimated probability density function that results from measuring the raw difference between the average violation rate in battleground states and those in non-battleground states. The dotted line shows the measured difference in the actual dataset of −0.119 (reported in Panel A of Table IV). Panel B shows the battleground state effect as measured in panel regressions which include facility and year fixed effects. The dotted line shows the battleground state effect of of −0.059 as measured from the actual dataset (reported in column (1) of Panel C in Table V. This figure is discussed in Section 4.6.

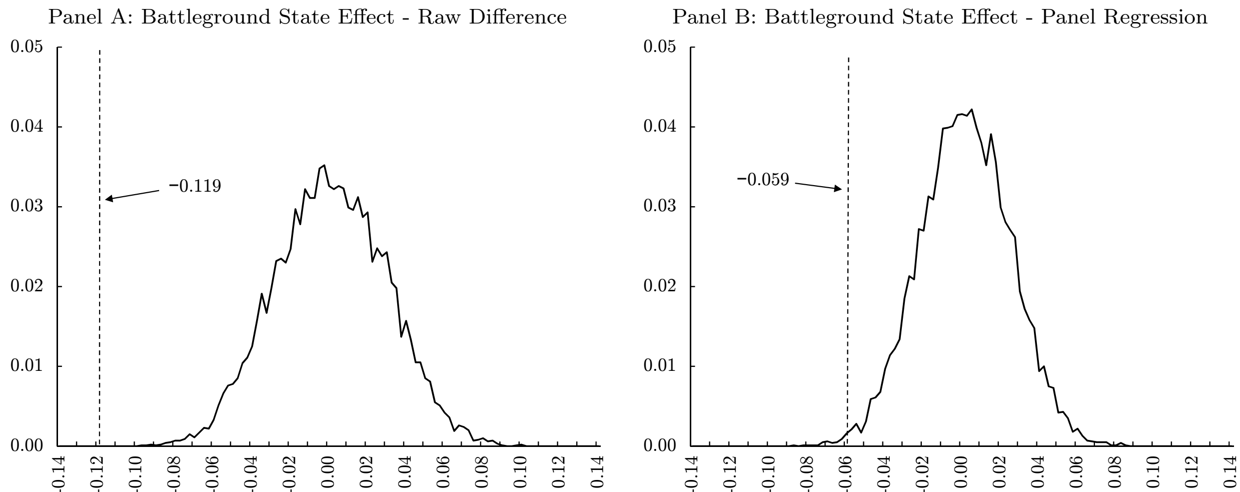


**Figure 7: Relative Violation Rates with Random Battleground States – Ornstein-Uhlenbeck (1976 – 2014)**

This figure displays estimated probability density functions of the difference between NPDES violation rates for border facilities located in battleground and non-battleground states, and where battleground state status is randomly assigned by modeling the percentage of votes to Democrats using an Ornstein-Uhlenbeck process. Starting with the actual votes to Democrats in the 1968 presidential election, the changes of votes to Democrats fore each state $i$ is modeled using an Ornstein-Uhlenbeck process:

$$dV_{i,t} = (\alpha_i - \beta_i V_{i,t})dt + \sigma_i dB_{i,t}.$$

Model parameters are estimated separately for each state and Brownian motions are correlated. For each election cycle in each simulated path, battleground states are determined using the methodology described in Section 3.1. Results are based on 10,000 simulations. Panel A plots the unconditional probability that a state will be a battleground state in our simulations, along with the unconditional probability that a state will be a battleground state in the actual sample. Panel B shows the estimated probability density function that results from measuring the raw difference between the average violation rate in battleground states and those in non-battleground states. The dotted line shows the measured difference in the actual dataset of −0.119 (reported in Panel A of Table IV). Panel C shows the battleground state effect as measured in panel regressions which include facility and year fixed effects. The dotted line shows the battleground state effect of of −0.059 as measured from the actual dataset (reported in column (1) of Panel C in Table V. This figure is discussed in Section 4.6.

