The "Gorilla in the Closet": Regulatory Enforcement Under Federalism *

Jenna Anders
Romaine Campbell

October 30, 2023

Please click here for the latest version.

Abstract

How does federal regulatory capacity affect state enforcement outcomes? We provide a model in which a stronger federal regulatory agency can either strengthen or weaken states' negotiating position with their regulated entities. The optimal federal enforcement for the states is one that maximizes state-level negotiated penalties. We apply the model's insights in the context of environmental regulation to test whether the US Environmental Protection Agency (EPA) is too strong or too lenient in two environmental programs: the Clean Air Act and Superfund. First, using an EPA database of state-issued penalties, we show that when EPA's budget was cut in 2011, state-issued penalties for Clean Air Act violations shrank by 15%. Second, using a dataset with information about environmental remediation projects under state jurisdiction, we show that firms are more likely to begin cleanup projects during Democratic federal administrations. Our remediation analysis identifies the mechanism: while firm cleanup behavior is affected by EPA strength, cleanups conducted by the state are not, providing evidence that the effects operate through firm-state bargaining. We conclude that over one third of EPA's effect on environmental penalties is through its spillovers to state enforcement outcomes, and that states would benefit from a stronger EPA.

*Contact: janders@g.harvard.edu. We are grateful for guidance from Nathan Hendren, Larry Katz, and Jesse Shapiro. We also thank Joe Aldy, Kirill Borusyak, Steve Cicala, John Conlon, Zoe Cullen, Liz Engle, Ed Glaeser, Claudia Goldin, Benny Goldman, Emma Harrington, Oliver Hart, Sarah Jacobson, Casey McQuillan, Amanda Pallais, Dev Patel, and Hilary Sigman, Charlie Rafkin, Melanie Rucinski, Kim Sarnoff, Brit Sharoni, Anthony Yu, and participants in Harvard's public economics and environmental economics workshops. The project would not have been possible without generative input and guidance from David Hindin. We enjoyed many helpful and interesting conversations with current and former staff members at US EPA and state environmental agencies, and we are grateful they took time away from their important work to share their expertise with us. We thank MC Hanafee LaPlante and Nikolas Kirk for excellent research assistance. Finally, we gratefully acknowledge financial support from the National Science Foundation and the Chae Family Economics Research Fund. All errors are our own.

"The [Environmental Protection Agency] would ... act as a 'gorilla in the closet' for the cities and states to use to frighten polluters into submission. State regulators had long wished for a federal agency to play this role." - The Guardian: EPA's Formative Years[1]

1 Introduction

In the United States, executive regulatory agencies in the federal government (Department of Labor, Department of Transportation, etc.) often have counterpart agencies within the states that regulate similar domains. This overlapping jurisdiction may seem inefficient: why duplicate efforts? In this paper, we present a model of overlapping jurisdiction and characterize when and how a stronger federal government can increase welfare. The key idea is that the federal government provides a threat point for states to leverage when enforcing their own regulations. We consider the context of environmental protection, a setting with extensive overlapping jurisdiction: many environmental statutes can be enforced by either the United States Environmental Protection Agency (US EPA, or EPA) or states' environmental agencies.

The first administrator of the EPA, William Ruckelshaus, coined a term to describe the federal government's ability to affect state enforcement outcomes: "the gorilla in the closet." His idea was based on the reality that state authorities cannot unilaterally impose whatever penalties they'd like on violators of environmental statutes, but rather must negotiate penalties with firms. The EPA as "gorilla" would provide the states a federal agency to use to "frighten their polluters into submission" (EPA.gov, 1993).

The idea of the gorilla suggests that EPA is less concerned about (or less influenced by) regulatory costs to local firms and impacts on local economies than are states. Consistent with this idea, we find in data on enforcement actions that state penalties respond to local economic conditions but EPA penalties do not. With this evident discrepancy in regulator preferences, are current federal institutions helpful to the states, and if so, how?

[1]"The Guardian" was an internal publication at the EPA with multiple installments recording the agency's history. This installment, written by Dennis C. Williams, can be found online at https://www.epa.gov/archive/epa/aboutepa/guardian-epas-formative-years-1970-1973.html.

We develop a model of the federal agency as "gorilla" and provide an empirical test for whether EPA is too harsh given the state's objective function.

In our model, we place federal enforcement in the context of a bargaining game between the state and the firm over environmental enforcement outcomes, where the state can threaten to hand over the enforcement case to EPA. As in the legal reality of delegated authority in environmental programs, EPA can, at some cost, threaten to sanction the state for unsatisfactory enforcement outcomes (e.g., imposing additional reporting requirements or even taking over enforcement in a state). EPA affects state penalties in two ways. First, its penalty serves as an outside option for the states, providing them with a threat point to use in negotiations with firms. Second, its threat of sanctions on the state expands the set of firm penalty offers the state will credibly reject.

The welfare impact on states of EPA strength is non-monotonic. At low levels of EPA enforcement, higher federal penalties improve the bargaining position of the state and increase penalty offers from firms. At high levels of EPA enforcement, EPA involvement becomes unattractive to the state, and the state accepts lower firm offers rather than send the firm to the EPA. This non-monotonic comparative static provides us with an empirical test with normative implications. Specifically, EPA strength is lower than the states' optimal level if and only if increases in EPA strength increase state penalties collected. By observing the effect of changes in EPA strength on state penalties, we can infer whether or not states would benefit from a stronger EPA.

We study variation in EPA strength in two environmental programs: the Clean Air Act (CAA) and Superfund. Our CAA analysis exploits federal budget cuts which reduced EPA's workforce, and our Superfund analysis exploits changes in presidential administrations.[2] The latter also provides additional evidence of the gorilla effect's mechanism: that it operates through firm-state negotiations and not state enforcement preferences.

We first estimate the effect of changes in EPA resources on penalties collected by states for violations under the CAA. We exploit US EPA agency budget cuts which led to a 15%

[2]Superfund involves a natural control group that CAA lacks for analyzing effects of presidential administrations; meanwhile, only CAA's frequency of outcomes enable analysis of the dramatic but one-time budget cuts.

reduction in the EPA workforce between 2011 and 2016. After the EPA's budget cuts, the number of federal formal enforcement actions issued for CAA violations decreased by almost 50%. Federal enforcement is largely conducted through EPA's 10 regional offices, which saw different reductions in their enforcement actions issued after the budget cuts. We exploit these differences across regional EPA offices in a differences-in-differences framework, where the outcome is *state* penalty size, from EPA's database of state-reported CAA penalty data. After the budget cut, in EPA regions which were more affected, state penalties decreased by more, even though EPA was not itself involved in these cases.[3] Our estimates suggest that halving the number of EPA's formal enforcement actions in a region reduces state penalty size by about 15% ($2,000) of average penalty size. That state penalties shrink suggests that EPA strength is below the states' optimal level.

Our second context is the federal and California Superfund programs, which compel companies liable for environmental contamination to clean it up. State cleanup programs are not under direct EPA oversight and so, unlike in CAA enforcement, there is no centralized federal data repository for these programs. We collect data from California, extracting information from hundreds of documents stored online and in physical records rooms to measure the speed of site clean-up and the estimated costs and environmental details of the cleanup projects.

These data span 30 years, allowing us to exploit a different source of variation in EPA strength: political party of the US presidential administration. At the federal level, EPA collects higher penalties during Democratic administrations than during Republican administrations, indicating EPA involvement is more costly for firms during Democratic administrations. The Superfund context also includes a control group: a subset of sites, called "orphan sites," do not have viable responsible parties to conduct cleanups, so the state cleans up the site itself instead of negotiating a cleanup with a firm. We combine these in a difference-in-difference analysis, using cleanup speed, a measure of firm coop-

[3] We provide evidence that the parallel trends assumption holds: before the budget cuts, (eventual) declines in regional office activity do not predict state outcomes. We see some evidence that there is also an extensive margin effect on the number of state penalties issued; we note that under reasonable assumptions about state enforcement behavior, this would lead us to underestimate the magnitude of the negative effect on average penalty size.

eration in this context, as our outcome.

We show that cleanups orchestrated by firms under state oversight are significantly less likely to begin major cleanup projects during Republicans presidencies, when the EPA is a more lenient enforcer; meanwhile there is no significant effect among orphan sites. With additional data extracted from cleanup project documentation, we provide suggestive evidence that firms also choose less expensive cleanup projects under Republican presidencies. Because we see firm cooperation increase and not decrease when the EPA becomes harsher, we conclude that the EPA is not too harsh for the states' liking. Moreover, as orphan sites' cleanup speed is an outcome of state behavior and not state-firm bargaining, the null result for orphan sites serves as validation of our modeling assumption that changes in state enforcement outcomes are not driven by states' fear of being sanctioned by EPA (which, if operative, would affect outcomes even in the absence of firm-state bargaining).

Together, our results show that federal agency strength matters for state outcomes. We can use our estimates from the Clean Air Act to ask how much of the EPA budget's effect on penalties collected comes from the EPA's own enforcement outcomes versus its spillover effects on the states. We find that over one third of the EPA budget's total effect on penalties comes from the "gorilla" effect.

Related literature. We provide insights into the consequences of a federalist government by incorporating the importance of firm bargaining power in regulatory outcomes. Prior literature cast the social benefit of the federal government as either to internalize spillovers across jurisdictions, to prevent a regulatory race-to-the-bottom, or to provide services more efficiently (Buchanan, 1950; Tiebout, 1961; Oates, 1972; Dijkstra and Fredrikssonn, 2010; Chang et al., 2014; Slatterey, 2022; Tang, 2022).[4] We show that the structure of federal and state policy can affect state's interactions with firms. We also propose a sufficient statistics test for whether the states would benefit from a stronger

[4]Several papers in the environmental federalism literature explicitly consider the role of decentralization in overall pollution levels (Sigman, 2002, 2005; Lipscomb and Mobarak, 2017). The broader federalism literature mostly focuses on the design of policy and not its implementation; we share a focus on regulatory *enforcement* with Woods (2006) and Konisky and Woods (2010).

federal government.

Our paper further relates to work estimating regulator preferences or providing evidence of different preferences across regulatory bodies (Jung and Makowsky, 2014; Lim and Yurukoglu, 2018; Earnhart and Frieson, 2021; Kang and Silveira, 2021; Tang, 2022). Our model adds to a smaller literature studying whether differences in regulator preferences can be beneficial to the regulators (Barro and Gordon, 1983; Rogoff, 1985; Gutiérrez and Philippon, 2019). Our sufficient statistics test, which allows us to map reduced form empirical results to welfare implications, brings to this literature methods from public economics which are usually applied to questions of optimal taxation (Baily, 1978; Chetty, 2006, 2009; Piketty et al., 2014; Allcott et al., 2014; Hendren, 2016, 2021; Farhi and Gabaix, 2020; Kleven, 2021).

Finally, we bring new data to and ask new questions in the environmental enforcement literature. Our dataset of California's Superfund program allows us to identify a novel factor in the efficacy of a large state-run program.[5] The Clean Air Act is a more established setting for studies of enforcement (e.g. Evans, 2016; Evans et al., 2018; Blundell et al., 2020), but there is little empirical analysis on the EPA as a "gorilla." We believe the closest papers to ours are Evans and Stafford (2019) and Blundell (2020), which both study formal and informal sanction threats EPA has used to affect state enforcement behavior.[6] Relative to these papers, we propose and identify the role of state bargaining with firms as a mechanism for the effect of EPA oversight, and we propose welfare implications.

The paper proceeds as follows. Section 2 describes the regulatory context. Section 3 provides the model. Section 4 describes the data we use. Sections 5 and 6 present our empirical analyses of state Clean Air Act penalties and California Superfund cleanups, respectively. Section 7 concludes.

[5]Prior Superfund work documents substantial health effects of Superfund cleanups (Currie et al., 2011; Persico et al., 2022) and has mixed evidence on capitalization of benefits (Greenstone and Gallagher, 2008; Gamper-Rabindran and Timmins, 2011; Gamper-Rabindran et al., 2011). Given the program's unusually punitive enforcement, there is also an extensive legal literature on Superfund. Mintz (1988) specifically discusses the role of political leadership at the EPA in Superfund implementation in the 1980s.

[6]Evans and Stafford (2019) show that when the EPA published a "Watch List" which identified high-priority violators, state CAA enforcement activity increased for facilities at risk of Watch List listing. Blundell (2020) shows that when EPA determined Florida's CAA enforcement on certain facilities to be lacking, compliance improved among these facilities.

2 Regulatory context

We study two environmental programs: the Clean Air Act, which regulates air emissions from currently operating facilities, and federal and state Superfund programs, which oversee cleanup of environments contaminated with hazardous substances. In this section, we begin with overviews of each program. Then, we highlight one distinction which is particularly relevant in our context: the Clean Air Act is a delegated statute and Superfund is not, meaning the US EPA has more authority to oversee state enforcement in the Clean Air Act than in hazardous substances cleanups.

2.1 Clean Air Act

The Clean Air Act (CAA), passed in 1970 and amended in 1990, is a multifaceted statute. We focus on the CAA's regulation of stationary sources (also known as point sources) from 2002 to 2019. Under CAA, stationary sources are subject to requirements on pollution control equipment and operating permits, and they additionally have emissions limits. Both excess emissions and procedural noncompliance constitute violations which can be penalized.

Our analysis uses the size of CAA penalties issued by the states as our main outcome. Penalties are largely determined by the economic benefit of noncompliance (to achieve a deterrence effect) and the gravity of the violation (EPA, 1991). The gravity of the violation is not only limited to the extent of possible emissions exceedance, but can also include other considerations, such as a company's net worth. EPA also explicitly allows for adjustments for "public interest" (to avoid plant closings and bankruptcies) and "litigation risk" (admitting lower penalties when the court case is weaker). In many cases, each individual day of noncompliance constitutes a separate offense, so that the final penalty can depend on the duration of noncompliance.

The Clean Air Act is a federal statute but, like many federal statutes created in the 1970s, allows for enforcement authority to be delegated the states. States authorized with "primacy" in their enforcement are the primary entity responsible for enforcement of the federal law (Norwood, 2015). All fifty states currently have CAA primacy for Title V sources. States conduct over 95% of inspections and issue over 80% of formal enforcement actions for violations.[7]

States can, and do, ask EPA for support and assistance on cases "when the weight of the EPA is needed" (Earnhart and Frieson, 2021). This can mean collaborating on an enforcement action, or alternatively asking EPA to handle a case. EPA also retains the right to enforce independently. For example, the EPA "does not delegate ... the authority to make decisions that are likely to be nationally significant."[8] US EPA also has direct jurisdiction over some facilities (e.g., in Indian country, federally-owned facilities, etc.).

CAA enforcement actions brought by the US EPA are largely brought through the 10 regional offices. Regional offices are relatively independent: they have different organizational structures, different priorities, and different enforcement cultures (Fiorino, 1995; Engelberg et al., 2011).

2.2 Superfund

The federal Comprehensive Environmental Response, Compensation, and Liability Act (CERCLA) was passed in December of 1980. In this project, features of the federal program are relevant because it provides a threat California enforcers can use in their dealings with firms; our outcomes come from the California counterpart to the federal Superfund program.

The federal Superfund program. The CERCLA Congressional bill created a trust fund (the "Superfund"), funded through appropriations and earmarked corporate taxes, for the US EPA to use for site cleanup and enforcement actions against parties responsible for contamination. Initially, the idea of the Superfund was to allow EPA to move freely with expensive remediation projects, funded through the trust fund, before recovering their costs from liable parties (EPA.gov, 2005b). In 1986, however, the program shifted to an "enforcement first" approach (EPA.gov, 2005a), in which EPA uses its enforcement

[7] Authors' calculations using data from EPA Enforcement and Compliance History Online (ECHO).
[8] https://www.epa.gov/caa-permitting/delegation-clean-air-act-authority.

power to compel companies to conduct the cleanups themselves. Regardless, the EPA cannot use the Superfund money for (non-emergency) cleanup actions unless a site is listed on the National Priorities List (NPL). Once a site is on the NPL, CERCLA grants the federal government extensive power.[9]

State Superfund programs. Many states run their own Superfund programs modeled after the federal program, complete with their own Superfund trust funds allocated by the state legislature.[10] Upon discovery of a contaminated site, these states will often attempt to address the problem in a similar fashion to the approach in the federal Superfund program. These programs are not under federal oversight but address environmental issues that the federal Superfund program could also address.

States exploit this overlapping jurisdiction in their dealings with liable parties. In Figure 1, we show two letters (found in Maine and California state agency records rooms) from states to parties liable for contamination in which the states explicitly threaten to refer sites to the US EPA if the liable parties are not cooperative enough (i.e., they are not cleaning up the site quickly enough or to the appropriate extent). In our analyses, we consider how the strength of this threat affects the speed of cleanups in the California state Superfund program.[11]

Federal non-emergency Superfund-funded response actions must be approved by state governors.[12] Thus, the threat of federal involvement in a contaminated site is moderated to a substantial degree by the state.

[9]The EPA generally tries to negotiate agreements with companies wherein the company agrees to remediate the site. However, if the company is unwilling to negotiate, the EPA can issue unilateral orders, and if the company does not abide by these orders, the EPA can sue them in court for damages and penalties. Liability under CERCLA has unusually broad scope, and courts can order triple damages (fines up to three times the costs incurred by EPA).

[10]In 2001, all states had state laws enabling cleanup enforcement, but only 24 were funded by legislative appropriations (Environmental Law Institute, 2002).

[11]As put in a testimony made in a 2002 Congressional hearing on behalf of the U.S. Public Interest Research Group: "The success of... state programs heavily depends on the Federal Superfund program providing a credible deterrent against polluters who refuse to clean up sites under state programs" (US Senate, 2002). Among other reasons, firms may prefer state enforcement to federal because they believe it will involve lower transaction costs and a smaller decrease in property values, or because they believe they will have more influence over the state process (Environmental Law Institute, 1990, p. 59-60).

[12]CERCLA §9611(h)(i)

2.3 Sanctions

In Section 3, we introduce a model of the gorilla whereby the EPA can, at a cost, threaten to sanction states for unsatisfactory enforcement of environmental statutes. The sanctions available to the EPA differ by program.

Clean Air Act sanctions: The Clean Air Act is a delegated environmental program. EPA's methods of imposing costs on states with unsatisfactory implementation of delegated programs include withholding grant funds, increasing oversight of state processes and decisions, and revoking state primacy (U.S. EPA, 1984; Engelberg et al., 2011). EPA can also issue its own enforcement actions against specific facilities (called "overfiling") if it doesn't consider the state to be taking timely action against noncompliance. In each of these sanctions options, policy guidance is to take a "constructive approach" and, before taking action, "give the state a chance to explain and/or correct" problems that might otherwise result in sanctions.[13]

Superfund sanctions: As the state Superfund programs are independent programs and not delegated federal programs, the EPA's recourse against lenient states is more limited in this context. The most realistic option for "sanctions," broadly understood is through reduced funding of grants supporting state remediation (cleanup) efforts. The EPA can also initiate emergency actions, including restricting land use and suing firms, without explicit state consent.

3 Model

The model casts EPA involvement in a case as the states' outside option when negotiating penalties with firms.[14] The states can benefit from an EPA which has different preferences from its own, and specifically, an EPA which issues larger penalties than the states do.

[13]McCarthy (1997) collected data on EPA sanctions for inadequate state plans to attain ambient air quality standards and found that between 1990 and 1997, EPA imposed additional offset requirements 14 times and withheld a portion of state highway funds twice. Overfiling is also understood to be rare in practice (US Senate, 1997).

[14]In a 1990 survey of state CAA directors, 89% agreed with the statement that "[the] threat of EPA intervention strengthens state position with polluters." (Tobin, 1992)

Even so, it is possible for EPA to be *too* harsh, as states will shy away from involving it in enforcement if it carries too big a stick. The model provides a sufficient statistics test for whether EPA is too harsh or too lenient: we show that if (and only if) a stronger EPA raises state penalties, then the EPA is weaker than the states would like.

In reality, EPA has another tool for affecting state penalties: it can threaten to sanction states for unsatisfactory enforcement outcomes. In this section, after setting up the model, we first derive results when EPA's costs of sanctions are infinite, essentially depriving it of this tool, to make clear the key intuition in the model. We then reduce EPA's cost of sanctions, show that this introduces a threat to our normative conclusions, and show how our empirical analysis addresses this threat.

3.1 Set-up

There are three actors: EPA, a representative state, and a representative firm. There is a set of environmental violations by firms determined in the base period. The actors interact to decide a penalty size[15] for the enforcement case of a given violation v.

