ENFORCEMENT SPILLOVERS: LESSONS FROM STRATEGIC INTERACTIONS IN REGULATION AND PRODUCT MARKETS[i]

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KEYWORDS: leakage, general deterrence, enforcement, strategic substitutes, pollution regulation

JEL CODES: K42, L51, Q58

Abstract

We explore enforcement spillovers - when sanctions at one entity influence behavior at other entities. We model spillovers arising from a regulatory channel and from a channel not previously emphasized: product market interactions. Our model motivates empirical hypotheses, which we test using data from Clean Water Act manufacturers. We find that penalties create positive spillovers for other facilities facing the same regulatory authority, such that enforcement actions reduce pollution at facilities in the same regulatory jurisdiction. However, penalties generate negative spillovers for facilities in the same industry facing a different authority, such that enforcement actions increase pollution at facilities in the same industry but facing a different regulator. Reductions in pollution in a state issuing a fine are about 50% offset by increases in pollution in nearby states. This is the first paper to explain and systematically document this 'enforcement leakage'.

1. Introduction

Without enforcement, regulations are just discretionary guidelines. Philosophers have studied the public enforcement of law since Bentham (1789) and economists have formally proposed theories of punishment since at least Becker (1968) and Stigler (1970).[1] Empirical evidence shows that inspections and sanctions can deter harm in regulatory settings as diverse as financial oversight; environmental, natural resource, and energy; food, drug, and occupational safety; and health administration.[2] Nonetheless, economists and policymakers still have an incomplete understanding of the mechanisms linking punishment with outcomes at regulated entities. Of particular interest in this paper are the economic channels driving enforcement spillovers, the form of general deterrence that arises when sanctions levied against one entity "spill over" to influence behavior at other regulated entities.

Enforcement spillovers have been documented for both individuals and firms. Every dollar in revenue collected from an income tax audit spills over to generate many dollars in increased revenues from individuals not audited (Dubin, Graetz, and Wilde 1987, 1990; Alm 2012). Inspections for television license fees in Austria influence compliance at non-inspected households (Rincke and Traxler 2011). Environmental compliance following water and air pollution enforcement activity increases almost as much at neighboring facilities as at penalized facilities (Shimshack and Ward 2005; Gray and Shadbegian 2007).

The economic mechanism typically postulated to link enforcement actions directed towards one agent to the behavior of other agents is a reputational learning channel, following Sah's (1990) work on social osmosis in crime. In an uncertain regulatory environment, potential violators update beliefs about their own expected penalties based on recent experiences of those around them. When

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[1] Polinsky and Shavell (2000) survey the literature.
[2] Cohen (1998); Baker (2003); Jackson and Roe (2009); Ruser and Ruser (2010); Leeth (2012); Gray and Shimshack (2011) review the evidence.

agents face a common regulator, spillovers naturally arise (Heyes and Kapur 2009).[3] If enforcement actions foster a "regulator reputation" for toughness, positive regulatory spillovers result (Shimshack and Ward 2005, 2008; Gray and Shadbegian 2007; Rincke and Traxler 2011).[4] Negative regulatory spillovers arise if facilities believe that enforcement actions against one facility reduce enforcement resources available for targeting others. In general, the direction and magnitude of regulatory spillovers depend on the nature of uncertainty about regulatory scrutiny and the process by which facilities update beliefs.

Although the empirical and theoretical work to date explains enforcement spillovers by reference to facility interactions in the regulatory environment, we consider the implications for enforcement spillovers when facilities also interact through a different channel: the output market. Facilities within a regulatory jurisdiction span a wide range of product market relationships. Some facilities produce identical commodities, some produce near substitutes, some have no interactions with one another in output markets, and so on. We show that that these strategic interactions in product markets may drive enforcement spillovers. In fact, product market mechanisms could fully explain, reinforce, or counteract enforcement spillovers driven by the regulatory interactions previously emphasized.

We first develop an enforcement and compliance model that highlights facilities' simultaneous interactions in the output market and the regulatory environment. For tractability and to match our later empirical setting, we emphasize the implications of these two channels of interactions for facilities' optimal levels of a pollution externality. In the spirit of Bulow, Geanakoplos, and Klemperer (1985), we allow one facility's actions in the output market to change another facility's strategies via

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[3] Heyes and Kapur (2009) are primarily concerned with optimal regulator behavior under different enforcement missions. Although we draw from their model, our paper has fundamentally different research questions and objectives.
[4] Positive regulatory spillovers arise when other facilities respond by becoming less aggressive (reduce output, reduce emissions). With negative regulatory spillovers, facilities become more aggressive in response to the marginal enforcement action on another facility.

changes in marginal benefits of production. Building from Heyes and Kapur (2009), we also allow a regulator's actions against one facility to directly influence the perceived regulatory scrutiny for another facility via changes in marginal expected penalties. An innovation of our model is the combination of these effects to reveal when and how enforcement spillovers arise. A key novel feature of our model is that enforcement spillovers can be driven by interactions in the regulatory environment, in the product market, or both.

Our model implies testable hypotheses, which we explore in the context of the Clean Water Act (CWA). We investigate monthly enforcement, pollution, and compliance data for several hundred large U.S. manufacturers over many years using empirical specifications that more fully account for the range of interactions among plants than the previous literature. We find three main empirical results. First, CWA enforcement actions spill over to reduce pollution at other facilities in the same industry and facing the same state regulatory authority as the sanctioned facility. These positive enforcement spillovers are most consistent with a strong regulator reputation mechanism swamping countervailing product market interactions. Second, CWA enforcement spillovers extend beyond the industry of the sanctioned facility and reduce pollution at facilities in other industries that face the same state regulator. Third, CWA enforcement actions spill over to *increase* pollution at facilities in the same industry and geographic area but facing a different regulatory authority. This latter result is new to the literature and consistent with product market interactions driving spillovers.

One natural policy implication is that enforcement actions appear to have a multiplier effect within the same regulatory jurisdiction. Although this effect has been noted in the existing literature, we show that it is not restricted to spillovers within the same industry. As such, the bang per buck from enforcement actions is larger than previously expected when considering effects within the regulatory jurisdiction. A more cautionary policy implication arises from negative enforcement

spillovers stemming from the previously unexplored product market mechanism. Here, enforcement actions have the potential to generate a form of unintended "leakage" for facilities in the same industry but other regulatory jurisdictions. A back of the envelope calculation using our empirical estimates suggests that around 50% of positive enforcement spillovers within a regulatory jurisdiction are offset by negative enforcement spillovers outside of the jurisdiction. This paper provides early evidence explaining and documenting leakage from regulatory enforcement.[5]

2. Modeling Enforcement Spillovers

2.1. Setup and Results

We consider a model in which each facility $i = 1,2$ chooses output and emissions, denoted q_i and e_i, to maximize expected profit. Although limited to two facilities, the model we develop has the advantage of being highly agnostic about market structure and the nature of product market competition. The two-facility model is also sufficient to show a wide range of possible enforcement spillovers, including those we later explore empirically.

In our model, expected profit depends on revenues, production costs, and expected regulatory costs. Facility i's revenues vary with its own output, as well as potentially the output of the other facility, given by q_{-i}. We denote facility i's revenue function as $R_i = R_i(q_i, q_{-i})$ and assume $\frac{\partial R_i}{\partial q_i} > 0, \frac{\partial^2 R_i}{\partial q_i^2} < 0$. Facility i's production costs depend on its own output and emissions and are denoted $C_i = C_i(q_i, e_i)$, where we assume $\frac{\partial C_i}{\partial q_i} > 0, \frac{\partial^2 C_i}{\partial q_i^2} > 0, \frac{\partial C_i}{\partial e_i} < 0, \frac{\partial^2 C_i}{\partial e_i^2} > 0, \frac{\partial^2 C_i}{\partial q_i \partial e_i} < 0$. Facility i's expected regulatory costs are a function of its emissions, the regulatory pressure it faces, and possibly the

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[5] Gray and Shadbegian (2007) found that regulatory activity increased compliance at neighboring facilities in the same state but not at neighboring facilities in other states. Like us, their point estimates suggested that regulatory actions (in their case, inspections) reduced compliance in neighboring states, but their empirical results were not statistically significant. The authors informally explained any possible adverse out-of-state impacts as likely due to air pollution transport issues unrelated to this paper's contributions. More generally, a growing literature documents emissions leakage stemming from partial regulation (Fowlie 2009, Bushnell and Mansur 2011, Baylis, Fullerton, and Karney 2014, Cunningham, Bennear, and Smith 2016, Fischer, Guttormsen, and Smith 2016, Gibson 2018).

regulatory pressure faced by other facility. The parameter ρ_i denotes the regulatory pressure faced by facility i. The expected regulatory cost function for i is given by $F_i = F_i(e_i, \rho_i, \rho_{-i})$ where ρ_{-i} denotes the regulatory pressure faced by the other facility. We assume $\frac{\partial F_i}{\partial e_i} > 0, \frac{\partial^2 F_i}{\partial e_i^2} \geq 0$, and $\frac{\partial^2 F_i}{\partial e_i \partial \rho_i} > 0$, implying that increased regulatory pressure raises the marginal expected regulatory costs for a facility. F reflects any cost spurred or leveraged by regulator attention. The expected profit function for facility i is then: $\pi_i = R_i(q_i, q_{-i}) - C_i(q_i, e_i) - F_i(e_i, \rho_i, \rho_{-i})$.

Our formulation, motivated by Bulow, Gneanakoplos, and Klemperer (1985) and Heyes and Kapur (2009), allows for the possibility that facilities interact through up to two channels (i) the product market (that is, if $\frac{\partial^2 R_i}{\partial q_i \partial q_{-i}} \neq 0$), and (ii) the regulatory environment (that is, if $\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0$). By definition, the two facilities produce strategic complements if $\frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} > 0$ and strategic substitutes if $\frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} < 0$. We say that the facilities experience positive regulatory spillovers if $\frac{\partial^2 F_i(e_i, \rho_i, \rho_{-i})}{\partial e_i \partial \rho_{-i}} > 0$, or negative regulatory spillovers if $\frac{\partial^2 F_i(e_i, \rho_i, \rho_{-i})}{\partial e_i \partial \rho_{-i}} < 0$. If increased regulatory pressure (an enforcement action) against one facility signals a regulator reputation for toughness thereby increasing the marginal expected regulatory costs for the other facility, then positive regulatory spillovers arise. If instead increased regulatory pressure against one facility leads other facilities to perceive that fewer resources are available for targeting them, then negative regulatory spillovers occur.

Each facility chooses output and emissions to maximize the expected profit function. Depending upon the nature of facilities' interactions, the solution to the model may result in enforcement spillovers. Enforcement spillovers arise when a facility's optimal level of emissions varies with the regulatory pressure faced by the other facility, when $\frac{\partial e_i}{\partial \rho_{-i}} \neq 0$. We characterize spillovers as positive if $\frac{\partial e_i}{\partial \rho_{-i}} < 0$ and negative if $\frac{\partial e_i}{\partial \rho_{-i}} > 0$.

This stylized model provides a simple yet general framework for examining the conditions under which interactions in the product market and/or the regulatory environment result in enforcement spillovers and the direction and magnitude of those spillovers. Interactions in the product market can be absent, characterized by strategic substitutes, or characterized by strategic complements. Interactions in the regulatory environment can be absent, characterized by positive spillovers, or characterized by negative spillovers. Strategic complements and positive regulatory spillovers both result in positive enforcement spillovers while strategic substitutes and negative regulatory spillovers lead to negative enforcement spillovers. The nature of enforcement spillovers in a market with strategic substitutes (complements) and positive (negative) regulatory spillovers depends on the relative strengths of facilities' interactions in the two settings.

Table 1 summarizes the empirical predictions that arise from this general model.[6] Less stylized versions of the model with additional structure highlight how enforcement spillovers may operate with many facilities and varying assumptions about the nature and strength of interactions in the regulatory environment. Appendix A summarizes a Cournot model with many facilities producing strategic substitutes in the output market and simulations that show how total emissions respond to increased regulatory pressure on one facility. Intuitively, when one facility receives increased regulatory scrutiny, the effect on total emissions depends on the number of facilities in and outside the regulatory jurisdiction of the "targeted" facility, as well as the nature and strength of spillovers.[7] An important implication is that even if regulatory actions yield significant positive spillovers within jurisdictions (as suggested by earlier empirical studies), the effect on total emissions may be modest if regulatory

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[6] An earlier version of the paper included propositions and proofs of the results summarized in Table 1.

[7] When facilities produce strategic substitutes but have no interactions in the regulatory environment, the net effect on total emissions of an asymmetric increase in regulatory pressure is akin to the effect of partial regulation (Fowlie 2009). That is, the net effect of regulating only one facility on total emissions is smaller than if the facilities produced unrelated products (that is, with no strategic interactions in the product market). This "squeezing the balloon" effect is an important insight.

spillovers are offset by countervailing product market interactions. This may occur despite the magnitude of product market spillovers being relatively small at the individual facility-level.

2.2. Empirical Predictions

To formalize the model predictions most relevant to our empirical application, we focus on settings that satisfy the following four conditions: (1) regulatory interactions are confined to facilities facing the same primary regulatory authority, (2) facilities facing the same primary regulatory authority experience positive regulatory spillovers on average, (3) interactions in the product market are confined to facilities in the same industry, and (4) facilities in the same industry produce strategic substitutes on average. Condition (1) implies no regulatory spillovers among facilities facing different primary regulatory authorities. Condition (3) implies no product market interactions among facilities in different industries. Conditions (2) and (4) follow from the existing empirical literature. All of these conditions are empirically refutable. Given these conditions, the following empirical predictions about overall enforcement spillovers follow from our model:

<u>Empirical Prediction 1:</u> Facilities in different industries and facing the same primary regulatory authority will experience positive overall enforcement spillovers.