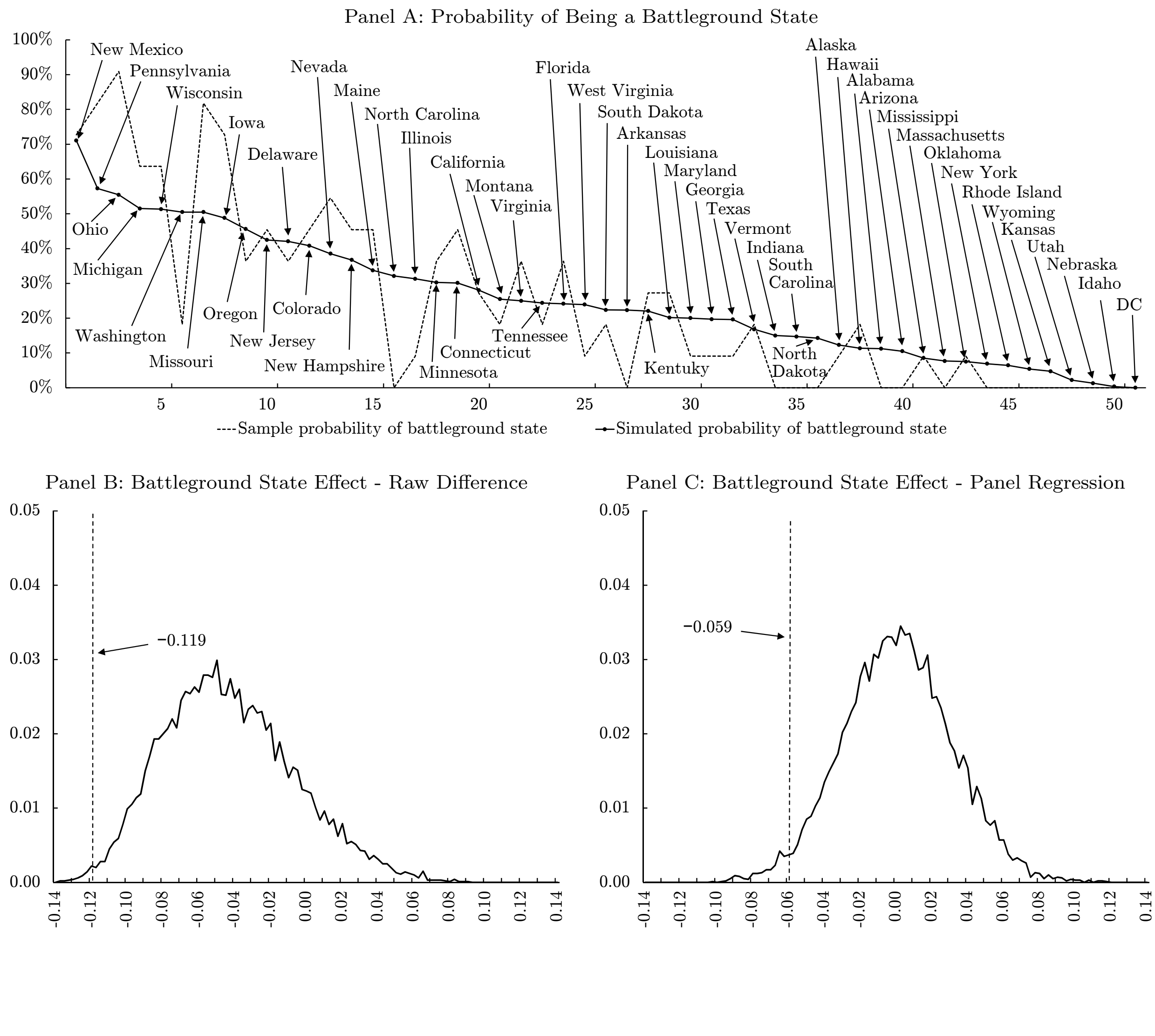


**Figure 8: Violation Rates for State and EPA Administered NPDES Programs (1976 – 2014)**

This figure displays the violation rates for NPDES facilities that are located either in non-battleground or battleground states. It includes facilities in the industrial subsample. Panel A displays the rates for facilities located in states to which the EPA has delegated the administration of the NPDES program to state regulators, and Panel B displays the rates for facilities located in states in which the EPA runs the NPDES program directly. Panels C and D display the the number of states in the battleground and non-battleground samples used in Figures A and B, respectively. This figure is discussed in Section 4.8.