The firm commits a violation if its benefit from violating exceeds a randomly drawn cost. Once the violation is established, the firm offers a penalty to the state, and the state either accepts it or sends the case to the EPA. Let $I_S = 1$ if the state issues the penalty (i.e., accepts the firm's offer), and 0 if EPA issues the penalty. Let p denote penalty size; $p = p_{s_v}$ when the state issues the penalty, and $p = p_e$ when the EPA issues the penalty. For each case, the state has a uniformly drawn comparative advantage in enforcement $\zeta_v \sim U[\underline{\zeta}_v, \overline{\zeta}_v]$, with $\overline{\zeta}_v > 0$. This comparative advantage could reflect, for example, states' specialization in the environmental issues most relevant to their geographic and demographic characteristics. ζ_v could also represent the state's political cost of involving the EPA.

Finally, EPA has the ability to threaten to sanction the state. In reality, EPA has several sanctioning tools at its disposal which vary in the costs the sanctions impose on the state.

[15] In the model, we use penalty size as the enforcement outcome. However, this could be thought of as any enforcement outcome that is costly to the firm and still beneficial for the state. In our Superfund context, we use cleanup as the enforcement outcome.

At the extreme, it can revoke the state's enforcement primacy. Examples of lesser sanctions include imposing additional reporting requirements and requiring federal review of state enforcement actions.[16] Regardless of the sanction, EPA policy is to give the state an opportunity to correct their behavior before EPA imposes sanctions on the state. In our model, EPA must pay a cost to threaten sanctions, even before implementing them. If EPA chooses to threaten the state with sanctions, it chooses a level k of sanctions to threaten, which costs EPA $c(k)$. We assume costs of sanction threats are linear: $c(k) = ck$.

3.1.1 Agents' preferences and technology

The firm is a cost minimizer. It draws a compliance cost $\eta_v \sim F$ and violates if its compliance cost exceeds its expected penalty, which is $-I_S p_{s_v} - (1 - I_S) p_e$.

The state has strictly concave utility over penalty size. For generic penalty size p, it trades off the environmental benefits of a penalty $b(p)$ (deterrence) with the economic harm that issuing the penalty will incur, $\tau(p)$. We assume $b'(p) > 0, b''(p) < 0$ and $\tau'(p) > 0, \tau''(p) > 0$.

Specifically, the utility of the state from penalty size p and sanctions $k(p_{s_v})$ is

$$U_S(p, k) = b(p) - \tau(p) + I_S(\zeta_v - k).$$

Denote the state's preferred penalty

$$p_s^* = \arg\max b(p) - \tau(p).$$

The EPA's utility from penalty size a is similar, although it pays a cost of imposing sanctions and it weighs economic harms of enforcement differently than the states so. It also receives ζ_v if the state issues the penalty, and not otherwise.

$$U_E(p, k) = b(p) - \beta\tau(p) - ck + I_S\zeta_v.$$

[16] https://www.epa.gov/sites/default/files/2014-06/documents/state-oversight-strategy.pdf

Denote EPA's preferred penalty

$$p_e^* = \arg\max b(p) - \beta\tau(p).$$

If $\beta < 1$, then EPA will prefer a penalty larger than the states do.

Technology. EPA has a workforce N which allows it to get some share $\sigma(N)$ of its preferred penalty, with $\sigma(N) \in [0,1]$, $\sigma'(N) > 0$, for cases it handles.

EPA strength. When we refer to EPA strength, we are referring to the determinants (N, β) of the penalty EPA issues for cases it handles. A "stronger" EPA is one that would issue a higher penalty, either because of a larger workforce N or because of a lower weight β on economic harm.

3.1.2 Timing

Since states have enforcement primacy in our context, our model allows the state to choose whether it or EPA issues the penalty. If the state handles the case, the state must accept whatever penalty the firm offers, and whatever sanctions EPA threatened. If EPA handles the case, EPA unilaterally issues its own penalty.

The sequence of actions taken about a given violation v is:

0. The firm draws η_v and decides whether to commit a violation. If it does, then...

1. The firm makes a penalty offer p_{s_v} to the state.

2. The EPA pays $c \cdot k$ to issue sanction threat k.

3. The state either accepts the firm's offer p_{s_v}, or it rejects the offer and sends the case to EPA.

4. If the state sends the case to EPA, EPA chooses p_e^* and issues penalty $p_e = \sigma(N)p_e^*$.

There are a few features the timing worth elaborating on. First, our assumption that EPA chooses its sanctions after the firm's offer reflects the fact that EPA can adjust its sanctions depending on the firm's offer. It also gives a more realistic equilibrium outcome than a model where EPA chooses sanctions before firm offers: with this alternative timing, EPA always threatens sanctions, while in reality, sanctions are rarely threatened. That EPA threatens sanctions before states accept or reject an offer captures EPA's institutional policy to allow states an opportunity to correct their behavior if EPA believes enforcement is inadequate: official EPA policy dictates that EPA give states every opportunity to correct their enforcement behavior after warnings like sanction threats. In some cases, mandatory waiting periods (known as "sanction clocks") are codified in the environmental statute. Second, our assumption that EPA chooses its preferred penalty p_e^* last gives us that EPA does not internalize the effects of its preferred penalties on the state's decision.

3.2 Equilibrium

All proofs are in Appendix E.

Let $u_S(p) = b(p) - \tau(p)$ and $u_E(p) = b(p) - \beta\tau(p)$. That is, u represents only the portion of each agent's utility derived from environmental benefits and economic harm.

Since the state has enforcement primacy (i.e., it decides who handles the case), it will only allow EPA to take the case if the utility it would receive from EPA involvement is higher than the utility it would receive from accepting the firm's offer. The relative utility the state would get from rejecting vs. accepting the firm's offer depends on (1) its utility from EPA's penalty and (2) the threat of sanctions it faces.

Sanction threats increase state penalties by making EPA involvement a more credible threat to the firm—that is, by expanding the set of firm offers the state will credibly reject. If the EPA threatens sanctions, the state will have a good reason to reject penalty offers (since rejecting the offer will allow them to avoid sanctions). Firms know this, and raise penalty offers.

In equilibrium, firm offers ensure both (1) the state has at least as high utility from accepting the offer as rejecting it and (2) the EPA does not have an incentive to threaten the state with sanctions. This is true for firm offers $p_{\tilde{s}_v}$ such that

$$\underbrace{u_S(p_{\tilde{s}_v})}_{\text{State utility from firm offer}} \geq \underbrace{u_S(p_e) - \zeta_v}_{\text{Pure outside option value}} + \underbrace{\frac{u_E(p_e) - u_E(p_{\tilde{s}_v}) - \zeta_v}{c}}_{\text{Sanctions threat}} \tag{1}$$

As long as there exists at least one $p_{\tilde{s}_v}$ which satisfies Expression (1), the firm offers $p_{\tilde{s}_v} = \min\{p_{\tilde{s}_v}\}$, and the state accepts the firm's offer. For $\zeta_v < 0$ (when the EPA has a comparative advantage in enforcement on a given case), it is possible that there does not exist a $\tilde{s}_v$ which satisfies Expression (1). In this situation, regardless of the firm's offer, the state will reject the firm's offer and send the case to the EPA (given the EPA's sanction threats).

In reality, very few cases get sent by the state to the EPA. For this reason, going forward, we suppose $\overline{\zeta}_v > \underline{\zeta}_v > 0$.

In equilibrium, $k = 0$, and $I_S = 1$. The mass of violations are those satisfying $\eta_v > p_{\tilde{s}_v}$.

3.2.1 Without Sanctions ($c \to \infty$)

To illuminate the intuition behind our proposed test, we start by depriving EPA of sanction power, setting c arbitrarily high. Now, in Equation 1, firm offers are the minimum $p_{\tilde{s}_v}$ such that $u_S(p_{\tilde{s}_v}) = u_S(p_e) - \zeta_v$.

Our first result is that the state penalties will never exceed the state's preferred penalty p_{s^*}, regardless of EPA's penalty p_e.

Proposition 1. *For sufficiently high c, equilibrium state-issued penalties are lower than the state's preferred penalty ($p_{\tilde{s}_v} < p_s^*$) and EPA's penalty ($p_{\tilde{s}_v} \leq p_e$).*

The logic behind the first inequality is illustrated by Figure 2. Recall that the firm offers the lowest penalty possible that gives the state equal utility to EPA involvement. Because p_s^* maximizes the state's utility, any penalty offer higher than p_s^* has a corresponding penalty offer *lower* than p_s^* which gives the state equal utility. The firm will always choose this lower penalty offer.

That the state can never attain p_s^* comes from our assumption that $\zeta_v > 0$. Even if EPA's penalty is the state's preferred penalty ($p_e = p_s^*$), the firm can take advantage of the fact that the state prefers to handle the case itself, and can offer the state something lower. If $\zeta_v \leq 0$, the first inequality would be weak ($p_{\bar{s}_v} \leq p_s^*$).

The second inequality follows trivially from firm optimization: the firm gets no benefit from offering a penalty higher than p_e^*.

Our second result is that when increasing EPA strength increases state penalties, it is also increasing state welfare, and vice versa.

Proposition 2. *For sufficiently high c,* $\frac{dU_S}{dp_e} > 0$ *if and only if* $\frac{dp_{\bar{s}}}{d[\sigma(N)p_e^*(\beta)]} > 0$.

Notice in Figure 2 that there exists an EPA penalty such that strengthening EPA (i.e., increasing p_e further) strictly decreases penalty offers from firms, which in turn strictly decreases state welfare. With Proposition 1, we can conclude that an increase in EPA strength improves state welfare if and only if it increases state penalties.

3.2.2 With Sanctions ($c < \infty$)

Statutorily, EPA has an additional tool for affecting state penalties: it has oversight authority of state enforcement, and it can sanction states for unsatisfactory enforcement outcomes. Sanctions are rarely observed in reality. However, even when EPA's sanctions are off the equilibrium path, they can still affect firm penalty offers.[17]

In this section, we allow the reality of EPA sanction power.[18] EPA's sanction power makes an additional assumption necessary to maintain the conclusions of Propositions 1 and 2. We show in the next subsection that it also rationalizes a harsher EPA.

When EPA has sanction power, firm offers are weakly higher than in the case without sanction offers. For low enough sanctioning costs, this can drive firm offers above p_s^*:

[17] As noted in Tobin (1992), "Faced with possible sanctions... state agencies can assert [to firms] that they have no choice but to enforce the mandates that the federal government has imposed on them."

[18] We aim to capture the spirit of EPA oversight described in the conclusion of a 1984 policy memo on oversight in delegated programs: *"[This oversight policy] will demonstrate our desire to work with and assist states in a positive manner... while at the same time retaining our commitment to maintain high national environmental standards through appropriate sanctions and independent action, as necessary."* Found at https://www. epa.govsites/default/files/2019-12/documents/epa_policy_on_oversight_of_delegated_environmental_ programs_1984.pdf. Our model emphasizes the role of sanctions and not independent action. Independent action is understood to be unusual; moreover, it could be recast as sanctions to the state if states face a utility cost of losing control of an enforcement case.

the state requires a penalty larger than its preferred penalty because accepting only its preferred penalty would induce EPA to impose sanctions. Specifically,

Proposition 3. *For a given ζ_v, state penalties exceed p_s^* if and only if $c < \frac{u_E(p_e)-\zeta_v-u_E(p_s^*)}{u_S(p_s^*)+\zeta_v-u_S(p_e)}$.*

To have $p_{s_v} < p_s^*$ for all β, N, and ζ_v, it is sufficient, but not necessary, for $c > \frac{1-\underline{\zeta}_v}{1+\underline{\zeta}_v}$.

When state penalties can exceed s^*, we can no longer conclude that higher state penalties improve state welfare. Proposition 3 clarifies the costs c that are needed such that state penalties will not exceed p_s^*, ensuring that our normative conclusions (Proposition 2) are valid.

Proposition 3 motivates our Superfund analysis. In "orphan sites," states can choose their own enforcement outcomes (cleanup pace), instead of being beholden to negotiations with firms. They will choose their preferred outcome p_s^* unless EPA sanctions are binding. If EPA costs of threatening sanctions are sufficiently low, the state will have to choose $p_{s_v} > p_s^*$ and moreover, will change its choice of p_{s_v} when EPA strength changes. If EPA's sanction costs are sufficiently high, the state will choose p_s^* regardless of the EPA's strength. We will show that enforcement outcomes do not change with EPA strength when the state has complete control over enforcement outcomes and does not need to negotiate with firms; thus we can infer that EPA sanctions are not binding.

Our final proposition is that when EPA has sanction power, the state's utility is maximized when EPA and state preferences diverge.

Proposition 4. *For $0 < c < \infty$ and $\forall\ N$, $\arg\max_\beta U_S < 1$.*

The intuition: the state's comparative advantage weakens its bargaining position, as the firm can exploit the fact that the state prefers to handle cases itself rather than send it to the EPA. EPA willingness to sanction compensates for this, strengthening state bargaining power. While an EPA which underweights economic harm relative to the state (β<1) provides a worse outside option penalty for the state relative to an EPA which shares the state's preferences, it also more readily threatens sanctions; for intermediate values of β, this expands the set of firm offers the state will credibly reject.

3.3 Discussion

3.3.1 EPA vs. states' utility functions

Does EPA's objective function differ from the states'? Appendix Figure A.1 provides descriptive evidence that state penalties are lower when the local unemployment rate is higher, while EPA penalties are not. Under the assumption that a penalty causes more economic harm during bad economic times than good, this suggests that EPA cares less about the economic harms of penalties than does the state. In Appendix C, we provide additional empirical evidence that this modeling assumption is realistic.

3.3.2 Implications for optimal policy

Finally, we discuss optimal policy in the context of our model. Let the social planner's objective function be

$$U_{SP} = b(p) - \beta^{SP}\tau(p) + \zeta_v I_S - k - \kappa(N).$$

The social planner has her own weight on the economic harm of enforcement which can differ from the EPA's weight. Like the state and the EPA, the social planner also receives ζ_v if the state issues the penalty. She must also pay EPA's cost of threatening sanctions k (although $k = 0$ in equilibrium), and she pays a cost to fund EPA's workforce, $\kappa(N)$. We allow her to also change EPA's preferences, which in this section we denote β^{EPA}, at no cost.

Generalizing Proposition 2. Proposition 2 allows us to draw normative conclusions from our empirical effects, but its statement is limited to state welfare. If the state's objective function differs from the social welfare function, what can we say about social welfare? As long as the social planner's optimal penalty is higher than the state's preferred penalty ($\arg\max_a U_{SP} > \arg\max_a U_S$), we can generalize Proposition 2: U_{SP} is increasing in EPA strength if and only if state penalties are increasing in EPA strength.

The assumption $\arg\max_a U_{SP} > \arg\max_a U_S$ may be reasonable if, for example, states' concern about economic harm of enforcement is partially about trans-state movement of industry. A counterexample would be a model where investing in EPA resources is particularly expensive (large $\kappa'(N)$), so that obtaining the state penalties comes at too high of a cost.

Optimal β. Proposition 4 can be extended to the social planner. To the extent that changing β is costless for the social planner, a social planner with $\beta^{SP} \leq 1$ prefers an EPA with $\beta^{EPA} < \beta^{SP}$.

4 Data

4.1 Clean Air Act

State penalty data. The data for our Clean Air Act analyses come from an EPA database called ICIS-AIR, available from EPA.gov, which includes enforcement and facility data for stationary sources of emissions. We use data from 2001-2020. The formal enforcement action data include penalty size, settlement date, and enforcement agency (state, federal, and local); the data on facilities include facility location, industry, and current operating status. We exclude enforcement activities by non-state local authorities, since our model does not accommodate these. US EPA categorizes stationary sources by their emissions potential and only requires that the states submit data on formal enforcement actions for major and synthetic minor sources; thus, we limit our data to this subsample.[19] The definition of a "formal enforcement action" (FEA) is somewhat at the states' discretion. For consistency across states, we consider an FEA one that has a non-zero penalty associated with it.[20]

We adjust penalty amounts to 2010 dollars. The raw penalty data include very large outliers. For example, while the 95th percentile penalty issued by the EPA in our sample

[19]Sources are categorized by the quantity of regulated pollutants they emit or have the potential to emit. This sample accounts for about 23% of currently operating facilities registered with the EPA. However, in additional data obtained from the Florida Department of Environmental Protection, we see that these facilities account for 58% of violations that resulted in formal enforcement actions (in Florida) between 2000 and 2022.

[20]There also appears to be some misreporting on the zero-penalty margin. For example, one state staff member told us that they define a FEA as an enforcement action that has a penalty associated with it, and yet, over 10% of this state's FEAs appear to have no penalty in US EPA's data.

period is \$133,526 (in 2010 dollars), the largest penalty in the data is \$26 million. EPA often uses median penalty values when presenting summary statistics for this reason. We instead top-code state (EPA) penalties at the 90th percentile of the state (EPA) penalty distribution, and then log-transform the penalties. We show that the results are similar but less (more) precise when values are top-coded at the 95th (85th) percentile.

Violations. Our focus in this section is on average penalty size for state formal enforcement actions. We do not have systematic data on violations for the penalties we observe. However, we requested such data from several states and received it from one (the Florida Department of Environmental Protection).

In the Florida data, the plurality of violations (35%) that resulted in formal enforcement actions were discovered by direct inspections; an additional 20% were discovered by file review. The median violation was resolved within six months of being discovered, although the longest 3% of violations took over 2 years to resolve. We are able to categorize roughly three-quarters of violations into "procedural" and non-procedural violations (our own distinction), and find that 58% are procedural: i.e., they relate to incomplete permitting, late testing, etc., and not to excess emissions.

Emissions. For certain analyses, we include facility-reported data on total air emissions from EPA's Toxic Release Inventory (TRI). We use these data as a proxy control for facility size. TRI reporting is only required for a subset of facilities, based on number of employees, chemicals emitted, and emissions quantity. We observe roughly half of penalized facilities in the TRI. Appendix Figure A.2 shows that TRI emissions are strongly correlated with penalty size.

4.2 Hazardous substances (Superfund)

Our data on environmental remediation projects come from the California Department of Toxic Substances Control (DTSC), which is a department within the California Environmental Protection Agency (CalEPA).[21] We rely on the database the DTSC uses to track

[21]The CalEPA was created in 1991 (DTSC, 2023); before this, the cleanups were handled by the toxic substances control division of the California Department of Health Services.

their cleanup projects internally, "EnviroStor." For each site, EnviroStor includes a history of relevant activities (site assessments, cleanup decisions, results from post-cleanup monitoring, etc.), as well as limited site characteristics (location, acreage, funding source). Our main outcome uses the dates of "remedial actions," which are large cleanup projects meant to either contain or remediate the contaminaton.

Many environmental remediation efforts began in the 1980s, or even earlier. As one might expect, some early remediation projects have less extensive coverage in the online database EnviroStor. However, when DTSC project managers update the database with new activities, they are instructed to retroactively log dates and documents of any missing prior major activities. To explore the possibility of sample selection in the early period of our sample, we visited four DTSC records rooms across California (the two Los Angeles offices, the Berkeley office, and the Sacramento office) to view paper records from early sites.[22] We found little evidence of major activities in the paper documents that were not logged online, suggesting limited sample selection.

Sample Restriction. In our main analysis, we limit the sample to sites under DTSC jurisdiction. We further restrict the sample to sites that are over 3 acres. Using acreage as a proxy for site complexity and threat to human health, we argue that the threat of EPA involvement is much less credible for small sites. Indeed, the probability of becoming a Superfund site is three times as large for sites over 3 acres versus sites under (Appendix Figure A.3).[23]

Outcome: Remedial actions dates. Our main outcome uses the date that remedial actions began on sites under state oversight. In the model, the probability of a cleanup activity in a given year corresponds to penalty size s: it is costly for the firm but has environmental benefits. Remedial action completion dates (i.e., the date the state approved a completed remedial action) are a logged activity in EnviroStor; however, remedial action start dates are not. These dates are, however, usually available within uploaded forms. For every remedial action we observe in the database, we search documents uploaded to

[22]We're deeply indebted to the DTSC records staff and project managers for their help with this effort; it was clear this was not a typical use of the records rooms.

[23]We also show in appendix figures robustness to different size cutoffs.

EnviroStor for remedial action start dates. In our main results, we replace remedial action completion dates with the start dates listed in the certification forms where available. Since there is some judgement involved in determining remedial action start dates,[24] we present results using remedial action end dates as a robustness check.

Remedial alternatives and estimated costs. Before a remediation project begins under DTSC oversight, DTSC requires that the responsible firm(s) propose and assess multiple remedial alternatives, or options for cleanup, that vary in how extensive and how expensive they are. For example, alternatives for remediation of contaminated soil might include no action (required as an alternative for all projects, at zero cost); monitoring of soil and groundwater for a certain length of time; and excavation and disposal of contaminated soil. The firm is required to estimate and report the projected costs of each remedial alternative. Benefits are also considered, but are generally only discussed qualitatively. The firm and DTSC then agree on a single remedial alternative to pursue.

