

ENFORCEMENT SPILLOVERS: LESSONS FROM STRATEGIC INTERACTIONS IN
REGULATION AND PRODUCT MARKETS¹

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ABSTRACT

We explore enforcement spillovers - when sanctions at one entity influence behavior at other entities. Our model illustrates when spillovers arise from a regulatory channel and when they arise from a channel not previously emphasized: product markets. Our model motivates empirically-refutable 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 but generate negative spillovers for facilities in the same industry facing a different authority. This is the first paper to explain and systematically document this ‘enforcement leakage’.

KEYWORDS: general deterrence, strategic substitutes, regulation, enforcement, pollution policy
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I. 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.³

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 et al. 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

¹ Polinsky and Shavell (2000) survey this literature.

² Cohen (1998); Baker (2003); Jackson and Roe (2009); Ruser and Ruser (2010); Leeth (2012); Gray and Shimshack (2011).

³ Other authors have used the phrases ‘enforcement spillovers’ or ‘enforcement externalities’ to describe geographic spillovers in criminal settings, where enforcement threats induce criminals to shift crime to other areas (e.g. Bronars and Lott 1998). This is not the subject of the present paper.

beliefs about their own expected penalties based on recent experiences of those around them. When agents face a common regulator, spillovers naturally arise (Heyes and Kapur 2009).⁴ 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).⁵ Negative regulatory spillovers arise if enforcement actions against one facility reduce enforcement resources available for targeting other facilities. 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.

The empirical and theoretical work to date explains spillovers by reference only to facility interactions in the regulatory environment as described above. 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.

This paper’s contribution begins by formalizing the insight that these strategic interactions in product markets may drive enforcement spillovers. For example, if two plants produce strategic substitutes so that less aggressive strategies by one increase marginal profits of the other, then enforcement actions levied against one plant will increase output and externalities at the other plant. Enforcement spillovers, which can be negative or positive, arise even in the absence of interactions in the regulatory environment (i.e., even when facilities are located in different regulatory jurisdictions). Thus, enforcement spillovers driven by product market mechanisms could fully explain, reinforce, or

⁴ 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.

⁵ Positive regulatory spillovers arise when other facilities respond by becoming less aggressive (e.g., reduce output, reduce emissions). With negative regulatory spillovers, facilities become more aggressive in response to the marginal enforcement action on another facility.

counteract enforcement spillovers driven by the regulatory interactions emphasized in the existing literature.

We develop an enforcement and compliance model that formalizes facilities' simultaneous interactions in both 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 model a duopoly in which one facility's actions in the output market can change the other facility's strategies via 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 the other 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. To be precise, a key novel feature of our model is that spillovers can be driven by interactions in the regulatory environment, in the product market, or both.

Simulations refine our conceptual results and produce testable hypotheses that we then 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. 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 as producers of strategic substitutes 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 as much as 70% of positive enforcement spillovers within a state are offset by negative enforcement spillovers outside of the state. Despite an increasing understanding of emissions leakage stemming from partial regulation (Fowlie 2009, Bushnell and Mansur 2011, Baylis et al. 2014, Cunningham et al. 2016, and Fischer et al. 2016), we believe this is the first paper to directly explain and systematically document leakage from regulatory enforcement.⁶

II. Modeling enforcement spillovers

A. Setup

We propose a duopoly model in which each facility $i = A, B$ chooses output and emissions, denoted q_i and e_i , respectively to maximize expected profit. Expected profit depends on revenues, production costs, and expected regulatory costs. Facility i 's revenues vary with its own output as well

⁶ 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 inspections actually 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.

as (potentially) the output of the other facility, 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 regulatory pressure faced by the other facility. The parameters ρ_A and ρ_B denote the regulatory pressure faced by facilities A and B, respectively. The regulatory cost function for i is given by $F_i = F_i(e_i, \rho_i, \rho_{-i})$. We assume $\frac{\partial F_i}{\partial e_i} > 0, \frac{\partial^2 F_i}{\partial e_i^2} \geq 0, \frac{\partial^2 F_i}{\partial e_i \partial \rho_i} > 0$. F is not restricted to monetary fines alone and may reflect any cost spurred or leveraged by regulator attention. The expected profit function for facility i is then given by:

$$\pi_i = R_i(q_i, q_{-i}) - C_i(q_i, e_i) - F_i(e_i, \rho_i, \rho_{-i}). \quad (1)$$

Our formulation, motivated by Bulow et al. (1985) and Heyes and Kapur (2009), allows for the possibility that the two plants interact through up to two channels (i) the product market (i.e., if $\frac{\partial^2 R_i}{\partial q_i \partial q_{-i}} \neq 0$), and (ii) the regulatory environment (i.e., if $\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0$). By definition, facilities A and B produce strategic complements if $\frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} > 0$ and strategic substitutes if $\frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} < 0$ for $i = A, B$. We characterize interactions through the regulatory channel as either positive regulatory spillovers, which occur if $\frac{\partial^2 F_i(e_i, \rho_i, \rho_{-i})}{\partial e_i \partial \rho_{-i}} > 0$, or negative regulatory spillovers, which arise when $\frac{\partial^2 F_i(e_i, \rho_i, \rho_{-i})}{\partial e_i \partial \rho_{-i}} < 0$ for $i = A, B$. For example, positive regulatory spillovers might arise when enforcement actions against one facility signal a regulator's reputation for toughness and negative regulatory spillovers might arise when enforcement actions against one facility reduce enforcement resources for targeting other facilities.

The following first order conditions, which characterize the optimal levels of output and emissions for the two facilities, must be satisfied at an interior Nash equilibrium:

$$h_1 \equiv \frac{\partial \pi_A}{\partial q_A} = \frac{\partial R_A}{\partial q_A} - \frac{\partial C_A}{\partial q_A} = 0 \quad (2)$$

$$h_2 \equiv \frac{\partial \pi_A}{\partial e_A} = -\frac{\partial C_A}{\partial e_A} - \frac{\partial F_A}{\partial e_A} = 0 \quad (3)$$

$$h_3 \equiv \frac{\partial \pi_B}{\partial q_B} = \frac{\partial R_B}{\partial q_B} - \frac{\partial C_B}{\partial q_B} = 0 \quad (4)$$

$$h_4 \equiv \frac{\partial \pi_B}{\partial e_B} = -\frac{\partial C_B}{\partial e_B} - \frac{\partial F_B}{\partial e_B} = 0 \quad (5)$$

Let H denote the Hessian matrix of second-order partial derivatives. We assume the second-order conditions for maximization are satisfied and that H satisfies the property of diagonal dominance.⁷ Conditions (2) and (4) have the familiar interpretation that each facility's marginal revenues from production equal marginal costs of production at an optimum. Conditions (3) and (5) imply that each optimizing facility emits until the marginal benefits of polluting in terms of reduced production costs equal the marginal costs of polluting in terms of increased expected regulatory costs. In other words, this is the familiar Becker (1968) condition.

B. Characterizing enforcement spillovers

Our primary interest is characterizing the comparative static effects of increased regulatory pressure on facility A for outcomes at facility B.⁸ We use Cramer's Rule to solve for $\frac{\partial q_B}{\partial \rho_A}$ and $\frac{\partial e_B}{\partial \rho_A}$. An enforcement spillover arises whenever $\frac{\partial e_B}{\partial \rho_A}$ is non-zero. The following propositions illustrate the nature of our results. The appendix contains all proofs.

⁷ Matrix H satisfies diagonal dominance if its diagonal elements are such that $|H_{ii}| > \sum_{j \neq i} |H_{ij}|$ for all i (Bulow et al. 1983).

⁸ Regulatory pressure at facility A, of course, also impacts outcomes at facility A itself. An earlier version of this paper derives these specific deterrence results, which are intuitive. We omit them here since they are not the focus of the paper.

Proposition 1: If the two facilities produce strategic complements and face positive regulatory spillovers, then an increase in regulatory pressure on facility A reduces optimal output and emissions at facility B: $\frac{\partial q_B}{\partial \rho_A}, \frac{\partial e_B}{\partial \rho_A} < 0$.

Proposition 2: If the two facilities produce strategic substitutes and face negative regulatory spillovers, then an increase in regulatory pressure on facility A increases optimal output and emissions for facility B; $\frac{\partial q_B}{\partial \rho_A}, \frac{\partial e_B}{\partial \rho_A} > 0$.

Corollary 1: If the two facilities produce strategic complements and face negative regulatory spillovers, or if they produce strategic substitutes and face positive regulatory spillovers, then an increase in regulatory pressure on facility A has an ambiguous effect on output and emissions at facility B.

In the two cases addressed by Propositions 1 and 2, the incentives for facility B that arise from increased regulatory pressure on facility A through the product and regulatory channels reinforce each other. In the former case, positive enforcement spillovers (i.e., $\frac{\partial e_B}{\partial \rho_A} < 0$) arise while in the latter case, negative enforcement spillovers (i.e., $\frac{\partial e_B}{\partial \rho_A} > 0$) result. In the ambiguous cases covered by Corollary 1, the sign of the effect of an increase in regulatory pressure is determined by the relative strength of the two channels of strategic interaction. For example, if the facilities produce strategic substitutes and face positive regulatory spillovers but the former channel dominates, then an increase in regulatory pressure on facility A increases optimal output and emissions for facility B, $\frac{\partial q_B}{\partial \rho_A}, \frac{\partial e_B}{\partial \rho_A} > 0$.⁹

It is also illustrative to compare and contrast cases with and without product market spillovers, as well as cases with and without regulatory spillovers. To motivate our subsequent empirical setting,

⁹ The next section considers a more structured version of our model, which allows us to further explore the ambiguous cases covered by Corollary 1.

we focus on relationships between regulatory pressure at one facility and emissions outcomes at the other facility (i.e., the sign of $\frac{\partial e_B}{\partial \rho_A}$).

Proposition 3: For facilities that interact in the regulatory environment, the overall enforcement spillover effect when the facilities have independent demands is not equal to the overall spillover effect when facilities have interrelated demands.

Proposition 4: For facilities that interact in the product market, the overall enforcement spillover effect when the facilities have no strategic interactions in the regulatory environment is not equal to the overall enforcement spillover effect when they do.

Our model, and the intuitive propositions and corollary, yield general predictions for overall net enforcement spillovers given the direction and magnitude of interactions in product markets and regulatory environments.¹⁰ These predictions are summarized in Table 1.