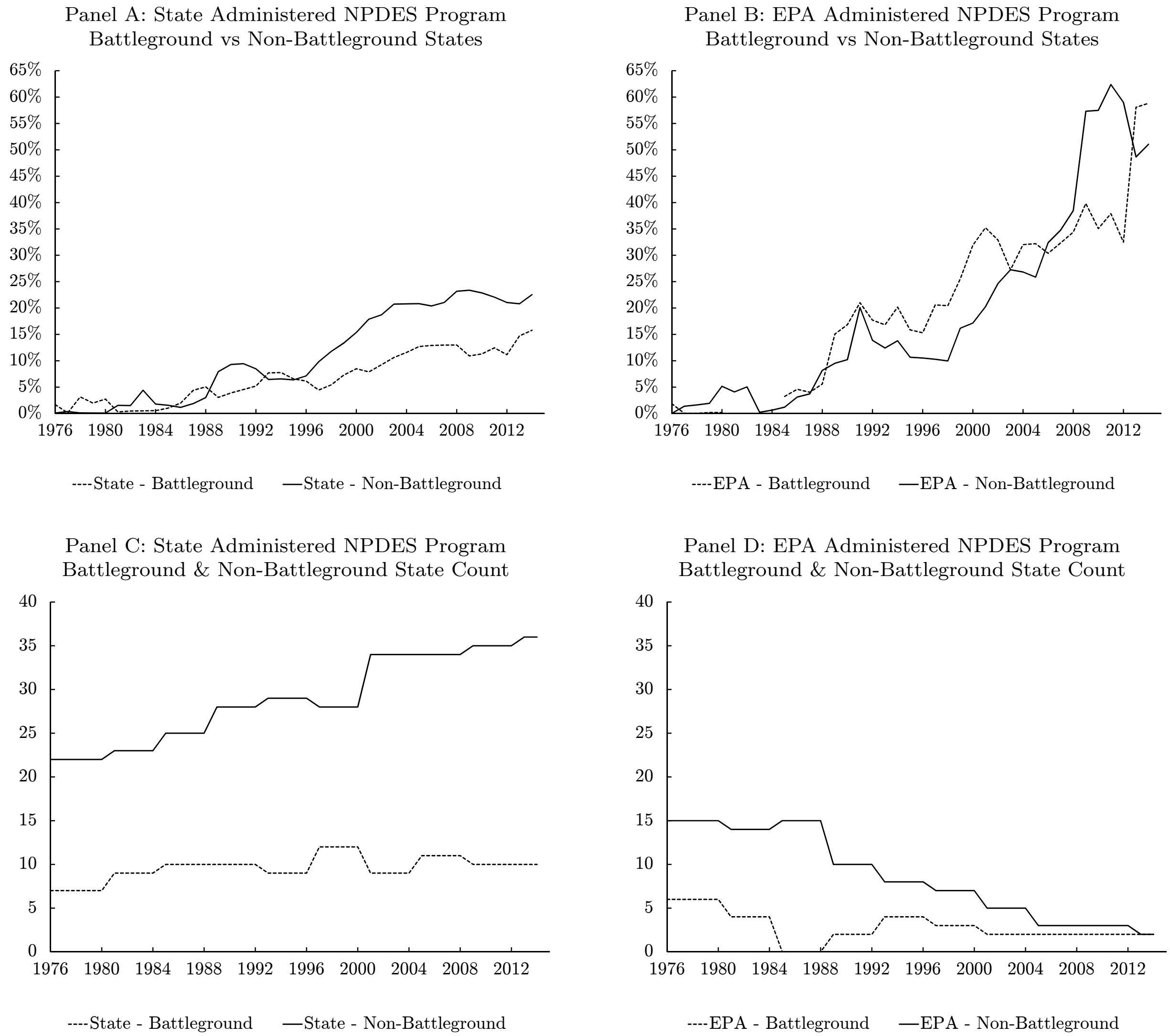


**Table I: Assigning Battleground State Status - 2012 Election**

This table contains an illustration of the methodology used to classify battleground states (including the District of Columbia) using the results of the 2012 presidential election. To determine battleground states, we first sort by the percentage of the vote given to Democrats in each state. We then cumulatively sum by electoral votes and subtract 270. Finally, a state is classified as a battleground state if the absolute value of the cumulative sum minus 270 is less than or equal to 75. This methodology is discussed in Section 3.1.