The remedial alternatives are included in report PDFs uploaded on EnviroStor. For all sites over three acres with the relevant documentation (103 sites in total), we access these reports and log the remedial alternatives, their costs, and the chosen alternative. In supplemental analyses, we use the cost of the chosen remedial alternative.

For additional details on our data for the Superfund and the CAA analysis, please see Appendix D.

4.3 Other data

For economic conditions, we use data on state and county unemployment rates from the Bureau of Labor Statistics Local Area Unemployment Statistics, and data on establishment counts and employment totals from the US Census's County Business Patterns dataset. We also use state government expenditures from the US Census's Annual Survey of State and Local Government Finances.

[24] We detailed our procedure in Appendix D.

5 Setting 1: EPA Budget Cuts (The Clean Air Act)

With evidence that EPA cares less about the economic harms of enforcement, we now turn to the empirical test implied by our model. We exploit budget cuts the US EPA faced after the 2011 Budget Control Act, and we focus on Clean Air Act enforcement.[25]

5.1 Empirical strategy

In the years following the 2011 Budget Control Act, EPA's full-time equivalent workforce fell by almost 20%.[26] EPA budget proposals submitted to Congress during this time explicitly reference their efforts to cut the payroll, and also note that the agency is focusing their enforcement efforts on the worst offenses. For example, the Fiscal Year 2012 EPA Budget in Brief (released in February 2011) begins with the sentence, "The [budget] request reflects the tough choices needed for our nation's short- and long-term fiscal health."

As we show in Section 5.2, US EPA enforcement actions for Clean Air Act violations fell significantly in the aftermath of the budget cuts, and average penalties increased, suggesting that federal enforcement actions focused only on the worst cases. We exploit additional variation driven by differences across EPA regional offices, which have famously idiosyncratic enforcement preferences and approachs (Engelberg et al., 2011).[27] Some regions' CAA enforcement decreased more substantially than other regions'; we treat this as treatment intensity and run an event study specification interacting treatment intensity with time period indicators.

[25]These budget cuts likely affected many (if not most) of the EPA's enforcement programs. We focus on the Clean Air Act because of data availability and context. Unlike the water programs, state penalty data is reliable dating back to at least 2002. Unlike hazardous substance and waste programs (Superfund and the Resource Conservation and Recovery Act (RCRA)), enforcement actions are high frequency and likely exhibit less substantial time trends.

[26]From conversations with EPA staff, we understand that much of this was from additional restrictions imposed on hiring new staff.

[27]Discussions with EPA staff suggest that program leadership in regional offices is a significant determinant of regional office enforcement behavior. We do not have data on the priorities of the EPA regional office program staff.

5.1.1 Specification

To test whether and how lower EPA strength affects state penalties, we run the following specification using the EPA database of formal enforcement actions issued by states and by EPA:

$$y_j = \beta D_{t(j)} \times (\text{Regional decrease})_{s(j)} + \delta_{s(j)} + \gamma_{i(j),t(j)} + \Gamma X_j + \epsilon_j \tag{2}$$

$y_{j,s,t}$ is the log of the penalty size in penalty j issued by state s in year t, to a firm in industry i. D indicates the penalty was issued in the post period (in 2011 or later). We also present an event study version of the regression, combining years into bins for precision.

In all specifications, we include state fixed effects and industry-by-year fixed effects, where an industry accords to 3-digit NAICS codes.[28] In our preferred specification ("baseline controls"), the vector of controls $\mathbf{X}_{j,s,t}$ includes indicators for deciles of the state unemployment rate (lagged 3 months) and indicators for deciles of facility emissions (from the TRI). In additional specifications, we include facility type controls[29] and a control for the total annual state expenditures.

"Regional decrease" is a continuous variable which encodes the extent to which each EPA regional office reduced their enforcement after the budget cuts. Specifically, we take the number of formal enforcement actions in the four years before the EPA budget cuts (2007-2010); subtract out the number of enforcement actions in the four years after (2012-2015); and divide by the former. The interpretation, then, is that a 100% reduction in *federal* formal enforcement actions (or, removing US EPA enforcement from the region) would correspond to a 100*β percent reduction in average *state* penalty size.

One concern with our definition of treatment intensity is that EPA enforcement declines may be correlated with changes in average state penalty size for other reasons not

[28]While states may adjust their enforcement across industries depending on EPA strength, inclusion of industry fixed effects improves precision by addressing changes in US industry composition over time. It also alleviates concerns that state-issued penalty decreases are driven by economic considerations for industries that suffered in the Great Recession.

[29]Specifically, these are an indicator for major facilities (see Section 4) and an indicator for non-public facilities.

captured in our model. For example, if compliance is improving overall, declines in EPA activity may simply reflect the fact that certain regions have less significant violations. We argue this is not the case for two reasons. First, as we will show in the results, average EPA penalty size is increasing in regions where enforcement actions are decreasing. This is consistent with EPA optimizing with increasingly tight constraints (needing to prioritize the worst violations) and not consistent with increasing compliance. Second, our controls for industry fixed effects mitigate concerns that secular declines in polluting industries are driving the results.

5.2 CAA Results

5.2.1 Descriptives

State penalties. Appendix Figure A.4 shows descriptives for the regression sample. Figure A.4B shows the distribution of state-issued penalties, in level terms, separately for before and after the EPA's budget cuts. The distribution is right-skewed, with the majority of state-issued penalties falling under $10,000 in 2010 dollars. Appendix Figure A.4A shows that the plurality of penalties issued are issued to firms in manufacturing industries. Other over-represented industries including oil and gas and utilities. Many of these industries faced secular declines during our sample period, which motivates our inclusion of industry-by-year fixed effects in our main specification.

EPA budget cuts. Our identifying variation uses budget cuts many federal agencies faced after the Budget Control Act of 2011. Figure 3A shows that the EPA workforce declined over 15% in the years following the budget cuts.[30] Correspondingly, Clean Air Act formal enforcement actions brought by the US EPA declined in the years after the budget cuts (Figure 3B).

As described in EPA annual fiscal year budget proposals, these budget cuts appear to lead the agency to focus its efforts on the worst offenders; i.e., in EPA enforcement decisions, the marginal enforcement cases are the ones with lower penalty sizes. Moreover,

[30]While many federal agencies saw budget cuts after the Budget Control Act, other agencies do not appear to have had the workforce declines that the EPA did (Appendix Figure A.5).

consistent with qualitative evidence of regions operating with substantial independence (Engelberg et al., 2011), this targeting happened within region, and not across regions.[31] Figure 4 shows that EPA regional offices with the largest decreases in enforcement activity also have the largest increases in average penalty size. The expansive discretion of regional offices motivates our use of variation in regional office reactions to budget cuts in our identification strategy.

5.2.2 Estimated effect of reduced resources

We begin by presenting an event study with no treatment intensity dimension: we regress penalty size on year with our baseline controls and state fixed effects. Figure 5 shows that average penalty size is unchanging in the pre-period and then drops in the post period. Averaging the estimated coefficients before versus after the budget cuts, penalties dropped by 25% ($p = 0.02$).

Table 1 adds treatment heterogeneity and presents the results of estimating the difference-in-difference specification in Equation (2). Our coefficients imply that a 10% reduction in US EPA (federal) activity results in about a 3% decrease in average penalty size for state-issued formal enforcement actions ($p = 0.036 - 0.044$ across specifications). This effect size is stable across specifications. In the first column, we run a regression with sparse controls: only the fixed effects and controls for the state unemployment rate and facility emissions. In the second column, we add facility type controls. In the final column, we also control for the state budget.

In the sample, the average penalty size is \$12,828 (with a standard deviation of \$13,593).[32] Thus, a decrease of 3% of average penalty size corresponds to a reduction of about \$400 per penalty. Overall, EPA formal actions decreased 50% after the 2011 budget cuts, suggesting that state penalties decreased by about \$2,000 on average.

To explore the possibility of pre-trends, Figure 6 estimates Equation (2) with bian-

[31] In Engelberg et al. (2011), a 2011 review by the EPA Office of Inspector General (OIG), the OIG recommended that the EPA centralize its enforcement efforts to effectively allocate its resources nationwide instead of within region. Of the five recommendations that the OIG made in this report, this is the only one that the EPA disagreed with.

[32] The median penalty is \$6,649.

nual year dummies interacted with treatment intensity. We combine the first two year dummies because of inconsistent penalty reporting from states in the early period of the sample.[33] Before the EPA budget cuts, states in regions that are eventually more and less affected have similar trends in average penalty size. After the EPA budget cuts, states in regions where the US EPA decreased enforcement more collected smaller penalties, on average, compared to states where the US EPA decreased enforcement less. We note that unlike the typical use case for an event study figure, we (largely) do not follow the same observations over time; rather, each year contains a new draw of facilities. For this reason, we calculate a p-value for Figure 6 pooling the post-period coefficients and the pre-period coefficients ($p = 0.046$).

In Appendix Table B.1, we explore changes in the distribution of penalties; we replace our main outcome with dummies indicating a penalty is under $Y dollars. Neither the largest nor the smallest penalties appear most affected; rather, penalty decreases are largest in the middle of the distribution ($1,000-$10,000, the 26th through the 68th percentile of the distrbution).

5.3 Robustness

5.3.1 Extensive margin response.

EPA strength may have affected the composition of penalties collected, and not only the size of penalties collected. Our model speaks to the final penalty size paid by individual firms—an intensive margin measure. Extensive margin effects affect the validity of our results to the extent that the types of violations being penalized differ before and after the EPA budget cuts.

First, we note that many related concerns would bias our effects towards zero. For example, if states are less likely to issue penalties conditional on violation severity when the EPA is weaker, we would be missing some low-severity violations in the post period,

[33] Appendix Figure A.6A shows that the share of penalties coded as zero in the data drops sharply after 2005. Appendix Figure A.6B shows that before 2005, eventual treatment intensity is correlated with the share of zeros in the data.

increasing average penalty size in the post period. One the other hand, if states are less willing to pursue the most severe violations—e.g., preferring to directly hand them to the EPA—then this would cause us the opposite issue: our effects would be overestimates.

To address this concern, we present results of a balanced panel at the state year level. We make two adjustments to our data to address the fact that some states have more CAA facilities than others.[34] First, we use as an outcome total penalty dollars *per facility* in the state. Second, we weight the panel by the number of facilities in a state.[35] The specification also includes controls for the state-year unemployment rate (in deciles, as in our main specification), as well as for the industry composition of penalties issued.

Appendix Figure A.7 produces the results. Total penalty dollars collected per CAA facility decrease more in states more affected by the EPA budget cuts, by about $20 per facility. The results are less precise than the main analysis ($p = 0.100$). That overall penalties collected decreased after the EPA budget cuts suggest that our results are not driven by sample selection.

5.3.2 Robust inference.

Our main analysis uses what Abadie et al. (2023) refer to as a model-based framework: we take the stance that errors are correlated within state over time, and we cluster by state level. An alternative, design-based approach to inference would suggest it is appropriate to cluster standard errors at the level at which treatment is assigned. Considering treatment to be regional EPA office strength, our treatment varies by regional office and before vs. after the EPA budget cuts. However, given the small number of clusters at this level (20), it's not clear how to appropriately calculate standard errors (Roth et al., 2023).

For an additional analysis for robust inference, we use wild bootstrap clustered standard errors Cameron et al. (2008). This is a common approach to robust inference with few clusters; however, Canay et al. (2021) (as referenced in Roth et al. (2023)) note that the

[34] Because of data limitations, our counts of facilities per state are time-invariant.

[35] Weighting the panel also makes the results more comparable to the main analysis, since the penalty-level data implicitly give more weight to states which have more facilities (and therefore issue more penalties).

validity of these standard errors requires assumptions about homogeneity of treatment effects across clusters, which in our setting may not hold. We cluster at the level of treatment assignment, treating regional EPA strength as treatment and clustering at the EPA region $\times$ post-period level. Appendix Table B.2 shows that the wild bootstrap clustered p-values ($p = 0.018 - p = 0.032$) are lower than the p-values when we cluster by state level.

6 Empirical Setting 2: Presidential Administrations (Hazardous Substances Cleanups)

Our second source of variation in EPA's strength uses changes in presidential administrations. EPA administrators are appointed by presidents (and confirmed by Congress) and affect the enforcement culture and capacity of the EPA; we provide evidence that a Republican-led EPA is a less harsh enforcer. While the Clean Air Act provides a useful context for studying changes in EPA resources, it is not sufficient for exploring effects of EPA leadership for two reasons. First, since the CAA data only span 2002-2020, they only include two changes in presidential administrations, and one was coincident with the Great Recession (which, as we show in Appendix C, independently affected state penalties). Second, we do not have a control group we expect to be less affected by changes in presidential administration.

Instead, we turn to a second environmental program: the Superfund program. Unlike the Clean Air Act, Superfund is not a delegated program, but states often have their own cleanup programs and use the federal program to threaten firms (Figure 1). Our cleanup data date back to the late 1980s and include a convenient control group. We use a difference-in-differences design, where our outcome is the pace of cleanup projects. The first difference is variation in presidential administration, which we argue changes firm expectations about the costs of entering the federal Superfund program. The second difference is in whether the cleanup is funded and orchestrated by a private party or state government.

This design allows to test whether changes in presidential administration, which we argue affects EPA strength through both its preferences and its capacity, affects state outcomes.

6.1 Empirical Strategy

6.1.1 Variation in EPA strength

For variation in the strength the EPA, we use changes in the party of the US president, since the headquarters and regional EPA administrators, as well as the assistant administrator who oversees waste cleanup, are all political appointees. Appointees are, for politicians, a "vital tool for controlling the bureaucracy" (alongside statutes, budget changes, and oversight investigations and hearings) (Lewis, 2010). While the career staff members at the EPA, who are on average quite left-leaning (Clinton et al., 2012; Spenkuch et al., 2021), may stay at the agency through many administrations, the administrators have substantial sway over the the culture, priorities, and capacity of the EPA. Republican-appointed administrators tend to be more closely tied to industry, suggesting they may be more business-friendly in their enforcement. Thus, the strength of the "gorilla" threat should be lower under Republican presidential administrations.

Evidence of the qualitative difference in EPA leadership across administrations can be found in EPA press releases giving backgrounds on regional EPA administrators, who are political appointees. We found press releases for eight of George W. Bush's initial ten appointees for regional administrator, and nine of Barack Obama's initial ten. Only one of Bush's regional appointees was noted to have worked in the non-profit sector before their EPA service, while four of the eight were in private law or business. Meanwhile, seven of Obama's appointees had non-profit sector backgrounds mentioned in the press releases, and only one of the nine mentioned private law or business.

For quantitative evidence that EPA involvement is more costly for firms during Democratic presidential administrations, we turn to data from federal enforcement across major

EPA programs.[36] Figure 7 shows the total sum of penalties assessed annually (in administrative orders with penalties) by the US EPA for violations of the Clean Air Act, the Clean Water Act (CWA), the Resource and Conservation Recovery Act (RCRA), and Superfund, under different presidential administrations. Across the board, EPA collects higher total penalties in Democratic years. Limiting the data to years after 1992, as early data are less complete, this difference is statistically significant at the 1% significance level for CWA, RCRA, and Superfund enforcement. In CAA, it is not significant at conventional levels (p=0.107).

6.1.2 Orphan sites control group

We exploit a key feature of the cleanup context to form a time-invariant control group: "orphan" sites. In California, orphan sites are sites with no viable parties to orchestrate the cleanup.[37]

Using orphan sites as a control group allows us to identify the effect of EPA preferences on firm cooperation with state enforcement. For both orphan sites and firm-led cleanups, the EPA's leniency is changing with the presidential party in power, and in theory, changes in EPA leniency could affect state-led cleanups (especially to the extent that the EPA can sanction the state). However, in state-led cleanups, the state has complete control over the pace of environmental cleanups, so that the relevant difference between orphan and firm-led cleanups is the control the state has over how it handles the environmental issue.

In the model, orphan sites can be thought of as cases where the state has total control over the enforcement outcome, p_s: there is no longer a firm to bargain with. States still get some utility from their own cleanup efforts, so they still have a preferred p_s^*. Thus,

[36] Source: https://echo.epa.gov/facilities/enforcement-case-search

[37] It can happen that this is because of responsible party recalcitrance; however, it is more commonly because an inability to pay. For example, one major cleanup site in California (Alco Pacific, Inc.) became an orphan site after the DTSC determined that the former owner of the defunct recycling facility owed $500,000 to the IRS and $1 million for a different environmental court judgement, was already in the process of selling his home to pay for these, and had no more than $100,000 in capital assets from his recycling company (source: DTSC regional file room records). The state began cleanup itself while it amassed evidence for a court case, and later sued several other potentially responsible parties to recoup its costs.

in the model, any effect of EPA preferences on orphan sites would operate through the state's fear of sanctions from the EPA. We view this as a test of the model's assumption that EPA sanctions are not binding on state behavior.

6.1.3 Outcome: Cleanup begins in a given year

We do not consider financial penalties in the Superfund context. Penalties do not exist for orphan sites, meaning we would not have a control group if we used penalties as an outcome. Projected cleanup costs, another negotiated outcome of enforcement, are difficult to find for sites, and have very large variation driven mostly by the geological and chemical characteristics of the sites. Instead, we use the probability of cleanup activities occurring in a given year, conditional on site age: i.e., cleanup pace. Our conversations with Superfund program staff, as well as prior academic and policy research on Superfund (Environmental Law Institute, 1990; Sigman, 2001), indicate that the pace of cleanup is a strong measure of firm cooperation. Cleanup pace has costs and benefits in terms of present discounted value; it is more expensive to complete a project faster, but it also confers environmental benefits.

6.1.4 Empirical Specification

We analyze cleanup site-year data from 1987-2016 using the following empirical specification:

$$y_{i,t} = \delta \text{Rep}_t + \beta \text{Rep}_t \times (\text{firm-led site})_i + \rho (\text{site age})_{i,t} + \mu_i + \epsilon_{i,t}, \tag{3}$$

where $y_{i,t}$ is whether site i had a remedial action in year t; Rep_t is an indicator equal to 1 in Republican presidential administration years and 0 otherwise; $(\text{site age})_{i,t}$ gives the number of years since we first observed the site (divided by 10 to make tables readable); and firm-led site$_i$ is a site-level indicate for whether the site's remedial action is orchestrated and funded by a firm (as opposed to the state); and μ_i are site fixed effects. If a stronger EPA increases firm cooperation, we expect $\beta < 0$.

Orphan sites may be different from firm-led sites. However, this empirical specification identifies the effect of EPA leadership on firm cooperation under the assumption that outcome trends do not change differently under different presidential administrations for firm-led versus orphan sites for reasons besides the bargaining power of the state.

Cox proportional hazard model. Our data are censored: once a site has been cleaned up, its outcomes are on longer observed. Thus, the remaining observable sites will be negatively selected. This becomes problematic if the sample selection differs by treatment status.

We include results using a Cox proportional hazards model. The identification assumption for the Cox model in this context is similar to the linear model—treatment status must not be (differentially by orphan vs. non-orphan status) correlated with anything unobserved which also affects cleanup likelihood (Fisher and Lin, 1999). We note that our setting is not a typical use case for a hazard model: in our setting, treatment status flips every 4-8 years and affects all sites at once. Because of our alternating treatment status, we are not obviously subject to the usual problem in survivorship models: that sites are differentially selected in treatment and control.

6.1.5 Hazardous Substances Results

Appendix Table B.3 gives descriptive statistics for the regression sample and the full sample (relaxing the acreage restriction). Sites in our sample are more likely to have cleanup activities than in the full sample. By construction, they are much larger. Within our regression sample, orphan sites are less likely to have any cleanup activities or be certified during the sample period.

In Table 2, we report the results of the main difference-in-differences regression (Equation 3). Overall (Column 1), sites under DTSC oversight are less likely to have remedial actions in years when the EPA is headed by a Republican appointee. This effect is driven entirely by firm-led sites; i.e., where the cleanup is orchestrated by the firm and not the state. Firm-led sites are three percentage points less likely to have a remedial action in a given year if the EPA is led by a Republican appointee ($b = 0.03$, $p < 0.01$). Orphan sites, which the state cleans, are not significantly more less likely to have remedial actions.

Furthermore, as shown in Column 4, this difference is statistically significant ($p = 0.028$).

We next explore whether cleanups are more thorough (and expensive) during Democratic presidential administrations. Here, we have limited data, and lack power. In particular, we only have data on 11 orphan sites. However, we present these results for two reasons: the costs to the firm (and state) more closely align with our model, and it also allows us to test whether cleanup *quality* is affected by EPA preferences.