<u>Empirical Prediction 1a:</u> Overall enforcement spillovers for facilities in the same industry and facing the same primary regulatory authority will not equal overall enforcement spillovers for facilities in different industries and facing the same primary regulatory authority.

<u>Empirical Prediction 2:</u> Facilities in the same industry and facing different primary regulatory authorities will experience negative overall enforcement spillovers.

<u>Empirical Prediction 2a:</u> Overall enforcement spillovers for facilities in the same industry and facing the same primary regulatory authority will not equal overall enforcement spillovers for facilities in the same industry and facing different primary regulatory authorities.

Empirical Prediction 3: Facilities in the same industry and facing the same primary regulatory authority will experience positive overall enforcement spillovers if regulatory channels dominate product market channels.

In subsequent sections, we investigate these predictions for a particular regulatory setting. Our empirical explorations represent joint analyses of the predictions and the underlying conditions. Nevertheless, economic intuition, input-output data, and the existing empirical literature suggest the underlying conditions hold on average. Regulator spillovers are unlikely to cross regulatory jurisdiction boundaries (Gray and Shadbegian 2007, Gray and Shimshack 2011); facilities facing the same primary regulatory authority generally experience positive regulatory spillovers where regulatory actions against any facility signal a regulator reputation for toughness to other facilities (Shimshack and Ward 2005, 2008; Gray and Shadbegian 2007; Rincke and Traxler 2011); strategic interactions in product markets are considerably stronger within industries than across industries (Porter 1986, OECD 2015);[8] and industrial facilities in the same broadly-defined industry produce strategic substitutes (Bushnell, Mansur, and Saravia 2008; Fowlie 2009).

3. Empirical Setting and Data

We investigate the empirical predictions outlined above using pollution, compliance, and enforcement data for a sample of large industrial facilities regulated under the U.S. Clean Water Act (CWA). CWA pollution and compliance outcomes are observed every month for all large manufacturing facilities. Water quality remains a serious issue in the United States, as more than 75% of the population lives within 10 miles of an impaired waterway. To understand our empirical framework, it's helpful to briefly characterize CWA enforcement.

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[8] Our subsequent empirical analysis focuses on facilities in the pulp and paper, organic chemicals, inorganic chemicals, petroleum, and iron and steel industries. OECD input-output data indicates that domestic flows of goods and services between these industries are typically small. All industry pairs have cross-industry flows of goods and services representing less than 6% of either industry's total domestic flows. For example, the petroleum, metals, and paper sectors, respectively, receive about 0.9%, 0.4%, and 4.0% of total domestic flows from the chemical sector.

3.1. CWA Regulation and Enforcement for Large Industrial Facilities

Under the CWA, the key regulatory jurisdiction is the state. Facilities in different states will not typically interact with the same primary regulator. Although legislation and guidance is largely set at the federal level, the overwhelming majority of CWA permitting, enforcement, and monitoring activity is delegated to states or local authorities. State regulators with 'primacy' typically conduct inspections and issue enforcement actions under the CWA.[9] State agencies are required to provide certain data to regional and federal EPA offices for review. Revocation of CWA primacy is legally permissible, but does not happen in practice. Regional and federal enforcement activities as regulatory 'backstops' for state inactivity are rare for the large CWA industrial facilities in our empirical sample.

For large CWA facilities, the primary monitoring strategy involves self-reported pollution.[10] Regulator inspections serve to verify the accuracy of self-reporting. Inspections also identify correctable problems and may support enforcement actions. Inspections can vary from brief reconnaissance inspections that visually examine effluents to rigorous, weeks-long compliance evaluations involving sampling, and equipment evaluations. Inspections going beyond reconnaissance will typically include reviews of record-keeping and self-reporting processes.

Although many options are available, the practical workhorse of CWA enforcement is administrative penalties. Regulators have a menu of cooperative and punitive enforcement instruments from which to choose including: warning telephone calls; warning letters; informal enforcement actions like notices of violation; formal administrative actions like administrative consent orders with and without penalties; civil cases; and criminal cases. The literature suggests that telephone calls,

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[9] In the few cases where states decline primary regulatory authority, or for a limited number of facilities and industries, EPA offices conduct their own inspections and issue their own sanctions. This has no implications for our subsequent empirical analysis, as all of our sample states maintained CWA primacy for all of our sample facilities except ME and MA. Results are statistically indistinguishable (and otherwise similar) if we omit ME and MA.

[10] Like other researchers and policy-makers, we assume that self-reported data for large CWA facilities are reasonably reliable. We check for evidence of strategic misreporting in Appendix B.

warning letters, notices of violation, and other informal enforcement actions appear to have limited impacts on CWA pollution or compliance (Gray and Shimshack 2011). Although civil and criminal cases are technically possible as ultimate enforcement threats, these actions are very rare for standard CWA violations.[11] Administrative sanctions are therefore the standard CWA enforcement pillar. These actions can include field citations in some states, but the majority are issued by state or regional administrative law judges. They can be imposed for paperwork or reporting errors but a large majority at least partially address pollution violations. A sanction may address multiple pollutants and violations.

Regulatory discretion is pronounced. Discretion is significant because resources are scarce, regulations and enforcement actions are technically and legally complex, and political economic factors are influential.[12] Federal enforcement guidelines dictate that all violations receive formal sanctions and that sanction severity vary with the level of harm, financial gain, compliance history, ability to pay, intent, fairness, and the strength of the legal evidence (U.S. EPA 1989). In practice, most violations are not formally sanctioned and typical penalty magnitudes are small fractions of penalties allowed under the statute. Gray and Shimshack (2011) and Shimshack (2014) explain the typical environmental enforcement processes in detail.

CWA enforcement intensity varies markedly across jurisdictions and across time within jurisdictions. Even conditional on facility characteristics, pollution discharges, and violations, the frequency and severity of CWA inspections and sanctions vary dramatically across states and over time within states (U.S. GAO 2009). Average levels of enforcement intensity in a jurisdiction (state) may be influenced by industrial composition, average emissions, average environmental quality,

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[11] Civil and criminal CWA cases are almost always imposed for operating without appropriate permits, for deliberate falsification of records, for unusual histories of persistent noncompliance, or for extreme environmental harm.
[12] See, for example, Gray and Deily (1991) and Grooms (2015) for interesting illustrations.

community pressure, environmental budgets, politics, and so on.[13] Short-run variation in enforcement around central tendencies is driven by idiosyncratic choices of individual regulators; administrative backlogs; shocks to administrative, negotiation, or legal costs; idiosyncratic political and budgetary realizations; atypical changes in community pressure on regulators; or other factors. Shimshack (2014) reviews the literature on empirical determinants of environmental regulator behavior in some detail, and Innes and Mitra (2015) provide an interesting illustration of short-run variation.

Sanctions, and variation in sanctions over time, are observable to firms. Qualitative surveys of compliance officials at regulated facilities indicate that most respondents are aware of enforcement actions at other industrial facilities (Carlough 2004; Thornton, Gunningham, and Kagan 2005). Enforcement authorities publicize penalties, trade journals summarize regulator actions, and facilities informally interact with one another. EPA and states issue quarterly and annual enforcement action reports that list penalties, enforcement details, and sanctioned facilities and owners. EPA and state agencies regularly input enforcement data into searchable public databases. Although many facilities do not regularly search databases themselves, the information can be publicized by intermediaries like trade associations and the media. Although penalty information is technically widely accessible, we note that information transmission among facilities is likely greatest within state and for facilities that are generally similar. Media coverage of enforcement actions often occurs in local newspapers or on regulation consultant blogs that are specific to industries or local areas.[14]

3.2. Data

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[13] In the analysis that follows, we make no attempt to empirically exploit persistent differences in enforcement intensity across jurisdictions. To the contrary, we undertake significant effort to remove cross-sectional differences from the analysis and instead empirically exploit short-run temporal variation around central tendencies in agencies' enforcement behavior.

[14] The role of the state as the primary enforcement jurisdiction extends to other environmental media and statues. Gray and Shadbegian (2007) conduct an explicit spatial analysis of air pollution in cities that cross state lines and note, "Our spatial analysis indicates significant positive spatial correlations in compliance: plants located near each other tend to have similar compliance rates….[but]…. this effect does not cross state borders — only plants in the same state behave similarly — reinforcing the importance of jurisdictional boundaries in a federal regulatory system where most of the enforcement activity is done by state regulators."

Our specific data sources are the EPA's Integrated Compliance Information System and the Permit Compliance System which track monthly facility-level self-reported discharges, permitted pollution limitations, inspections, and enforcement actions under the Clean Water Act. We focus on the conventional water pollutant total suspended solids (TSS) (EPA parameter 00530), as it is the pollution parameter most consistently measured, tracked, and reported monthly across a large number of industries. TSS is also correlated with other conventional pollutants, toxics, and other contaminants like nutrients.

Since the goal of our analysis is to examine spillovers within and across industries, we focus on manufacturing facilities in industries with many major facilities and substantial water pollution impacts. Our final sample contains facilities from the pulp and paper, inorganic chemicals, organic chemicals, petroleum refining, and steel industries. Four 2-digit SIC code industries (26, 28, 29, 33) include six 3-digit SIC code industries (261, 262, 281, 286, 291, 331) and eleven four-digit SIC code industries (2611, 2621, 2812, 2813, 2816, 2819, 2861, 2865, 2869, 2911, 3312). For our empirical analysis, we define industries (that is, product market interactions) by 3-digit SIC code, as this approach balances state and industry variation and coverage.[15] Our industries generate the bulk of industrial wastewater pollution in the United States. These industries also represent the bulk of major CWA facilities other than wastewater treatment plants, which are typically publicly owned and do not interact in product markets.

Our initial sample consists of "major" manufacturing facilities in the continental United States with continuously active CWA permits between January 1996 and May 2006. Our pollution and compliance analysis period is the 101 months spanning January 1998 to May 2006, so a full analysis

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[15] We acknowledge that 3-digit SIC industry classifications provide somewhat coarse approximations for actual plant-level strategic interactions in product markets. An ideal analysis would involve plant-level regulatory data that can be matched to actual strength of product market interaction measures between firms on a large scale. This is a promising subject for future research. Our data do, however, permit explorations of enforcement spillovers that have not been explored at all in the extant literature. We find stark empirical results with even coarse measures of strategic interactions.

period beginning in 1996 allows two years of enforcement lags.[16] We focus on major (that is, large or significant polluting) facilities because non-majors are not required to report pollution and compliance every month, and because states are not required to input monitoring, enforcement, and compliance information into EPA databases for non-major facilities.[17]

Consistent with our focus on enforcement spillovers within and across state-level regulatory jurisdictions, we focus on states with reasonable numbers of CWA majors over the time period of our analysis. Because the overwhelming majority of such states were in the eastern half of the country, our final sample includes states east of the Mississippi River plus the industrialized gulf states of Texas, Louisiana, and Oklahoma.[18]

3.3. Analysis Sample

Our final sample consists of 489 large manufacturing facilities. Figure 1 shows the locations of sample establishments. Facilities are somewhat clustered along major rivers and coasts, as perhaps expected. About 12%, 20%, 15%, 25%, 15%, and 13% of facilities are associated with the pulp; paper; inorganic chemicals; organic chemicals; petroleum refining; and steel industries, respectively.

The top panel of Table 2 summarizes aggregate monitoring and enforcement actions at sample facilities. In an average month, about 10 percent of facilities received at least a reconnaissance inspection. All facilities except for one were inspected at least once during our sample period. 86 facilities received 144 fines over the enforcement sample period. The median fine was $11,500, and

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[16] Time periods were chosen for data consistency. Reasonably high quality CWA discharges data became available in 1998. Data migration between data systems began in June 2006, and some pollution and compliance information was not consistently tracked in public EPA databases during migration periods.

[17] EPA regulatory universe documents suggest minor facilities outnumber major facilities by 10 to 15 fold. Since state and federal databases do not systematically track pollution and compliance outcomes for minor facilities, even the EPA does not know how aggregate water pollution discharges from minors compare to aggregate water pollution discharges from majors. One should be cautious extrapolating our results to minor facilities, where the regulatory environment often differs substantially.

[18] The overwhelming majority of mid-western and western states had fewer than five CWA manufacturing majors. New Hampshire also had few CWA majors and is omitted. Results are robust to including states with fewer than 5 majors or including western states with 5 or more majors.

fines were highly variable. Fine magnitudes should be interpreted relative to the economic gains from the specific triggering violation(s), rather than to operating profits of the facility itself.

Following the empirical environmental enforcement literature, our emissions measures are monthly average discharge quantities expressed as the percent of permitted pollution (Earnhart 2004; Shimshack and Ward 2008).[19] Violations occur when discharge ratios exceed one hundred percent. Our main analysis sample tracks TSS discharges from the 415 of 489 original sample facilities that reported TSS discharges for the majority of our pollution periods.[20]

The second, third, and fourth panels of Table 2 summarize pollution and compliance measures. Mean discharges for TSS pollution were about 26 percent of limits and the 25th and 95th percentiles were approximately 7 and 70 percent of the limits, respectively. These statistics suggest a high rate of average statutory compliance with permitted effluent limits, consistent with McClelland and Horowitz (1999) and Shimshack and Ward (2008). However, pollution discharges were highly variable, both across facilities and across time for the average facility.[21] In an average month, more than 1 percent of facilities were in violation. 126 facilities violated TSS standards 486 times during our sample period. The average TSS violation was more than two times the permitted limit, and dozens of violations were more than 10 times limits. Violations were more common in the early part of the sample, but not overly concentrated at a single point in time.

3.4. Empirical Variation across Time and Space

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[19] To be precise, since some plants may have multiple outfalls, our unit of observation is the plant-by-month maximum of monthly average discharge ratios across all possible outfalls. In a given month, the large majority of facilities discharge our specific pollution parameters from a single specific outfall. These outfalls remain constant over time. It is extremely unlikely that this convenient aggregation biases results (Shimshack and Ward 2008).