C. Interpreting the model: simulation

In this section, we simulate a stylized and more structured version of our model to illustrate key tensions and motivate hypotheses suitable for subsequent empirical testing. Our simulation imposes Cournot competition with linear demand, which results in product market interactions characterized by strategic substitutes. Bushnell et al. (2008) and Fowlie (2009) note that many industrial product markets are reasonably characterized by the Cournot framework.

We simulate an asymmetric Cournot oligopoly with N facilities. Facilities are one of three types, 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

¹⁰ Note that the duopoly model discussed here, which allows us to model product market competition for industries characterized by strategic substitutes or complements, also generalizes to a more competitive market where firms choose output and strategic competition disappears. The Cournot simulation in the following subsection illustrates spillovers as the number of firms becomes large and the strategic aspect of product market competition becomes negligible but product market spillovers remain very relevant.

facilities are located in jurisdiction B. We assume regulatory spillovers are confined within a regulatory jurisdiction.

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

$$\pi_i = (1 - q_i - q_{-i})q_i - \frac{q_i^2}{e_i} - e_i\gamma_j, i = 1, \dots, N, j = 1, A, B \quad (6)$$

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

$$q_1 = \frac{1}{N+1} [1 - 2N\sqrt{\gamma_1} + 2M\sqrt{\gamma_A} + 2(N-M-1)\sqrt{\gamma_B}] \quad (7)$$

$$q_A = \frac{1}{N+1} [1 + 2\sqrt{\gamma_1} - 2(N-M+1)\sqrt{\gamma_A} + 2(N-M-1)\sqrt{\gamma_B}] \quad (8)$$

$$q_B = \frac{1}{N+1} [1 + 2\sqrt{\gamma_1} + 2M\sqrt{\gamma_A} - 2(M+1)\sqrt{\gamma_B}] \quad (9)$$

$$e_j = \frac{q_j}{\sqrt{\gamma_j}}, j = 1, A, B. \quad (10)$$

We assume $N = 10$ and $\bar{\gamma} = 0.05$. We consider two values of ε , zero and 0.01, where the latter denotes a higher degree of regulatory pressure on the type-1 facility.

We structure our simulation such that the lone Type 1 facility is the only facility directly impacted by increased regulatory scrutiny. Specifically, our simulation illustrates the effects of a 20% increase in regulatory pressure on the type-1 facility under varying parameter values for 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.¹¹ Figures 1, 2 and 3 illustrate our results under three different distributions of facility types. In Figure 1, four facilities are type-A and 5 facilities are type-B (i.e., an equal number of facilities in the two regulatory jurisdictions). Figures 2 and 3 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 Figures 1-3, 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. This “squeezing the balloon” effect is an important insight in and of itself. When facilities produce strategic substitutes, the net effect on total emissions of an asymmetric increase in regulatory pressure is akin to the effect of partial regulation

¹¹ For sufficiently negative values of β , the type-1 facility's optimal choice of output and emissions is at a corner.

(Fowlie 2009) - the net effect of regulating only the type-1 facility on total emissions is smaller than if the facilities produced unrelated products (i.e., with no strategic interactions in the product market).

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 (Figure 1) 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 modestly negative, the reduction in industry emissions is an order of magnitude lower than the reduction in the type-1 facility's emissions.

The key lesson from the simulations is that, when one facility receives increased regulatory scrutiny, the effect on total emissions depends on the number of facilities in and outside the regulatory jurisdiction, as well as the nature and strength of spillovers. An interesting implication is that, even if regulatory actions yield significant positive spillovers within jurisdictions, the effect on total emissions may be modest if regulatory 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.

D. Interpreting the model: testable hypotheses

Theory and the simulation results suggest clear empirical predictions. To formalize these, we focus on settings in which (1) strategic interactions in the regulatory environment are confined to facilities facing the same regulatory authority, (2) facilities facing the same primary regulatory authority experience positive regulatory spillovers on average, (3) strategic interactions in the product

market are confined to facilities in the same industry, and (4) different facilities in the same industry produce strategic substitutes on average. Given these conditions, enforcement spillovers can be characterized by the following predictions:

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

Empirical Prediction 1a: 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.

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

Empirical Prediction 2a: 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.

Prediction 1 follows from our discussion above and the definition of positive regulatory spillovers in the absence of the product market channel. Prediction 1a follows from Proposition 3. Prediction 2 follows from the definition of strategic substitutes, as the regulatory spillover channel is absent. Prediction 2a follows from Proposition 4. Prediction 3 follows from Corollary 1 and its proof. All predictions follow the intuition that underlies Table 1.

In subsequent sections, we investigate these predictions for one regulatory setting. Our empirical explorations can be thought of as joint analyses of the predictions themselves and the

conditions outlined in the first paragraph of this subsection. Nevertheless, economic intuition, input-output data, and the existing empirical literature do suggest the underlying conditions hold on average. Regulator spillovers are unlikely to cross regulatory jurisdictions (Gray and Shadbegian 2007, Gray and Shimshack 2011); facilities facing the same primary regulatory authority experience positive regulatory spillovers (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);¹² and industrial facilities in the same broadly-defined industry produce strategic substitutes (Bushnell et al. 2008; Fowlie 2009).

III. Empirical setting and data

A. Clean Water Act regulation of large industrial facilities

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). We focus on CWA facilities largely for reasons related to data quality and completeness; CWA pollution and compliance outcomes are observed every month for all large manufacturing facilities. Moreover, 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.¹³ For large CWA facilities, monitoring and enforcement activities take several forms. The primary monitoring strategy relies on self-reported pollution discharges.¹⁴ Regulator inspections serve to verify the accuracy of self-reporting. Inspections also identify correctable problems and may

¹² 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.

¹³ See Shimshack (2014) for detailed discussion of U.S. environmental monitoring and enforcement institutions.

¹⁴ We, like other researchers and EPA policy-makers, will initially assume that self-reported data for large CWA facilities is reasonably reliable. Nevertheless, we check for evidence of strategic misreporting later in the paper.

support enforcement actions. Inspections can vary from brief reconnaissance inspections that visually examine effluents to rigorous, weeks-long compliance evaluations involving sampling, equipment evaluations, and record-keeping reviews.

CWA enforcement actions vary from informal phone calls to formal actions including civil litigation. Most formal enforcement actions are administrative orders, which may or may not be accompanied by monetary penalties. Although administrative sanctions can include field citations in some states, the bulk of these actions are issued by state or regional administrative law judges. Administrative sanctions can be imposed for paperwork or reporting errors but the large majority at least partially address pollution. Sanctions may address multiple pollutants and violations.

B. Key features of CWA enforcement and compliance

Four features of the CWA enforcement and compliance setting are particularly relevant for our empirical analysis. First, *the regulatory jurisdiction is the state*. The implication is that it is extremely unlikely that facilities in different states 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.¹⁵ 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 very rare for the large CWA industrial facilities in our empirical sample.

Second, *regulatory discretion is pronounced*. The implication is that facilities face uncertain regulatory environments and therefore benefit from regularly updating beliefs about their regulatory

¹⁵ 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.

environments. Discretion is significant because resources are scarce, regulations and enforcement actions are technically and legally complex, and political economic factors are influential.¹⁶ The frequency and severity of CWA inspections and sanctions vary substantially across states and over time within states, even conditional on facility characteristics, pollution discharges, and violations (U.S. GAO 2009). Federal enforcement guidelines dictate that all violations be formally sanctioned and that penalty severity vary with the level of harm, financial gain, compliance history, ability to pay, and intent (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 and vary substantially.

Third, *penalties and other significant sanctions are observable* to other (i.e., non-sanctioned) facilities and stakeholders. The implication is that facilities will typically be aware of recent regulatory activities at other facilities (especially if sanctioned facilities are similar). Enforcement authorities publicize penalties, trade journals summarize regulator actions, and facilities informally interact with one another. Qualitative surveys of firm compliance officials (i.e., at industrial facilities) indicate an awareness of enforcement actions at other industrial facilities among most (as much as 90% of) respondents (Carlough 2004; Thornton et al. 2005).

Fourth, *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, etc. 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 changes in pollution

¹⁶ See, for example, Gray and Deily (1991) and Grooms (2015) for interesting illustrations.

and compliance almost always involve marginal costs rather than new equipment installations (Shimshack and Ward 2008, Gray and Shimshack 2011).

C. Data

Our specific data sources are the EPA's Integrated Compliance Information System and the Permit Compliance System. These databases 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 highly 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). These 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 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

period beginning in 1996 allows two years of enforcement lags.¹⁷ We focus on major (i.e., large) 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.

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 For our empirical analysis, we initially define industries (i.e., product market interactions) by 3-digit SIC code, as this approach balances state and industry coverage. We later explore robustness of our results to an alternative industry definition.

D. Analysis sample

Our final sample consists of 489 large manufacturing facilities. The map in Figure 4 shows the locations of sample facilities. 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

¹⁷ 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.

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.

facilities received 144 fines over the enforcement sample period. The median fine was \$11,500, and fines were highly variable. As discussed in more detail later, these 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.

Figure 5 illustrates basic trends in inspections and fines.¹⁹ The number of inspections per year generally declines over time, with a steeper decline occurring near the end of our sample period. The number of fines per year follows no obvious trend, although Figure 5 depicts a relatively sharp decrease in the last two full years of the sample.²⁰ The total dollar amount of fines, not depicted in Figure 5, 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. A key point is that inspections and fines vary significantly across time and are not overly concentrated at single points in time.

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).²¹ 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. Most of the 74 facilities with missing data were either not required to report TSS discharges or reported no TSS discharges during

¹⁹ 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.

²⁰ Declines in CWA administrative fines after 2005 have been documented elsewhere (Gray and Shimshack 2011).

²¹ 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).

the sample period. A small number of facilities have unexplained missing data, but we are unable to predict missingness with any observable facility characteristic.²²

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.²³ 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.

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 6 illustrates cross-sectional variation in pollution, monitoring, and enforcement. The top panel of Figure 6 highlights variation across 3-digit SIC industries in our sample. 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

²² We explore several aspects of data reliability in subsequent sections.