| State | Percentage of Votes to Democrats | Electoral Votes | Cumulative Sum of Electoral Votes | Cumulative Sum Minus 270 | Battleground State |
|---|---|---|---|---|---|
| District of Columbia | 92.59 | 3 | 3 | −267 | |
| Hawaii | 71.70 | 4 | 7 | −263 | |
| Vermont | 68.25 | 3 | 10 | −260 | |
| New York | 64.28 | 29 | 39 | −231 | |
| Rhode Island | 64.02 | 4 | 43 | −227 | |
| Maryland | 63.32 | 10 | 53 | −217 | |
| California | 61.87 | 55 | 108 | −162 | |
| Massachusetts | 61.79 | 11 | 119 | −151 | |
| Delaware | 59.45 | 3 | 122 | −148 | |
| New Jersey | 58.95 | 14 | 136 | −134 | |
| Connecticut | 58.78 | 7 | 143 | −127 | |
| Illinois | 58.58 | 20 | 163 | −107 | |
| Maine | 57.86 | 4 | 167 | −103 | |
| Washington | 57.63 | 12 | 179 | −91 | |
| Oregon | 56.27 | 7 | 186 | −84 | |
| New Mexico | 55.30 | 5 | 191 | −79 | |
| Michigan | 54.80 | 16 | 207 | −63 | X |
| Minnesota | 53.94 | 10 | 217 | −53 | X |
| Wisconsin | 53.51 | 10 | 227 | −43 | X |
| Nevada | 53.41 | 6 | 233 | −37 | X |
| Iowa | 52.96 | 6 | 239 | −31 | X |
| New Hampshire | 52.84 | 4 | 243 | −27 | X |
| Colorado | 52.75 | 9 | 252 | −18 | X |
| Pennsylvania | 52.73 | 20 | 272 | 2 | X |
| Virginia | 51.97 | 13 | 285 | 15 | X |
| Ohio | 51.52 | 18 | 303 | 33 | X |
| Florida | 50.44 | 29 | 332 | 62 | X |
| North Carolina | 48.97 | 15 | 347 | 77 | |
| Georgia | 46.04 | 16 | 363 | 93 | |
| Arizona | 45.39 | 11 | 374 | 104 | |
| Missouri | 45.22 | 10 | 384 | 114 | |
| Indiana | 44.80 | 11 | 395 | 125 | |
| South Carolina | 44.69 | 9 | 404 | 134 | |
| Mississippi | 44.20 | 6 | 410 | 140 | |
| Montana | 42.97 | 3 | 413 | 143 | |
| Alaska | 42.68 | 3 | 416 | 146 | |
| Texas | 41.99 | 38 | 454 | 184 | |
| Louisiana | 41.26 | 8 | 462 | 192 | |
| South Dakota | 40.78 | 3 | 465 | 195 | |
| North Dakota | 39.89 | 3 | 468 | 198 | |
| Tennessee | 39.65 | 11 | 479 | 209 | |
| Kansas | 38.88 | 6 | 485 | 215 | |
| Nebraska | 38.87 | 5 | 490 | 220 | |
| Alabama | 38.78 | 9 | 499 | 229 | |
| Kentucky | 38.46 | 8 | 507 | 237 | |
| Arkansas | 37.85 | 6 | 513 | 243 | |
| West Virginia | 36.33 | 5 | 518 | 248 | |
| Idaho | 33.58 | 4 | 522 | 252 | |
| Oklahoma | 33.23 | 7 | 529 | 259 | |
| Wyoming | 28.84 | 3 | 532 | 262 | |
| Utah | 25.37 | 6 | 538 | 268 | |

**Table II: Battleground States by Presidential Election (1976 – 2012)**

This table contains a list of battleground states as determined by state voting in the indicated presidential election cycle. The method for determining whether a state was a swing state for a given presidential election is outlined in Section 3.1 and illustrated in Table I.

| State | 1976 | 1980 | 1984 | 1988 | 1992 | 1996 | 2000 | 2004 | 2008 | 2012 |
|---|---|---|---|---|---|---|---|---|---|---|
| Alaska | | | | | | | | | | |
| Alabama | | | | | | | | | | |
| Arkansas | | | | | | | | | | |
| Arizona | | | | | | | | | | |
| California | | | X | X | | X | | | | |
| Colorado | | | | X | X | | | X | X | X |
| Connecticut | | X | X | X | X | | | | | |
| Delaware | | | X | X | X | | | | | |
| Florida | | | | | | | X | X | X | X |
| Georgia | | | X | | | | | | | |
| Hawaii | X | | | | | | | | | |
| Iowa | X | X | | | X | X | X | X | X | X |
| Idaho | | | | | | | | | | |
| Illinois | | X | | | | | | | | |
| Indiana | | | | | | | | | | |
| Kansas | | | | | | | | | | |
| Kentucky | | | X | X | X | | | | | |
| Louisiana | | | | X | X | X | | | | |
| Massachusetts | | | | | | | | | | |
| Maryland | | | | | | | | | | |
| Maine | X | | X | X | X | | | | | |
| Michigan | | X | X | X | X | | X | X | | X |
| Minnesota | | | | | | | X | X | X | X |
| Missouri | X | X | X | X | | X | X | X | | |
| Mississippi | X | | | | | | | | | |
| Montana | | | | X | X | | | | | |
| North Carolina | | | | | | | | | | |
| North Dakota | | | | | | | | | | |
| Nebraska | | | | | | | | | | |
| New Hampshire | | | | | | X | X | X | X | X |
| New Jersey | | | X | | X | | | | X | |
| New Mexico | | | X | X | X | X | X | X | X | |
| Nevada | | | | | X | | X | X | X | X |
| New York | | | | | | | | | | |
| Ohio | X | X | X | X | | X | X | X | X | X |
| Oklahoma | X | | | | | | | | | |
| Oregon | X | X | | | | X | X | | | |
| Pennsylvania | X | X | | | X | X | X | X | X | X |
| Rhode Island | | | | | | | | | | |
| South Carolina | | | | | | | | | | |
| South Dakota | X | | | X | | | | | | |
| Tennessee | | | | | X | | X | | | |
| Texas | X | | | | | | | | | |
| Utah | | | | | | | | | | |
| Virginia | X | X | | | | | | | X | X |
| Vermont | | X | X | | | | | | | |
| Washington | | | | | | X | | | | |
| Wisconsin | X | | | | X | X | X | X | X | X |
| West Virginia | | | | | | | | | | |
| Wyoming | | | | | | | | | | |

**Table III: NPDES Industry and Facility Type Summary Statistics (1976 – 2014)**

This table contains descriptive data for the 288,490 unique NPDES facilities considered in this study. Panel A lists the number and proportion of facilities within each Fama-French 49 industry for the 127,467 unique facilities assigned a valid SIC code. Panel B lists the number and proportion of each facility type for the 189,097 unique facilities assigned a valid facility type code. This table is discussed in Section 3.2.1.