[20] Most of the 74 facilities with missing data were either not required to report TSS discharges or reported no TSS discharges during the sample period. A small number of facilities have unexplained missing data, but we are unable to predict missingness with any observable facility characteristic (See Appendix B).

[21] Theories emphasizing implications of stochastic discharges include Beavis and Walker (1983); Beavis and Dobbs (1987); Segerson (1988); Shimshack and Ward (2008).

The long-term trend in pollution discharges during our sample period is downward. Mean TSS pollution was approximately 10-20 percent higher for the first few months of 1998 than for the same months in 2006. Pollution variability increased somewhat between 1998 and 2001, but modestly declined along with mean discharges beginning in 2002. Discharges as a percent of limits exhibited mild seasonality throughout the sample period, with scaled pollution about 10 percent higher in the late winter/early spring than in the late summer/early fall.

Figure 2 illustrates basic trends in inspections and fines.[22] The number of inspections per year generally declines over time, with a steeper decline near the end of our sample period. The number of fines per year follows no obvious trend, although Figure 2 depicts a relatively sharp decrease in the last two full years.[23] The total dollar amount of fines, not depicted in Figure 2, is noisy and dominated by few large fines imposed in 2002 and 2003. Median observed fines are generally stable between 1996-2001 and 2004-2006, but experience marked increases in 2002 and 2003. Inspections and fines vary significantly across time and are not overly concentrated at single points in time.

Violation propensity and enforcement intensity vary within and across industries and jurisdictions. The top panel of Figure 3 highlights variation across our 3-digit SIC industries. Organic chemical facilities violated most frequently for the conventional pollutant TSS; pulp mills violated least frequently. Although organic chemical facilities were also fined most often, fine rates were roughly comparable to those in the pulp, inorganic chemical, and steel industries where fewer violations were committed per plant on average. The bottom panel of Figure 3 highlights cross-sectional variation across states.[24] Violations, inspections, and fines are highly variable across

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[22] Our fines are administrative fines, which are formal administrative actions accompanied by monetary penalties, indicated in our databases as a non-zero value for "penalty amount assessed." This represents the dollar amount of the assessed penalty as identified in the final administrative order.

[23] Declines in CWA administrative fines after 2005 have been documented elsewhere (Gray and Shimshack 2011).

[24] The bottom panel of Figure 3 includes data from EPA region four states only; this choice is arbitrary but illustrative as the variation depicted is similar across all states (not just those within region four).

jurisdictions but also not restricted to or excessively concentrated in any given set of states. Additional explorations show that within-state variation over time is large but idiosyncratic. [25] Changes in violation propensity and enforcement intensity in one state are not obviously correlated with changes in nearby states, and variation does not appear to be systematically driven by common shocks within a region.

4. Empirical Framework

Our empirical goal is to identify enforcement spillovers and the channels through which they arise. Our basic approach involves regressing pollution at a given CWA manufacturer in a given month on several enforcement spillover measures. Coefficients on the spillover measures, in principle, represent the impact of marginal changes in enforcement activity directed towards other facilities in the recent past on the pollution decisions of the average non-targeted facility.

4.1 Conceptual Underpinnings of an Empirical Model: Plant-level Decision Making

Our basic empirical approach is consistent with a view of regulated plants as rational decision-makers that equate expected marginal benefits and expected marginal costs of production and emissions choices. [26] Marginal benefit and cost factors may include plant and community characteristics, seasonality, national shocks, industry-specific shocks, and state-specific shocks. Expected costs of pollution outcomes are some function of expected regulatory penalties. It is worth

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[25] Within-variation in enforcement is systematically larger than between-variation. For example, datasets of fine indicators at the facility-month level, state-month level, or industry-month level exhibit "within" standard deviations roughly 4 to 5 times larger than "between" standard deviations. See Appendix Figures C1 and C2 for illustrative examples of variation in enforcement across time and space.

[26] Conventional water pollution compliance involves marginal costs to the facility. Industrial wastewater treatment involves primary, secondary, and tertiary treatments. Primary treatment involves simple screening and phase separation. Increasingly fine screens remove large solids, settling causes suspended solids to separate out via gravity and sedimentation, and forced air or simple density separation allows oil, grease, and so on to float to the top for skimming. Secondary treatment involves biological processes where microorganisms convert organic contaminants in wastewater to less harmful bio-solids and other bi-products. Tertiary treatment, although less common, involves chemical disinfection. All of these processes are highly sensitive to production volume and attention, and changes in conventional water pollution and compliance almost always involve marginal costs rather than new equipment installations (Shimshack and Ward 2008, Gray and Shimshack 2011).

reiterating that the expected effects of regulator fines and penalties may be functionally larger than the penalty amounts themselves suggest.[27]

Enforcement spillovers may arise due to changes in plants' marginal expected benefits and costs of production and emissions. Enforcement spillovers due to regulator reputation (that is, regulatory spillovers) are ultimately predicated on an implicit conceptual framework where plants form beliefs or expectations about uncertain current enforcement probabilities by observing and learning from regulator actions in the recent past. Thus, facilities may learn about their own expected penalties by observing fines and other penalties recently directed towards other facilities in their regulatory jurisdiction.[28] Enforcement spillovers due to interactions in product markets are driven by strategic interactions with other plants as outlined in Section 2. For example, we presume a given plant's net marginal benefits of production and pollution increase when recent net marginal benefits at plants producing strategic substitutes decrease in response to incremental enforcement pressure.

We note two departures from 'textbook' models of facility decision-making that are useful for our empirical application. First, we allow pollution to have a random component such that facilities imperfectly control their pollution discharges. Because actual pollution is a stochastic realization around a facility's intended discharges, observed discharges may fall below the standard (Beavis and Walker 1983; Beavis and Walker 1987; Segerson 1988; Bandyopadhyay and Horowitz 2006). With imperfect control of its discharges, even an over-compliant facility (that is, one with observed discharges below the standard) has an incentive to respond to a marginal increase in the expected penalty associated with a violation (Shimshack and Ward 2008). Second, facilities may jointly produce

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[27] True economic penalties may include reduced reputation with consumers, reduced employee satisfaction, increased community pressures, as well as increased threats of boycotts, letter writing campaigns, and citizen group actions. See Innes and Sam (2008), Bennear and Olmstead (2008), Langpap and Shimshack (2010), and Lyon and Maxwell (2012).

[28] Expected penalty formation of this sort is in the spirit of Sah (1990)'s work on social osmosis in crime and consistent with Shimshack and Ward (2005, 2008). One could model this process more formally in a Bayesian learning framework, but the implications are intuitive.

multiple pollutants. With jointness in production, a facility may be compliant on a conventional pollutant like TSS as a result of incentives created by expected penalties for violations on a different, yet jointly produced, pollutant. Jointness in production can induce facilities to respond to changing expectations by reducing TSS pollution (for example) even when average TSS pollution is well below permitted standards (Shimshack and Ward 2008). Recall that one reason we focus on TSS is because it is correlated with other pollutants. The practical implication of these real world considerations is that a facility, even one that is over-complying, may still respond at the margin in response to changing beliefs about expected penalties.

4.2. Key Variables

Our primary dependent variable, e_{it}, is the quantity of total suspended solids (TSS) emissions at facility i in month t expressed as a percent of permitted limits. We focus on scaled pollution quantity because TSS quantity is more commonly reported than TSS concentration for major CWA industrial facilities.[29] Quantity-based measures also map clearly to our model where production quantity and emissions are simultaneously determined.[30]

Our key explanatory variables are enforcement spillover measures, *SPILLOVERS$_{it}$*, which represent the number of monetary penalties assessed at facilities other than i in the 1 to 12 months preceding month t.[31] We choose annual lags to follow the related empirical environmental enforcement literature (Gray and Deily 1996, Earnhart 2004; Shimshack and Ward 2005; Gray and Shadbegian 2005; Shimshack and Ward 2008). We focus on administrative/civil fines because

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[29] TSS quantity is often an averaged measure of many pollution concentration measurements (typically not observed to the regulator) times daily flow. While industry-specific technology-based standards commonly address effluent concentrations, states frequently write permits based on the quantity standard for TSS. The choice of conventional water pollution quantity rather than concentration follows convention in the literature (Earnhart 2004, Shimshack and Ward 2005, 2008).

[30] We later explore sensitivity to a linear probability model where outcomes are defined by a 0/1 indicator function for a TSS violation at facility i in month t, 1[violation$_{it}$]. While violations will not necessarily vary directly with TSS pollution, we explore the measure as it is of regulatory interest.

[31] An earlier version of this paper shows that all major conclusions of the paper are robust to including more annual lags, such as 13 to 24 months preceding t.

criminal fines are not levied for typical CWA violations and because less formal sanctions may have limited impacts on facility behavior (Gray and Shimshack 2011; Shimshack 2014).

To match our testable predictions, the first explanatory variable is the number of fines at other facilities in the same state and industry in the 1 to 12 months preceding t. The coefficient on this variable is hypothesized to be negative if regulatory spillovers dominate product market spillovers and positive if product market spillovers dominate regulatory spillovers. The second explanatory variable is the number of fines at other facilities in the same state but a different industry in the 1 to 12 months preceding t. The coefficients on this variable is hypothesized to be negative; regulatory spillovers are positive and product market spillovers are zero for a net reduction in pollution. The third explanatory variable measures the number of fines at other facilities in the same industry and geographic area but a different state in the 1 to 12 months preceding t.[32] The coefficient on this variable is hypothesized to be positive; lagged enforcement actions on others outside of the state but in the same industry are hypothesized to trigger negative product market spillovers and zero regulatory spillovers for a net increase in pollution. In our main analysis, we restrict the same industry / different state measure to facilities located within 600 miles of, but in a different state than, the facility in question. Given the regional nature of many output markets, the regional nature of many input markets, and the importance of transportation costs for competition, we presume that similar facilities in Houston, TX and Mobile, AL may compete more directly (and thus experience measurable product market spillovers) than similar facilities in Houston, TX and Augusta, ME. Our 600-mile radius is arbitrarily chosen but approximates the sizes of the gulf coast region, the northeast corridor, and the mid-Atlantic region.[33]

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[32] The final panel of Table 2 provides sample means for our three spillover measures. The mean number of fines in the previous year on other facilities in the same state and 3-digit SIC industry is 0.22. The mean number of fines in the previous year on other facilities in the same state but a different industry is 0.57. The mean number of fines in the previous year on other facilities in same industry and same geographic area but different state is 1.07.

[33] We later demonstrate robustness to alternative radii. Earlier versions of this paper defined these spillover variables using all facilities in different states but in the same industry. Point estimates were, on average, similar but the standard errors

4.3. Empirical Research Design and Specifications

Baseline specifications, for facility i in period (month) t of season s and year y, take the form:

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + \delta OUT_{kt} + \mu_s + \gamma_y + t\alpha_i + \varepsilon_{it}. \qquad (1)$$

We flexibly control for common trends and seasonality with season and year dummies, μ_s and γ_y, respectively. Industry-by-month producer price index, PPI_{kt}, and industry-by-month national industry output, OUT_{kt}, capture common (demand) shocks within sector.[34] Facility-level fixed effects, α_i, control for time invariant (or nearly so) plant characteristics possibly correlated with both pollution and enforcement intensity, including industry, subindustry, production capacity, general technology, geography, the biophysical conditions of the receiving waters and the surrounding area, and community characteristics like income, education, and political affiliations. Facility-level fixed effects also minimize bias from enforcement targeting based on the average environmental performance of the facility, state, region, or industry. Baseline specifications also include facility-specific linear time trends, $t\alpha_i$, that allow technology adoption and local economic conditions to trend differently across facilities.

We attempt to minimize remaining endogeneity concerns with additional research designs, each with its own strengths and weaknesses. First, some specifications include state-by-year fixed effects, which address common shocks within a state and year that may be correlated with both pollution and enforcement spillover measures. In specifications with state-by-year fixed effects, after netting out the

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were larger. This finding is consistent with facilities outside of the regional radius not contributing meaningfully to the effects but simply adding statistical noise.

[34] We obtain PPI data from the Bureau of Labor Statistics. We cross-reference our SIC data to the NAICS codes used by PPI with the SIC-to-NAICS crosswalk (https://www.naics.com/sic-naics-crosswalk-search-results). We adopt the following mapping from 3-digit SIC code to NAICS code associated with the relevant PPI series: 261 to 32211, 262 to 32212, 281 to 3251, 282 to 3251, 291 to 32411, 331 to 331. Within the chemical industry, more refined PPI series are available only for 2003 onward, so we use PPI data for basic chemical manufacturing for both organic and inorganic chemicals. Monthly industrial production data come from the FRED database. OUT_{kt} measures the seasonally-adjusted industrial production index for industry k in month t. The index, from release G. 17 "Industrial Production and Capacity Utilization", reflects "the real output of all relevant establishments located in the United States, regardless of their ownership, but not those located in U.S. territories" (https://fred.stlouisfed.org/series/IPN32211S). 2012 serves as the base year for the series. We follow the same industry definitions as for the PPI data.

effects of controls, identification of parameters of interest comes only from atypical within-state deviations from state-average enforcement activities for that same year. Second, some specifications include industry-by-year fixed effects, which address common shocks within an industry and year (not already captured by PPI_{kt} and OUT_{kt}) that may be correlated with both pollution and enforcement spillover measures. In specifications with industry-by-year fixed effects, after netting out the effects of controls, identification of parameters of interest comes only from atypical within-industry deviations from industry-average enforcement activities for that same year. Third, some specifications include industry-by-period (month) fixed effects, which address common shocks within an industry in *any* given time period. This approach, which asks a lot of the data, controls for all common observed and unobserved time varying shocks common to facilities in the same sector. Identification of enforcement spillover effects comes only from within-industry differences in short-run state and regional enforcement intensity.