Table 3, Column 1, shows that firms choose less expensive remedial alternatives during Republican EPA administrations. The difference is large (over one-third of a standard deviation) and highly significant ($p = 0.013$). Column 2 includes the 12 orphan sites for which we were able to obtain these data. Even compared to the state's choices, firms still choose lower cost projects during Republican administrations, but the relationship is smaller (about 1/5 of a standard deviation) and less significant ($p = 0.212$).

6.1.6 Robustness

Censored data. One concern about our difference-in-differences model is that the resulting estimates are biased by a censoring problem. Sites that remain in the panel for longer are negatively selected: they may be harder to remediate or less of a priority.

First, we point out that sites' older ages are less likely to be under Republican administrations. Appendix Figure A.8 shows how the time patterns of site discovery and presidential administrations interact: sites are most likely to have a Republican EPA when they are young, and the least likely to have a Republican EPA when they are old.If the oldest sites are the most difficult to clean up, then older sites being under Democratic EPAs would bias us towards a positive effect of Republican EPAs.

We also run our regressions using a Cox proportional hazards model (Table B.4). The results are similar to our linear model—firm-led sites are significantly less likely to have remedial actions during Republican administrations.

State policy changes. State policy changes may be correlated with federal policy changes (for reasons besides a concern about federal involvement). We note that our orphan sites outcome should be affected by state policy changes, so that this only presents a threat to identification if state policy changes differentially for firm-led versus orphan sites under Democratic versus Republican US presidencies. In Table B.5, Panel A, we add controls for the political party of the California state governor; these do not meaningfully change our results.

Data decisions. Appendix Figure A.9 shows robustness to varying the acreage threshold for our site sample. The difference in clean-up probability across presidential administrations for firm-led sites remains highly significant. The magnitude of the interaction term (comparing the effect of presidential party for firm-led and orphan sites) shrinks somewhat at a threshold of 5 acres, and then remains stable as the sample is further restricted. Appendix Table B.6 shows two additional robustness checks. Panel A outcomes use the dates cleanup projects were approved by DTSC as complete instead of the dates they began, since end dates are entered by project managers as metadata in the database and do not require judgement calls. Panel B includes as outcomes removal actions in addition to remedial actions. Removal actions are less expensive projects than remedial actions, reducing the firms' incentive to avoid beginning these projects, so we expect a smaller treatment effect with this outcome. However, these projects are more frequent and address a potential concern that our orphan sites treatment effect is low only because of a low base value.

7 Conclusion

In this paper, we document several novel empirical findings. In two different settings, we show that characteristics of the EPA which affect its enforcement behavior also affect enforcement outcomes for state environmental agencies. A back-of-the-envelope calculation suggests that each individual EPA staff person removed (or not replaced) after the agency's budget cuts in 2011 cost the states about $1,100 per year in lost penalties (not including the environmental benefits of these penalties). This is consistent with a characterization, originally proposed at EPA, of a federal agency as a "gorilla in the closet" for the states in regulatory enforcement. We model the "gorilla" as affecting firm offers in a bargaining game between the state and its regulated entities, and we show how changing the resources or preferences of the federal agency can change firm offers to the state. State enforcement primacy provides us with a sufficient statistics characterization of the states' welfare effects from increasing federal strength, and our empirics reveal that EPA is currently less strong than what would maximize state welfare.

How much of EPA's total benefit is through its effects on state penalties? Our estimates suggest that states lost $2.7 million annually after EPA's budget cuts. Meanwhile, EPA's own penalties collected fell by about $7.7 million (13% of the 2008-2010 average).[38] This suggests that about 26% of EPA's total benefit is through the "gorilla" effect. Work that characterizes the benefit of federal regulation and enforcement, environmental or otherwise, should not neglect this spillover onto the states.

Finally, these results are relevant for current environmental issues. As the federal government struggles to pass major federal climate legislation, state and local governments are "stepping up" to address demand (Astor, 2022). There are myriad reasons federal climate legislation may be desirable. Our results suggest that states' efforts will be less effective without backup from a federal authority.

[38]To account for the lag in the effect of the budget cuts on penalties, we calculate this using the average annual federal penalty sum from 2014 through 2016, compared to the average annual federal penalty sum 2008-2010.

References

Abadie, Alberto, Susan Athey, Guido W Imbens, and Jeffrey M Wooldridge, "When Should You Adjust Standard Errors for Clustering?," *The Quarterly Journal of Economics*, 2023, *138* (1), 1–35.

Allcott, Hunt, Sendhil Mullainathan, and Dmitry Taubinsky, "Energy policy with externalities and internalities," *Journal of Public Economics*, 2014, *112*, 72–88.

Astor, Maggie, "As Federal Climate-Fighting Tools Are Taken Away, Cities and States Step Up," *The New York Times*, July 2022.

Baily, Martin, "Some Aspects of Optimal Unemployment Insurance," *Journal of Public Economics*, 1978, *10* (3), 379–402.

Barro, Robert J. and David B. Gordon, "Rules, Discretion, and Reputation in a Model of Monetary Policy," *Journal of Monetary Economics*, 1983, *12* (1), 101–121.

Blundell, Wesley, "When Threats Become Credible: A Natural Experiment of Environmental Enforcement from Florida," *Journal of Environmental Economics and Management*, 2020, *101*, 102288.

_ , Gautam Gowrisankaran, and Ashley Langer, "Escalation of Scrutiny: The Gains from Dynamic Enforcement of Environmental Regulations," *American Economic Review*, 2020, *110* (8), 2558–2585.

Buchanan, James M., "Federalism and Fisal Equity," *American Economic Review*, 1950, *40* (4), 583–599.

Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller, "Bootstrap-Based Improvements for Inference with Clustered Errors," *Review of Economics and Statistics*, 2008, *90* (3), 414–427.

Canay, Ivan A., Andres Santos, and Azeem M. Shaikh, "The Wild Bootstrap with a "Small" Number of "Large" Clusters," *Review of Economics and Statistics*, 2021, *103* (2), 346–363.

Chang, Howard F., Leah G. Traub, and Hilary Sigman, "Endogenous Decentralization in Federal Environmental Policies," *International Review of Law and Economics*, 2014, *37*, 39–50.

Chay, Kenneth Y. and Michael Greenstone, "The Impact of Air Pollution on Infant Mortality: Evidence from Geographic Variation in Pollution Shocks Induced by a Recession," *The Quarterly Journal of Economics*, 2003, *118* (3), 1121–1167.

Chetty, Raj, "A General Formula for the Optimal Level of Social Insurance," *Journal of Public Economics*, 2006, *90* (10-11), 1879–1901.

— , "Sufficient Statistics for Welfare Analysis: A Bridge Between Structural and Reduced-Form Methods," *Annual Review of Economics*, 2009, *1* (1), 451–488.

Clinton, Joshua D., Anthony Bertelli, Christian R. Grose, David E. Lewis, and David C. Nixon, "Separated Powers in the United States: The Ideology of Agencies, Presidents, and Congress," *American Journal of Political Science*, 2012, *56* (2), 341–354.

Currie, Janet, Michael Greenstone, and Enrico Moretti, "Superfund cleanups and infant health," *American Economic Review*, 2011, *101* (3), 435–41.

Dijkstra, Bouwe R. and Per G. Fredrikssonn, "Regulatory Environmental Federalism," *Annual Review of Resource Economics*, 2010, *2*, 319–339.

DTSC, CA, "Our History," February 2023. https://dtsc.ca.gov/about-dtsc/our-history/.

Earnhart, Dietrich and Lana Frieson, "Enforcement Federalism: Comparing the Effectiveness of Federal Punishment versus State Punishment," *Environmental and Resource Economics*, 2021, *78* (227-255).

Engelberg, Dan, Kathlene Butler, Charles Brunton, Anthony Chirigotis, Julie Hamann, Tiffine Johnson-Davis, and Genevieve Soule, "EPA Must Improve Oversight of State Enforcement," Report 12-P-0113, U.S. Environmental Protection Agency Office of Inspector General December 2011.

Environmental Law Institute, "Enhancing State Superfund Capabilities: Nine-State Study," Technical Report, U.S. Environmental Protection Agency 1990.

— , "An Analysis of State Superfund Programs: 50-State Study, 2001 Update," Technical Report 2002.

EPA, U.S., "Clean Air Act Stationary Source Civil Penalty Policy," Available online 1991. https://www.epa.gov/sites/default/files/documents/penpol.pdf.

EPA.gov, "The Guardian: EPA's Formative Years, 1970-1973," 1993. https://www.epa.gov/archive/epa/aboutepa/guardian-epas-formative-years-1970-1973.html.

— , "Interview with Henry Longest," Superfund 25th Anniversary Oral History Project August 2005.

— , "Interview with Lois Gibbs," Superfund 25th Anniversary Oral History Project August 2005.

Evans, Mary F., "The Clean Air Act Watch List: An Enforcement and Compliance Natural Experiment," *Journal of the Association of Environmental and Resource Economists*, September 2016, *3* (3), 525–778.

— **and Sarah L. Stafford**, "The Clean Air Act Watch List and Federal Oversight of State Enforcement Efforts," *Journal of Environmental Economics and Management*, 2019, *93*, 170–184.

— **, Scott M. Gilpatric, and Jay P. Shimshack**, "Enforcement Spillovers: Lessons from Strategic Interactions in Regulation and Product Markets," *The Journal of Law & Economics*, November 2018, *61* (4), 555–769.

Farhi, Emmanuel and Xavier Gabaix, "Optimal Taxation with Behavioral Agents," *American Economic Review*, 2020, *110* (1), 298–336.

Feng, Kuishuang, Steven J. Davis, Laixing Sun, and Klaus Hubacek, "Drivers of the US CO_2 Emissions 1997-2013," *Nature Communications*, 2015, *6* (7714).

Finkelstein, Amy, Matthew J. Notowidigdo, Frank Schilbach, and Jonathan Zhang, "Lives vs. Livelihoods: The Impact of the Great Recession on Mortality and Welfare," 2023. http://users.nber.org/ñotom/research/FNSZ_Great_Recession_july2023.pdf.

Fiorino, Daniel J., *Making Environmental Policy*, Berkeley: University of California Press, 1995.

Fisher, Lloyd D. and D. Y. Lin, "Time-Dependent Covariates in the Cox Proportional-Hazards Regression Model," *Annual Review of Public Health*, May 1999, *20*, 145–157.

Gamper-Rabindran, Shanti and Christopher Timmins, "Hazardous waste cleanup, neighborhood gentrification, and environmental justice: Evidence from restricted access census block data," *American Economic Review*, 2011, *101* (3), 620–24.

— **, Ralph Mastromonaco, and Christopher Timmins**, "Valuing the benefits of superfund site remediation: Three approaches to measuring localized externalities," Technical Report, National Bureau of Economic Research 2011.

Greenstone, Michael and Justin Gallagher, "Does hazardous waste matter? Evidence from the housing market and the superfund program," *The Quarterly Journal of Economics*, 2008, *123* (3), 951–1003.

Gutiérrez, Germán and Thomas Philippon, "How EU Markets Became More Competitive Than US Markets: A Study of Institutional Drift," 2019. http://germangutierrezg.com/GutierrezPhilippon_Europe_2019.pdf.

Hendren, Nathaniel, "The Policy Elasticity," *Tax Policy and the Economy*, 2016, *30*, 51–89.

— , "Measuring Ex Ante Welfare in Insurance Markets," *Review of Economic Studies*, 2021, *88*, 1193–1223.

Jung, Juergen and Michael D. Makowsky, "The Determinants of Federal and State Enforcement of Workplace Safety Regulations: OSHA Inspections 1990-2010," *J Regul Econ*, 2014, *45* (1), 1–33.

Kang, Karam and Bernardo S. Silveira, "Understanding Disparities in Punishment: Regulator Preferences and Expertise," *Journal of Political Economy*, 2021, *129* (10), 2947–2992.

Kleven, Henrik, "Sufficient Statistics Revisited," *Annual Review of Economics*, 2021, *13*, 515–538.

Konisky, David M. and Neal D. Woods, "Exporting Air Pollution? Regulatory Enforcement and Environmental Free Riding in the United States," *Political Research Quarterly*, December 2010, *63* (4), 771–782.

Lewis, David E., *The Politics of Presidential Appointments: Political Control and Bureaucratic Performance*, Princeton University Press, 2010.

Lim, Claire S. H. and Ali Yurukoglu, "Dynamic Natural Monopoly Regulation: Time Inconsistency, Moral Hazard, and Political Environments," *Journal of Political Economy*, 2018, *126* (1), 263–312.

Lipscomb, Molly and Ahmed Mushfiq Mobarak, "Decentralization and Pollution Spillovers: Evidence from Re-drawing of County Borders in Brazil," *Review of Economic Studies*, 2017, *84*, 464–502.

McCarthy, James E., "Highway Fund Sanctions for Clean Air Act Violations," Technical Report, Congressional Research Service 1997.

Mintz, Joel A., "Agencies, Congress and Regulatory Enforcement: A Review of EPA's Hazardous Waste Enforcement Effort, 1970-1987," *Environmental Law*, 1988, *18* (4), 683–777.

Norwood, Kellen, "Federal Preemption of State and Local Law," in William Scheiderich, ed., *Municipal Law Deskbook*, American Bar Association, 2015.

Oates, Wallace E., *Fiscal Federalism*, Harcourt Brace Jovanovich, 1972.

Persico, Claudia, David Figlio, and Jeffrey Roth, "The Developmental Consequences of Superfund Sites," *Journal of Labor Economics*, 2022, *38* (4), 1055–1097.

Piketty, Thomas, Emmanuel Saez, and Stefanie Stantcheva, "Optimal Taxation of Top Labor Incomes: A Tale of Three Elasticities," *American Economic Journal: Economic Policy*, 2014, *6* (1), 230–271.

Rogoff, Kenneth, "The Optimal Degree of Commitment to an Intermediate Monetary Target," *The Quarterly Journal of Economics*, 1985, *100* (4), 1169–1189.

Roth, Jonathan, Pedro H.C. Sant'Anna, Alyssa Bilinski, and John Poe, "What's Trending in Difference-in-Differences? A Synthesis of the Recent Econometrics Literature," *Journal of Econometrics*, 2023, *235* (2), 2218–2244.

Sigman, Hilary, "The Pace of Progress at Superfund Sites: Policy Goals and Interest Group Influence," *The Journal of Law & Economics*, April 2001, *44* (1), 315–343.

_ , "Interational Spillovers and Water Quality in Rivers: Do Countries Free Ride?," *American Economic Review*, September 2002, *92* (4), 1152–1159.

_ , "Transboundary Spillovers and Decentralization of Environmental Policies," *Journal of Environmental Economics and Management*, 2005, *50* (2005), 82–101.

Slatterey, Cailin, "Bidding for Firms: Subsidy Competition in the US," December 2022. Working Paper.

Spenkuch, Jorg L., Edoardo Teso, and Guo Xo, "Ideology and Performance in Public Organizations," *NBER Working Paper No. 28673*, 2021.

Tang, Johnny, "Regulatory Competition in the US Life Insurance Industry," November 2022. Working Paper.

Tiebout, Charles M., "An Economic Theory of Fiscal Decentralization," in Universities-National Bureau Committee for Economic Research, ed., *Public Finances: Needs, Sources, and Utilization*, Princeton University Press, 1961, pp. 79–96.

Tobin, Richard J., "Environmental Protection and the New Federalism: A Longitudinal Analysis of State Perceptions," *Publius: The Journal of Federalism*, Winter 1992, *22*, 93–107.

U.S. EPA, "EPA Policy on Oversight of Delegated Environmental Programs," April 1984. https://www.epa.gov/sites/default/files/2019-12/documents/epa_policy_on_oversight_of_delegated_environmental_programs_1984.pdf.

US Senate, "Enforcement of Environmental Laws," 1997. Senate Hearing 105-173.

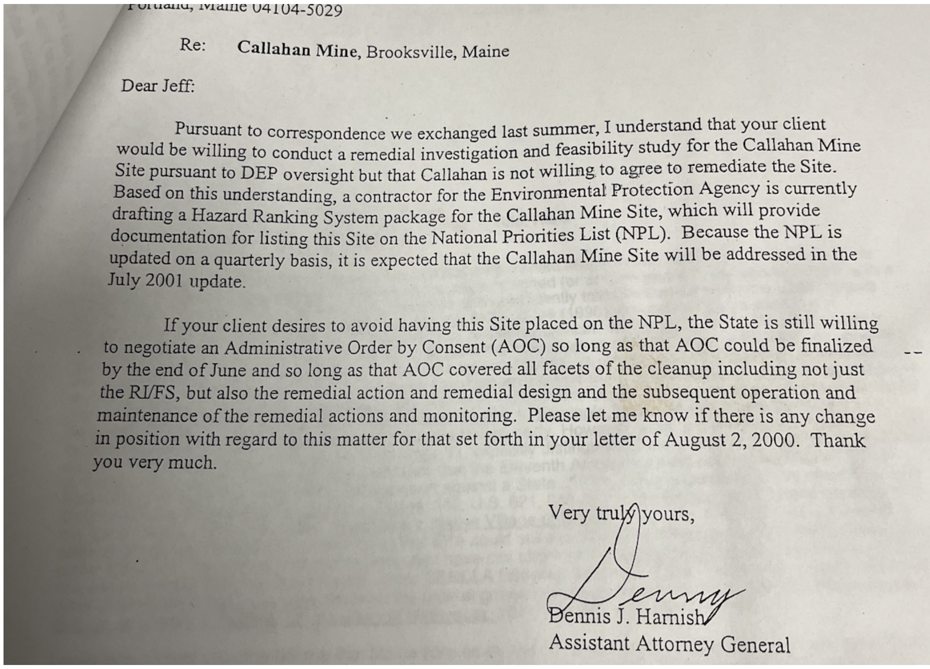
_ , "Oversight of the Environmental Protection Agency's Management of the Superfund Program," 2002. Senate Hearing 107-996.

Woods, Neal D., "Interstate Competition and Environmental Regulation: A Test of the Race-to-the-Bottom Thesis," *Social Science Quarterly*, 2006, *87* (1), 174–189.

8 Figures

Figure 1: The Gorilla in Action

(A) Callahan Mine (Maine)



Portland, Maine 04104-5029

Re: **Callahan Mine**, Brooksville, Maine

Dear Jeff:

Pursuant to correspondence we exchanged last summer, I understand that your client would be willing to conduct a remedial investigation and feasibility study for the Callahan Mine Site pursuant to DEP oversight but that Callahan is not willing to agree to remediate the Site. Based on this understanding, a contractor for the Environmental Protection Agency is currently drafting a Hazard Ranking System package for the Callahan Mine Site, which will provide documentation for listing this Site on the National Priorities List (NPL). Because the NPL is updated on a quarterly basis, it is expected that the Callahan Mine Site will be addressed in the July 2001 update.

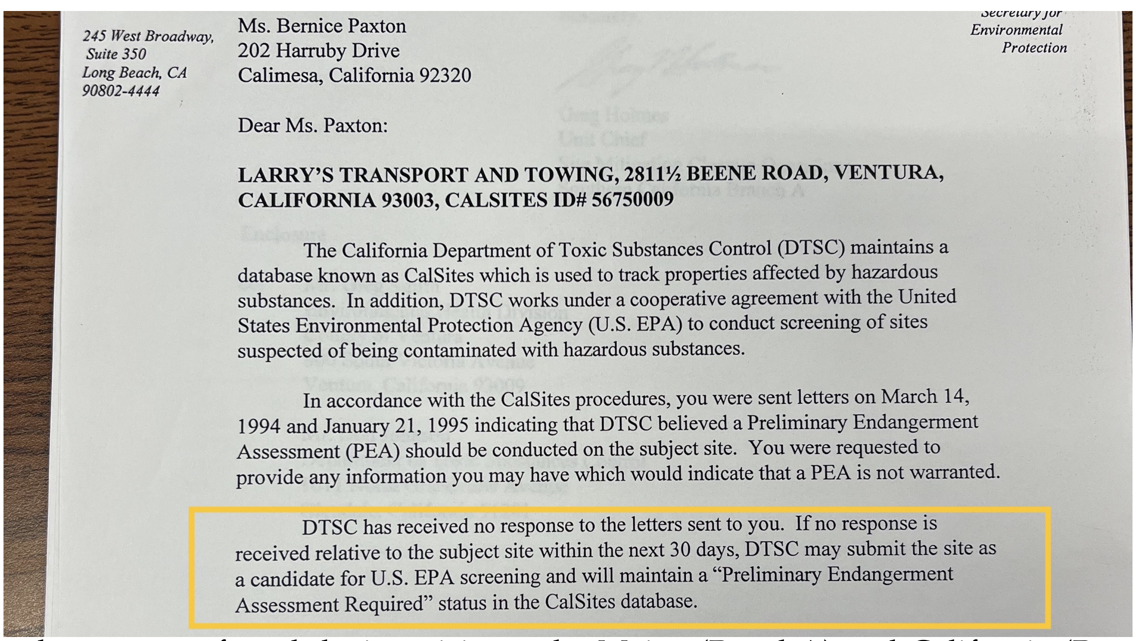
If your client desires to avoid having this Site placed on the NPL, the State is still willing to negotiate an Administrative Order by Consent (AOC) so long as that AOC could be finalized by the end of June and so long as that AOC covered all facets of the cleanup including not just the RI/FS, but also the remedial action and remedial design and the subsequent operation and maintenance of the remedial actions and monitoring. Please let me know if there is any change in position with regard to this matter for that set forth in your letter of August 2, 2000. Thank you very much.