Panel A: Facility Industry Statistics

| Industry | Number of Facilities | Percent of Total | Industry | Number of Facilities | Percent of Total |
|---|---|---|---|---|---|
| Agriculture | 7,442 | 5.84 | Defense | 92 | 0.07 |
| Food products | 1,972 | 1.55 | Precious metals | 518 | 0.41 |
| Candy and soda | 218 | 0.17 | Non-metallic and metal mining | 5,555 | 4.36 |
| Beer and liquor | 111 | 0.09 | Coal | 1,771 | 1.39 |
| Tobacco products | 9 | 0.01 | Petroleum and natural gas | 3,009 | 2.36 |
| Recreation | 678 | 0.53 | Utilities | 5,995 | 4.70 |
| Entertainment | 691 | 0.54 | Communication | 41 | 0.03 |
| Printing and publishing | 70 | 0.05 | Personal services | 5,117 | 4.01 |
| Consumer goods | 542 | 0.43 | Business services | 2,554 | 2.00 |
| Apparel | 71 | 0.06 | Computers | 31 | 0.02 |
| Healthcare | 418 | 0.33 | Computer software | 5 | 0.00 |
| Medical equipment | 94 | 0.07 | Electronic equipment | 287 | 0.23 |
| Pharmaceutical products | 302 | 0.24 | Measuring and control equipment | 106 | 0.08 |
| Chemicals | 2,280 | 1.79 | Business supplies | 624 | 0.49 |
| Rubber and plastic products | 1,028 | 0.81 | Shipping containers | 344 | 0.27 |
| Textiles | 358 | 0.28 | Transportation | 5,697 | 4.47 |
| Construction materials | 7,962 | 6.25 | Wholesale | 5,567 | 4.37 |
| Construction | 25,855 | 20.28 | Retail | 2,978 | 2.34 |
| Steel works | 1,239 | 0.97 | Restaurants, hotels, and motels | 764 | 0.60 |
| Fabricated products | 914 | 0.72 | Banking | 37 | 0.03 |
| Machinery | 1,166 | 0.91 | Insurance | 21 | 0.02 |
| Electrical equipment | 293 | 0.23 | Real estate | 5,515 | 4.33 |
| Automobiles and trucks | 574 | 0.45 | Trading | 25 | 0.02 |
| Aircraft | 167 | 0.13 | Other (incudes sewage & irrigation) | 26,071 | 20.45 |
| Shipbuilding and railroad | 289 | 0.23 | Unknown | 161,023 | |

Panel B: Facility Type Statistics

| Facility Type | Number of Facilities | Percent of Total | Facility Type | Number of Facilities | Percent of Total |
|---|---|---|---|---|---|
| County government | 1,309 | 0.69 | Mixed ownership (public/private) | 2,332 | 1.23 |
| Corporation | 10,788 | 5.71 | Non-government | 3 | 0.00 |
| Municipality | 8,199 | 4.34 | Privately owned | 135,025 | 71.41 |
| District | 2 | 0.00 | School district | 152 | 0.08 |
| Federal | 2,121 | 1.12 | State government | 5,362 | 2.84 |
| Gov owned/contractor operated | 5 | 0.00 | Tribal government | 180 | 0.10 |
| Individual | 704 | 0.37 | Unknown | 99,393 | |
| Municipal or water district | 22,915 | 12.12 | | | |

## Table IV: Summary Statistics (1976 – 2014)

This table contains summary statistics for violation rates of EPA facilities. Panel A contains summary statistics for the subsample of facilities located in battleground states that are no more than 25 miles away from a facility located in a non-battleground state with the same 3-digit SIC code (and vice versa). Panels B, C, and D contains summary statistics for the full sample, industrial facilities (those with a valid SIC code), and for non-industrial facilities (those without a valid SIC code), respectively. Battleground state status is determined by the outcome of the most recent presidential election. Differences between the violation rates in battleground states and non-battleground states for each sample are also reported along with *t*-stats. This table is discussed in Sections 3.2.2 and 4.