²³ Theories emphasizing implications of stochastic discharges include Beavis and Walker (1983); Beavis and Dobbs (1987); Segerson (1988); Shimshack and Ward (2008).

comparable to those in the pulp, inorganic chemical, and steel industries where fewer violations were committed per plant on average. Inspection rates were similar across industries. The bottom panel of Figure 6 highlights cross-sectional variation across states. The bottom panel reflects data from EPA region five states only; this choice is arbitrary but illustrative as the variation depicted is similar across all states (not just those within region five). Two key points emerge across the two panels of Figure 6. First, the data are broadly consistent with earlier assertions of variability and discretion across regulatory jurisdictions. Second, despite clear variability, violations, inspections, and fines are not excessively concentrated across space; states and industries experience violations, inspections, and fines in multiple periods.

E. 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 in 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).²⁴ 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.

²⁴ 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.

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 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.

IV. Empirical framework

A. Conceptual underpinnings

Our empirical goal is to identify enforcement spillovers and the channels through which enforcement spillovers arise. Our basic approach involves regressing pollution at a given facility 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.

While this basic set-up follows our earlier modeling framework, applying the insights of our model to the CWA setting warrants some additional discussion. There are two features of our conceptual model that simplify the water pollution discharge decisions facing facilities. First, we model pollution as a fully-determined choice of a facility and therefore not subject to other random factors. Second, the facilities in our conceptual model emit only one pollutant. In practice and in our

empirical application, however, pollution may have a random component such that facilities imperfectly control their pollution discharges and facilities jointly produce multiple pollutants.

In addition, facilities in our sample pollute on average below their discharge limits such that any increases (decreases) in pollution that arise from enforcement spillovers may not result in more (fewer) violations. Although economic intuition and our model suggest that changes in expected penalties will alter polluting behavior, one might question this presumption in the presence of systematic overcompliance. However, in the presence of stochastic and/or jointly produced discharges, spillovers generate economic incentives to alter pollution in response to increased regulatory pressure on other facilities even when mean pollution is relatively low. When facilities imperfectly control their discharges, observed pollution is a stochastic realization around a facility's intended charges and therefore may fall below the standard (Beavis and Walker 1983; Beavis and Walker 1987; Segerson 1988; Bandyopadhyay and Horowitz 2006). Even a facility with observed discharges below the standard has an incentive to respond to a marginal increase in the expected penalty associated with an accidental violation (Shimshack and Ward 2008). Thus randomness can induce facilities to respond to changing enforcement expectations that might arise through enforcement spillovers even when average pollution is well below permitted standards (Shimshack and Ward 2008). Additionally, 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.

Emissions decisions are presumed to be a function of factors influencing expected marginal benefits and expected marginal costs. Empirically, such marginal benefit and cost factors may include

plant and community characteristics, seasonality, national shocks, industry-specific shocks, state-specific shocks. Expected costs of pollution are, of course, also related to expected regulatory penalties. Enforcement spillovers due to regulator reputation effects are predicated on an implicit conceptual framework where plants' behavior is related to their beliefs or expectations about regulatory pressures at their facility and at other facilities. Beliefs are not directly observed, however. Even if they were, beliefs may well be correlated with the error-term in pollution regressions.

As discussed in detail below, our approach of regressing pollution discharges on enforcement spillover measures addresses these natural concerns, and can be thought of as functionally equivalent to a just-identified proxy variable approach.²⁵ In the spirit of Sah (1990)'s work on social osmosis in crime and Shimshack and Ward's (2005, 2008) empirical implementations, the conceptual idea is that plants are presumed to form beliefs or expectations about uncertain current enforcement probabilities by observing and learning from regulator actions in the recent past. Although a facility's own enforcement history would be endogenous because regulatory targeting introduces a correlation with the error term, we take the general view that lagged enforcement actions on *other* facilities, conditional on extensive covariates and fixed effects, are reasonably exogenous sources of variation from the facility's perspective.²⁶

It is worth reiterating here that the expected effects of regulator fines may be functionally larger than the penalty amounts themselves suggest. True economic penalties arising from regulator actions include monetary fines, but also negotiation costs, court costs, degraded relationships with the regulator, and future permitting problems. Fines may leverage additional compliance channels like

²⁵ One could think of this as a reduced form of a deterrence regression with the enforcement spillover measure (the proxy) as an instrument for beliefs.

²⁶ We discuss attempts to minimize several remaining sources of endogeneity in the next section. We note here that one might be concerned that a firm owning a plant that attracts enforcement attention in one state (jurisdiction) attracts greater enforcement attention at one or more of its plants located in other states (jurisdictions). This is extremely unlikely in practice, given devolved responsibilities to state agencies and very limited interactions between state agencies on day-to-day enforcement matters. We discuss related concerns in later sensitivity analyses.

activist pressures, consumer pressures, and input market pressures. True economic penalties might include reduced reputation with consumers, reduced employee satisfaction, increased community pressures, and increased threats of boycotts, letter writing campaigns, and citizen group actions. See, for example, Innes and Sam (2008), Benneer and Olmstead (2008), Langpap and Shimshack (2010), and Lyon and Maxwell (2012). Economic penalties should be interpreted relative to the marginal gain in profitability from a pollution violation, rather than relative to overall facility revenues.

B. Variables and Specifications

Our primary dependent variable is the quantity of total suspended solids (TSS) emissions at facility i in month t , e_{it} , expressed as a percent of permitted limits. We focus on scaled pollution quantity rather than scaled pollution concentration for two reasons. First, TSS quantity is more commonly reported than pollution concentration for major CWA industrial facilities.²⁷ Second, this measure maps clearly to our model where production quantity and emissions are simultaneously determined. TSS quantity is an averaged measure of daily pollution concentrations (typically not observed to the regulator) times daily flow in millions of gallons. In addition to models with TSS pollution as the dependent variable, we also explore sensitivity to a linear probability model where outcomes are defined by the 0/1 indicator function for a TSS violation at facility i in month t , $I[violation_{it}]$. While violations will not necessarily vary directly with TSS pollution, we explore this measure as it is of direct regulatory interest.

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 and in the 13 to 24 months preceding month t . We choose the 1 year and 2 year lags following the empirical environmental enforcement literature (Gray and Deily 1996, Earnhart 2004;

²⁷ 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).

Shimshack and Ward 2005; Gray and Shadbegian 2005; Shimshack and Ward 2008). We focus on administrative/civil fines because criminal fines are not levied for typical CWA violations. The literature generally suggests that less formal sanctions not involving financial penalties have limited impacts on facility behavior (Gray and Shimshack 2011; Shimshack 2014).

To match our testable predictions, the first group of explanatory variables includes the number of fines at other facilities in the same state but different industry in the 1 to 12 months preceding t . Measures for 13 to 24 month lags are similarly defined. Coefficients on these variables are hypothesized to be negative; lagged enforcement actions on others in the same state but different industries are hypothesized to trigger positive regulator spillovers and zero product market spillovers for a net reduction in pollution. The second group of explanatory variables includes the number of fines at other facilities in the same state and industry in the 1 to 12 (or 13 to 24) months preceding t . Coefficients on these variables are hypothesized to be negative if regulatory spillovers dominate product market spillovers and positive if product market spillovers dominate regulatory spillovers.

The third group of key explanatory variables measures the number of fines at other facilities in the same industry and geographic area but a different state in the 1 to 12 (or 13 to 24) months preceding t .²⁸ Coefficients on these variables are 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 these same industry / different state measures 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

²⁸ 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.

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 to loosely represent the size of the gulf coast region, the northeast corridor, and the mid-Atlantic regions (for example). We later demonstrate robustness to alternative radii.²⁹

Key control variables include the industry-by-month producer price index (PPI), denoted PPI_{kt} .³⁰ PPI_{kt} controls for demand shocks common to all facilities within industry k that could be correlated with output, pollution, and enforcement intensity directed towards the sector. Other controls include season-of-year dummies (μ_s), as both pollution and enforcement can vary seasonally. Year dummies (γ_y) control for annual economic and technological shocks common across all facilities in the sample. Final control variables include a facility's own recent monitoring and enforcement actions, I_{it} . The vector I_{it} includes indicators for the following events: facility i was inspected in the 1 to 12 months prior to t , facility i was inspected in the 13 to 24 month prior to t , facility i was fined in the 1 to 12 months prior to t , and facility i was fined in the 13 to 24 months prior to t .³¹

In order to control for time invariant (or nearly so) facility characteristics, we include facility-level fixed effects, α_i . Facility-level fixed effects capture facility characteristics possibly correlated with both pollution and enforcement intensity like industry, subindustry, production capacity, general technology, geography, and the biophysical conditions of the receiving waters and the surrounding

²⁹ Earlier versions of this paper defined these spillover variables using all facilities in different states but in the same sector. Point estimates were, on average, similar but the standard errors were considerably larger. This finding is consistent with facilities outside of the regional radius not contributing meaningfully to the effects but simply adding statistical noise.

³⁰ 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.

³¹ We are not directly interested in the causal interpretation of these specific deterrence measures. Nevertheless, since it may be possible that they are endogenous via time varying regulator targeting, we also demonstrate in Appendix Table 5 that key general deterrence enforcement spillover estimates are essentially unaffected by excluding or including them.

area. Facility-level fixed effects also capture possible confounders associated with community characteristics like income, education, and political affiliations. Notably, facility-level fixed effects eliminate bias from enforcement targeting based on the average environmental performance of the facility, state, region, or industry. Identification is within-group.

For facility i in month t of season s and year y , our regressions take the general form:

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + I_{it}\delta + \mu_s + \gamma_y + \varepsilon_{it}. \quad (9)$$

Variable constructions, facility-level fixed effects, and control variables address many standard threats to plausible causal attribution. We attempt to minimize any remaining endogeneity concerns with additional research designs. We augment regression (9) with facility-specific linear time trends, $t\alpha_i$, to generate specification (10):

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + I_{it}\delta + \mu_s + \gamma_y + t\alpha_i + \varepsilon_{it}. \quad (10)$$

Facility-specific time trends address variation in technology adoption and local economic trends across facilities. We also augment regression (9) with state-by-year fixed effects, τ_{jy} , to generate specification (11):

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + I_{it}\delta + \mu_s + \tau_{jy} + \varepsilon_{it}. \quad (11)$$

State-by-year fixed effects address common shocks within a state that may be correlated with both pollution and enforcement spillover measures.³² Our preferred specification is of the form (12), which augments regression (9) with industry-by-year fixed effects, θ_{ky} , and facility-specific linear time trends as follows:

$$e_{it} = \alpha_i + SPILLOVERS_{it}\beta + \rho PPI_{kt} + I_{it}\delta + \mu_s + t\alpha_i + \theta_{ky} + \varepsilon_{it}. \quad (12)$$

Industry-by-year fixed effects address common shocks within an industry (not already captured by PPI_{kt}) that may be correlated with both pollution and enforcement spillover measures. Such

³² 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 (11).

confounders may be a potential concern when estimating within-industry enforcement spillovers due to product market effects.