Very truly yours,

Denny

Dennis J. Harnish
Assistant Attorney General

(B) Larry's Truck and Towing (California)



245 West Broadway,
Suite 350
Long Beach, CA
90802-4444

Secretary for
Environmental
Protection

Ms. Bernice Paxton
202 Harruby Drive
Calimesa, California 92320

Dear Ms. Paxton:

LARRY'S TRANSPORT AND TOWING, 2811½ BEENE ROAD, VENTURA, CALIFORNIA 93003, CALSITES ID# 56750009

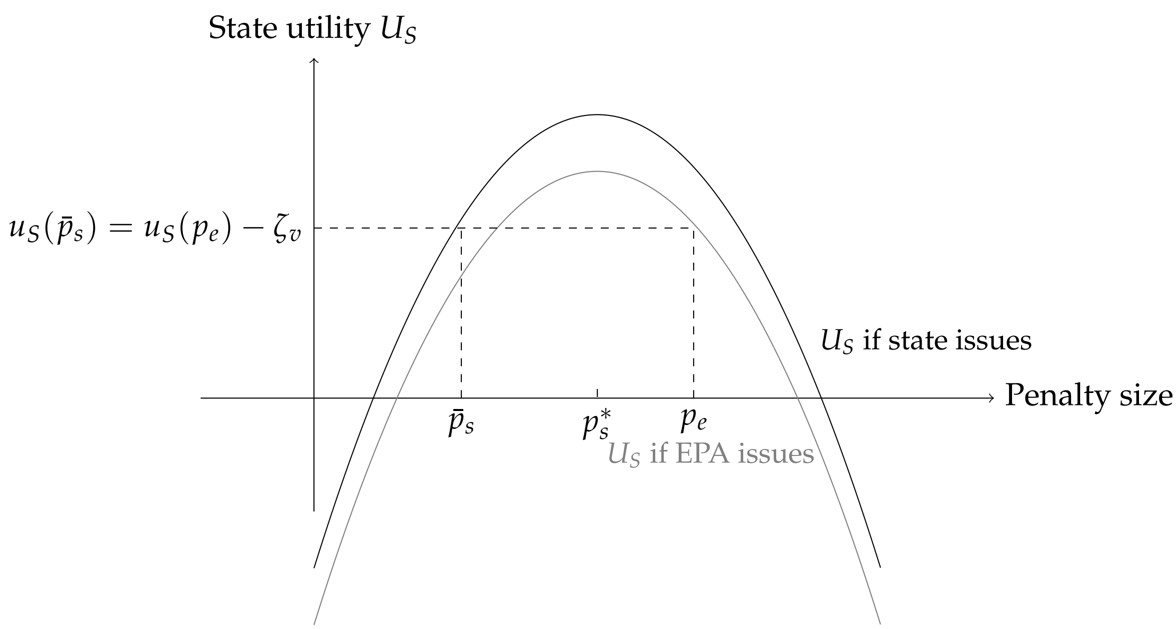
The California Department of Toxic Substances Control (DTSC) maintains a database known as CalSites which is used to track properties affected by hazardous substances. In addition, DTSC works under a cooperative agreement with the United States Environmental Protection Agency (U.S. EPA) to conduct screening of sites suspected of being contaminated with hazardous substances.

In accordance with the CalSites procedures, you were sent letters on March 14, 1994 and January 21, 1995 indicating that DTSC believed a Preliminary Endangerment Assessment (PEA) should be conducted on the subject site. You were requested to provide any information you may have which would indicate that a PEA is not warranted.

DTSC has received no response to the letters sent to you. If no response is received relative to the subject site within the next 30 days, DTSC may submit the site as a candidate for U.S. EPA screening and will maintain a "Preliminary Endangerment Assessment Required" status in the CalSites database.

These letters were found during visits to the Maine (Panel A) and California (Panel B) environmental agency records rooms. We include them as examples of states invoking the gorilla in their dealings with companies.

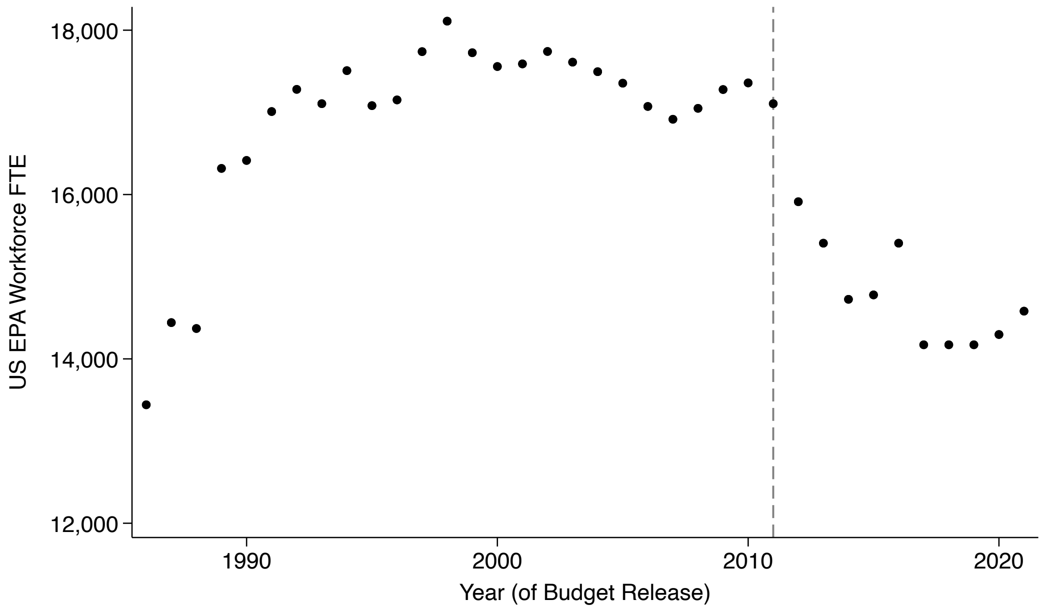
Figure 2: Equilibrium Determination of Firm's Penalty



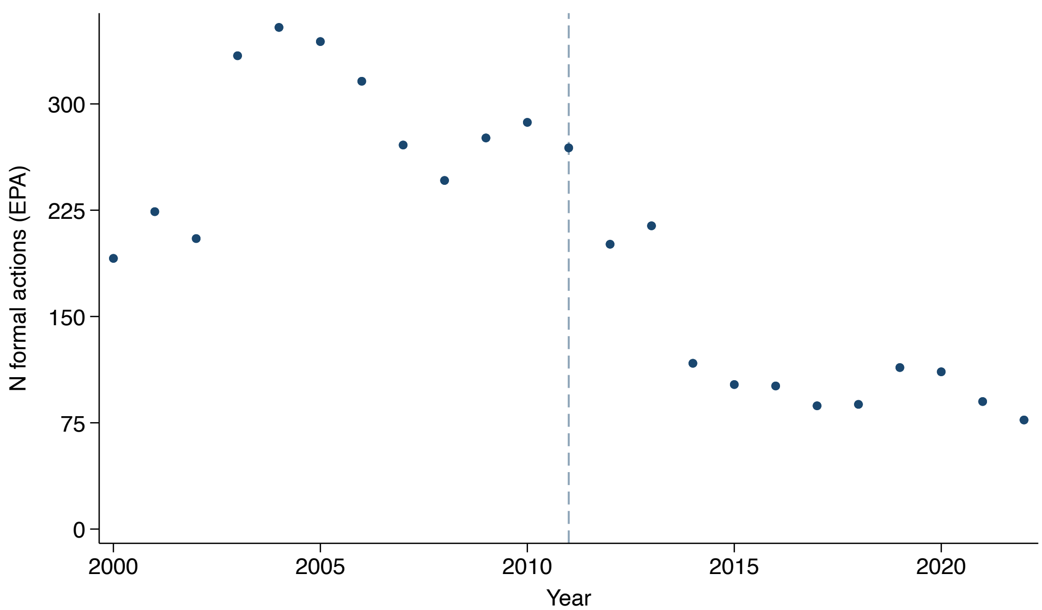
The figure illustrates how, when the EPA does not have sanction power, a harsher EPA can decrease equilibrium penalties collected by states. The black (gray) curve shows the state's utility over penalty size when the state (EPA) issues the penalty. EPA penalty e (i.e., the penalty the case would receive if the state sent the case to the EPA) is marked. The dashed horizontal line indicates the utility level the state receives from sending the case to the EPA; where it intersects the state's utility curve if the state handles the case provides the value for the equilibrium firm offer $\bar{p}_s$.

Figure 3: US EPA Budget Cuts

(A) Workforce FTE By Year

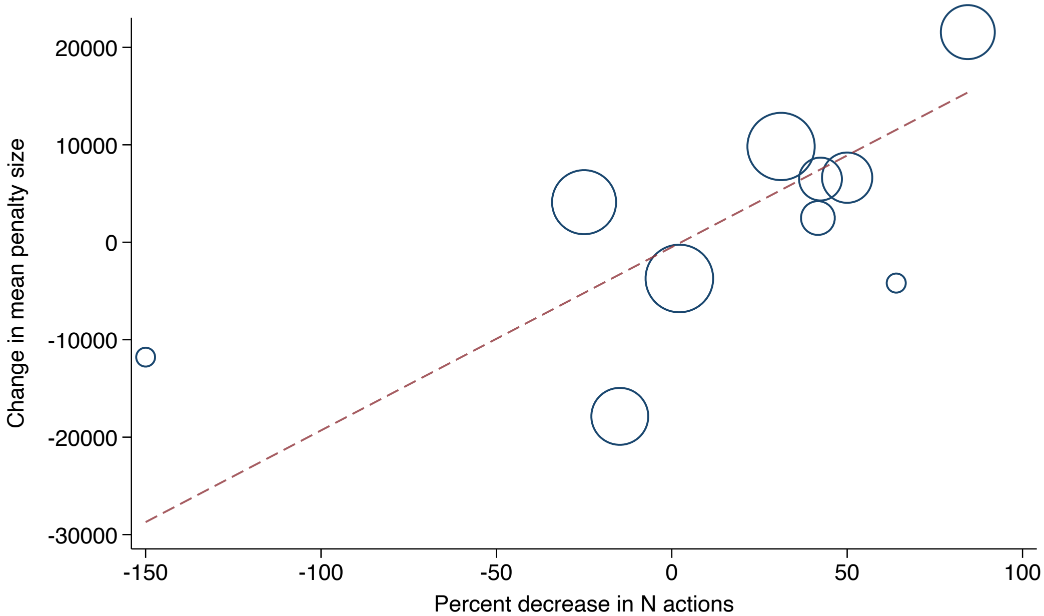


(B) Total US EPA Formal Actions under CAA



Panel A shows the number of workforce FTE budgeted for the US EPA by year. Source: https://www.epa.gov/planandbudget/budget. Panel B shows the number of formal enforcement actions the US EPA settled for Clean Air Act violations in each year. Refer to Appendix D for details on data construction. Panel B shows that the EPA regional offices with the largest decreases in enforcement actions (in percent terms) also had the largest increases in average penalty size.

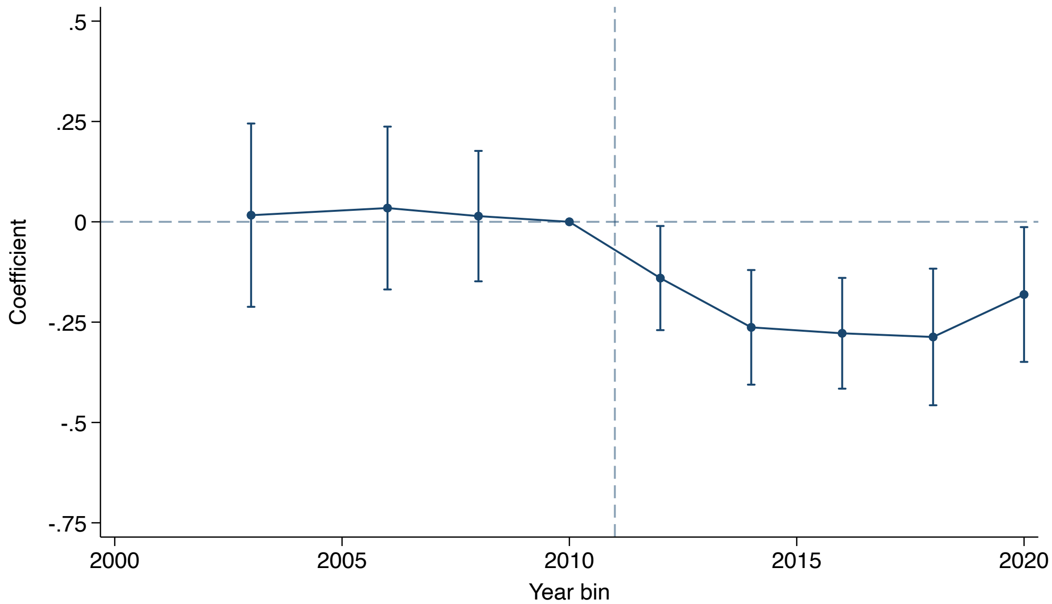
Figure 4: By US EPA Regional Office: Δ N Actions vs. Δ Average Penalty



The x-axis shows the percent *decrease* in number of formal enforcement actions, so that higher values correspond to larger decreases. The x-axis value is calculated the percent difference in the number of formal enforcement actions between 2007-2010 and 2012-2015. The regions are weighted by the number of CAA facilities located in their region across the sample period.

Figure 5: Clean Air Act Results

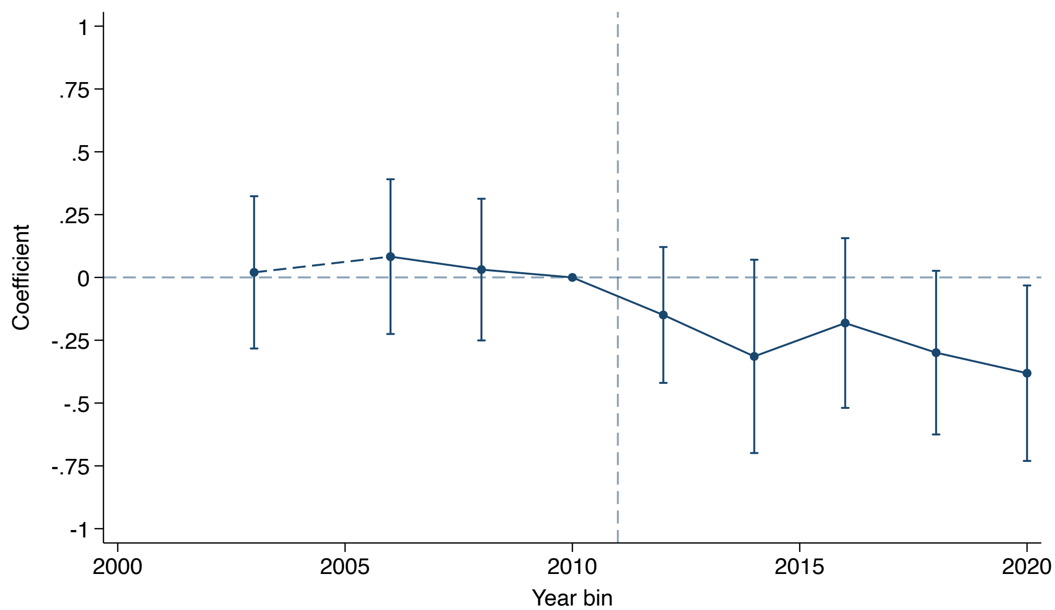
State Penalty Size over Time



The figure plots coefficients from regressing penalty size on year bin dummies, with our baseline controls (state unemployment and facility emissions bins) and state fixed effects.

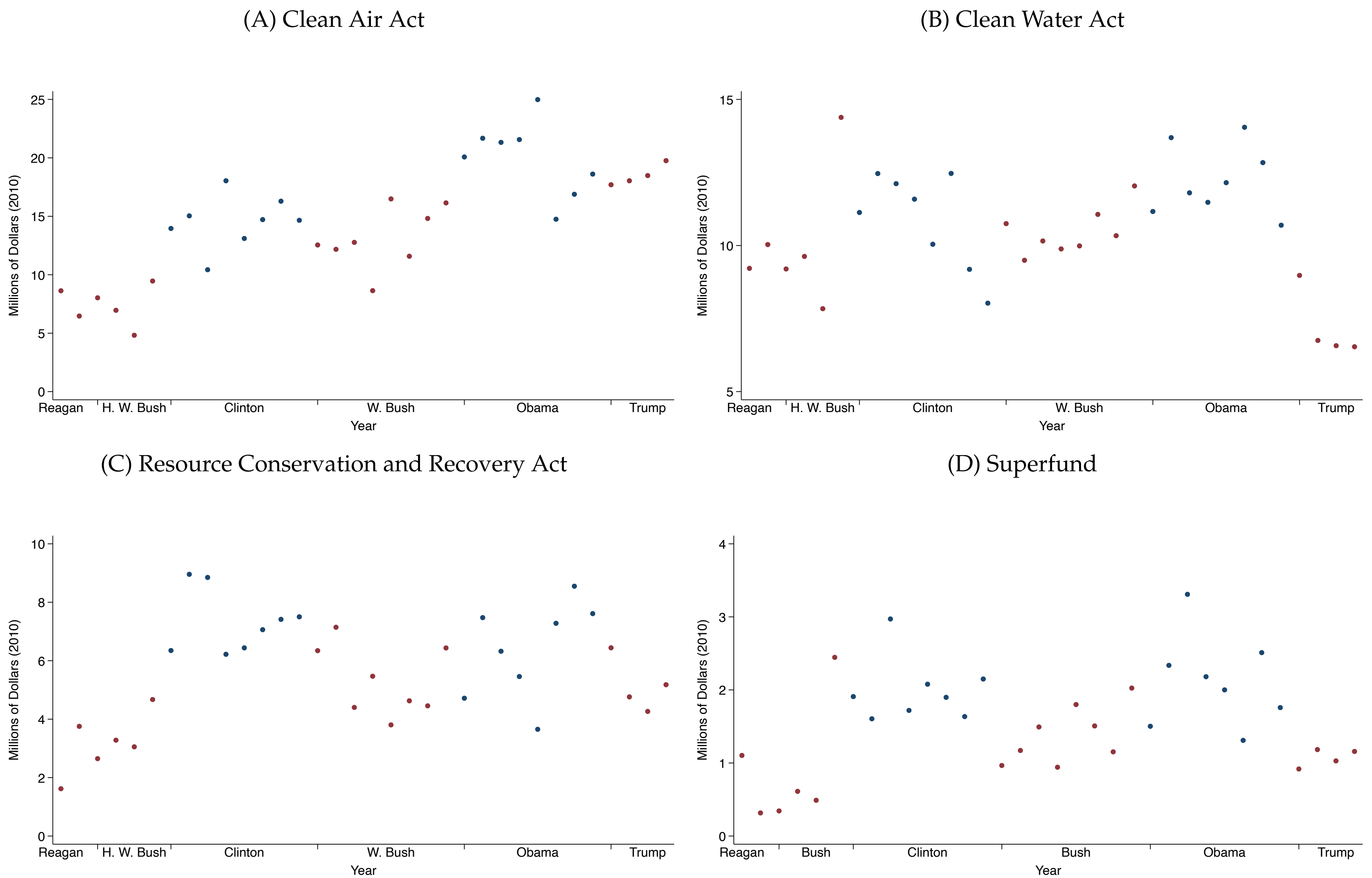
Figure 6: Clean Air Act Results

Difference-in-Differences



The figure shows the β coefficients from estimating Equation 2 as an event study, binning penalties into two-year bins. The outcome is penalty size. Whiskers show the 95% confidence intervals.