Panel A: Border Facilities

| | All Observations ($N = 123,514$) | | Battleground States ($N = 61,164$) | | Non-Battleground States ($N = 62,350$) | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation | Difference | *t*-stat |
| All violations | 0.169 | 0.375 | 0.109 | 0.312 | 0.228 | 0.420 | −0.119 | −56.5 |
| Effluent violations | 0.117 | 0.321 | 0.076 | 0.265 | 0.157 | 0.364 | −0.081 | −44.8 |
| Compliance schedule violations | 0.019 | 0.137 | 0.015 | 0.120 | 0.024 | 0.152 | −0.009 | −11.7 |
| Single event violations | 0.021 | 0.144 | 0.016 | 0.125 | 0.026 | 0.160 | −0.011 | −12.9 |
| Permit schedule violations | 0.044 | 0.205 | 0.020 | 0.141 | 0.067 | 0.250 | −0.047 | −40.6 |

Panel B: Full Sample

| | All Observations ($N = 2,746,253$) | | Battleground States ($N = 856,757$) | | Non-Battleground States ($N = 1,889,496$) | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation | Difference | *t*-stat |
| All violations | 0.115 | 0.319 | 0.078 | 0.268 | 0.132 | 0.338 | −0.054 | −142.2 |
| Effluent violations | 0.075 | 0.264 | 0.056 | 0.229 | 0.084 | 0.278 | −0.029 | −89.3 |
| Compliance schedule violations | 0.020 | 0.141 | 0.007 | 0.082 | 0.026 | 0.160 | −0.019 | −132.5 |
| Single event violations | 0.008 | 0.092 | 0.005 | 0.073 | 0.010 | 0.099 | −0.004 | −41.0 |
| Permit schedule violations | 0.026 | 0.160 | 0.019 | 0.137 | 0.030 | 0.169 | −0.011 | −54.9 |

Panel C: Industrial Facilities

| | All Observations ($N = 1,570,146$) | | Battleground States ($N = 393,356$) | | Non-Battleground States ($N = 1,176,790$) | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation | Difference | *t*-stat |
| All violations | 0.146 | 0.353 | 0.098 | 0.297 | 0.163 | 0.369 | −0.065 | −111.0 |
| Effluent violations | 0.105 | 0.307 | 0.063 | 0.244 | 0.119 | 0.324 | −0.055 | −113.2 |
| Compliance schedule violations | 0.021 | 0.142 | 0.012 | 0.110 | 0.023 | 0.151 | −0.011 | −50.1 |
| Single event violations | 0.013 | 0.113 | 0.011 | 0.103 | 0.014 | 0.116 | −0.003 | −14.4 |
| Permit schedule violations | 0.030 | 0.170 | 0.024 | 0.154 | 0.032 | 0.175 | −0.007 | −24.4 |

Panel D: Non-Industrial Facilities

| | All Observations ($N = 1,176,107$) | | Battleground States ($N = 463,401$) | | Non-Battleground States ($N = 712,706$) | | | |
|---|---|---|---|---|---|---|---|---|
| | Mean | Standard Deviation | Mean | Standard Deviation | Mean | Standard Deviation | Difference | *t*-stat |
| All violations | 0.073 | 0.260 | 0.061 | 0.239 | 0.081 | 0.273 | −0.020 | −42.5 |
| Effluent violations | 0.035 | 0.185 | 0.049 | 0.216 | 0.027 | 0.161 | 0.022 | 60.1 |
| Compliance schedule violations | 0.020 | 0.139 | 0.002 | 0.047 | 0.031 | 0.173 | −0.029 | −132.2 |
| Single event violations | 0.002 | 0.050 | 0.001 | 0.029 | 0.004 | 0.059 | −0.003 | −32.3 |
| Permit schedule violations | 0.022 | 0.146 | 0.015 | 0.120 | 0.026 | 0.160 | −0.012 | −46.0 |

**Table V: Battleground States and the Probability of an NPDES Violation (1976 – 2014)**

This table contains the results of panel regressions in which the independent variable is one if a facility is found to have at least one violation in a given year and zero otherwise. Column (1) examines the sample of border facilities, or those facilities in non-battleground states that are no more than 25 miles away from a facility in a battleground state with the same 3-digit SIC code (and vice versa). Column (2) examines the full sample, while columns (3) and (4) include industrial and non-industrial facilities (those with valid SIC codes and those without, respectively). The independent variable of interest is a dummy variable equal to one if the facility is located in a battleground state (as determined by the most recent presidential election) and zero otherwise. The models in Panel A include industry-by-year fixed effects, while the models in Panel B includes state and industry-by-year fixed effects, and the models in Panel C include facility and industry-by-year fixed effects. The models in Panel C also includes a number of state-level control variables: the natural log of the number of electoral votes for each state, the state-level unemployment rate, and the average per-capital income (in 1000s), and changes to these variables. Standard error estimates are adjusted for two-way clustering by state and year, and *t*-statistics are reported in parenthesis, with * and ** representing statistical significance at the 0.10 and 0.05 levels, respectively. This table is discussed in Section 4.4.