In our main analysis, we estimate standard errors clustered at the state-level to test the predictions laid out in section IID. Specifically, for same state / different industry spillover measures, we test a null hypothesis that $\beta = 0$ against an alternative hypothesis that $\beta < 0$. For same industry / different state spillover measures, we test a null hypothesis that $\beta = 0$ against an alternative hypothesis that $\beta > 0$. For same state / same industry spillover measures with ambiguous theoretical predictions, we test a null of $\beta = 0$ against an alternative hypothesis that $\beta \neq 0$. In Appendix Table 3, we note that statistically significant and economically meaningful results are robust to standard errors clustered at the facility-level and the industry-level, as well as to standard errors two-way clustered at the state and month level.

C. Identifying Assumptions

Although we make no attempt to explain plants' persistent average pollution choices, our empirical exploration investigates how *variation* around a given plant's typical environmental performance responds to *variation* in enforcement actions levied against *other* facilities. Ultimate sources of variation in lagged enforcement activity at other facilities in the same state, in different states, in the same industry, or in different industries may include: 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.³³

In our empirical model, we maintain that *lagged* enforcement actions directed towards *other* facilities are a plausibly exogenous source of identifying variation, conditional on controls and fixed

³³ See Shimshack (2014) for an overview of determinants of environmental regulatory enforcement behavior, and Innes and Mitra (2015) for an interesting illustration.

effects. More formally, preferred specifications assume $E(e_{it} | SPILLOVERS_{it}, \alpha_i, \mu_s, \gamma_y, PPI_{kt}, \tau_{jy}) = 0$ or $E(e_{it} | SPILLOVERS_{it}, \alpha_i, \mu_s, \gamma_y, PPI_{kt}, \theta_{ky}) = 0$.³⁴ In words, we assume spillovers are exogenous conditional on controls like PPI; facility, year, and season fixed effects; and either state-by-year fixed effects or industry-by-year fixed effects. We later explore sensitivity to alternative choices. Nevertheless, before proceeding, it is worth noting how our empirical model plausibly addresses several natural concerns.

In order to minimize reverse causality from possible regulatory targeting, we do not identify enforcement impacts from variation in a facility's own enforcement activity. In order to minimize omitted variable bias, we do not identify enforcement impacts from any average differences in enforcement intensity across facilities that might arise due to facility characteristics, costs of compliance, community socio-demographics and pressures, local environmental quality, persistent regulatory characteristics, etc. We do not identify enforcement impacts from variation induced by seasonality or longer-run time trends common to all facilities.

In specifications with state-by-year fixed effects, after netting out the effects of controls, identification of parameters of interest comes only from atypical within-state deviations from state-average enforcement activities for that same year. These specifications rule out bias from confounding annual shocks common to facilities in a state. Consider, for example, a political or economic shock that simultaneously decreased both manufacturing output (and thus pollution) and that state's environmental enforcement intensity. This would not bias spillover estimates unless the shock was correlated at the short-run monthly level with both anomalous pollution and anomalous enforcement intensity within the state and within the year.

³⁴ Our data vary significantly between-group and within-group. Nevertheless, we also present results from specifications that do not include state-by-year or industry-by-year fixed effects to illustrate that our main results are not driven by removing too much meaningful variation from the data.

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. These specifications rule out bias from annual shocks common to all facilities in a sector. Consider, for example, a positive demand shock that simultaneously increased both manufacturing output (and thus pollution) and environmental enforcement intensity in a given industry. This would not bias spillover estimates unless the demand shock was correlated at the short-run monthly level with both anomalous pollution and anomalous enforcement intensity within the sector and within the year.

V. Results

Table 3 presents estimated coefficients on the enforcement spillover measures associated with equations (9)-(12). Before interpreting our key enforcement spillover results, we briefly note the impact of control variables. Industry-by-month producer price index (PPI) is positively related to pollution discharges, as expected. This supports the hypothesis that observed TSS pollution is at least partially determined by production. Consistent with summary statistics, we find that pollution declines significantly over our sample period and varies seasonally with highs in late winter and lows in the late summer and early fall. As expected, signs and significance on facility-level fixed effects and facility-specific time trends (when included) vary substantially. Idiosyncratic specific deterrence measures, like lagged own inspections and own fines, are consistently negatively related to subsequent pollution but typically not statistically significant.³⁵

A. Estimated enforcement spillover effects

³⁵ These insignificant coefficients may represent truly small incremental deterrence effects from a facility's own enforcement and monitoring actions, at least conditional on general deterrence signals about overall regulator behavior. Alternatively, small and insignificant negative specific deterrence coefficients may suggest some positive bias from targeting-induced reverse causality. As shown in Appendix Table 5, all other results are robust to including or omitting these control variables.

Table 3 presents results from our main analysis. The first and second rows indicate 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 2.4 to 3.4 percentage points in the year following the marginal fine on other facilities in same state and industry. These results translate into around a 9 to 13 percent overall reduction relative to the mean discharge ratio. These general deterrence effects from fines in the same state and industry persist through time; the discharge ratio fell about 2.2 to 3.6 percentage points in the second year following the marginal fine on other facilities in the same state and industry.

The third 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 third row indicate that TSS discharge ratios fell by 0.5 to 1.0 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 4 percent overall reduction relative to the mean. Results in the fourth row indicate no general deterrence effects from fines in the same state and different sectors that persist into a second year. It appears that within state but different industry effects may decay rapidly (in this case, to the null of zero after one year).

The fifth and six rows of Table 3 indicate that facilities' average discharge ratios increase on average following fines on other facilities in the same industry and general geographic area, but located in different regulatory jurisdictions (states). The one-year lag results are statistically significant at the 10 percent level for all specifications and statistically significant at the 5 percent level for the preferred specification that controls for common industry shocks outside of the enforcement process. Results reported in the fifth row indicate that TSS discharge ratios increased by 0.7 to 0.9 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 to 4 percent overall increase relative to the mean. Results in the sixth row provide only suggestive evidence that these effects persist beyond one year.

Table 4 presents results from linear probability regressions with the TSS violation indicator, $I[violation_{it}]$, as the dependent variable. Although none of our theoretical channels require enforcement spillovers to operate directly on compliance outcomes, as discussed in the previous sections, all effects could potentially impact statutory violations as well as pollution itself. Row 1 of Table 4 suggests facility violations declined significantly in the year following fines on other facilities in the same state and industry. Consistent with the earlier literature (Shimshack and Ward 2005; Gray and Shadbegian 2007), these results are large. TSS violations fell by approximately 25 to 50 percent overall (relative to the mean violation propensity) following the marginal fine on others in the same state and industry. We also find evidence that fines impact facilities in the same state but different sectors as well. Row 3 of Table 4 suggests that TSS violations by facilities in the same state but different sectors declined by approximately 5 to 9 percent overall (relative to the mean violation propensity) following the marginal fine. We find some suggestive evidence in rows 5 and 6 that TSS violations may increase by facilities in the same state but different sectors, but the results do not appear strong initially. Coefficients on fines in the same industry but different state in the previous year are small and statistically insignificant but become larger and statistically significant after longer lags. Overall, results are 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 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.

B. 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 (i.e., pollution shifts from a facility owned by a firm to another facility owned by the same firm).³⁶ Of particular concern is the possibility that our negative within-industry/out-of-state enforcement 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, production shifting across states is not necessarily consistent with our product market spillover mechanism. We therefore 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 5 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) are systematically 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 are again similar to those in Table 3, and patterns of statistical significance remain similar except that same state / different sector results are no longer significant. Most notably, magnitudes of same industry / different state spillovers are smaller than those for single-plant firms

³⁶ 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.

and smaller than comparable results in Table 3. In short, we find no evidence that negative spillover results are driven by multi-plant firms shifting production from high enforcement states to low enforcement states.

C. Sensitivity

One concern with our most novel results is that a positive demand shock to an industry might result in increased output from all facilities in the sector, and perhaps that demand shock also triggers greater enforcement directed towards some facilities/states in the sector but not others. In this case, detected enforcement spillovers from facilities in the same industry but different states could not be interpreted causally. Of course, we already condition on producer price index (PPI) to capture many common demand shocks within an industry. Moreover, key specifications in Tables 3 and 4 include industry-by-year fixed effects, so the demand shock must induce an idiosyncratic output and enforcement response within the industry and within the year. As a practical matter, the necessarily nuanced enforcement agency response is unlikely. Nevertheless, we further attempt to minimize any such omitted variable bias by augmenting specifications of the form of (12) with region-by-industry-by-year fixed effects. In these specifications, spillover measures could only be biased due to demand shocks not correlated with output price yet still co-moving with idiosyncratic deviations in enforcement within region, within industry, and within year. Appendix Table 1 presents results, which are similar to our main results. Point estimates for key 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.

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

in our main analysis we restricted 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. Appendix Table 2 therefore explores robustness to different geographic radii. Point estimates and general patterns of statistical significance are extremely similar for all spillover measures operating on facilities within the same state, which is expected as these facilities are not directly affected by radii choices. We also 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). All one-year lag results in row 5 are statistically significant at the 10 percent level and statistically significant at the 5 percent level for the preferred specifications. As perhaps expected, empirical magnitudes of out-of-state spillovers are larger when geographic area radii are smaller and magnitudes are smaller when geographic area radii are larger.³⁷

Other possible concerns involve clustering and industry definition choices. For all presented specifications, we clustered standard errors at the state-level. Appendix Table 3 explores sensitivity to clustering at the industry level and facility level, as well as to two-way clustering at the facility-by-month level. Results are robust, and in some cases statistically significant at smaller alphas. We defined spillover variables at the 3-digit level in order to balance depth and breadth of coverage. Appendix Table 4 explores sensitivity to defining enforcement spillover variables at the 2-digit industry level. Signs are generally consistent with the 3-digit results but point estimates are systematically smaller and noisier on average, consistent with expectations given less precisely defined industrial categories.

C. Interpreting empirical results in the context of model-generated hypotheses

³⁷ Similar investigations to those in Table 5 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.