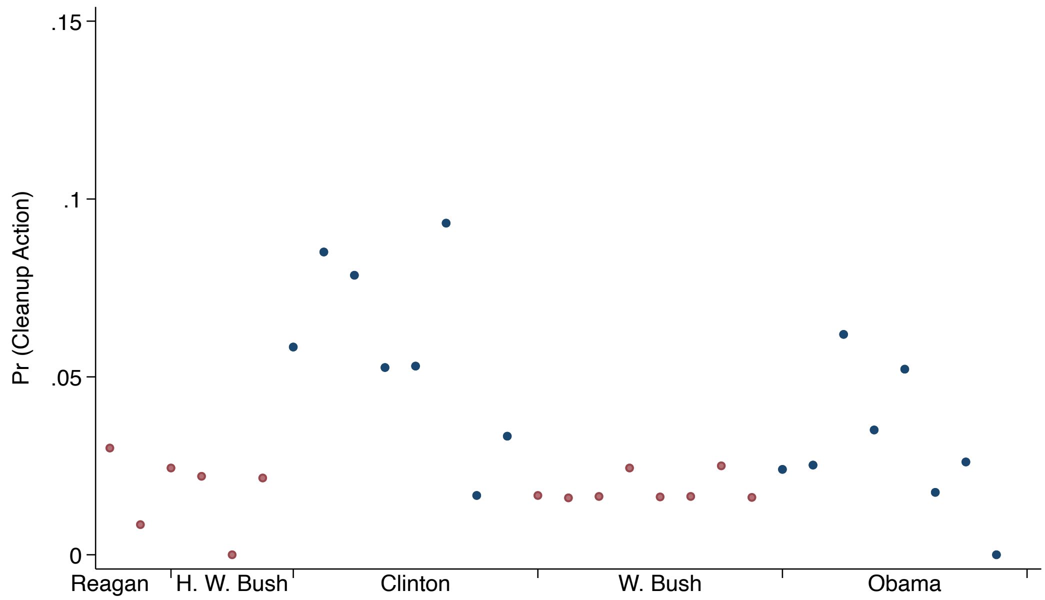
Figure 7: Total US EPA Penalties Collected by Presidential Administration



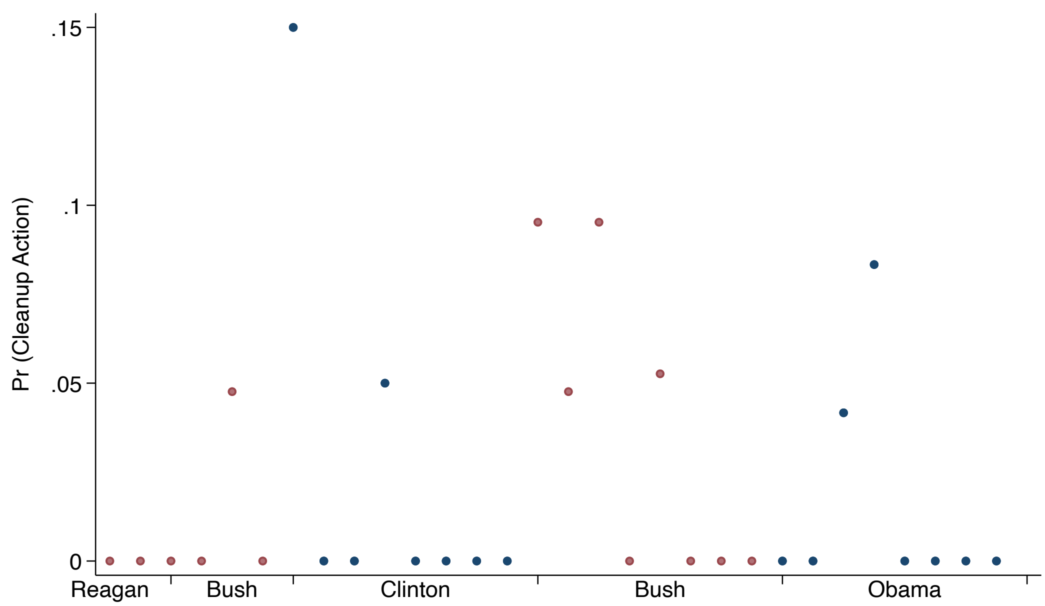
The figure shows the total dollar amount (in millions, adjusted to 2010 dollars) of penalties issued by the US EPA for violations under each of the major environmental statutes, by presidential administration. The data were downloaded from EPA "Enforcement Case Search" tool. Only penalties assessed in "Final Order with Penalties" cases are included. Cases are assigned to the year they were settled, and penalty amounts winsorized at the 5th and 95th percentile. Cases that were settled more than three years after they were filed are dropped from the data, as are cases missing a filing or settlement date. Early data is less complete; EPA reports that data quality before November 2000 has not been assessed.

Figure 8: Remedial Actions by Year

(A) PRP-led sites



(B) Orphan Sites



The figure shows the probability of a remedial action completion in a given year for PRP-led vs. orphan sites, residualized on site fixed effects and a linear control for time since initial documentation.

9 Tables

Table 1: State Penalty Size (CAA)

	Baseline Controls	+ Facility Controls	+ State Budget Control
Post × Regional decrease	-0.293**	-0.285**	-0.284**
	(0.138)	(0.137)	(0.139)
Major facility		0.260***	0.261***
		(0.046)	(0.045)
Non-public facility		0.025	0.023
		(0.057)	(0.057)
State budget (per capita)/1k			0.000
			(0.066)
State FE	X	X	X
Year FE	X	X	X
Industry X Year FE	X	X	X
Penalty Mean	8.75	8.75	8.75
Penalty SD	1.34	1.34	1.34
Obs	21,315	21,315	21,285
R^2	0.26	0.27	0.27

The table shows the results from estimating Equation 2. All columns include state fixed effects, year fixed effects, and 3-digit NAICS industry fixed effects interacted with years. The post period begins in 2011, and "Regional decrease" gives the treatment intensity, as measured by the regional decrease in federal enforcement actions after the budget cuts. Baseline controls are controls for emissions bins (a proxy for facility size) and state unemployment rate bins. The third column adjusts for state-year budgets, as measured by total state expenditures, from the Census Annual Survey of State and Local Government Finances. Standard errors are clustered by state. Data are from 2001-2020. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

Table 2: California State Superfund: Probability of Any Remedial Actions

	All	Firm-led	Orphan	All
Republican admin. (federal)	-0.027***	-0.032***	0.007	0.008
	(0.006)	(0.006)	(0.017)	(0.015)
(Years since discovery)/10	0.008**	0.009**	0.006	0.008**
	(0.004)	(0.004)	(0.008)	(0.004)
Rep Adm × Firm-led				-0.040**
				(0.016)
Site FE	X	X	X	X
Mean During Dem Adms	0.04	0.05	0.02	0.04
N Sites	246	212	36	246
R^2	0.08	0.08	0.08	0.08

The table shows the probability that a cleanup site under California DTSC jurisdiction experienced a remedial action in a given year, using the linear probability model defined in Equation 3. The data are at the site-year level. Sites can experience multiple remedial actions and are removed from the panel when they are certified as no longer requiring additional remediation. The first and final columns use all sites at least 3 acres in size; the second and third columns use subsamples of firm-led and orphan (state-led) cleanups, respectively. All columns have site fixed effects. Standard errors are clustered by site. Data are from 1987-2016. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

Table 3: Projected Costs of Chosen Remedial Alternatives

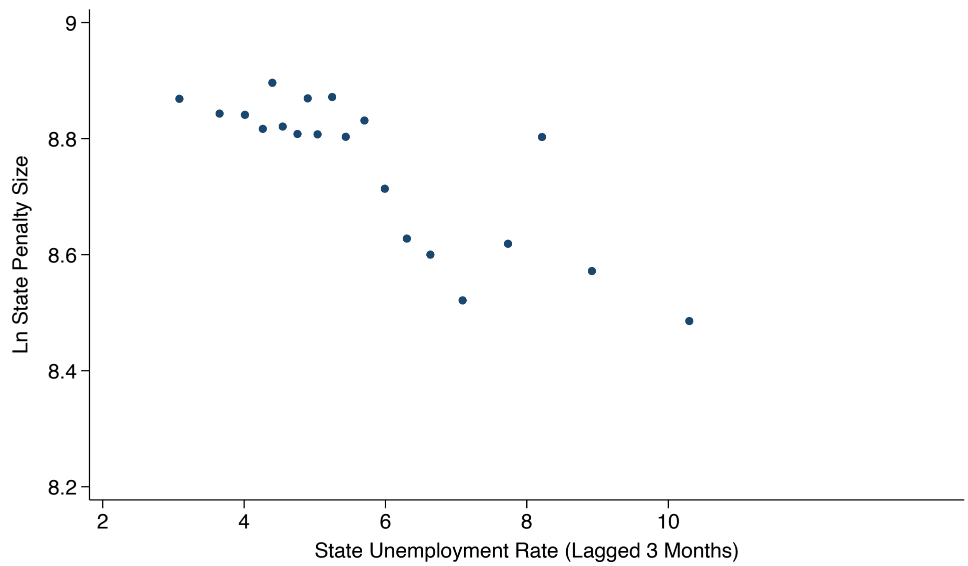
	Firm-led sites	All sites
Republican administration (federal)	-5.583**	-2.298
	(2.198)	(1.587)
Firm-led		6.853***
		(2.603)
Rep adm × Firm-led		-3.449
		(2.747)
N Sites	92	103
Mean During Dem Adms	9.55	9.08
SD During Dem Adms	15.56	15.06
R^2	0.04	0.07

The table shows that firms choose less expensive remedial actions for cleanups under California DTSC jurisdiction during Republican presidencies. The first column includes only firm-led cleanups, and the second column includes all cleanups. The outcome is the project cost of the chosen remedial alternative. The data are at the site level and only include sites under 3 acres where we were able to find information on projected costs. Sites are assigned to years based on when the document with the remedial alternatives was finalized. Standard errors are clustered at the site level. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

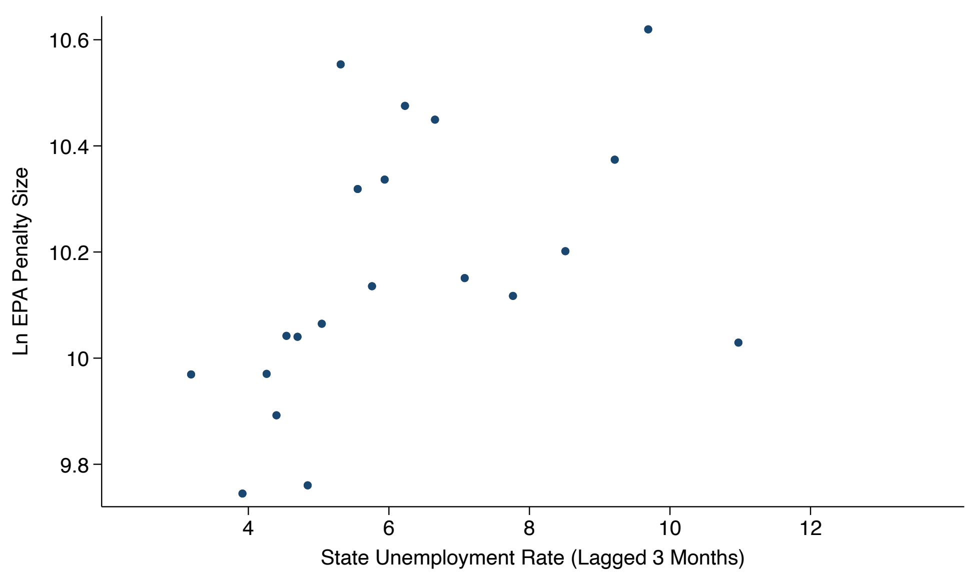
A Appendix Figures

Figure A.1: Standardized Penalties vs. States Unemployment Rates

(A) State Penalties

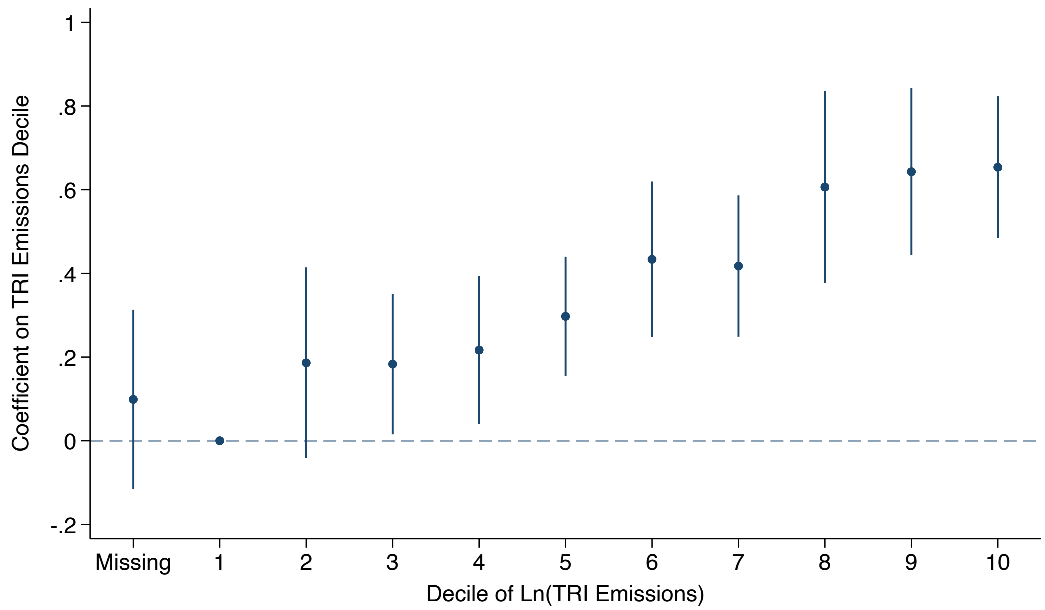


(B) US EPA Penalties



The figure uses data from (log) penalties in formal enforcement actions by states and by EPA for violations of the Clean Air Act by major and synthetic minor stationary sources (see Section 4 for more information). Panel A shows log state-issued penalty size against state unemployment rates, lagged 3 months. Panel B repeats this for EPA-issued penalties. Scatterplots are residualized on state fixed effects and year fixed effects.

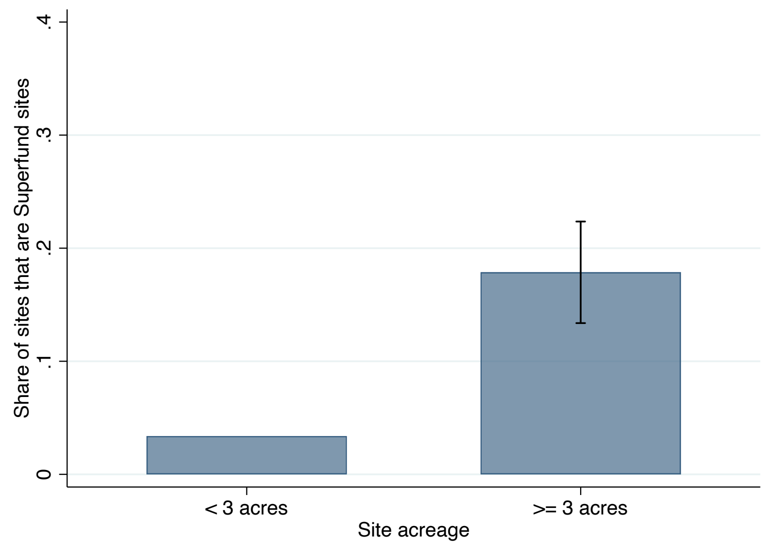
Figure A.2: CAA: Log Penalty Size vs. TRI Emissions



TRI emissions deciles are calculated using the log of total air emissions reported in EPA's Toxic Release Inventory (TRI). The coefficients are on dummies for the TRI deciles (and a dummy for missing TRI data), where outcome is log penalty size. The regression includes state fixed effects and industry-by-year fixed effects.

Figure A.3: Probability of NPL listing

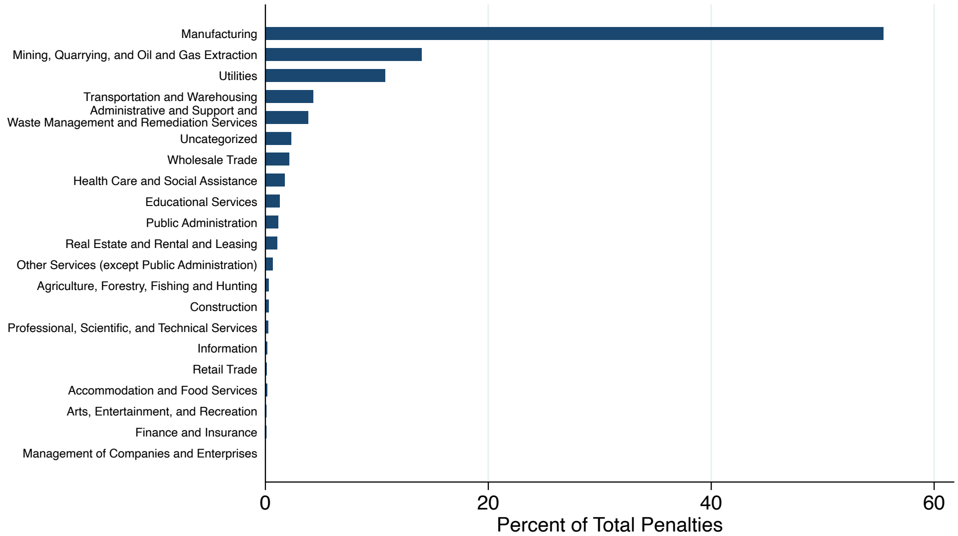
By Site Acreage



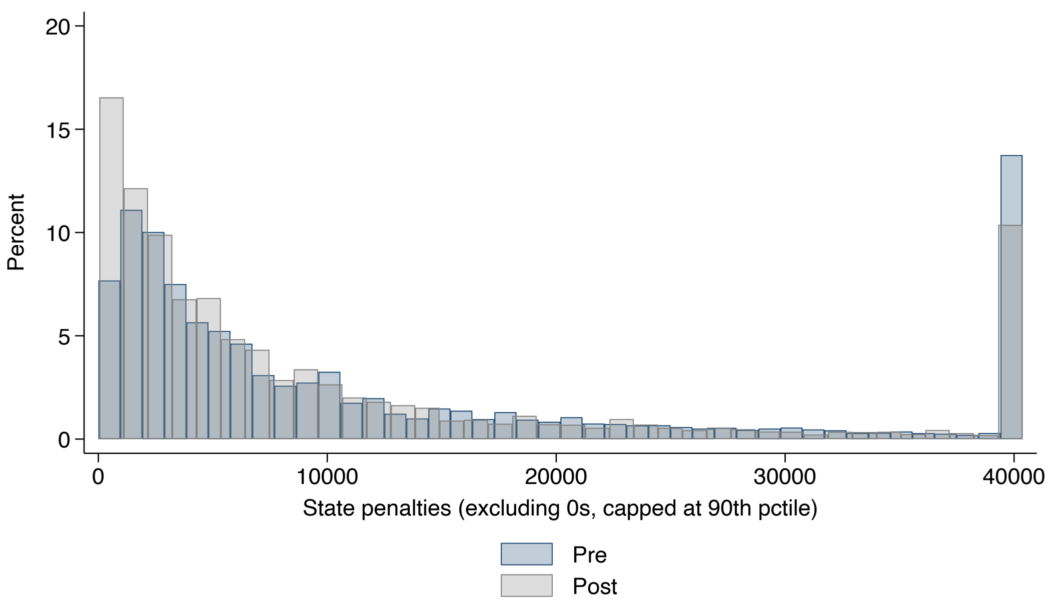
The figure shows that CA DTSC sites over 3 acres are substantially more likely to be listed as federal Superfund sites (i.e., on the National Priorities List) by the EPA.

Figure A.4: Clean Air Act Descriptives

(A) Distribution of Industries in CAA Penalty Data

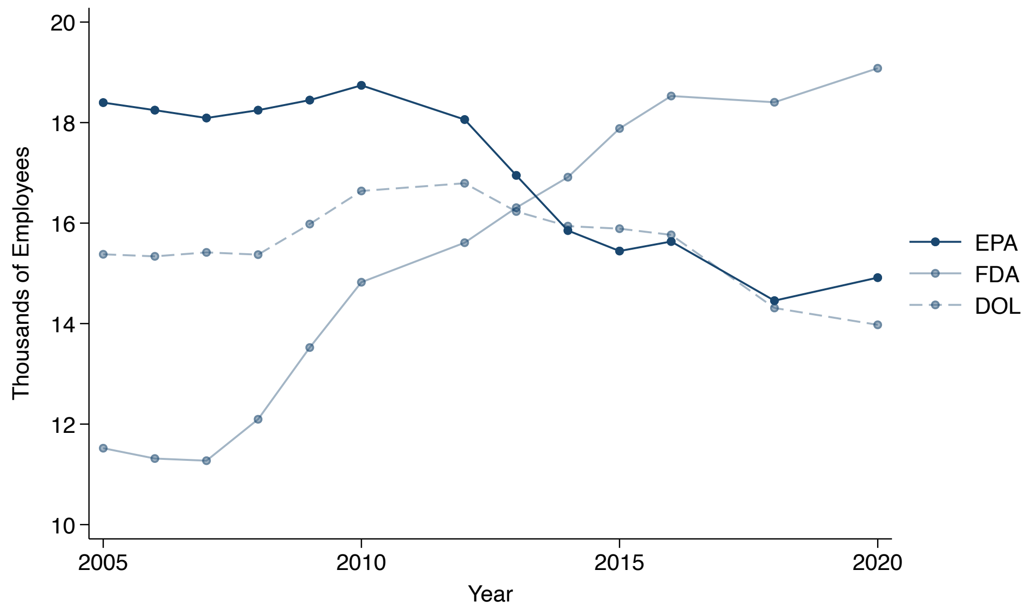


(B) State Penalty Distribution



Panel A shows the share of state-issued CAA penalties which fall into each 2-digit NAICS industry group. Panel B shows the distribution of state penalties before versus after the 2011 EPA budget cuts.