These additional specifications, for facility *i* in period *t* of season *s* and year *y*, take the form:

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + \delta OUT_{kt} + \mu_s + \tau_{jy} + \varepsilon_{it} \,. \tag{2}$$

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + \delta OUT_{kt} + \mu_s + t\alpha_i + \theta_{ky} + \varepsilon_{it} \,. \tag{3}$$

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + t\alpha_i + \theta_{kt} + \varepsilon_{it} \,. \tag{4}$$

τ_{jy} are state-by-year fixed effects, θ_{ky} are industry-by-year fixed effects, and θ_{kt} are industry-by-month fixed effects. [35]

In our main analysis, we estimate standard errors clustered at the state-level to test the predictions laid out in section 2. Specifically, for the same state / same industry spillover measure with ambiguous theoretical predictions, we test a null of β = 0 against an alternative hypothesis that β ≠ 0.

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[35] Because some states contain reasonably small numbers of facilities, state-by-year dummies and facility-specific trends were occasionally highly correlated. We thus omit facility-specific trends from regressions of the form (2). PPI and OUT are perfectly correlated with industry-by-month fixed effects and are omitted from specifications of the form (4).

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For the same state / different industry spillover measure, we test a null hypothesis that $\beta = 0$ against an alternative hypothesis that $\beta < 0$. For the same industry / different state spillover measure, we test a null hypothesis that $\beta = 0$ against an alternative hypothesis that $\beta > 0$.

5. Empirical Results

5.1. Estimated Enforcement Spillovers

Figure 4 summarizes estimated coefficients and 90% confidence intervals for our key spillover measures (Jann 2014). Before going into the empirical magnitudes and details, we note three immediate qualitative results. First, a facility's pollution falls following the incremental fine at another plant in the same state and sector. Second, a facility's pollution falls following the incremental fine at another plant in the same state but in a different sector, but less so than if the fined facility was in the same sector. Third, a facility's pollution increases following the incremental fine at a plant in the same sector and region but located in a different state (that is, facing a different regulator).

Table 3 presents our main results in more detail. Columns of results tables reflect different specifications with different fixed effect structures and approaches to controls. The first three rows of results tables present enforcement spillover coefficients and standard errors. The first row presents results for 'same state / same sector' spillovers, the second row presents results for 'same state / different sector' spillovers, and the third row presents results for 'different state / same sector' spillovers. In Table 3, the first row indicates that, on average, facilities' discharge ratios declined significantly in the years following fines on other facilities in the same state and industry. The coefficients in the first row indicate that TSS discharge ratios fell by 1.9 to 3.5 percentage points in the year following the marginal fine on other facilities in same state and industry. These results translate into around a 7 to 14 percent overall reduction relative to the mean discharge ratio. The second row of Table 3 indicates that facilities' discharge ratios also declined significantly following fines on other

facilities in the same state but in a different industry. Results reported in the second row indicate that TSS discharge ratios fell by 0.6 to 1.3 percentage points in the year following the marginal fine on other facilities in the same state but in different industry. This translates to a roughly 2 to 5 percent overall reduction relative to the mean. The third row of Table 3 indicate that facilities' average discharge ratios increased significantly on average following fines on other facilities in the same industry and general geographic area but located in different regulatory jurisdictions (states). Results reported in the third row indicate that TSS discharge ratios increased by 0.7 to 0.8 percentage points in the year following the marginal fine on other facilities in the same state and geographic area but in a different state. These results translate into around a 3 percent overall increase relative to the mean.

5.2. Omitted Variable Bias Concerns

We maintain that lagged enforcement actions directed towards *other* facilities are a plausibly exogenous source of identifying variation, conditional on controls and the various fixed effects approaches.[36] Our empirical model directly addresses several natural identification concerns with omitted political economic or demand shocks by including extensive fixed effects and controls. In addition, we estimate a model with region-by-industry-by-year fixed effects. In this specification, enforcement leakage spillover measures could only be biased due to shocks uncorrelated with output and output price yet still co-moving with idiosyncratic deviations in enforcement within region, within industry, and within year. We find nearly identical results.[37]

We investigate the potential for omitted variable bias in key leakage estimates using the coefficient stability validation tests proposed by Oster (2017) and in the broader spirit of Altonji,

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[36] Recall that we discuss potential drivers of idiosyncratic variation in enforcement intensity across time in Section 3.

[37] Appendix Table C1 presents results with region-by-industry-by-year fixed effects. Point estimates for enforcement spillovers from facilities in the same state are moderated by the inclusion of region-by-industry-by-year fixed effects while those for key enforcement spillovers from facilities in different states are now larger in magnitude and more precisely estimated.

Elder, and Taber (2005).[38] We find that selection on unobservables would have to be 1.2 times greater than selection on all included covariates in the model to drive our key enforcement leakage parameter to zero. In other words, validation tests suggest that only implausibly strong selection on unobservables could bias our enforcement leakage estimate so much that it goes to zero. The tests' identified set suggests that the minimum possible bias-adjusted enforcement leakage coefficient on the enforcement leakage measure would still be positive and statistically significant at about 0.18.

We also explore whether including leads of enforcement spillover measures influences our results. If omitted and serially correlated common shocks are driving our results, one might expect that our key coefficients on lagged enforcement spillovers would be sensitive to including leads of enforcement spillover measures. However, our results are not sensitive to including leads. Coefficients on key lagged enforcement spillover variables tend to be statistically indistinguishable from our main estimates or larger in magnitude when we include leads. The coefficients on the leads related to our most novel enforcement leakage parameter (the one capturing same industry, different state spillovers) are not typically statistically significant and do not exhibit consistent sign patterns.[39] We view this as further evidence that omitted and serially correlated common shocks are unlikely to drive our results.

5.3. Other Sensitivity

One possible concern with our analysis is that we define key out-of-state enforcement spillover measures by fines at other facilities in the same industry and general geographic area. In particular, our

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[38] Appendix Table C2 presents results from Oster (2017) coefficient stability tests. In our implementation, we compare unsaturated models where we regress pollution outcomes solely on enforcement leakage measures with saturated models where we regress pollution on all enforcement spillover measures, facility fixed effects and facility linear time trends, year controls, seasonality controls, sector-by-month PPI and sector-by-month industrial production. We follow suggested rules of thumb, assuming Rmax = min $\{1.3*R^2, 1\}$, where R^2 is the overall R^2 from the fully saturated model (Oster 2017).
[39] Appendix Table C3 shows detailed results when lags and leads are included. Note that including leads requires us to omit as many years of data from the analysis as the number of years of leads included, so Table C3 shows results with one year of leads one year of data omitted. Note that coefficients on different state / same sector leads are not significant. Coefficients on same state / same sector leads are (when significant) opposite signs from the lags, which suggests shocks would bias lagged spillover estimates towards zero when omitted and serially correlated with lagged spillovers of interest. Coefficients on same state / different sector leads are the same signs from the lags, which suggests shocks would bias lagged spillover estimates away from zero when omitted and serially correlated with lagged spillovers of interest.

main analysis restricts the same industry / different state measures to facilities within 600 miles of the facility (but still in different states). The general geographic region restriction is motivated by the economic logic of transportation costs and regional markets, but the 600-mile radius itself was chosen subjectively. Results, however, are robust to different geographic radii.[40]

Other possible concerns relate to clustering choices and the absence of controls for specific deterrence in our specifications. For all presented specifications, we cluster standard errors at the state-level. Results are reasonably robust to clustering at the industry level and facility level, as well as to two-way clustering at the facility-by-month level.[41] We also establish that the general deterrence enforcement spillover estimates of primary interest are essentially unaffected by the inclusion or omission of specific deterrence measures.[42]

6. Interpretation

6.1. Pollution Shifting within a Firm or Changes in Total Pollution?

Our plant-level CWA data do not identify parent companies and so we do not separately identify absolute changes in a firm's total pollution from shifts in pollution within a given firm (that is, pollution shifts from one facility owned by a firm to another facility owned by the same firm).[43] Of particular concern is the possibility that our negative same industry / different state enforcement

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[40] Appendix Table C4 presents robustness results for 500 and 700 mile radii. Results show that point estimates and general patterns of statistical significance are extremely similar across all radii for spillover measures operating on facilities within the same state, which is expected as these facilities are not directly affected by radii choices. We continue to see that facilities' discharge ratios increase on average following fines on other facilities in the same industry and general geographic area, but located in a different regulatory jurisdiction (state). As expected, empirical magnitudes of out-of-state spillovers are larger when geographic area radii are smaller and smaller when geographic area radii are larger. Similar investigations to those in Appendix Table C4 revealed that enforcement actions on other facilities in the same industry but located strictly more than 700 miles away had no impact on pollution and compliance. This result is reassuring if one interprets this exercise as a sort of falsification exercise.

[41] Appendix Table C5 presents results with alternative clustering strategies. We note that results with industry-level clusters may be subject to small cluster issues and should be interpreted with caution.

[42] Appendix Table C6 present results when we include specific deterrence indicator variables. Additional variables reflect whether facility i was inspected in the 1 to 12 months prior to t and whether facility i was fined in the 1 to 12 months prior to t. We do not offer a causal interpretation of specific deterrence measures as they may be endogenous.

[43] The issue of production shifting relates to a strand of the trade and environment literature that explores the relationship between environmental regulation and foreign direct investment. See Hanna (2010) for a recent contribution to this literature.

spillover result could be driven by parent firms simply shifting production from facilities in high enforcement states to facilities in low enforcement states. Of course, our existing results reveal clear enforcement-induced shifts in economic incentives in either case. These incentives have bearing for the equilibrium timing and location of pollution regardless of whether results are driven by production shifting or absolute changes in pollution.

Nevertheless, we gathered data on plant ownership from the EPA's Facility Registry System (FRS) and replicated our analysis for facilities owned by known single-plant firms vs. facilities owned by known multiple-plant firms. FRS parent company ownership information is regrettably incomplete and we are unable to identify ownership for many facilities in our sample, but Table 4 presents results for those facilities for which ownership information was reasonably reliable. Columns (1)-(4) replicate the results in Table 3 for single-plant firms only. Signs and patterns of statistical significance are similar to Table 3, and significant spillover results (both positive and negative) tend to be larger in magnitude than those in Table 3. Columns (5)-(8) replicate the results in Table 3 for plants owned by known multiple-plant firms. Signs and patterns of statistical significance remain similar to those in Table 3 except that same state / different industry results are no longer significant. Magnitudes of same industry / different state spillovers are smaller than those for single-plant firms and smaller than comparable results in Table 3. Thus, we find no evidence that negative spillover results are driven by multi-plant firms shifting production from high enforcement states to low enforcement states.

6.2. Product Market Effects or Other Mechanisms?

We assert that empirically detected enforcement leakage outside of the regulatory jurisdiction is most naturally consistent with shifting production quantity due to product market interactions. To bolster this assertion, we perform a placebo test based on enforcement spillovers to facilities in different states and different industries. Our model predicts that facilities in a different state and a

different sector will experience no enforcement spillovers, as facilities in different states should have no regulatory interactions and facilities in different sectors should have no (or small) product market interactions. Other mechanisms do not necessarily share these predictions. Results from the placebo test are in Table 5. All coefficients on the placebo measures in row 4 of Table 5 are statistically insignificant, as predicted. All previously estimated enforcement spillover coefficients are similar to those in our main analysis in Table 3. The placebo test bolsters our assertion that enforcement leakage is attributable to product market effects and not some other mechanism.

6.3. Changes across the Distribution of Pollution or Changes in Violations?

Although our mechanisms do not require enforcement spillovers to operate directly on compliance outcomes, all effects could potentially impact statutory violations as well as pollution itself. We therefore explore linear probability regressions with a scaled TSS violation indicator, *1[violation$_{it}$]*, as the dependent variable.[44] We find that facility violations decline significantly in the year following fines on other facilities in the same state and industry. We also find suggestive, but less robust, evidence that fines impact facilities in the same state but different sectors as well. We find no consistent evidence that TSS violations increase at facilities in the same industry but different states. Overall, pollution and violation results are collectively most consistent with enforcement-induced TSS reductions within the state coming from avoided high pollution levels, which would reduce both pollution and violations. Enforcement-induced increases in TSS pollution outside of the state are more consistent with increases in pollution occurring across the pollution distribution, which would increase average pollution discharges but not necessarily translate into many more violations in the short run.

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[44] Appendix Table C7 presents results from linear probability regressions with a violation indicator as the dependent variable. Consistent with the earlier literature (Shimshack and Ward 2005; Gray and Shadbegian 2007), the 'same state / same industry' results are large. TSS violations fell by approximately 17 to 57 percent overall (relative to the mean violation propensity) following the marginal fine on others in the same state and industry. TSS violations by facilities in the same state but different sectors also declined by approximately 9 to 22 percent overall (relative to the mean violation propensity) following the marginal fine. Coefficients on 'different state / same industry' are not statistically significant.

6.4. Interpreting Results in the Context of Model-generated Hypotheses

We interpret our empirical results as consistent with our main theoretical predictions. Our first prediction asserts that facilities in the same regulatory jurisdiction but different industries will experience positive enforcement spillovers. We found empirically that a given facility's pollution declined around 2 to 5 percent the year following the marginal fine on other facilities in the same state but a different industry. Prediction 2 states that facilities in the same industry but different regulatory jurisdictions will experience negative enforcement spillovers. Empirical point estimates indicate that a given facility's pollution increased around 3 percent the year following the marginal fine on other facilities in the same industry and geographic area but different states. Prediction 3 maintains that facilities in the same industry facing the same regulatory authority will experience positive enforcement spillovers provided regulatory channels dominate product market channels. We found empirically that a given facility's pollution declined 7 to 14 percent following the marginal fine on other facilities in the same state and industry. This result is consistent with strong regulatory channels and weaker product market channels for large manufacturers in the CWA setting.