Interpreting empirical results necessitates several caveats. First, our industry classifications are coarse approximations to actual plant-level strategic interactions in product markets. We acknowledge that an ideal analysis would involve plant-level regulatory data that: (1) can be reliably matched to firm ownership, and (2) can be matched to real strength of product market interaction measures between firms on a large scale.³⁸ This is a promising subject for future research. Nevertheless, our data do permit explorations of enforcement spillovers that have not been explored at all in the extant literature. Even with potentially unrefined industry proxies for strategic interaction boundaries, we detect enforcement spillovers that are statistically significant, meaningfully large and most naturally interpreted through the lens of regulatory and output market mechanisms.

Second, our model suggests that enforcement spillovers affect pollution through production. We acknowledge that the ideal data would allow us to observe plant-level pollution and production outcomes in order to more completely document the empirical mechanisms, as observed enforcement spillovers could operate through both production choices and abatement per unit production choices.³⁹ Nevertheless, we note that our dependent variable is a quantity-based pollution measure that is a direct function of production, and it is highly positively correlated with industry-specific output price. Also, as a practical matter this paper's key insights address regulation, and we observe the regulatory outcomes of direct interest (performance and violations).

Subject to the above caveats, 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 4 percent the year following the marginal fine on other

³⁸ We are unaware of datasets that would reliably facilitate either of these steps for CWA data.

³⁹ Plant-level monthly production measures are unavailable in Clean Water Act datasets. Even highly detailed establishment-level datasets, like the confidential Census Longitudinal Business Database, do not provide necessary monthly data that might complement this study.

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 to 4 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 9 to 13 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, at least at first blush. 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. We found empirically 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.⁴⁰ The sign of the difference in coefficients is consistent with the theory.

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

⁴⁰ Regression coefficients in Table 3 columns (1) – (4) on 'fines on others 1-12 months ago, same state, same industry' and 'fines on others 1-12 months ago, different state, same industry' are statistically different from one another at or around the 5 percent level.

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.⁴¹ However, at first glance, 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 empirical results find smaller spillovers. A likely explanation is that regulatory channels themselves operate more strongly for facilities in the same industry; in the spirit of Sah (1991), facilities may be most likely to be aware of and/or most likely to extract signals from, enforcement actions levied towards facilities more like themselves. This makes sense if different states focus on different pollutants, some industries have more or less political clout within a given state, etc. Our model generates predictions consistent with this empirical finding when we allow for stronger regulatory spillovers within industries than across industries.⁴²

VI. Discussion and conclusion

It was long believed that the impact of enforcement was on the subsequent behavior of the sanctioned firm alone. Then, legal scholars began asserting that penalties might spillover to enhance compliance and improve regulatory performance at non-sanctioned facilities facing the same

⁴¹ Regression coefficients in Table 3 columns (1) – (4) on ‘fines on others 1-12 months ago, same state, same industry’ and ‘fines on others 1-12 months ago, same state, different industry’ are statistically different from one another at or around the 1 percent level.

⁴² We thank careful readers for highlighting practical reasons to suspect that spillovers will be greatest for plants in the same state and industry. To see this more formally, reconsider our simulation results with a 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 different 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.

regulatory authority (Braithwaite and Makkai 1991; Ayres and Braithwaite 1992, Thornton et al. 2005). A handful of law and economics studies eventually developed models and/or detected such enforcement spillovers empirically (Shimshack and Ward 2005; Gray and Shadbegian 2007; Heyes and Kapur 2009; Rincke and Traxler 2011). The key lesson was that the “bang per buck” of regulatory enforcement was perhaps bigger than expected; interpreting the effect of enforcement by examining the sanctioned facility alone may understate the implications of enforcement within the same regulatory jurisdiction.

Our model, simulations, and empirical evidence first confirm those same lessons but then show that they are incomplete because they rely only on regulatory interaction mechanisms and ignore product market interactions. We document that enforcement indeed spills over to improve regulatory performance within jurisdictions on average, but that enforcement may also spill over to *reduce* regulatory performance outside the jurisdiction on average. To be precise, in our specific pollution context, we show that enforcement actions could 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 rapidly developing regulatory leakage literature that emphasizes partial regulation. We believe ours 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 cannot answer this question definitively in any generalizable way. Nevertheless, back of the envelope calculations based on our CWA investigations provide some rough context for one empirical setting. Our results suggest that the marginal fine induces: facilities in the same state and sector to reduce pollution about 9 to 13 percent in the year following the fine;

facilities in the same state but different sectors to reduce pollution about 2 to 4 percent in the year following the fine; and facilities in the same industry and geographic region but in different states to increase pollution about 2 to 4 percent in the year following the fine. 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 and area but a different state is relatively large. Our total net results suggest about 68% leakage:⁴³ enforcement spillovers reducing pollution in the state issuing the fine are about 70% offset by enforcement spillovers increasing pollution in other states.

One policy implication is that greater coordination across decentralized regulatory authorities may be necessary to mitigate enforcement leakage. In a broad sense, this paper contributes to our understanding of pros and cons of federalism. In a more specific sense, this paper highlights that the current decentralized enforcement regime in the U.S. and other developed nations (i.e. where a federal EPA supports and oversees actual enforcement efforts of states or localities) may require great coordination of effort. As the new U.S. EPA administration considers a movement towards further decentralization of enforcement responsibilities and a diminished role for the federal EPA, the specific policy lessons from our research are particularly timely.

⁴³ These calculations use one year lag coefficients from preferred specifications in Table 3, as well as the mean number of facilities within group. $-2.638 * 10$ facilities affected on average by same state / same sector measures; $-0.761 * 22$ facilities affected on average by same state / different sector measures; and $0.843 * 35$ facilities affected on average by same sector and area / different state measures. $[\.843*35] / [(2.638*10)+(.761*22)] \approx 0.68$. These back of the envelope calculations assume homogeneity in emissions and emissions limits across all facilities.

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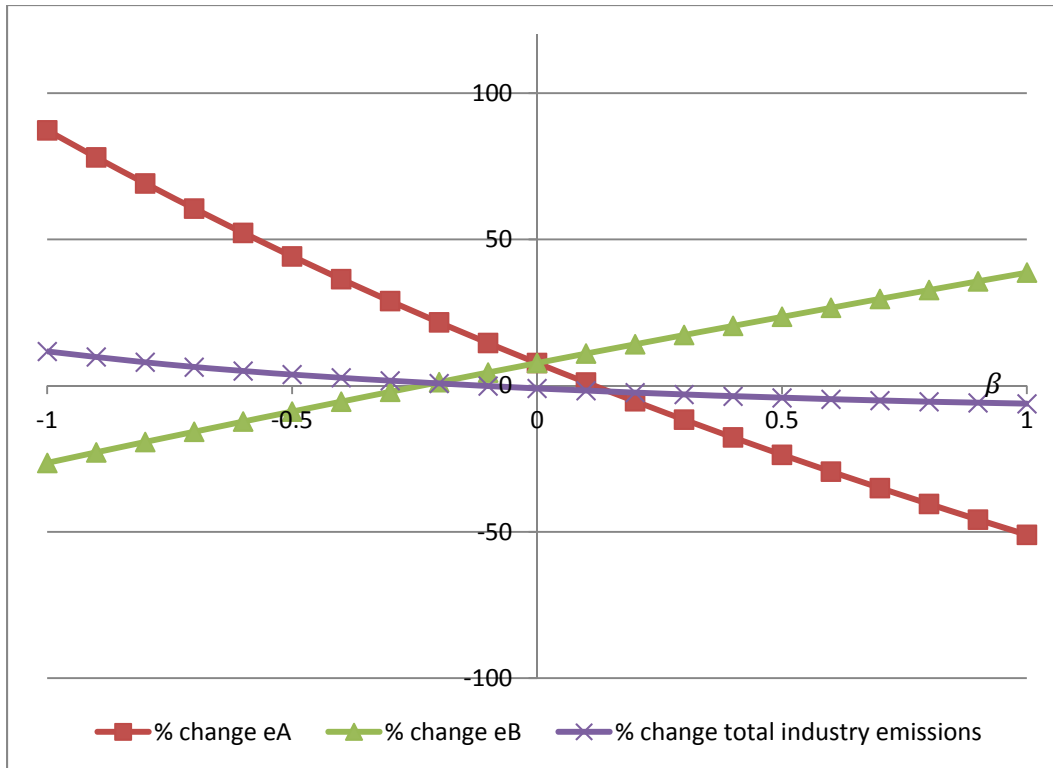


Figure 1: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β . $N_A = 5$, $N_B = 5$. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same state A and in the other state B to increase pollution. As regulatory spillovers increase (β becomes increasingly positive), facilities in the same state A emit less in response to regulatory threat perceptions while facilities in the other state 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.

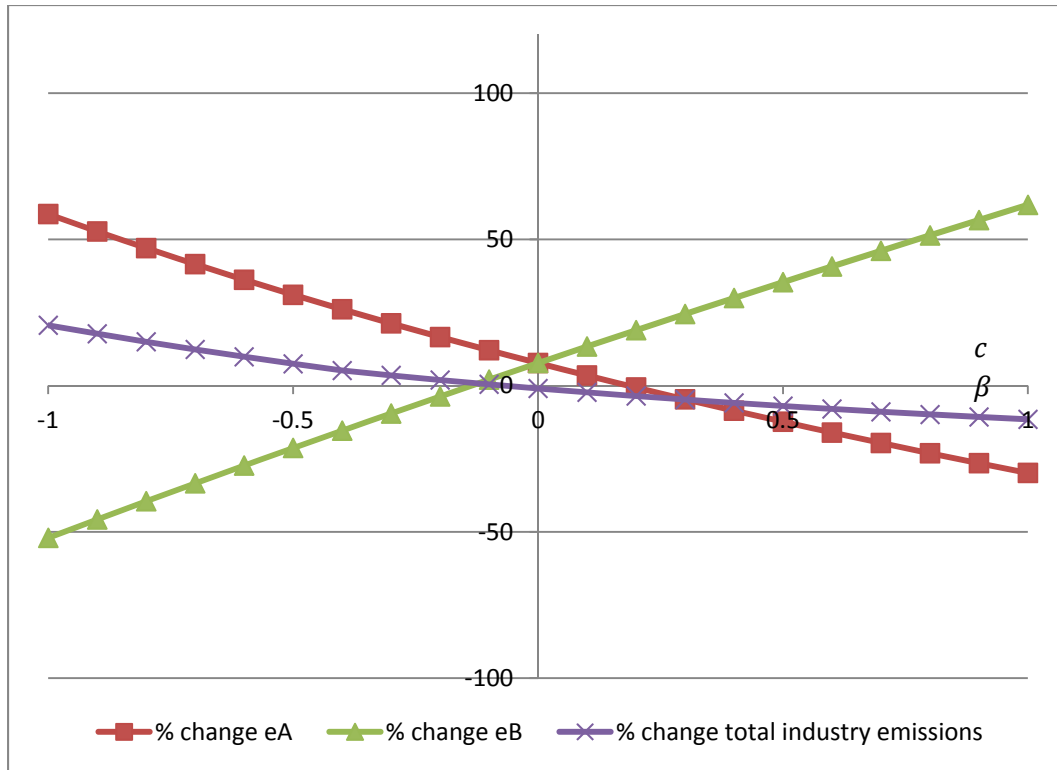


Figure 2: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β . $N_A = 8$, $N_B = 2$. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same state A and in the other state B to increase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