| | Border Facilities (1) | Full Sample (2) | Industrial Facilities (3) | Non-Industrial Facilities (4) |
|---|---|---|---|---|
| **Panel A: Industry-by-Year Fixed Effects** | | | | |
| Battleground state | −0.131** | −0.052 | −0.087** | −0.018 |
| | (−3.02) | (−1.59) | (−2.38) | (−0.35) |
| Observations | 123,514 | 2,746,253 | 1,570,146 | 1,176,107 |
| R-squared | 0.144 | 0.112 | 0.143 | 0.006 |
| **Panel B: State and Industry-by-Year Fixed Effects** | | | | |
| | (1) | (2) | (3) | (4) |
| Battleground state | −0.067** | −0.026 | −0.045* | 0.004 |
| | (−1.96) | (−1.41) | (−1.87) | (0.12) |
| Observations | 123,514 | 2,746,253 | 1,570,146 | 1,176,107 |
| R-squared | 0.208 | 0.167 | 0.190 | 0.104 |
| **Panel C: Facility and Industry-by-Year Fixed Effects** | | | | |
| | (1) | (2) | (3) | (4) |
| Battleground state | −0.059** | −0.038** | −0.050** | −0.014 |
| | (−2.21) | (−2.01) | (−2.21) | (−0.53) |
| ln(state population) | −0.033 | −0.132 | −0.021 | −0.648** |
| | (−0.09) | (−0.67) | (−0.10) | (−2.24) |
| State unemployment | 0.006 | 0.000 | −0.002 | 0.004 |
| | (0.79) | (0.00) | (−0.32) | (0.46) |
| State per-capita income (1000s) | −0.006 | −0.006 | −0.006 | −0.002 |
| | (−0.71) | (−0.92) | (−0.96) | (−0.29) |
| Δ ln(state population) | −0.007 | 0.002 | −0.001 | 0.004 |
| | (−0.34) | (0.55) | (−0.14) | (1.01) |
| Δ State unemployment | 0.004 | 0.001 | 0.002 | −0.001 |
| | (0.68) | (0.32) | (0.39) | (−0.19) |
| Δ State per-capita income (1000s) | 0.002 | 0.001 | 0.002 | −0.001 |
| | (0.93) | (0.63) | (0.88) | (−0.39) |
| Observations | 123,514 | 2,746,253 | 1,570,146 | 1,176,107 |
| R-squared | 0.496 | 0.472 | 0.536 | 0.541 |

**Table VI: Alternative Battleground State Classifications and NPDES Violations (1976 – 2014)**

This table contains results for panel regressions involving border samples which consider alternative specifications of battleground states, namely those classified as such by Strömberg (2008), Shaw (1999, 2006), and the website RealClearPolitics.com. These alternative specifications cover time periods that differ from the full period considered in this study (shown in the second row below). The dummy variable "alternative battleground" uses these alternative battleground state classifications while the dummy variable "battleground" uses the method described in Section 3.1. The regression models mirror those in Panel A of Table V, and includes industry-by year fixed effects in addition to the variable of interest. Standard error estimates are adjusted for two-way clustering by state and year, and *t*-statistics are reported in parenthesis, with * and ** representing statistical significance at the 0.10 and 0.05 levels, respectively. This table is discussed in Section 4.5.

| | Strömberg (2008) (2001 – 2008) | | Shaw (1999, 2006) (1989 – 2008) | | Real Clear Politics (2005 – 2014) | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Battleground | −0.169** | | −0.124** | | −0.210** | |
| | (−2.47) | | (−2.60) | | (−2.88) | |
| Alternative battleground | | −0.187** | | −0.121** | | −0.126** |
| | | (−3.30) | | (−3.02) | | (−2.32) |
| Observations | 37,604 | 31,903 | 71,828 | 71,845 | 49,374 | 44,221 |
| R-squared | 0.124 | 0.139 | 0.110 | 0.113 | 0.150 | 0.117 |

**Table VII: Battleground States and the Probability of an NPDES Violation with Interaction Effects (1976 – 2014)**

This table contains the results of panel regressions in which the independent variable is one if a facility is found to have at least one violation in a given year and zero otherwise. All regressions examine the border facilities sample which includes those facilities in non-battleground states that are no more than 25 miles away from a facility in a battleground state with the same 3-digit SIC code (and vice versa). Model (1) includes state and year fixed effects, model (2) includes facility and year fixed effects, and model (3) includes facility fixed effects and industry-by-year fixed effects, as well as the control variables in Table V. Panel A includes a battleground state dummy variable for frequent and infrequent battleground states. We classify a state as being a "frequent" battleground state if the number of times it was a battleground state in our sample was above the median. Panel B includes an interaction between the battleground state dummy variable and dummy variables that indicate whether the president in a given period is a Democrat or a Republican. Panel C includes an interaction between the battleground state dummy and dummy variables that indicate whether a given year falls in the first, second, third, or fourth year of a four-year presidential election cycle (and where the fourth year corresponds to the election year). Standard error estimates are adjusted for two-way clustering by facility and year, and *t*-statistics are reported in parenthesis, with * and ** representing statistical significance at the 0.10 and 0.05 levels, respectively. This table is discussed in Section 4.7.