Empirical support for our more nuanced theoretical predictions is more mixed. Model prediction 1a asserts that enforcement spillovers for facilities in the same regulatory jurisdiction and industry will not equal spillovers for facilities in the same jurisdiction and different industries. Here, the implicit assumption is that strategic interactions among facilities within these two groups stemming from the regulatory channel will be similar while those arising from the product market channel will differ. We found empirically that spillovers were statistically different at or around the 5 percent level for facilities in the same state and industry vs. the same state and different industry. However, the sign of the difference in coefficients is inconsistent with our theoretical expectations. When facilities in the same industry produce strategic substitutes and all facilities in the same state

face identical positive regulatory spillovers, our model predicts larger positive total enforcement spillovers for facilities in the same state but different industries as compared to facilities in the same state and industry. Our main empirical results show smaller spillovers.[45] Prediction 2a states that enforcement spillovers for facilities in the same industry and same jurisdiction will differ from spillovers for facilities in the same industry and geographic area but different jurisdictions. This prediction implicitly assumes that strategic interactions among facilities within these two groups arising through the product market channel will be similar; the only difference therefore will be those attributable to the regulatory channel. Our main empirical results show that spillovers were indeed statistically different at or around the 5 percent level for facilities in the same industry and state vs. the same industry and different state. The sign of the difference is consistent with the theory.

7. Discussion and Conclusion

Early empirical evidence quantified the impact of enforcement on the subsequent behavior of the sanctioned firm. Then, scholars began discussing and measuring how penalties might spillover to enhance compliance and improve regulatory performance at non-sanctioned facilities facing the same regulatory authority (Braithwaite and Makkai 1991; Ayres and Braithwaite 1992, Thornton, Gunningham, and Kagan 2005; Shimshack and Ward 2005; Gray and Shadbegian 2007; Heyes and Kapur 2009; Rincke and Traxler 2011). The key lesson was that interpreting the effect of enforcement by examining the sanctioned facility alone may understate the implications of enforcement.

Our model and empirical evidence first confirm those same lessons but then show that they are incomplete because they rely only on regulatory interaction mechanisms and overlook product market interactions outside the jurisdiction. We document that enforcement indeed spills over to improve regulatory performance within jurisdictions on average, but that enforcement may also spill over to

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[45] One can imagine many reasons that regulatory spillovers could be greater for plants in the same state and industry than for plants in the same state and different industry, but these are not the focus of the present conceptual framework.

reduce regulatory performance outside the jurisdiction on average. In our specific CWA pollution context, we show that enforcement actions result in a "squeezing the balloon" effect - reducing emissions among facilities within the enforcement jurisdiction but increasing emissions among facilities in the same industry but located in other jurisdictions. Although the mechanism is different, this follows the intuition of the rapidly developing regulatory leakage literature that emphasizes partial regulation. We believe this is the first paper to explain and systematically document "enforcement leakage".

One natural question is the relative magnitude of negative enforcement spillovers, or enforcement leakage, in practice. We do not answer this question definitively in any generalizable way. Nevertheless, back of the envelope calculations based on our CWA investigations of large industrial facilities provide some rough context for one empirical setting. Based on our empirical results and the mean numbers of facilities in states, regions, and industries, we calculate leakage effect point estimates between 50% and 58% depending on specification. Point estimates and confidence intervals calculated from the main results in Table 3 are: specification (1): 55.7%** [90% CI: 11.8%, 99.6%]; specification (2): 57.7%** [90% CI: 26.9%, 88.5%]; specification (3): 49.9%** [90% CI: 10.8%, 89.0%]; and specification (4): 58.1%** [90% CI: 3.5%, 112.7%].[46] In words, our point estimates suggest enforcement spillovers reducing pollution in the state issuing the fine are 50% to 58% offset by enforcement spillovers increasing pollution in other states. These results arise in a setting where the number of facilities in the same state and industry is relatively small but the number of facilities in the same industry but a different nearby state is relatively large.

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[46] Greater than 100% leakage means that spillover-induced total increases in pollution outside the state are greater than spillover-induced decreases in pollution within the state. Calculations use the delta method to determine intervals for non-linear combinations of parameter estimates. Bootstrapped estimates and intervals are similar. Specific calculations use the mean numbers of facilities within groups and the 1-12 months ago enforcement spillover empirical estimates. For example, for specification [1], -1.927 * 10 facilities affected on average by same state / same industry measures; -1.335 * 22 facilities affected on average by same state / different industry measures; and 0.775 * 35 facilities affected on average by same industry / different state measures yields [.775*35] / [(1.927*10)+(1.335*22)] ≈ 0.557. Calculations assume homogeneity in emissions and emissions limits across all facilities and ignore specific deterrence effects.

This paper contributes to our understanding of the pros and cons of federalism. One broad policy implication from our results is that greater coordination across decentralized regulatory authorities may be necessary to mitigate enforcement leakage. These insights may apply to any decentralized administrative law enforcement system, although the nature and empirical strength of spillovers in contexts like antitrust, occupational safety, and food safety warrant further research. In a more specific sense, this paper highlights that the current decentralized environmental enforcement regime in the U.S. (that is, where a federal EPA supports and oversees actual enforcement efforts of states or localities) may require greater coordination of effort. As the current U.S. EPA administration advocates for movement towards further decentralization of enforcement responsibilities and a diminished role for the federal EPA, the specific policy lessons from this research may be particularly timely.

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β
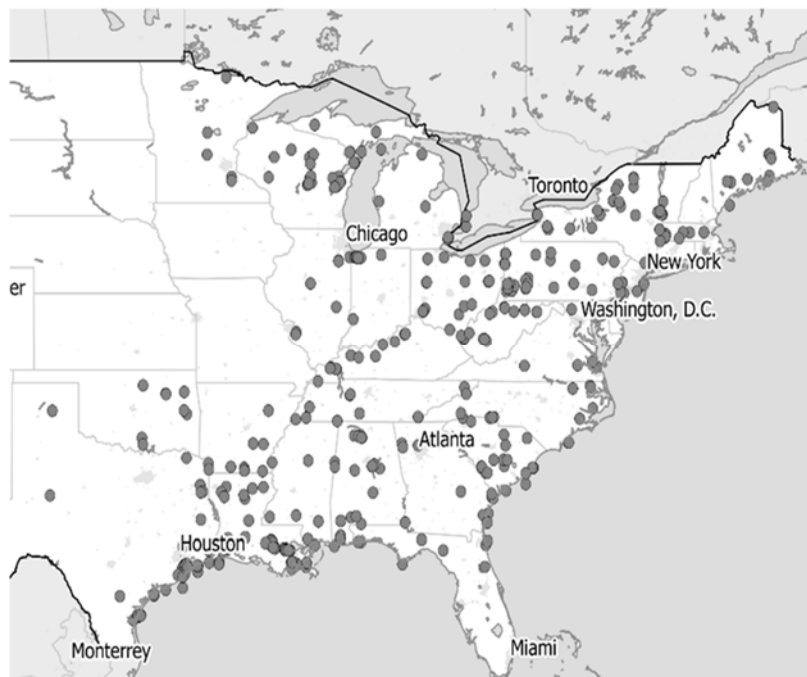
Figure 1. Sample facilities. The 489 final sample industrial facilities are located in the eastern half of the United States.

Figure 2. Total inspections and fines over time. The number of inspections per year generally declines over time. The number of fines per year follows no obvious trend, but declines at the end of the period.
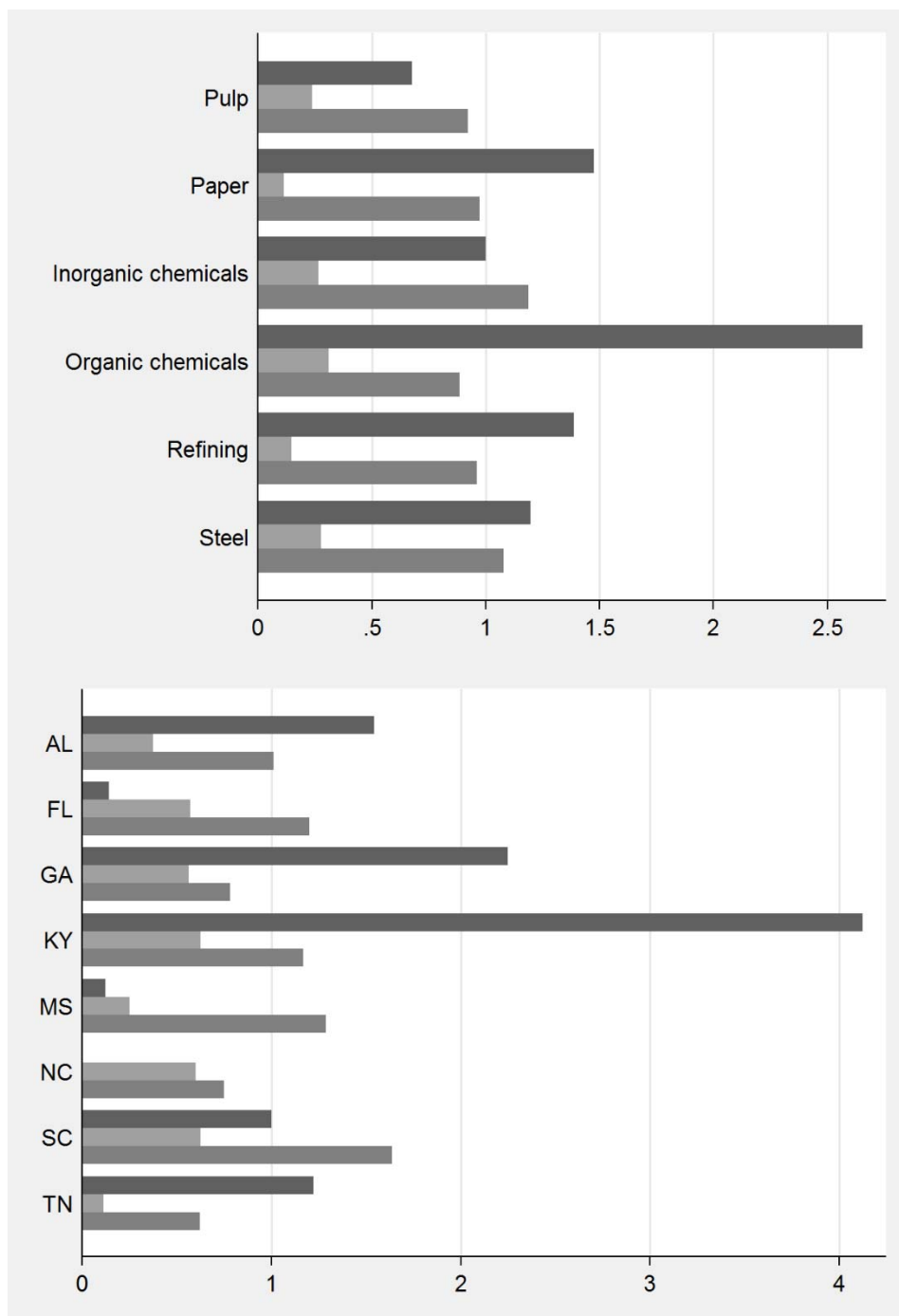
Figure 3. Cross-industry and cross-state variation in violations, fines, and inspections. For each industry or state, the top (darkest) bar represents violations per plant, the middle (lightest) bar represents fines per plant, and the bottom (intermediate tone) bar represents tens of inspections per plant. The top panel illustrates variation in violations and enforcement across 3-digit industries. The bottom panel illustrates variation in violations and enforcement across all states in EPA region 4. Region 4 is arbitrarily chosen for illustration purposes.
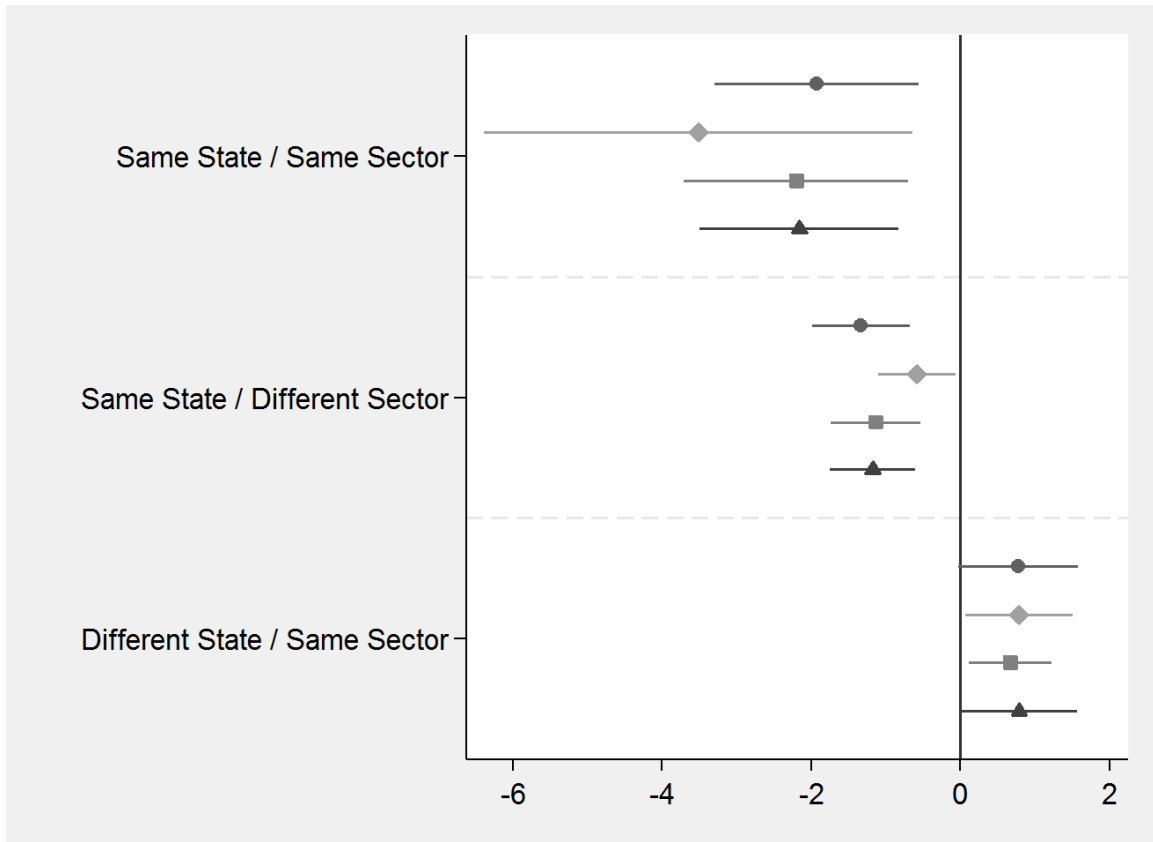
Figure 4. Estimated spillover effect point estimates with 90% confidence intervals. The table summarizes our main results, with the four estimates per category corresponding to the four main specifications. Magnitudes represent percentage point differences in scaled TSS pollution. We find significant and robust reductions in pollution following fines on others in the same state and the same sector. We find significant and robust reductions in pollution following fines on others in the same state and a different sector. We find significant and robust increases in pollution following fines on others in a different state and the same sector.