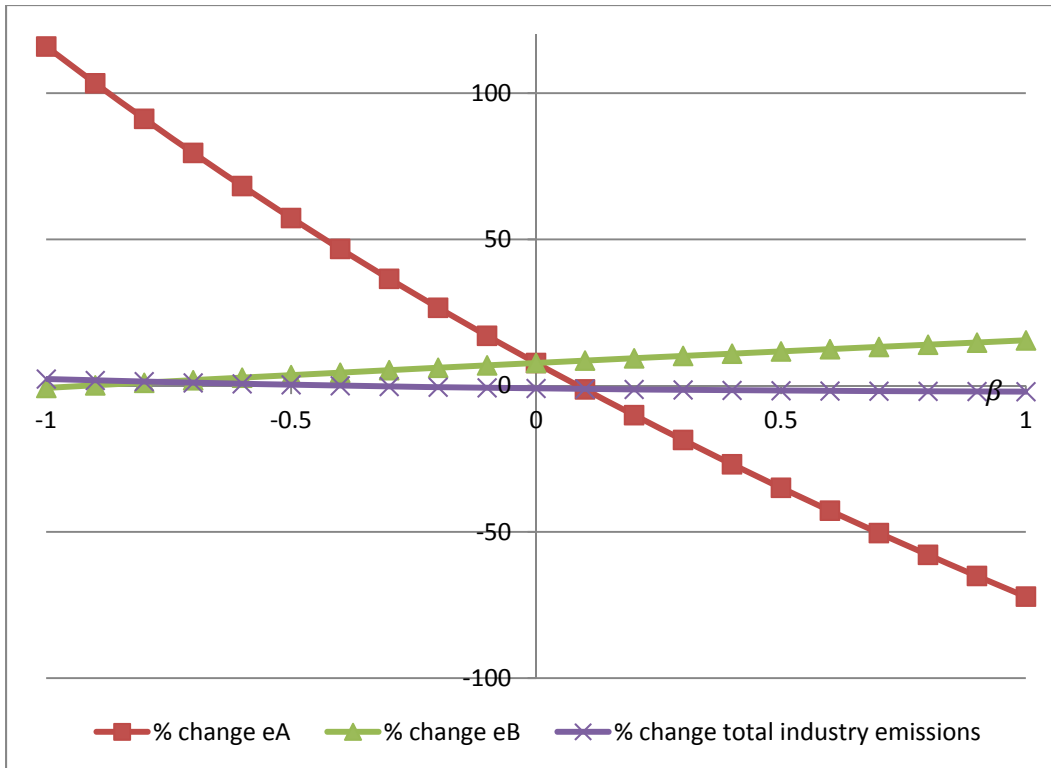


Figure 3: Simulation effects of increased regulatory pressure on one facility in jurisdiction A for different values of β . $N_A = 2$, $N_B = 8$. When regulatory spillovers (as measured by β) are zero, product market effects cause other facilities in the same state A and in the other state B to increase pollution. As regulatory spillovers increase (β becomes increasingly positive), facilities in the same state A emit less in response to regulatory threat perceptions while facilities in the other state B emit more due to product market effects. The product market effect dominates when the number of facilities in B is relatively large.

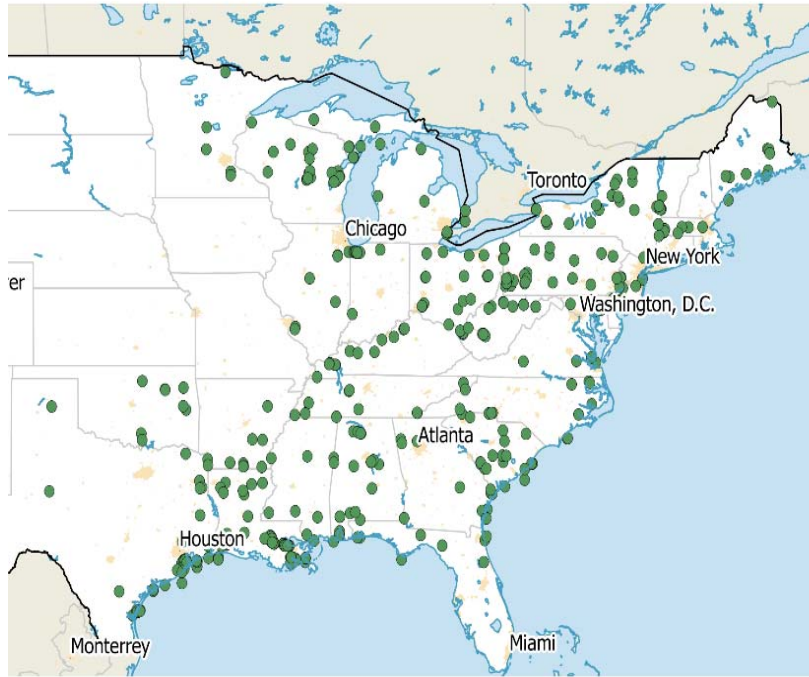


Figure 4. Sample facilities. The 489 final sample industrial facilities are located in the eastern half of the United States.

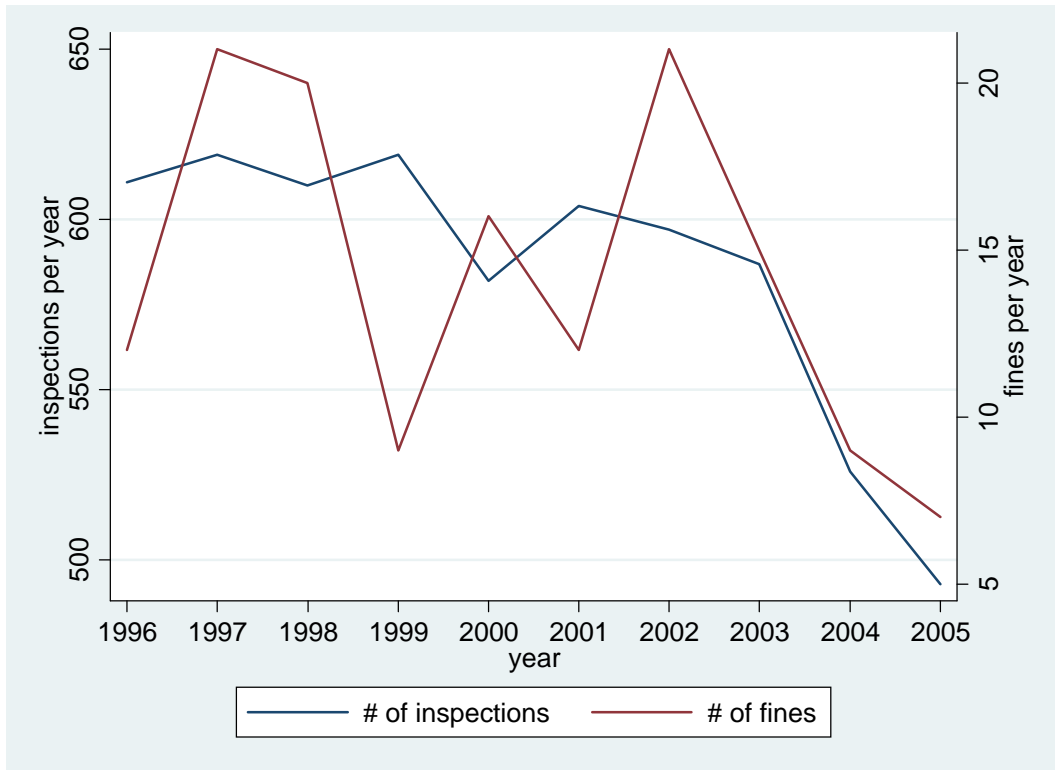


Figure 5. Total inspections and fines over time. The number of inspections per year generally declines over time. The number of fines per year follows no obvious trend, but declines at the end of the period.