| Panel A: Battleground State Effects by Battleground State Frequency | | | |
|---|---|---|---|
| | (1) | (2) | (3) |
| Battleground state × frequent | −0.164** | −0.138** | −0.101** |
| | (−3.58) | (−2.93) | (−2.04) |
| Battleground state × infrequent | −0.071* | −0.038 | −0.039* |
| | (−1.76) | (−1.01) | (−1.69) |
| R-squared | 0.169 | 0.228 | 0.528 |
| **Panel B: Battleground State Effects by President Party Affiliation** | | | |
| | (1) | (2) | (3) |
| Battleground state × Republican president | −0.119** | −0.062 | −0.067** |
| | (−2.53) | (−1.64) | (−2.33) |
| Battleground state × Democrat president | −0.143** | −0.074** | −0.046 |
| | (−3.36) | (−2.02) | (−1.40) |
| R-squared | 0.145 | 0.208 | 0.522 |
| **Panel C: Battleground State Effects by Year in Presidential Cycle** | | | |
| | (1) | (2) | (3) |
| Battleground state × year 1 | −0.140** | −0.074** | −0.060** |
| | (−3.31) | (−2.09) | (−2.16) |
| Battleground state × year 2 | −0.136** | −0.071* | −0.058** |
| | (−3.03) | (−1.93) | (−2.00) |
| Battleground state × year 3 | −0.124** | −0.062* | −0.057* |
| | (−2.84) | (−1.73) | (−1.89) |
| Battleground state × year 4 | −0.124** | −0.062 | −0.062** |
| | (−2.69) | (−1.56) | (−1.96) |
| R-squared | 0.145 | 0.208 | 0.521 |
| **All Panels** | | | |
| Control variables | | | X |
| Industry-by-year fixed effects | X | X | X |
| State fixed effects | | X | |
| Facility fixed effects | | | X |
| Observations | 123,514 | 123,514 | 123,514 |

**Table VIII: Battleground State Effects and State or EPA Administered NPDES Program (1976 – 2014)**

This table contains results for panel regressions involving the industrial subsample for states to which the EPA has delegated the administration of the NPDES program to state regulators as well as those states in which the EPA administers the NPDES program directly. The independent variable of interest is a dummy variable equal to one if the facility is located in a battleground state (as determined by the most recent presidential election) and zero otherwise. The models in columns (1) and (2) include industry-by-year fixed effects, and the models in columns (3) and (4) add state fixed effects. Finally, the models used in columns (5) and (6) include facility fixed effects, industry-by-year fixed effects, as well as state-level control variables. Standard error estimates are adjusted for two-way clustering by state and year, and *t*-statistics are reported in parenthesis, with * and ** representing statistical significance at the 0.10 and 0.05 levels, respectively. This table is discussed in Section 4.8.

| | State NPDES | EPA NPDES | State NPDES | EPA NPDES | State NPDES | EPA NPDES |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Battleground | −0.095** | 0.018 | −0.058** | 0.042** | −0.064** | 0.023 |
| | (−2.55) | (0.76) | (−2.21) | (2.56) | (−2.51) | (1.16) |
| ln(state population) | | | | | −0.087 | −0.442** |
| | | | | | (−0.34) | (−3.12) |
| State unemployment | | | | | 0.001 | −0.008 |
| | | | | | (0.24) | (−1.48) |
| State per-capita income (1000s) | | | | | −0.007 | 0.014** |
| | | | | | (−0.97) | (2.56) |
| Δ ln(state population) | | | | | 0.003 | −0.014** |
| | | | | | (0.52) | (−2.15) |
| Δ State unemployment | | | | | 0.002 | −0.001 |
| | | | | | (0.53) | (−0.22) |
| Δ State per-capita income (1000s) | | | | | 0.001 | −0.001 |
| | | | | | (0.53) | (−0.63) |
| Industry-by-year fixed effects | X | X | X | X | X | X |
| State fixed effects | | | X | X | | |
| Facility fixed effects | | | | | X | X |
| Observations | 1,455,109 | 115,037 | 1,455,109 | 115,037 | 1,455,109 | 115,037 |
| R-squared | 0.143 | 0.264 | 0.191 | 0.287 | 0.466 | 0.529 |