Table 1: Predicted overall enforcement spillovers

Interaction in regulatory environment	Overall enforcement spillover
Panel A. Interaction in the output market: none	
None	Zero
Positive spillovers	Positive
Negative spillovers	Negative
Panel B. Interaction in the output market: strategic substitutes	
None	Negative
Positive spillovers	Ambiguous
Negative spillovers	Negative
Panel C. Interaction in the output market: strategic complements	
None	Positive
Positive spillovers	Positive
Negative spillovers	Ambiguous

Table 2. Pollution and enforcement: summary statistics

Monitoring and enforcement

# inspections (Facility months with inspection)	Mean inspections per facility month	# fines	Median fine amount	Mean fine amount
6,057	0.10	144	$11,500	$95,800

Monthly TSS pollution (as percent of allowable discharges)

Mean	Max	25th Pctile	75th Pctile	95th Pctile	Std. Dev.	Between Std. Dev.	Within Std. Dev.
25.8%	1938%	7.1%	35.5	70.3%	43.8	24.8	35.9

Monthly TSS compliance

Total Number of violations	Number of violators	Mean violation size
486	126	235% of cap

TSS violations by year (partial year 2006 not included)

1998	1999	2000	2001	2002	2003	2004	2005
68	82	73	72	56	43	42	39

Lagged enforcement across state jurisdictions and across industries

Mean # fines 1-12 months ago in same state, same industry	Mean # fines 1-12 months ago in same state, different industry	Mean # fines 1-12 months ago in same area but different state, same industry
0.22	0.57	1.07

Table 3. Spillover effects of enforcement actions on total suspended solids discharges

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago	-1.927**	-3.511**	-2.201**	-2.157**
same state, same industry	(0.801)	(1.682)	(0.884)	(0.781)
Fines on others 1-12 months ago	-1.335**	-0.584**	-1.137**	-1.176**
same state, different industry	(0.384)	(0.304)	(0.354)	(0.338)
Fines on others 1-12 months ago	0.775*	0.791**	0.670**	0.788**
different state, same industry	(0.472)	(0.422)	(0.324)	(0.458)
Facility-specific trends	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES
Observations	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415

NOTES: All specifications include or nest industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. Significance levels reflect tests of the predictions laid out in section 2. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Table 4. Spillover effects of enforcement actions on TSS discharges: Single plant firms vs. Plants from multiple plant firms

	Known single plant firms				Plants owned by known multi-plant firms			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fines on others 1-12 months ago same state, same industry	-2.281*	-6.233**	-2.627**	-2.567**	-2.424**	-2.415**	-2.754**	-2.496**
	(1.084)	(1.309)	(0.856)	(0.750)	(0.336)	(0.377)	(0.455)	(0.535)
Fines on others 1-12 months ago same state, different industry	-4.712**	-1.886**	-4.566**	-4.088**	-0.392	-0.261	-0.353	-0.376
	(1.555)	(0.514)	(1.306)	(1.049)	(0.298)	(0.582)	(0.334)	(0.297)
Fines on others 1-12 months ago different state, same industry	2.169*	1.946**	1.923**	3.316**	0.649**	0.569**	0.452*	0.322
	(1.324)	(0.863)	(0.835)	(1.340)	(0.257)	(0.238)	(0.266)	(0.304)
Facility-specific trends	YES	NO	YES	YES	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES	NO	NO	NO	YES
Observations	8,243	8,243	8,243	8,243	12,711	12,711	12,711	12,711
Number of facilities	85	85	85	85	130	130	130	130

NOTES: All specifications include or nest industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. Significance levels reflect tests of the predictions laid out in section 2. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Table 5. Placebo test including spillover measures for plants in different states and different industries.

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago	-1.879**	-3.501**	-2.169**	-2.168**
same state, same industry	(0.840)	(1.681)	(0.888)	(0.785)
Fines on others 1-12 months ago	-1.306**	-0.575**	-1.089**	-1.201**
same state, different industry	(0.386)	(0.296)	(0.339)	(0.337)
Fines on others 1-12 months ago	0.790**	0.865**	0.663**	0.786**
different state, same industry	(0.453)	(0.405)	(0.327)	(0.456)
Fines on others 1-12 months ago	0.093	0.182	0.120	-0.037
different state, diff. industry	(0.132)	(0.139)	(0.125)	(0.135)
State-by-year fixed effects	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES
Observations	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415

NOTES: All specifications include or nest industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. Significance level for key enforcement spillovers reflect tests of the predictions laid out in section 2. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

APPENDIX A—SIMULATION

We model an asymmetric oligopoly with N facilities of three different types. Facilities are either type-1, type-A, or type-B, and are located in one of two regulatory jurisdictions, A or B. We assume only one type-1 facility, M type-A facilities, and $N - M - 1$ type-B facilities. Type-1 and type-A facilities are located in jurisdiction A, and therefore face the same regulatory authority. Type-B facilities are located in jurisdiction B. We assume regulatory spillovers are confined within a regulatory jurisdiction. Our simulation characterizes the impacts of an increase in regulatory pressure on the type-1 facility on the emissions choice of type-A facilities, which interact with the type-1 facility in <u>both</u> the product market and regulatory environment, and on the emissions choice of type-B facilities, which interact with the type-1 facility <u>only</u> in the output market.

We model facilities in a Cournot framework, as Bushnell, Mansur, and Saravia (2008) and Fowlie (2009) note that industrial product markets like those in our empirical setting are often characterized by the Cournot framework. Normalize inverse demand to $P = 1 - Q$ where $Q = \sum_{i=1}^{N} q_i$. Assume production costs for facility i are $C_i(q_i, e_i) = \frac{q_i^2}{e_i}$ and define facility i's emissions per unit of output as $\omega_i = \frac{e_i}{q_i}$. Given this cost function, facility i has constant production costs per unit of output equal to $\frac{1}{\omega_i}$. A facility i of type-j faces a regulatory cost function parameterized as $F_i(e_i) = e_i \gamma_j$ where γ_j varies across facility types $j = 1, A, B$. γ_j, which represents the increased regulatory costs associated with a one unit increase in emissions for a type-j facility, depends on the degree of regulatory pressure faced by the facility itself as well as any regulatory spillovers. For the type-1 facility, $\gamma_1 = \bar{\gamma} + \varepsilon$. For all type-A facilities, $\gamma_A = \bar{\gamma} + \beta\varepsilon$ with $\beta \in [-1,1]$; $\gamma_B = \bar{\gamma}$ for all type-B facilities. The sign and magnitude of β indicate the nature and strength of regulatory spillovers in jurisdiction A with positive values of β close to 1 for strong positive spillovers and negative values of

β close to -1 for strong negative spillovers. With this structure, the profit function for a facility i of type-j is given by

$$\pi_i = (1 - q_i - q_{-i})q_i - \frac{q_i^2}{e_i} - e_i \gamma_j, i = 1, \dots, N, j = 1, A, B \qquad (A1)$$

This model yields a convenient analytical solution of the following form:

$$q_1 = \frac{1}{N+1}\left[1 - 2N\sqrt{\gamma_1} + 2M\sqrt{\gamma_A} + 2(N - M - 1)\sqrt{\gamma_B}\right] \qquad (A2)$$

$$q_A = \frac{1}{N+1}\left[1 + 2\sqrt{\gamma_1} - 2(N - M + 1)\sqrt{\gamma_A} + 2(N - M - 1)\sqrt{\gamma_B}\right] \qquad (A3)$$

$$q_B = \frac{1}{N+1}\left[1 + 2\sqrt{\gamma_1} + 2M\sqrt{\gamma_A} - 2(M + 1)\sqrt{\gamma_B}\right] \qquad (A4)$$

$$e_j = \frac{q_j}{\sqrt{\gamma_j}}, j = 1, A, B. \qquad (A5)$$

Comparative static results for the effects of an increase in regulatory pressure on the type-1 facility (i.e., an increase in ε) imply clear predictions about the nature of enforcement spillovers.

We design numerical simulations to illustrate how the magnitudes of predicted enforcement spillovers vary with market and regulatory conditions. Recall that the parameter β measures the nature and strength of regulatory spillovers in jurisdiction A with positive values of β close to 1 for strong positive spillovers and negative values of β close to -1 for strong negative spillovers. Our simulation assumes $N = 10$ and $\bar{\gamma} = 0.05$.

We structure our simulation such that the lone type-1 facility is the only facility <u>directly</u> impacted by increased regulatory scrutiny. Specifically, our simulation illustrates the effects of a 20% increase in regulatory pressure on the type-1 facility (i.e., an increase in ε from zero to 0.01) under varying values of β and under different distributions of facility types. We focus our discussion on the results most germane to our empirical analysis—the effects on emissions of a type-A facility, on emissions of a type-B facility, and on total industry emissions. The type-1 facility always reduces

emissions in response to the higher ε although larger values of β dampen the magnitude of the effect.[1]

Appendix Figures A1, A2 and A3 illustrate our results under three different distributions of facility types. In Appendix Figure A1, four facilities are type-A and 5 facilities are type-B (i.e., an equal number of facilities in the two regulatory jurisdictions). Appendix Figures A2 and A3 assume seven and one type-A facilities, respectively.

As a benchmark, consider the case of no regulatory spillovers. With $\beta = 0$, the effects of increased regulatory pressure on the type-1 facility are driven entirely by facilities' interactions in the product market (i.e., strategic substitutes). As illustrated in Appendix Figures A1-A3, regardless of the distribution of facility types, an increase in ε increases emissions for all type-A and type-B facilities when $\beta = 0$. Total industry emissions fall modestly as the reduction in the type-1 facility's emissions slightly dominates. Note however that, even in the absence of regulatory spillovers, the emissions reduction of the targeted facility (i.e., type-1) is almost entirely offset by the increased emissions from non-targeted facilities arising from the product market interactions.

With non-zero values of β, the total industry effects of increased regulatory pressure on the type-1 facility are driven by the joint impacts of the two channels of strategic interactions. For example, with positive regulatory spillovers ($\beta > 0$) the two channels of interaction between type-1 and type-A facilities work in opposing directions. Given five facilities located in each jurisdiction (Appendix Figure A1) and β greater than about 0.2, the effect of positive regulatory spillovers dominates the effect of strategic substitutes so the type-A facilities reduce emissions when ε increases. Type-B facilities respond with an increase in emissions. While the change in total emissions is

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[1] For sufficiently negative values of β, the type-1 facility's optimal choice of output and emissions is at a corner.

modestly negative, the reduction in industry emissions is an order of magnitude lower than the reduction in the type-1 facility's emissions. [2]

As a final exercise, we allow for variation in the regulatory spillover within and across industries. Reconsider the baseline case of 10 facilities operating in the same industry, five of which are located in the same regulatory jurisdiction. Let $\beta = 0.6$, $\bar{\gamma} = 0.05$ and $\varepsilon = 0.01$. When one of these five facilities faces increased regulatory pressure, the other four facilities in the same jurisdiction each reduce emissions by 29.41%. Now consider the case of an industry with 10 facilities, five of which are subject to a regulator that takes an action against a facility in a <u>different</u> industry. If the cross-industry regulatory spillover is the same as within-industry (i.e., $\beta = 0.6$), then facilities in the same jurisdiction (but not industry) as the targeted facility will each reduce emissions by 32.24%, a greater reduction than the within-industry effect of 29.41% because the regulatory spillover is not offset by product market interactions. However, if the cross-industry regulatory spillover is half as strong (i.e., $\beta = 0.3$), then the reduction in emissions is only 16.81%, significantly smaller than the within-industry effect.