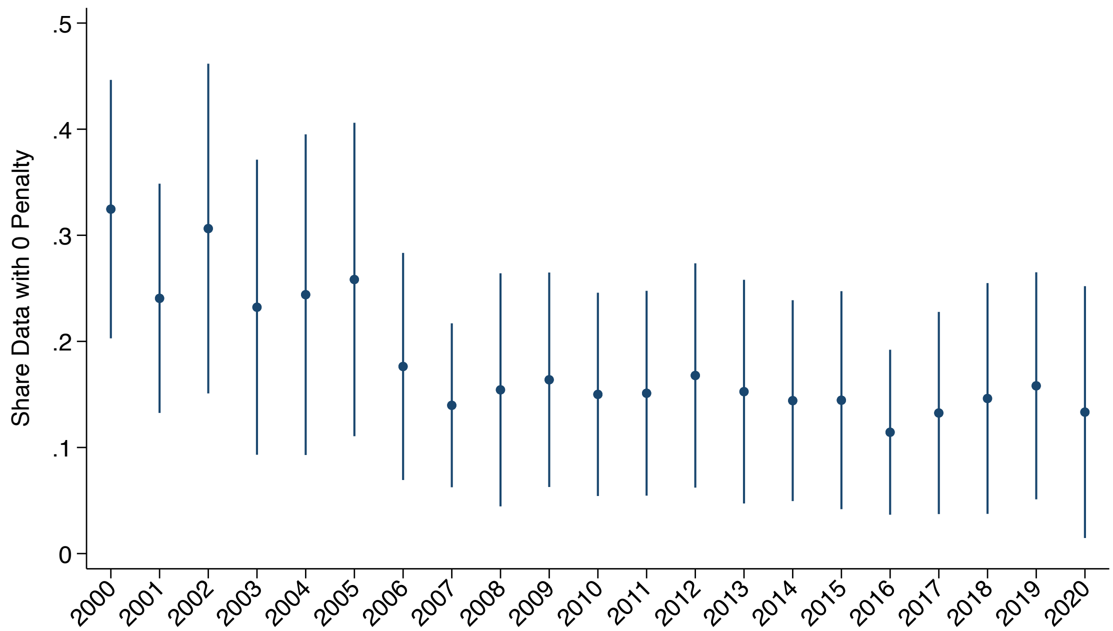
Figure A.5: Workforce Declines: EPA vs. Similar Agencies



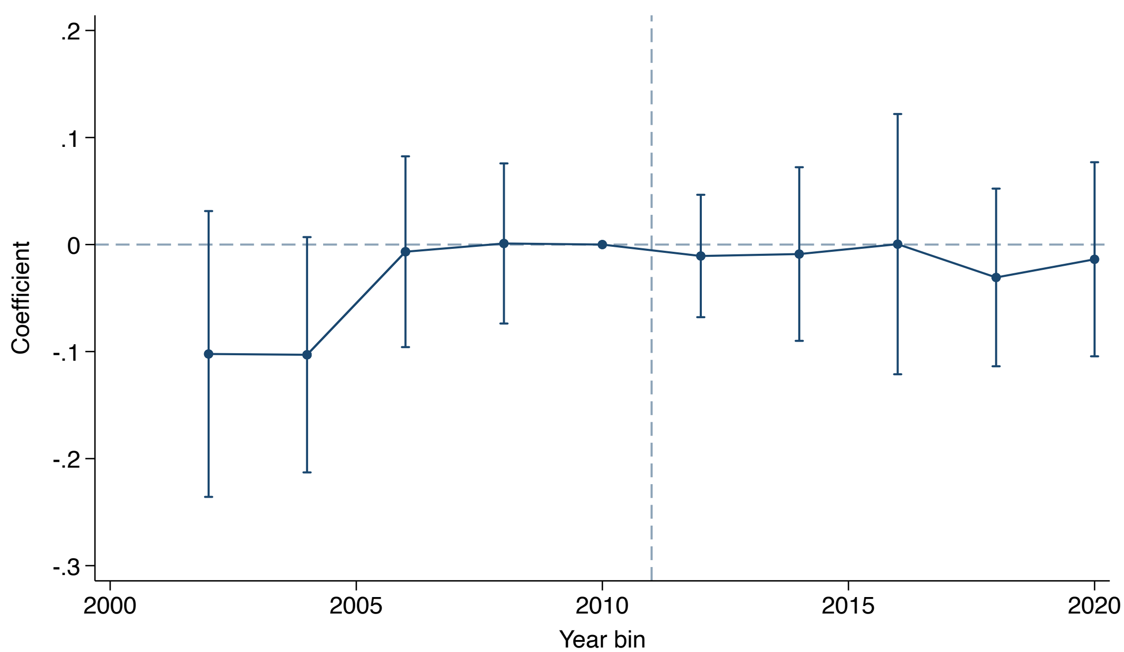
The figure plots total employment in the EPA during the sample period against employment in two similarly-sized agencies: the Food and Drug Administration (FDA) and the Department of Labor (DOL). Source: US Office of Personnel Management (Fedscope). Accessed here: https://www.fedscope.opm.gov/

Figure A.6: Inconsistent Penalty Reporting in the Early Sample Period

(A) Share Zero Penalties by Year



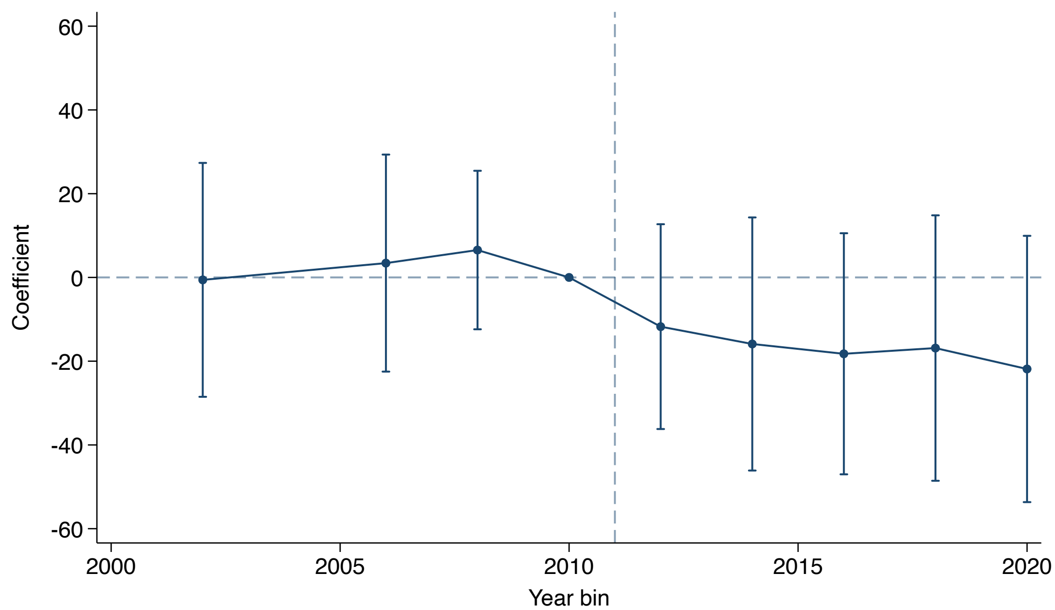
(B) Outcome: Indicator for Penalty Coded as Zero



Panel A shows the share of state-issued formal enforcement actions in ICIS-AIR which have a penalty coded as zero, by year. Zero penalties become much less common starting around 2005-2006. Panel B replicates Figure 6, replacing the outcome with an indicator for a penalty coded as 0.

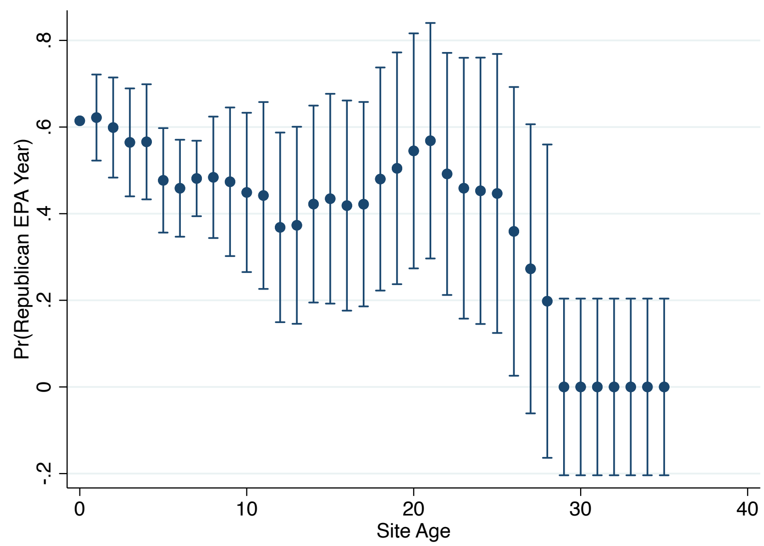
Figure A.7: Clean Air Act Results

Total Penalties Issued (Per CAA Facility)



The figure shows how the sum of penalties issued per facility changes over time by states' treatment intensity. The figure uses a state-year-level dataset. The outcome is the sum of penalties issued per CAA facility in the state, where the number of CAA facilities in the state is calculated from EPA's ICIS-AIR "air facilities" dataset and includes facilities which are no longer operating. States are assigned treatment values as in Figure 6. The regression includes controls for the state-year unemployment rate and the composition of industries in the penalty data, and is weighted by the number of CAA facilities in the state throughout the entire sample period. Standard errors are clustered at the state level. Whiskers show the 95% confidence intervals.

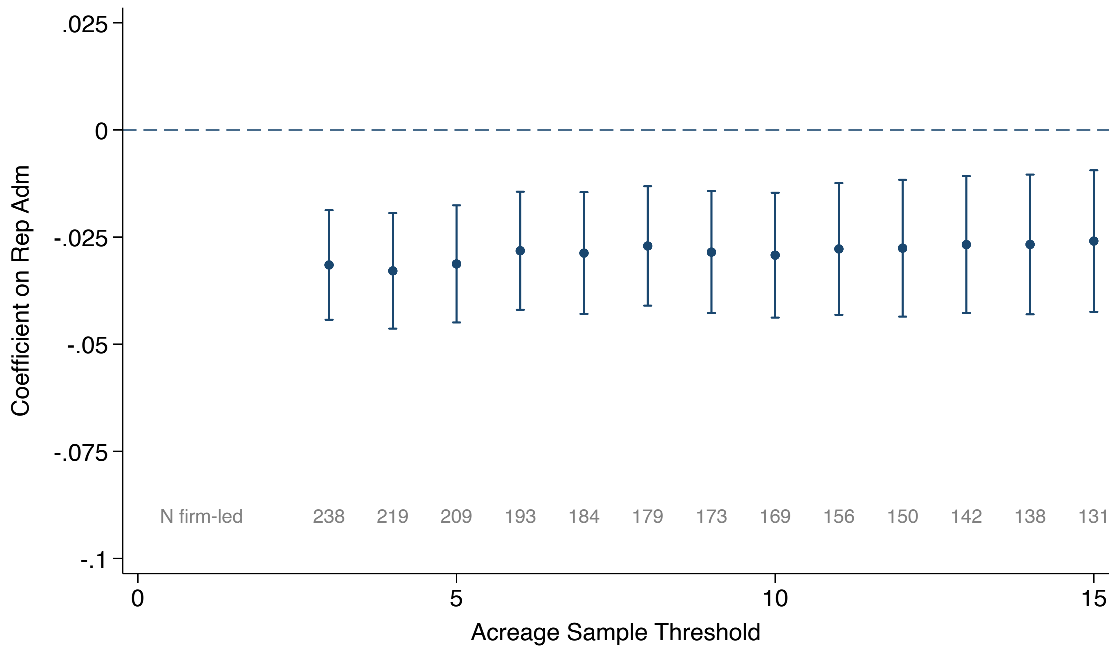
Figure A.8: Probability of Republican EPA by Site Age



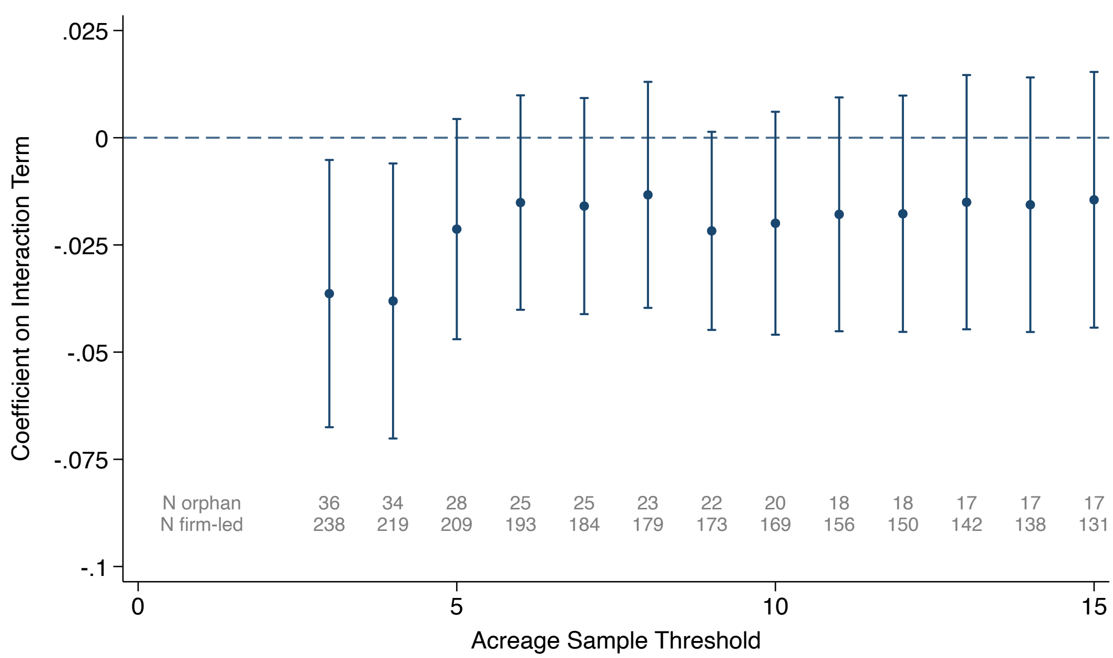
Using the site-year panel, the figure shows the coefficients from a single regression of an indicator for a Republican EPA in a given year on fixed effects for site age in that year. The figure shows that the oldest ages of a site are least likely to be under a Republican EPA. Standard errors are clustered at the year level.

Figure A.9: Superfund: Robustness to Acreage Restriction

(A) Main Effect (Table 2, Column 2)



(B) Interaction Effect (Table 2, Column 4)



The figure shows the coefficients from Table 2 varying the acreage cutoff for the regression sample. The number of orphan and firm-led sites in each regression are displayed in gray at the bottom of each figure.

B Appendix Tables

Table B.1: Clean Air Act: Effects Across Penalty Distribution

	Pr penalty size is...			
	$< 1k$	$< 5k$	$< 10k$	$< 20k$
Post × Regional decrease	0.0579*	0.116**	0.0642*	0.0306
	(0.0308)	(0.0513)	(0.0340)	(0.0253)
Share Penalties	0.09	0.42	0.61	0.76
Obs	21,422	21,422	21,422	21,422
R^2	0.15	0.19	0.18	0.17

Using the regression specified in Equation 2, this table shows effects on penalty sizes throughout the penalty distribution. States in EPA regions with larger enforcement decreases have overall decreases in penalty size; however, this decrease is concentrated among medium-sized penalties. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

Table B.2: CAA: Wild Cluster Bootstrap p-values

	(1) Baseline Controls	(2) + Facility Controls	(3) + State Budget Control
Post $\times$ Regional decrease	-0.291	-0.283	-0.284
p-value	0.018	0.022	0.032

The table shows wild cluster bootstrap p-values for the main coefficient in Table 1. We cluster at the region-by-post-period level, since our treatment of EPA strength varies at the region level as well as pre- vs. post-budget cuts.

Table B.3: Site Cleanup Descriptives

	All Sites	Regression Sample		
		All	Firm-led	Orphan
Share Ever Remedial Action	0.21	0.41	0.43	0.28
Share Ever Removal Action	0.46	0.52	0.53	0.47
Share Ever Certified	0.45	0.46	0.48	0.36
Median Acreage	3.3	16.3	16.9	11.0
Share 2+ Media Affected	0.57	0.58	0.59	0.53
Count	492	248	212	36

The table shows descriptive statistics for the DTSC cleanup regression sample, as it compares to all "State Response" sites in the data (see Appendix D for more information on the sample selection). The regression sample is limited to sites that cover at least 3 acres. The sum of firm-led sites and orphan sites exceeds the total number of sites in the regression sample because two sites switch from firm-led to orphan during the sample period. The first three rows show the share of sites that had the specified cleanup activities during our sample period (1987-2016). The last row shows the share of sites that have contamination in more than one media (soil, groundwater, etc.).

Table B.4: Hazard Ratios from Cox Hazard Model

	All	Firm-led	Orphan	Interaction
Rep-appointed EPA admin	0.321***	0.285***	0.667	0.734
	(0.0761)	(0.0740)	(0.369)	(0.464)
Orphan funded				0.639
				(0.262)
Rep Adm × Firm-led				0.382
				(0.262)
N Sites	276	234	36	270

The table shows hazard ratios from a Cox hazard model. Standard errors are clustered by site. Data are from 1987-2016 and limited to sites at least 3 acres in size. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

Table B.5: Probability of Cleanup: Robustness

State Political Environment

	All	Firm-led	Orphan	All
Republican admin. (federal)	-0.027***	-0.033***	0.008	0.007
	(0.006)	(0.006)	(0.017)	(0.018)
(Years since discovery)/10	0.010**	0.011**	0.001	0.010**
	(0.004)	(0.005)	(0.008)	(0.004)
Rep CA governor	0.005	0.009	-0.016	0.005
	(0.006)	(0.006)	(0.011)	(0.007)
Rep Adm $\times$ Firm-led				-0.040**
				(0.019)
Mean During Dem Adms	0.04	0.05	0.02	0.04
N Sites	246	212	36	246
R^2	0.08	0.08	0.08	0.08

The table replicates Table 2, adding a control for the political party of the California governor in each year. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

Table B.6: Probability of Cleanup: Robustness

Data Decisions

	All	Firm-led	Orphan	All
Panel A. Using project completion dates				
Republican admin. (federal)	-0.028***	-0.035***	0.017	0.018*
	(0.006)	(0.007)	(0.012)	(0.009)
(Years since discovery)/10	0.020***	0.020***	0.019*	0.020***
	(0.004)	(0.005)	(0.010)	(0.004)
Rep Adm × Firm-led				-0.052***
				(0.011)
Site FE	X	X	X	X
Mean During Dem Adms	0.05	0.05	0.02	0.05
N Sites	246	212	36	246
R^2	0.09	0.10	0.08	0.10
Panel B. Including removal actions				
Republican admin. (federal)	-0.019**	-0.027***	0.026	0.020
	(0.010)	(0.010)	(0.032)	(0.030)
(Years since discovery)/10	-0.021***	-0.023***	-0.009	-0.021***
	(0.008)	(0.009)	(0.018)	(0.008)
Rep Adm × Firm-led				-0.046
				(0.031)
Site FE	X	X	X	X
Mean During Dem Adms	0.08	0.09	0.05	0.08
N Sites	246	212	36	246
R^2	0.11	0.11	0.12	0.12

The table replicates Table 2, using metadata for project completion dates instead of manually extracted project start dates. *, **, *** indicate coefficients are significant at the 10%, 5%, and 1% significance level, respectively.