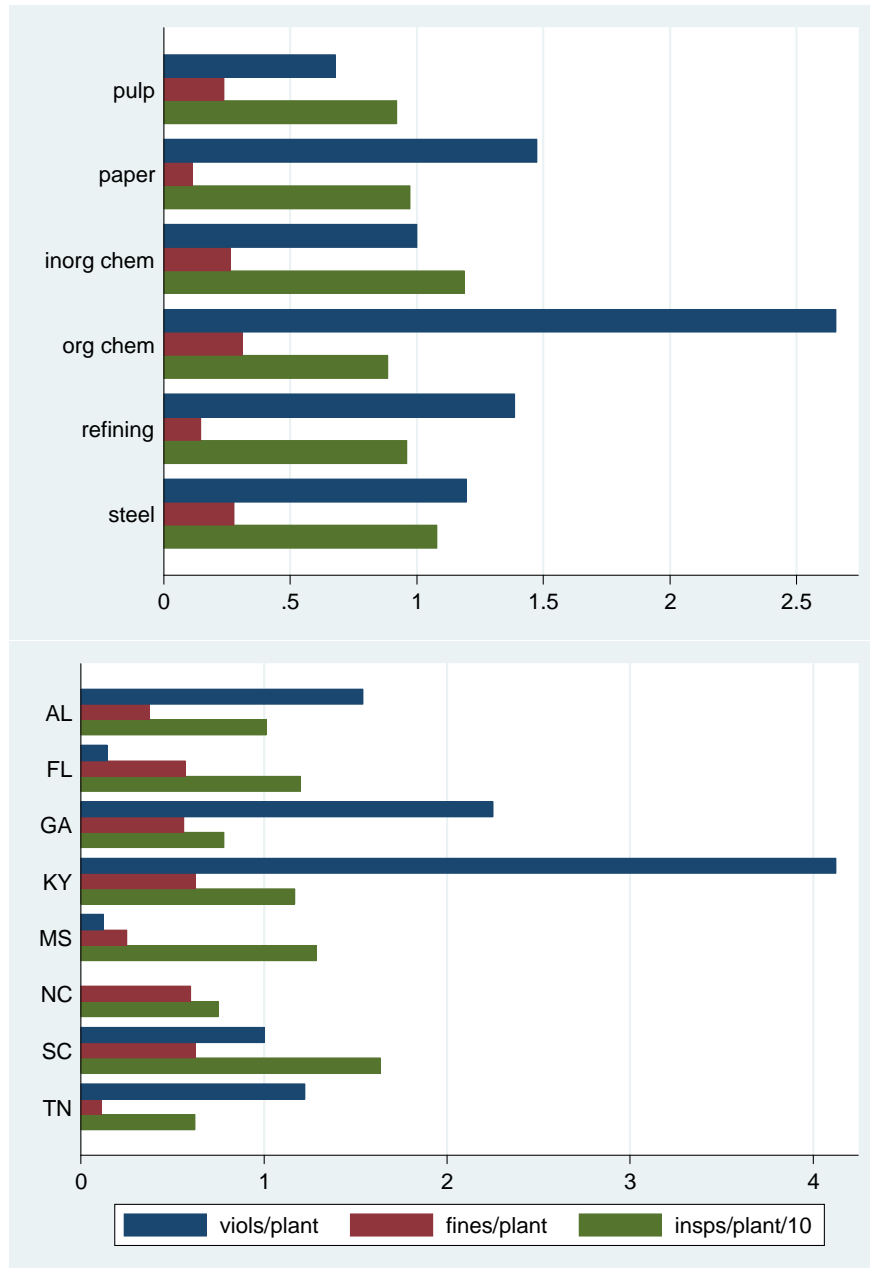


Figure 6. Cross-industry and cross-state variation in violations per plant, fines per plant, and tens of inspections per plant. The top panel illustrates variation in violations and enforcement across 3-digit industries. The bottom panel illustrates variation in violations and enforcement across all states in EPA region 5, which is arbitrarily chosen for illustration. Note that more violations are not necessarily associated with more fines or more inspections.

Table 1: Predicted net enforcement spillovers

Interaction in regulatory environment	Interaction in output market	Overall enforcement spillover	Sign of $\frac{\partial e_B}{\partial \rho_A}$
None	None	Zero	0
None	Strategic substitutes	Negative	+
None	Strategic complements	Positive	-
Positive spillovers	None	Positive	-
Positive spillovers	Strategic substitutes	Ambiguous	?
Positive spillovers	Strategic complements	Positive	-
Negative spillovers	None	Negative	+
Negative spillovers	Strategic substitutes	Negative	+
Negative spillovers	Strategic complements	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	25 th Pctile	75 th Pctile	95 th 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			

NOTES: TSS pollution and compliance summary statistics reflect 41,390 facility-by-month TSS observations on the 415 facilities reporting TSS discharges regularly during our January 1998 to May 2006 analysis sample period.

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

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago same state, same industry	-3.217** (1.097)	-2.429** (0.947)	-3.360** (1.443)	-2.638** (0.997)
Fines on others 13-24 months ago same state, same industry	-3.614** (1.186)	-2.311** (1.027)	-2.955** (0.888)	-2.215** (0.937)
Fines on others 1-12 months ago same state, different industry	-0.831** (0.277)	-0.974** (0.269)	-0.452* (0.310)	-0.761** (0.373)
Fines on others 13-24 months ago same state, different industry	1.080 (0.930)	1.032 (0.890)	1.408 (1.135)	1.115 (0.822)
Fines on others 1-12 months ago different state, same industry	0.670* (0.470)	0.905* (0.598)	0.859* (0.556)	0.843** (0.417)
Fines on others 13-24 months ago different state, same industry	0.388 (0.392)	0.557 (0.497)	0.421 (0.369)	0.734* (0.484)
Facility-specific trends	NO	YES	NO	YES
State-by-year fixed effects	NO	NO	YES	NO
Industry-by-year 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), lagged own inspections and fines, 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.

Table 4. Spillover effects of enforcement actions on TSS violations

	Linear Probability	Linear Probability	Linear Probability	Linear Probability
	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago same state, same industry	-0.636** (0.092)	-0.306** (0.113)	-0.694** (0.123)	-0.270** (0.113)
Fines on others 13-24 months ago same state, same industry	-0.501** (0.137)	-0.049 (0.238)	-0.482** (0.147)	-0.015 (0.241)
Fines on others 1-12 months ago same state, different industry	-0.055* (0.038)	-0.110** (0.060)	-0.290** (0.080)	-0.095* (0.059)
Fines on others 13-24 months ago same state, different industry	0.177 (0.139)	0.153 (0.145)	0.049 (0.162)	0.131 (0.140)
Fines on others 1-12 months ago different state, same industry	0.012 (0.076)	0.030 (0.086)	-0.022 (0.065)	0.015 (0.076)
Fines on others 13-24 months ago different state, same industry	0.183** (0.073)	0.190** (0.070)	0.158** (0.084)	0.241** (0.080)
Facility-specific trends	NO	YES	NO	YES
State-by-year fixed effects	NO	NO	YES	NO
Industry-by-year 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), lagged own inspections and fines, 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 an indicator for a TSS pollution violation, where pollution exceeds allowable limits.

Table 5. 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	-6.526** (1.086)	-2.899** (1.318)	-5.768** (1.329)	-3.143* (1.575)	-2.235** (0.225)	-3.281** (0.619)	-2.620** (0.236)	-3.597** (0.842)
Fines on others 13-24 months ago same state, same industry	-9.581** (2.646)	-4.827** (1.571)	-3.858** (1.517)	-4.358** (1.802)	-1.991** (0.735)	-3.215** (1.281)	-2.577** (0.354)	-3.233** (1.401)
Fines on others 1-12 months ago same state, different industry	-2.023** (0.590)	-3.219** (0.916)	-0.438 (0.696)	-3.009** (0.927)	-0.481 (0.381)	-0.013 (0.293)	-0.454 (0.396)	0.045 (0.401)
Fines on others 13-24 months ago same state, different industry	3.775 (2.519)	3.476 (2.282)	5.981 (3.762)	2.715 (1.776)	0.351 (0.572)	0.712 (0.789)	0.137 (1.027)	1.022 (0.724)
Fines on others 1-12 months ago different state, same industry	2.281* (1.402)	2.879* (1.783)	2.521* (1.537)	2.923** (1.217)	0.516** (0.238)	0.671** (0.263)	0.543** (0.285)	0.425* (0.312)
Fines on others 13-24 months ago different state, same industry	1.945 (1.673)	2.542 (2.047)	1.679 (1.508)	3.476** (1.668)	-0.228 (0.363)	0.126 (0.394)	-0.289 (0.398)	0.023 (0.480)
Facility-specific trends	NO	YES	NO	YES	NO	YES	NO	YES
State-by-year fixed effects	NO	NO	YES	NO	NO	NO	YES	NO
Industry-by-year 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 industry-by-month producer price index (PPI), lagged own inspections and fines, 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: PROOFS

Proofs rely on the comparative static effects of a change in ρ_A . We derive these comparative static results before proceeding to the proofs. The elements of the Hessian matrix of second-order partial derivatives follows:

$$h_{11} \equiv \frac{\partial^2 \pi_A}{\partial q_A^2} = \frac{\partial^2 R_A}{\partial q_A^2} - \frac{\partial^2 C_A}{\partial q_A^2}$$

$$h_{12} \equiv \frac{\partial^2 \pi_A}{\partial q_A \partial e_A} = -\frac{\partial^2 C_A}{\partial q_A \partial e_A} > 0$$

$$h_{13} \equiv \frac{\partial^2 \pi_A}{\partial q_A \partial q_B} = \frac{\partial^2 R_A}{\partial q_A \partial q_B}$$

$$h_{14} \equiv \frac{\partial^2 \pi_A}{\partial q_A \partial e_B} = 0$$

$$h_{21} \equiv \frac{\partial^2 \pi_A}{\partial e_A \partial q_A} = -\frac{\partial^2 C_A}{\partial e_A \partial q_A} = h_{12} > 0$$

$$h_{22} \equiv \frac{\partial^2 \pi_A}{\partial e_A^2} = -\frac{\partial^2 C_A}{\partial e_A^2} - \frac{\partial^2 F_A}{\partial e_A^2} < 0$$

$$h_{23} \equiv \frac{\partial^2 \pi_A}{\partial e_A \partial q_B} = 0$$

$$h_{24} \equiv \frac{\partial^2 \pi_A}{\partial e_A \partial e_B} = 0$$

$$h_{31} \equiv \frac{\partial^2 \pi_B}{\partial q_B \partial q_A} = \frac{\partial^2 R_B}{\partial q_B \partial q_A}$$

$$h_{32} \equiv \frac{\partial^2 \pi_B}{\partial q_B \partial e_A} = 0$$

$$h_{33} \equiv \frac{\partial^2 \pi_B}{\partial q_B^2} = \frac{\partial^2 R_B}{\partial q_B^2} - \frac{\partial^2 C_B}{\partial q_B^2}$$

$$h_{34} \equiv \frac{\partial^2 \pi_B}{\partial q_B \partial e_B} = -\frac{\partial^2 C_B}{\partial e_B \partial q_B} > 0$$

$$h_{41} \equiv \frac{\partial^2 \pi_B}{\partial e_B \partial q_A} = 0$$

$$h_{42} \equiv \frac{\partial^2 \pi_B}{\partial e_B \partial e_A} = 0$$

$$h_{43} \equiv \frac{\partial^2 \pi_B}{\partial e_B \partial q_B} = -\frac{\partial^2 C_B}{\partial e_B \partial q_B} = h_{34} > 0$$

$$h_{44} \equiv \frac{\partial^2 \pi_B}{\partial e_B^2} = -\frac{\partial^2 C_B}{\partial e_B^2} - \frac{\partial^2 F_B}{\partial e_B^2} < 0$$

We assume the matrix H is negative definite, which requires, $|H| > 0$, $h_{11}h_{22} - h_{12}h_{21} > 0$ and $h_{11} <$

0. The determinant of H is given by:

$$(h_{11}h_{22} - h_{12}h_{21})(h_{33}h_{44} - h_{34}h_{43}) - h_{13}h_{31}h_{22}h_{44}.$$

Since the first term in parentheses and the last term are positive, $|H| > 0$ requires $h_{33}h_{44} - h_{34}h_{43} >$

0. This implies $h_{33} < 0$ since $h_{44} < 0$. The signs of h_{13} and h_{31} depend on whether the facilities produce strategic substitutes or complements.