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[2] Note that in the standard Cournot framework, where facilities have constant marginal cost, the extent of "leakage" due to production shifting from facilities in the regulatory jurisdiction of the "targeted" facility to those in other jurisdiction is limited only by market concentration. As the industry approaches perfect competition with the number of facilities in other jurisdictions very large the leakage approaches 100%.
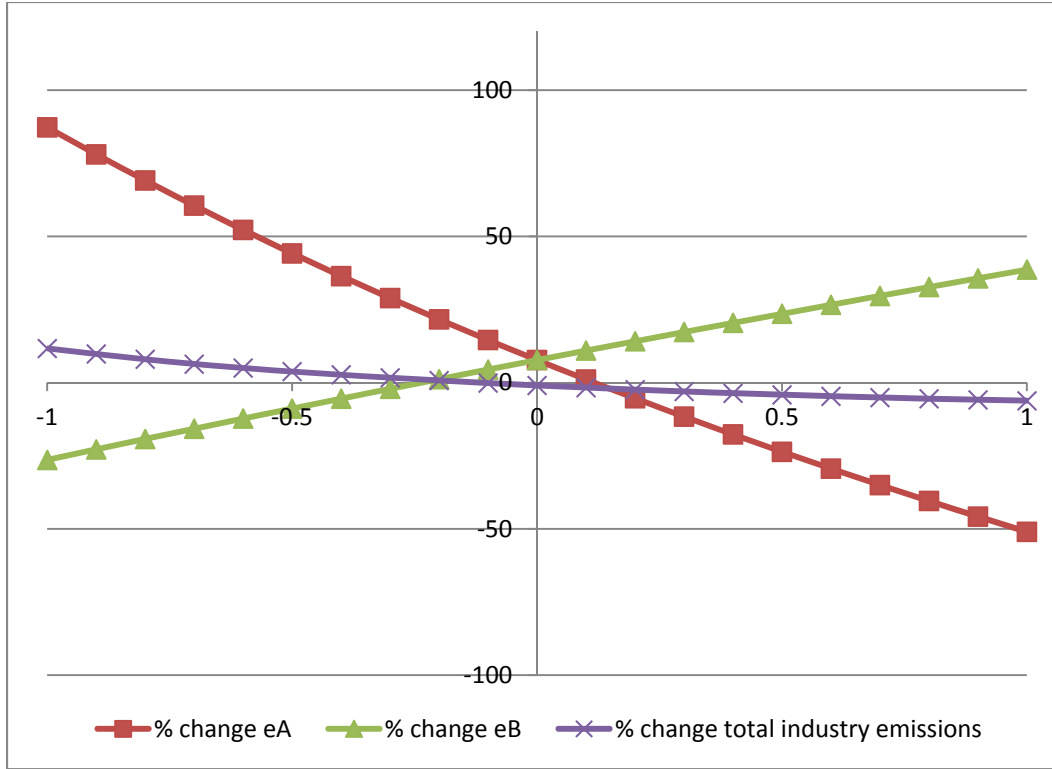
Figure A1: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β. $N_A = 5$, $N_B = 5$. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same jurisdiction A and in the other jurisdiction B to increase pollution. As regulatory spillovers increase (β becomes increasingly positive), facilities in the same jurisdiction A emit less in response to regulatory threat perceptions while facilities in the other jurisdiction B emit more due to product market effects. Product market effects nearly offset regulatory spillover effects when the number of facilities in A and B are equal.

Figure A2: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β. N_A = 8, N_B = 2. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same jurisdiction A and in the other jurisdiction B to increase pollution. As regulatory spillovers increase (β becomes increasingly positive), facilities in the same jurisdiction A emit less in response to regulatory threat perceptions while facilities in the other jurisdiction B emit more due to product market effects. Regulatory spillover effects dominate when the number of facilities in A is relatively large.
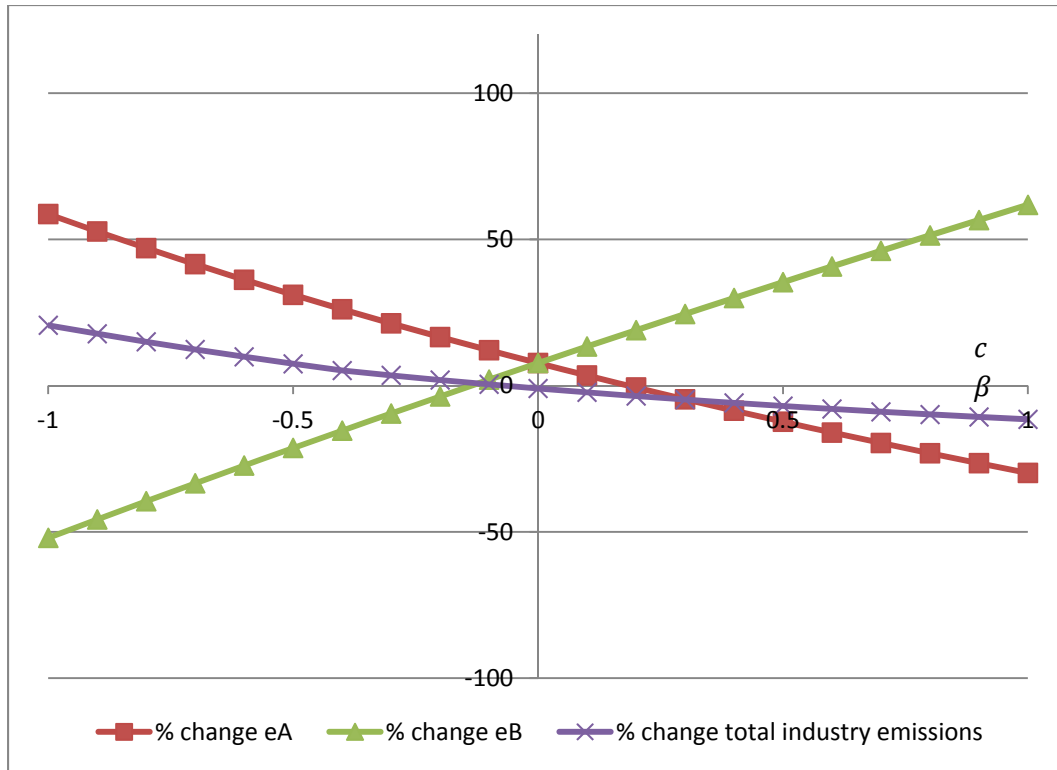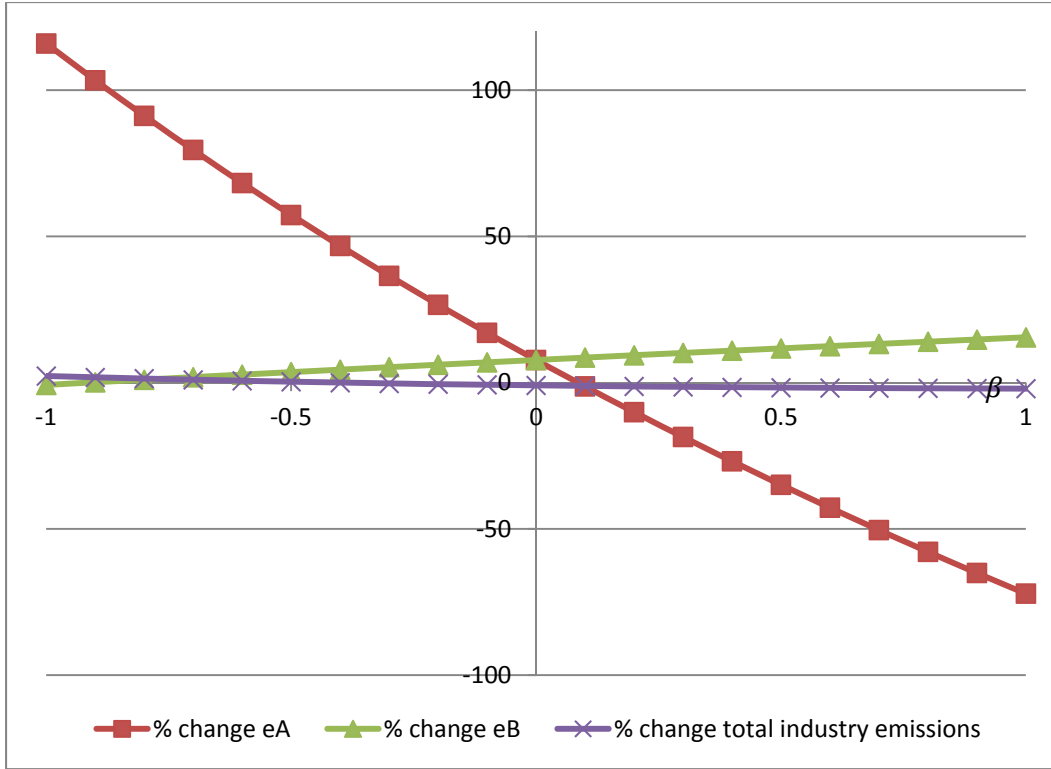
Figure A3: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β. N$_A$ = 2, N$_B$ = 8. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same jurisdiction A and in the other jurisdiction B to increase pollution. As regulatory spillovers increase (β becomes increasingly positive), facilities in the same jurisdiction A emit less in response to regulatory threat perceptions while facilities in the other jurisdiction B emit more due to product market effects. Product market effects dominate when the number of facilities in B is relatively large.

APPENDIX B – SELF REPORTING

A natural question with self-reported data is whether plants strategically non-report or misreport discharges. We believe systematic non-reporting and misreporting are unlikely in our context. Theory suggests that well-designed self-reporting regimes will be incentive compatible if penalties for intentional misreporting are large relative to penalties for act-based violations, and if penalties for intentional misreporting are borne by both principles and agents (Cohen 1992, Kaplow and Shavell 1994). These conditions are met for large CWA facilities. Sanctions for intentional misreporting are severe, and may include incarceration for both employees and managers (Uhlmann 2009). In contrast, penalties for typical violations of permitted pollution limits are relatively modest and do not involve incarceration (Shimshack 2014). Moreover, independent government reviews and a growing empirical literature fail to reject the accuracy of major industrial facilities' CWA self-reports (U.S. EPA 1999; Laplante and Rilstone 1996; Shimshack and Ward 2005; Chakraborti and Shimshack 2012).

Nevertheless, we explored reporting issues empirically for our dataset. To examine non-reporting, we estimated the empirical determinants of missingness in our sample. In our main analysis, less than 4.1 percent of facility-month discharge reports are missing. These instances are most likely uncoded, yet legally permitted, zero discharges. To minimize concerns that missing reports might be strategically missing, we attempted to predict missingness by regressing a missing discharges indicator variable on expected pollution determinants. We were unable to meaningfully predict missingness; reassuringly, we found no significant relationships between missingness and lagged pollution, lagged inspections, and lagged enforcement actions at the facility.

The ideal test of strategic misreporting of pollution data would compare self-reported discharges to objectively measured actual discharges. Unfortunately, not even CWA regulators conduct such direct checks. However, following Laplante and Rilstone (1996), Shimshack and Ward (2005), and Chakraborti and Shimshack (2012), it seems reasonable to suspect that plants report more accurately in the presence of a regulatory inspector. If plants underreport in the absence of an inspector, but report accurately in the presence of an inspector, then one might expect a positive correlation between reported pollution and contemporaneous inspections (after controlling for other pollution determinants and regulatory targeting factors).[1] We regressed our pollution measures on contemporaneous inspections and the full slate of explanatory variables discussed in the next section, and we found no relationship between reported pollution and contemporaneous inspections. Point estimates were small and negative, rather than positive, and t-statistics were below 1. We also replicated the analysis for full sampling inspections only, where regulators spend long periods of time on-site, and continued to find no statistically significant relationship between reported pollution and contemporaneous inspections.

A final concern is that perhaps strategic misreporting occurs only when plants perceive their regulatory environment is unusually harsh. To investigate this concern, we reinvestigated the relationship between reported pollution and contemporaneous inspections, as above, but only for periods where the plant was fined in the past year. The presumption is that plants may be subject to (or at least perceive) increased regulatory scrutiny in the period following a fine. Even

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[1] It is technically possible that plants could exactly scale back pollution to the average reported level when an inspector is present (to cover for misreporting in other periods). We would not detect such behavior in our analysis, but such outcomes are unlikely.

in these cases, we found no statistical difference between reported pollution when an inspector was present and when an inspector was absent.

In sum, although we are only able to conduct imperfect checks of reporting accuracy, both institutional factors and data explorations suggest strategic non-reporting and misreporting are unlikely to be pervasive in our dataset.