C Additional analysis: Support for assumption that $\beta \neq 1$

Our model We begin by asking whether EPA appears to have different preferences than the states. In this section, we provide evidence that average state penalty size responds to local economic conditions, but EPA penalty size does not, suggesting that states and EPA have different objective functions. In the model, this would indicate that $\beta \neq 1$, raising the question of whether EPA is too harsh or too lax for the states.

C.1 Empirical Strategy

We present two analyses using Clean Air Act penalties data to compare the role of economic conditions in state versus federal enforcement decisions. In both analyses, we show that state enforcement outcomes are more related to economic factors than are federal enforcement outcomes.

Our first analysis uses penalties issued in 2005-2006 and 2010-2011 to show that after the Great Recession, industries more affected by the recession faced lower average penalties from states but not from EPA. We use the following regression specification, separately for EPA penalties and for state penalties:

$$\begin{aligned} \text{Ln(Penalty Size)}_{j,i,s,t} = {} & \alpha \mathbb{1}(\text{Low-growth industry})_{i,s} \\ & + \beta \mathbb{1}(\text{Low-growth industry})_{i,s} \times D_t + \delta_s I_S + \gamma_t + \rho_i + \epsilon_{j,s,t} \end{aligned} \tag{C.1}$$

where Ln(Penalty Size)$_{j,i,s,t}$ is the natural log of the size of penalty j in industry i, state s, and year t. D indicates the penalty was issued after the Great Recession (2010-2011), I_S indicates the penalty was issued by the state, and $(\text{Low-growth industry})_{i,s}$ indicates the facility was in an industry with a bottom-quantile growth rate from 2005 to 2009. We calculate leave-out industry growth rates as

$$\text{industry growth}_{i,s} = \frac{\left[\left(\sum_{s' \neq s} x_{i,s',2009}\right) - x_{i,s,2009}\right] - \left[\left(\sum_{s' \neq s} x_{i,s',2005}\right) - x_{i,s,2005}\right]}{\sum_{s' \neq s} x_{i,s',2005} - x_{i,s,2005}},$$

where x is the total number of establishments or total employment, depending on the specification, in 3-digit NAICS industry i in state s in the specified year. Because we use leave-out growth rates, the ranking of industries differs slightly across states. Because of the limited number of EPA penalties issued, state fixed effects are only included in the regressions using state-issued penalties. In additional specifications, we add controls for facility emissions.

C.2 Results

We document that penalties issued by states appear to be more sensitive to economic conditions than are penalties issued by US EPA. Prior research has shown that air pollution is lower in bad economic times (Chay and Greenstone, 2003; Feng et al., 2015; Finkelstein et al., 2023), suggesting that our estimates of states' lower penalties after the recession may be driven by less severe violations. However, emphasizing the *difference* between state and EPA enforcement in good versus bad times belies this concern.[39]

First, we compare the relationship between state unemployment rates (lagged by three months) and penalty size. Figure A.1 shows this relationship for state-issued penalties and for EPA-issued penalties. There is a distinct downward-sloping relationship for states, and no such relationship for EPA-issued penalties. Of course, this could be driven by lower state enforcement capacity during bad economic times.

We turn to our main analysis of whether penalties responded to the economic shock of the Great Recession. Appendix Figure A.4A shows the distribution of industries represented in the penalty data. More than half of penalties are issued to manufacturing facilities, which make up almost the entirety (over 98%) of industries in the bottom quartile of industry growth between 2005 and 2009.

Table C.1 shows the results of estimating Equation C.1. We see that in industries hit hardest by the Great Recession, state penalties decrease after the Great Recession, while

[39]We also note that a plurality (if not majority) of CAA violations that result in formal enforcement actions are not necessarily for excess emissions but rather for procedural noncompliance such as inappropriate equipment and processes. Violations of, for example, abatement equipment requirements, should not be less likely in bad economic times.

EPA penalties do not. This is true whether industry decline is measured using the number of establishments or total employment, although the effect is larger using the number of establishments. Using establishment counts as our measure of industry growth, penalties are 25% lower in hard-hit industries relative to other industries and relative to penalties before the Great Recession. The results are also robust to including state-by-year fixed effects instead of state and year fixed effects and to using midpoints of employment ranges where employment data is suppressed. Altogether, it appears that states treat suffering industries differently from other industries, but EPA does not; however, we are not able to reject that the coefficients are the same.[40]

As we noted, the comparison of state penalties to EPA penalties should address most concerns that systematically varying emissions are driving our results. However, we do include two additional analyses related to this concern. First, Appendix Table C.2 uses the sample of all TRI-reporting major and synthetic minor facilities (not only those with penalties) and estimates effects of the recession on reported emissions. For this analysis, we are able to use facility fixed effects. Our employment measure of industry growth is significant related to emissions, but the magnitude of the effect is small (less than 5% of a standard deviation). In Appendix Table C.3, we replicate Table C.1 including a control for facility emissions reported in the TRI. While reported emissions are strongly related to penalty size, this control has minimal effect on the coefficient of interest, suggesting that changes in emissions are not driving the results.

Together, these results show that EPA's penalties do not respond in the same way as state penalties do to economic conditions. Under the assumption that penalties do more damage to firm profits or employment in bad economic times, this suggests EPA down-weights economic harms of enforcement relative to state preferences. In this case, EPA might prefer higher penalties than states do. Is EPA *too* harsh for the states, or could a

[40]One concern with this analysis is that selection into state versus EPA enforcement could differ in suffering industries. If states are less likely to handle specifically the worst violations in suffering industries, we might see this pattern. In this case, states may not be concerned about economic harms from enforcement but may face political constraints on their enforcement. While we cannot test for selection specifically on violation severity, we see that facilities in low-growth industries are not significantly more likely to face EPA enforcement (versus state enforcement) after the Great Recession relative to other industries.

harsher gorilla help the states more? To answer this question, we next turn to implementing the model's empirical test in two contexts.

C.3 Tables

Table C.1: State vs. EPA Penalty Patterns

	Establishments		Employment	
	Ln(State penalty)	Ln(EPA penalty)	Ln(State Penalty)	Ln(EPA penalty)
1[Low-growth] $\times$ 1[2010-2013]	-0.25***	0.10	-0.15*	0.09
	(0.08)	(0.39)	(0.08)	(0.36)
Issuer FE	X	X	X	X
Year FE	X	X	X	X
Industry FE	X	X	X	X
Mean Ln(Penalty)	8.8	10	8.8	10
SD Ln(Penalty)	1.3	1.8	1.3	1.8
Obs	4,754	379	4,754	379
R^2	0.22	0.09	0.22	0.09

The outcome uses data from the two years before (2006-2007) and the two years after (2010-2011) the Great Recession. The specification is given in Equation C.1. The first set of columns measures industry growth using the number of establishments, and the second set of columns uses total employment. Within each set, the first column shows that penalties issued by states are lower after the Great Recession in industries hit harder by the Great Recession, relative to other industries and relative to penalties issued before the Great Recession, and the second column shows that penalties issued by EPA do not exhibit this pattern.
1[Low-growth] indicates the facility is in an industry with bottom-quartile growth from 2005 to 2009, as measured by the percent change in the number of establishments (total employment) nationwide, excluding establishments (employment) in the state issuing the penalty. This variable is calculated using data from U.S. Census's County Business Patterns. 1[2010-2011] is an indicator for the penalty being issued in 2010-2011. *, *** indicates the coefficient is significant at the 10% and 1% significance levels, respectively.

Table C.2: Great Recession: Log Emissions as Outcome

	Establishments	Employment
1[Low-growth] × 1[2010-2011]	0.03	-0.14***
	(0.04)	(0.03)
Facility FE	X	X
Year FE	X	X
Mean Ln Emissions	8.60	8.60
SD Ln Emissions	3.11	3.11
Obs	36,954	36,954
R^2	0.93	0.93

The table uses the sample of all TRI-reporting major and synthetic minor facilities in 2006-2007 and 2010-2011. We estimate Equation C.1, replacing the outcome with the log of reported emissions, and replacing state and industry fixed effects with facility fixed effects. Standard errors clustered at the state level are in parentheses. *** indicates statistical significance at the 1% significance level.

Table C.3: Great Recession: Emissions Controls

	Establishments		Employment	
	Ln(State penalty)	Ln(EPA penalty)	Ln(State Penalty)	Ln(EPA penalty)
1[Low-growth] × 1[2010-2011]	-0.26***	0.12	-0.14*	0.12
	(0.08)	(0.53)	(0.08)	(0.34)
Ln(TRI Air Emissions)	0.06***	0.17**	0.06***	0.17**
	(0.01)	(0.06)	(0.01)	(0.06)
State FE	X		X	
Year FE	X	X	X	X
Industry FE	X	X	X	X
Obs	4,754	379	4,754	379
R^2	.24	.14	.24	.14

The table replicates Table C.1, adding controls for TRI-reported emissions. Standard errors clustered at the state level are in parentheses. *, **, *** indicate statistical significance at the 10%, 5%, and 1% significance level, respectively.

D Data Appendix

D.1 Superfund

The data we use from the California Department of Toxic Substances Control (DTSC) come largely from their online database, called EnviroStor. EnviroStor is used internally to track cleanup projects, and much of it is accessible online so that interested members of the public can learn more about hazardous waste sites in California.

D.1.1 EnviroStor Sample

EnviroStor does not contain every site in Califronia with hazardous substance contamination. Sites that are not under DTSC jurisdiction are not included. For example, most petroleum contamination (which is often the result of leaking underground storage tanks) falls under the jurisdiction of the California State Water Resources Control Board.

At the same time, EnviroStor contains more sites than are relevant to this project. We apply sample criteria which exclude the the following:

Site types. The DTSC runs several programs which evaluate sites for potential contamination—these are largely military bases and sites proposed for acquisition or development by school districts. We limit the sample to "State Response" sites. This excludes "Evaluation" sites, which were largely historical or current programs that assess public property (schools and military sites) to check for contamination, as well as sites where contamination is (or was at one point) suspected but not confirmed. This is a large share of sites in the database (41.7%), but a much smaller share of documented activities (11.4%). We also exclude sites under the "Cal-Mortgage" program, a loan program for non-profit and public entities which requires environmental review (1% of all sites, and 0.1% of all activities).

The other large class of cleanup projects that are excluded from the sample are voluntary cleanups.

Referred sites. We exclude sites that were referred to other agencies (such as regional water boards) or other California environmental programs (such as the Resource Conservation and Recovery Act, or RCRA), since DTSC does not generally track activities at these sites. This criterion excludes an additional 21% of sites in EnviroStor.

No Action Required. Finally, we exclude sites with an EnviroStor "status" of "No Action Required." These are sites where contamination was not found in levels high enough to require cleanup. While 9% of the entire EnviroStor sample has this status, the vast majority are covered by the site types we exclude (required evaluations for public entities); this criterion only excludes an addition 1% of the sample beyond the above criteria.

D.1.2 Remedial Action Dates

We find remedial action dates from several sources. End dates come from the dates of "Remedial Action Completion Reports" in EnviroStor, which correspond to the date that the DTSC approved the remedial action completion.

Our main specification uses remedial action *start* dates as our outcome. These require examining the contents of the documents uploaded into EnviroStor. The source we prioritize for remedial action (RA) start dates is certification forms. Certification forms are uploaded after all required remediation activities—including, occasionally, monitoring for a period of time—are complete at a site. These forms are standardized and include a field for project start and end dates. There are two reasons we are not be able to get RA start dates for all projects from certification forms. The most common reason is that the site is not yet certified. The second reason is that the certification form does not specify the RA start date — it might instead give the date the DTSC ordered the cleanup, or the date the site assessment began. We are able to get start dates from certification forms for 59 of 148 remedial actions in the sample.

For the remainder of remedial actions during our sample period, we next turn to additional reports uploaded to EnviroStor. Where remedial action completion reports follow design and implementation plans (which describe the plan for the remedial action implementation) within less than a year, we consider the design and implementation plan date the start date. Otherwise, we turn to the Remedial Action Completion Report itself, a technical document often hundreds of pages in length. This report often includes the dates of the project implementation, usually in its introduction. If not in the introduction, dates can sometimes be found in dated documents in appendices—for example, in daily field reports or in date-stamped photographs of project implementation.

Where we are unable to find dates in certification forms or in technical reports, we use the RA Completion Report date. We use this for 11 of the 148 remedial actions in the data.

D.1.3 Judgement calls

We note two additional judgement calls in these data. First, two site (The "Wickes Forest Industries" sites and the "McNamara and Peepe Lumber Mill" site) began as firm-led sites and became orphan sites later, when the firms responsible for the contamination declared bankruptcy. Instead of using the funding source noted in EnviroStor, which gives the current funding source for the project, we consider these a firm-led sites until the first state work order (an indication of a response action which is state-funded) was issued.

Second, some activities uploaded into EnviroStor as Remedial Action Completion Reports use "removal action" terminology in the reports. (Activities are categorized based on their expected costs, where remedial actions are more expensive and involved than removal actions.) It's unclear whether these activities should be considered remedial actions or removal actions. We consider these remedial actions, deferring to the judgement of the project manager who uploaded the document in EnviroStor, but we show robustness to including all remedial *and* removal actions in our results.

D.2 Clean Air Act

D.2.1 Penalties

We adjust penalties to 2010 dollars using the BLS Consumer Price Index.

Some facilities have multiple enforcement actions on a given settlement date. We collapse the data so that a facility has at most one observation on any given date. When a facility has multiple enforcement actions on a given date, if the enforcement actions are associated with different penalty amounts, we sum the penalties within the date. If, however, the enforcement actions are associated with the same penalty amount (less than 5% of all facility-dates), we consider this a duplicate entry, and we keep only the first enforcement action.

Sometimes, EPA and a state will bring a case jointly. In the model, we consider joint state-EPA cases to be instances when the state "rejects" the penalty and reports it to the EPA. To exclude joint state-EPA cases in our data, we exclude any state enforcement actions that happen during the same month as an EPA enforcment action.

D.2.2 Toxic Release Inventory

We use EPA's "Basic Plus" files, and specifically, the "total air emissions" entry. For facilities ever observed in the TRI, we impute emissions in years when the facility is missing from the data using the last-reported year. We do not replace 0's (which EPA uses to indicate missing values as well as true 0s) in this process. In our main specification, we bin log TRI emissions into deciles, add a group for missing or zero TRI emissions, and include fixed effects for the emissions deciles and the missing TRI data control.

E Math Appendix

E.1 Model in Section 3

E.1.1 Equilibrium firm offers.

First, note that when the state has a comparative advantage in enforcement ($\zeta_v > 0$), for any EPA penalty p_e, the state will always accept some offer less than p_e (since with $\zeta_v > 0$, $u_S(p_e) + \zeta_v > u_S(p_e)$). Thus, being a cost-minimizer, the firm prefers state enforcement to EPA enforcement for any case with $\zeta_v > 0$. The firm will make the lowest offer possible that the state will accept.

Note also that the optimal sanctions threat for EPA to make, should it make any, is $k^* = u_S(p_{\bar{s}_v}) + \zeta_v - u_S(p_e)$. This is the threat that will convince the state to reject the case. If EPA offers less than this, the state will not reject the case, and the EPA will see no benefit to threatening sanctions at all.[41] Since sanction threats are costly, EPA will not choose a higher k than necessary, either. Thus, EPA chooses between $k = 0$ and $k^* = u_S(p_{\bar{s}_v}) + \zeta_v - u_S(p_e)$.

Imagine the firm offered less than $p_{\bar{s}_v} = \min\{p_{\tilde{s}_v}\}$, where $p_{\tilde{s}_v}$ is defined in Equation 1. A lower p_{s_v} reduces k^*, and increases the EPA's benefit of handling the case ($u_E(p_e) - u_E(p_{\bar{s}_v}) - \zeta_v$), such that $u_E(p_e) - u_E(p_{\bar{s}_v}) - \zeta_v > c * k^*$. In this case, EPA will threaten sanctions, the state will reject the case (by the definition of k^*), and the firm will face the EPA's penalty. This is costly for the firm, so the firm will not offer less than $p_{\bar{s}_v} = \min\{p_{\tilde{s}_v}\}$.

E.1.2 Proof of Proposition 1

Figure 2 gives a graphical proof.

For an algebraic proof: Let $c = \infty$. Equilibrium firm offers $p_{\bar{s}_v}$ are the minimum value that satisfies $u_S(p_{\bar{s}_v}) + \zeta_v = u_S(p_e)$.

[41] That EPA sees no benefit of sanctions unless the sanctions threat convinces the state to reject the case is a feature of the timing. If EPA chose its sanction threat before the firm made its offer (or if this was a repeated game), EPA would face a continuous problem of optimal sanction choice.

We prove this in cases. Both cases use that $u_S(p_e) + \zeta_v > u_S(p_e)$.

From here, we suppress v subscripts.

Case 1: assume that $p_e \leq p_s^*$. Then $u'_S(p_e) > 0$. Given this, $u_S(p_s) + \zeta < u_S(p_e) + \zeta$ for $p_s < p_e$. Since the firm is a cost minimizer, it will reduce its offer until $u_S(p_e) + \zeta = u_S(p_e)$.

Case 2: assume that $p_e > p_s^*$. By our assumptions on $b(p)$ and $\tau(p)$, there exists an $p_s < p_s^*$ such that $u(p_s) = u(p_e)$. From here, we are in Case 1.

E.1.3 Proof of Proposition 2

Let $c = \infty$, so that the firm offer $p_{\bar{s}}$ satisfies $u_S(p_{\bar{s}}) + \zeta = u_E(p_e)$. Then Proposition 2 follows easily from Proposition 1: we use that, by the assumptions on b and τ, U_S is increasing in penalty size p_s if and only if $p_s < p_s^*$. From Proposition 1, we know that $p_s < p_s^*$. Thus, if EPA strength increases firm offers, it also increases state welfare.

E.1.4 Proof of Proposition 3

Suppose that $c > \frac{u_E(p_e) - \zeta - u_E(p_s^*)}{u_S(p_s^*) + \zeta - u_S(p_e)}$ and $c < \infty$. Then we have that $c[u_S(p_s^*) + \zeta - u_S(p_e)] > u_E(e) - \zeta - u_E(p_s^*)$, i.e., that $ck > u_E(p_e) - \zeta - u_E(p_s^*)$. Thus, if the firm offers p_s^*, EPA's cost of imposing sufficient sanctions on the state that the state would reject the firm's offer outweighs EPA's benefit of getting the case. Because EPA's benefit of getting the case is strictly decreasing in firm offers (as long as $p_s < p_e$), there is no $p_s > p_s^*$ such that EPA will want to sanction the state, and so there is no reason for the firm to make an offer that exceeds p_s^*.

By a similar logic, if $c < \frac{u_E(p_e) - \zeta - u_E(p_s^*)}{u_S(p_s^*) + \zeta - u_S(p_e)}$, EPA would sanction the state if it accepted p_s^*, and so the state would reject an offer of p_s^*, subjecting the firm to EPA's (higher) penalty. Thus the firm will offer above p_s^*.

E.1.5 Proof of Proposition 4

Proof. First, we note that it is sufficient to prove this for $\sigma(N) = 1$.

Next, we show that firm offers are lower than the state's preferred penalty when $\beta = 1$: Let $\beta = 1$, so that $p_e = p_s^*$. Write $u = u_S = u_E$. The firm's offer is such that $(1 + c)[u(p_s) -$

$u(p_s^*) + \zeta] = 0$. The p_s that satisfies this condition is lower than p_s^*.

Finally, we show that when $\beta = 1$, firm offers are increasing in $(1 - \beta)$. We fully differentiate Equation 1 and rearrange terms to get

$$\frac{dp_{\bar{s}}}{dp_e} = \frac{\tau(p_e) - \tau(\bar{p}_s) - \frac{dp_e}{d(1-\beta)}[u'_S(p_e) + \frac{1}{c}u'_E(p_e)]}{\frac{dp_e}{d(1-\beta)}[u'_S(p_{\bar{s}}) + \frac{1}{c}u'_E(p_{\bar{s}})]}$$

When $\beta = 1$ and $\sigma(N) = 1$, we have that $u'_S(p_e) = u'_E(p_e) = 0$. We are left with

$$\frac{dp_{\bar{s}}}{dp\big|_{\beta=1} = \frac{\tau(p_e) - \tau(p_{\bar{s}})}{\frac{dp_e}{d(1-\beta)}[u'_S(p_{\bar{s}}) + \frac{1}{c}u'_E(p_{\bar{s}})]}}$$

Proposition 1 gives us that $p_{\bar{s}} < p_e$, so that $\tau(p_e) - \tau(p_s) > 0$ and $u'_S(p_{\bar{s}}) = u'_E(p_{\bar{s}}) > 0$. Thus $\frac{dp_{\bar{s}}}{dp_e}\big|_{\beta=1} > 0$.

$\square$