Appendix B References

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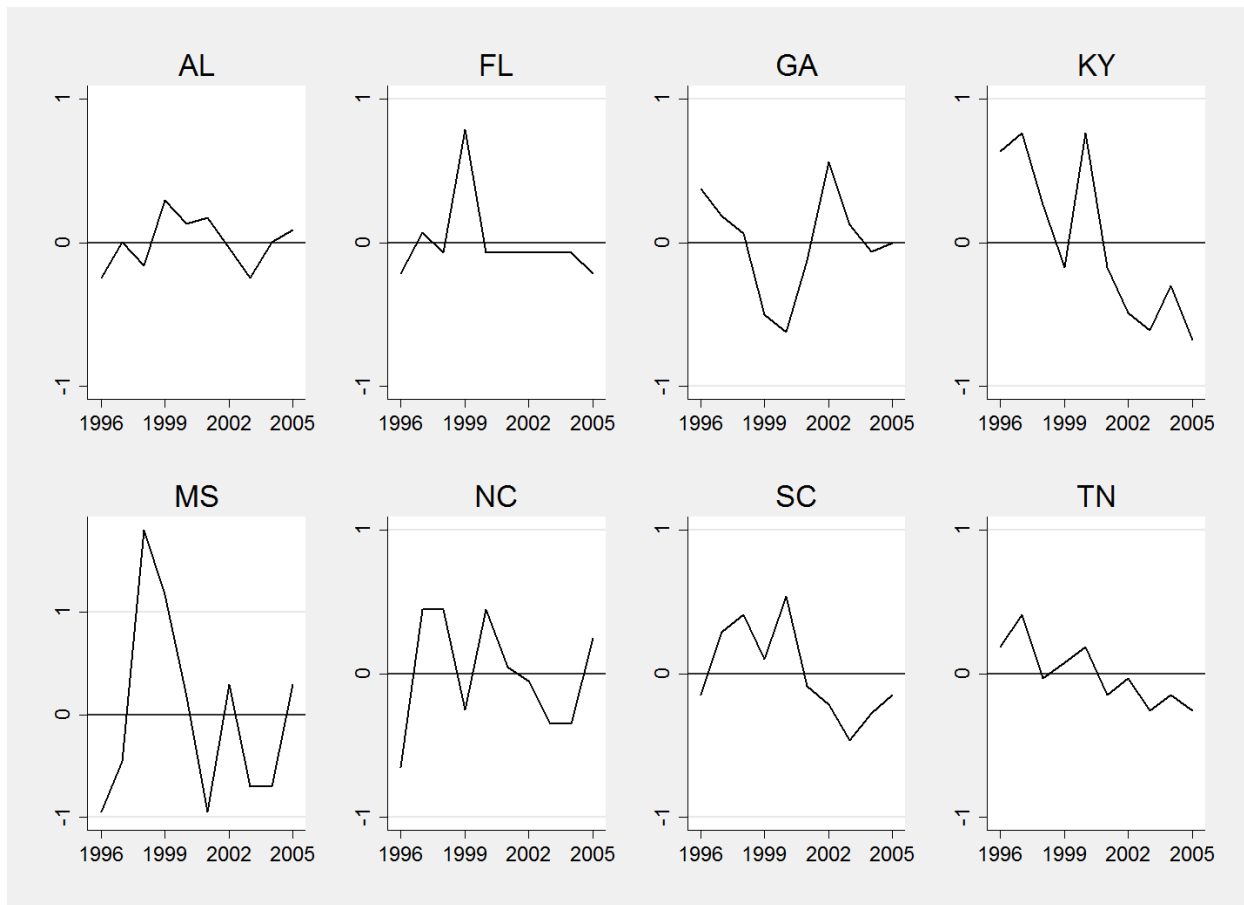
Appendix Figure C1. Variation in inspection activity across time for selected states: Demeaned inspections per plant per year for Region 4 states. Region 4 states are simply illustrative.
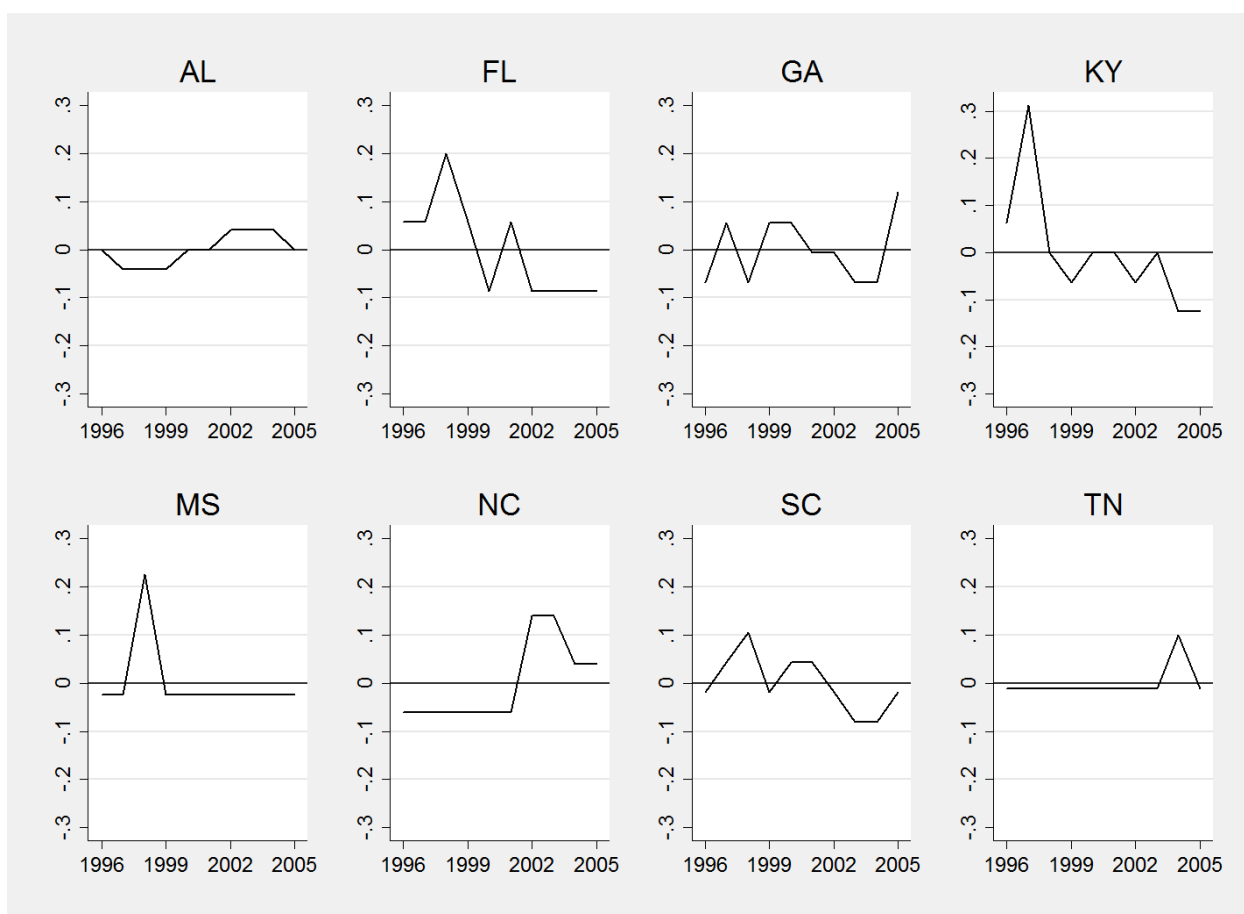
Appendix Figure C2. Variation in enforcement activity across time for selected states: Demeaned fines per plant per year for Region 4 states. Region 4 states are simply illustrative.

Appendix Table C1. Sensitivity to region-by-industry-by-year fixed effects

	(1)	(2)
Fines on others 1-12 months ago same state, same industry	-2.201**	-2.255**
	(0.884)	(0.752)
Fines on others 1-12 months ago same state, different industry	-1.137**	-0.738**
	(0.354)	(0.283)
Fines on others 1-12 months ago different state, same industry	0.670**	0.703**
	(0.324)	(0.342)
Facility-specific trends	YES	YES
Industry-by-year fixed effects	YES	NO
Region-by-industry-by-year Fixed effects	NO	YES
Observations	40,210	40,210
Number of facilities	415	415

NOTES: All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits. For region-by-industry-by-year fixed effects, we define region following EPA conventions, except that we group regions 1-3 into one "super region" to achieve balance in facility numbers and because geographic distances between plants in the mid-Atlantic and Northeastern US are relatively small. 26%, 20%, 21%, and 32% of sample facilities are in regions 1-3, region 4, region 5, and region 6, respectively.

Appendix Table C2. Oster (2017) – style coefficient stability tests

Key explanatory Variable	Unsaturated Model: Baseline effect, [R^2]	Saturated Model: Controlled effect, [R^2]	Identified Set	Delta for B=0, given Rmax
Fines on others 1-12 months ago different state, same industry	1.40 [0.002]	0.77 [0.389]	[0.18, 0.77]	1.182

NOTES: This table applies Oster (2017) tests to explore the validation results for the impact of enforcement leakage measures on pollution. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits. Baseline effects are from unsaturated models which include only the key explanatory variable. Controlled effects are from saturated models which include all spillover measures, facility fixed effects, year controls, season controls, facility-specific time trends, sector-by-month producer price index, and sector-by-month industrial production.

Appendix Table C3. Sensitivity to including one year of leads

	(1)	(2)	(3)	(4)
LAGS				
Fines on others 1-12 months ago	-3.096**	-4.168*	-3.521**	-3.565**
same state, same industry	(1.490)	(2.144)	(1.557)	(1.375)
Fines on others 1-12 months ago	-1.883**	-0.513*	-1.644**	-1.688**
same state, different industry	(0.736)	(0.384)	(0.566)	(0.385)
Fines on others 1-12 months ago	0.923*	0.939**	0.921**	0.879**
different state, same industry	(0.638)	(0.532)	(0.488)	(0.505)
LEADS				
Future fines on others 1-12 months	0.950**	-0.177	0.628	-0.225
same state, same industry	(0.448)	(0.479)	(0.405)	(0.728)
Future fines on others 1-12 months	-1.396**	-0.252	-1.126**	-1.192**
same state, different industry	(0.475)	(0.261)	(0.341)	(0.391)
Future fines on others 1-12 months	0.106	0.333	0.093	-0.609
different state, same industry	(0.308)	(0.338)	(0.336)	(0.733)
Facility-specific trends	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES
Observations	35,310	35,310	35,310	35,310
Number of facilities	415	415	415	415

NOTES: All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Appendix Table C4. Sensitivity to geographic radii

	500-mile radius for different state, same sector				700-mile radius for different state, same sector			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Fines on others 1-12 months ago	-1.905**	-3.520**	-2.139**	-2.087**	-2.015**	-3.556**	-2.345**	-2.468**
same state, same industry	(0.804)	(1.687)	(0.859)	(0.760)	(0.859)	(1.700)	(0.974)	(0.989)
Fines on others 1-12 months ago	-1.318**	-0.583**	-1.115**	-1.147**	-1.350**	-0.572**	-1.140**	-1.174**
same state, different industry	(0.388)	(0.306)	(0.365)	(0.353)	(0.393)	(0.302)	(0.356)	(0.336)
Fines on others 1-12 months ago	0.957**	1.062*	0.900**	1.004**	0.465**	0.520**	0.294**	0.290
different state, same industry	(0.527)	(0.613)	(0.427)	(0.589)	(0.252)	(0.252)	(0.166)	(0.233)
Facility-specific trends	YES	NO	YES	YES	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES	NO	NO	NO	YES
Observations	40,210	40,210	40,210	40,210	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415	415	415	415	415

NOTES: All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Appendix Table C5. Sensitivity to clustering choices

	Clustering at Industry level			Clustering at Facility level			Two-way Clustering at Facility-by-month level		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fines on others 1-12 months ago	-3.511**	-2.201**	-2.157**	-3.511*	-2.201**	-2.157**	-3.511**	-2.201**	-2.157**
same state, same industry	(1.078)	(0.815)	(0.827)	(1.804)	(1.113)	(1.021)	(1.670)	(1.013)	(0.956)
Fines on others 1-12 months ago	-0.584*	-1.137	-1.176	-0.584	-1.137*	-1.176*	-0.584	-1.137*	-1.176*
same state, different industry	(0.354)	(0.871)	(0.865)	(0.535)	(0.812)	(0.804)	(0.497)	(0.769)	(0.770)
Fines on others 1-12 months ago	0.791**	0.670**	0.788*	0.791**	0.670*	0.788	0.791**	0.670**	0.788*
different state, same industry	(0.248)	(0.320)	(0.505)	(0.412)	(0.423)	(0.648)	(0.376)	(0.376)	(0.586)
Facility-specific trends	NO	YES	YES	NO	YES	YES	NO	YES	YES
State-by-year fixed effects	YES	NO	NO	YES	NO	NO	YES	NO	NO
Industry-by-year fixed effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	YES	NO	NO	YES	NO	NO	YES
Observations	40,210	40,210	40,210	40,210	40,210	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415	415	415	415	415	415

NOTES: All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the specified level, are in parentheses. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits. Baseline specifications are omitted for space.

Appendix Table C6. Sensitivity to including or omitting specific deterrence measures

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago	-1.931**	-3.550*	-2.208**	-2.160**
same state, same industry	(0.793)	(1.744)	(0.881)	(0.782)
Fines on others 1-12 months ago	-1.337**	-0.619**	-1.137**	-1.179**
same state, different industry	(0.384)	(0.324)	(0.353)	(0.340)
Fines on others 1-12 months ago	0.775*	0.800**	0.671**	0.791**
different state, same industry	(0.473)	(0.431)	(0.321)	(0.448)
Dummy for own fine	0.019	-0.602	-0.284	-0.001
1-12 months ago	(2.559)	(2.849)	(2.628)	(2.627)
Dummy for own inspection	-0.820	-0.884	-0.847	-0.888
1-12 months ago	(0.917)	(0.908)	(1.011)	(1.111)
Facility-specific trends	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES
Observations	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415

NOTES: Results should be compared to those in Table 3. All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, facility fixed effects, and the listed specific deterrence measures. Standard errors, clustered at the state level, are in parentheses. ** $p<0.05$, * $p<0.10$. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Appendix Table C7. Spillover effects of enforcement actions on TSS violations

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago	-0.320**	-0.690**	-0.329**	-0.206**
same state, same industry	(0.094)	(0.127)	(0.105)	(0.086)
Fines on others 1-12 months ago	-0.144**	-0.270**	-0.123*	-0.104*
same state, different industry	(0.084)	(0.070)	(0.088)	(0.061)
Fines on others 1-12 months ago	-0.005	-0.038	-0.064	0.027
different state, same industry	(0.091)	(0.063)	(0.084)	(0.096)
Facility-specific trends	YES	NO	YES	YES
State-by-year fixed effects	NO	YES	NO	NO
Industry-by-year fixed effects	NO	NO	YES	NO
Industry-by-month fixed effects	NO	NO	NO	YES
Observations	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415

NOTES: All specifications include industry-by-month producer price index (PPI) and output (OUT), year fixed effects, season fixed effects, and facility fixed effects. Standard errors, clustered at the state level, are in parentheses. ** $p<0.05$, * $p<0.10$. The dependent variable is based on an indicator for a TSS pollution violation, where pollution exceeds allowable limits. To ease interpretation, we scale the 0/1 variable by 100 to make it 0/100 (instead of 0/1) in order to reduce decimal places in coefficient estimates (so that the coefficient in row 1, column 1 is -0.00306 when the dependent variable is 0/1). Reported results are based on linear probability models.