Additional second-order partial derivatives required for the comparative static effects of a change in ρ_A follow:

$$h_{1\rho_A} \equiv \frac{\partial^2 \pi_A}{\partial q_A \partial \rho_A} = 0$$

$$h_{2\rho_A} \equiv \frac{\partial^2 \pi_A}{\partial e_A \partial \rho_A} = -\frac{\partial^2 F_A}{\partial e_A \partial \rho_A} < 0$$

$$h_{3\rho_A} \equiv \frac{\partial^2 \pi_B}{\partial q_B \partial \rho_A} = 0$$

$$h_{4\rho_A} \equiv \frac{\partial^2 \pi_B}{\partial e_B \partial \rho_A} = -\frac{\partial^2 F_B}{\partial e_B \partial \rho_A}.$$

If the facilities face positive (negative) regulatory spillovers, then $h_{4\rho_A} < 0$ ($h_{4\rho_A} > 0$). By Cramer's rule, the comparative static effects of a change in ρ_A are given by:

$$\begin{aligned}\frac{\partial q_A}{\partial \rho_A} &= \frac{1}{|H|} [h_{12}h_{2\rho_A}(h_{33}h_{44} - h_{34}h_{43}) - h_{22}h_{13}h_{34}h_{4\rho_A}] \\ \frac{\partial e_A}{\partial \rho_A} &= \frac{1}{|H|} [h_{2\rho_A}(-h_{11}h_{33}h_{44} + h_{11}h_{34}h_{43} + h_{13}h_{31}h_{44}) + h_{13}h_{21}h_{34}h_{4\rho_A}] \\ \frac{\partial q_B}{\partial \rho_A} &= \frac{1}{|H|} [h_{34}h_{4\rho_A}(h_{11}h_{22} - h_{12}h_{21}) + h_{12}h_{31}h_{44}h_{2\rho_A}] \\ \frac{\partial e_B}{\partial \rho_A} &= \frac{1}{|H|} [-h_{33}h_{4\rho_A}(h_{11}h_{22} - h_{12}h_{21}) + h_{12}h_{31}h_{43}h_{2\rho_A}].\end{aligned}$$

Proof of Proposition 1:

If the facilities produce strategic complements and face positive regulatory spillovers, then $h_{31} > 0$ and $h_{4\rho_A} < 0$. Under these conditions, the term in brackets in the expression for $\frac{\partial q_A}{\partial \rho_A}$ is negative. Since $|H| > 0$, $\frac{\partial q_A}{\partial \rho_A} < 0$ in this case. In order to show $\frac{\partial e_A}{\partial \rho_A} < 0$, first note that the last term in brackets, $h_{13}h_{21}h_{34}h_{4\rho_A}$, is negative when $h_{31} > 0$ and $h_{4\rho_A} < 0$. We now show that if H satisfies diagonal dominance, then the term in parentheses, $-h_{11}h_{33}h_{44} + h_{11}h_{34}h_{43} + h_{13}h_{31}h_{44}$, is positive. The following inequalities hold under diagonal dominance:

- (i) $|h_{11}| > |h_{12}| + |h_{13}|$
- (ii) $|h_{22}| > |h_{21}|$
- (iii) $|h_{33}| > |h_{31}| + |h_{34}|$
- (iv) $|h_{44}| > |h_{43}|$

Multiply both sides of (iii) by $|h_{11}||h_{44}|$:

$$|h_{11}||h_{33}||h_{44}| > |h_{11}||h_{31}||h_{44}| + |h_{11}||h_{34}||h_{44}|.$$

The right-hand side of this expression exceeds the following:

$$|h_{13}||h_{31}||h_{44}| + |h_{11}||h_{34}||h_{43}|$$

since by (i), $|h_{11}| > |h_{13}|$, and by (iv), $|h_{44}| > |h_{43}|$. Combining equalities we have:

$$|h_{11}||h_{33}||h_{44}| > |h_{11}||h_{31}||h_{44}| + |h_{11}||h_{34}||h_{44}| > |h_{13}||h_{31}||h_{44}| + |h_{11}||h_{34}||h_{43}|.$$

This implies that the first term in parentheses, $-h_{11}h_{33}h_{44}$, which is positive, is larger in absolute value than the remaining two terms, $h_{11}h_{34}h_{43} + h_{13}h_{31}h_{44}$, which are negative. With $h_{2\rho_A} < 0$, the term in bracket is negative in the case of strategic complements and positive regulatory spillovers. Given $|H| > 0$, $\frac{\partial e_A}{\partial \rho_A} < 0$.

Proof of Proposition 2:

If the facilities produce strategic substitutes and face negative regulatory spillovers, then $h_{31} < 0$ and $h_{4\rho_A} > 0$. The proof of this proposition is analogous to that of Proposition 1 but takes these sign differences into account.

Proof of Corollary 1:

Corollary 1 follows directly from comparing the relevant comparative static results.

Proof of Proposition 3:

When the two facilities have independent demands, the overall enforcement spillover effect is given by:

$$\left. \frac{\partial e_B}{\partial \rho_A} \right|_{\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0, \frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} = 0} = -\frac{1}{|H|} [h_{33}h_{4\rho_A}(h_{11}h_{22} - h_{12}h_{21})].$$

With independent demands and positive (negative) regulatory spillovers, the overall enforcement effect is negative (positive) so increased regulatory pressure on facility A decreases (increases) facility B's emissions. When demands are also interrelated, the overall enforcement spillover effect becomes:

$$\left. \frac{\partial e_B}{\partial \rho_A} \right|_{\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0, \frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} \neq 0} = \frac{1}{|H|} [-h_{33}h_{4\rho_A}(h_{11}h_{22} - h_{12}h_{21}) + h_{12}h_{31}h_{43}h_{2\rho_A}].$$

Therefore, $\left. \frac{\partial e_B}{\partial \rho_A} \right|_{\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0, \frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} = 0} \neq \left. \frac{\partial e_B}{\partial \rho_A} \right|_{\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}} \neq 0, \frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} \neq 0}$.

Proof of Proposition 4:

When the two facilities have interrelated demands but no strategic interaction in the regulatory environment, the overall enforcement spillover effect is given by:

$$\frac{\partial e_B}{\partial \rho_A} \Big|_{\frac{\partial^2 F_i}{\partial e_i \partial \rho_{-i}}=0, \frac{\partial^2 R_i(q_i, q_{-i})}{\partial q_i \partial q_{-i}} \neq 0} = \frac{1}{|H|} [h_{12} h_{31} h_{43} h_{2\rho_A}].$$

With no regulatory spillovers, if the facilities produce strategic complements (substitutes), then the general deterrence effect is positive (negative) so increased regulatory pressure on facility A decreases (increases) facility B's emissions. The overall enforcement spillover effect when they strategically interact in both settings is given in the proof of Proposition 3. Comparing the two expressions yields the result.

ONLINE APPENDIX: SUPPLEMENTARY TABLES

Appendix Table 1. SENSITIVITY TO REGION-BY-INDUSTRY-BY-YEAR FIXED EFFECTS

	(1)	(2)
Fines on others 1-12 months ago same state, same industry	-2.638** (0.997)	-2.597** (0.846)
Fines on others 13-24 months ago same state, same industry	-2.215** (0.937)	-1.970* (1.068)
Fines on others 1-12 months ago same state, different industry	-0.761** (0.373)	-0.304 (0.292)
Fines on others 13-24 months ago same state, different industry	1.115 (0.822)	1.426 (0.760)
Fines on others 1-12 months ago different state, same industry	0.843** (0.417)	1.074** (0.526)
Fines on others 13-24 months ago different state, same industry	0.734* (0.484)	1.029** (0.548)
Facility-specific trends	YES	YES
Industry-by-year fixed effects	YES	YES
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), lagged own inspections and fines, 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. 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 since 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. The dependent variable is TSS pollution discharges, expressed as a percent of statutory limits.

Appendix Table 2. 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 same state, same industry	-3.203** (1.115)	-2.407** (0.959)	-3.370** (1.451)	-2.569** (0.948)	-3.305** (1.151)	-2.531** (1.009)	-3.421** (1.494)	-2.823** (1.081)
Fines on others 13-24 months ago same state, same industry	-3.645** (1.231)	-2.288** (1.015)	-3.007** (0.927)	-2.158** (0.886)	-3.674** (1.245)	-2.368** (1.069)	-2.971** (0.950)	-2.304** (0.975)
Fines on others 1-12 months ago same state, different industry	-0.770** (0.310)	-0.951** (0.288)	-0.438* (0.315)	-0.719** (0.404)	-0.827** (0.274)	-0.980** (0.264)	-0.435* (0.304)	-0.754** (0.374)
Fines on others 13-24 months ago same state, different industry	1.134 (0.967)	1.075 (0.921)	1.436 (1.162)	1.169 (0.851)	1.083 (0.942)	1.050 (0.914)	1.407 (1.148)	1.148 (0.833)
Fines on others 1-12 months ago different state, same industry	0.933* (0.652)	1.118* (0.689)	1.206* (0.818)	1.145** (0.607)	0.500* (0.305)	0.622* (0.411)	0.594* (0.407)	0.472** (0.236)
Fines on others 13-24 months ago different state, same industry	0.595 (0.631)	0.699 (0.669)	0.823 (0.706)	0.985 (0.773)	0.375 (0.406)	0.510 (0.510)	0.364 (0.388)	0.604 (0.489)
Facility-specific trends	NO	YES	NO	YES	NO	YES	NO	YES
State-by-year fixed effects	NO	NO	YES	NO	NO	NO	YES	NO
Industry-by-year 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), lagged own inspections and fines, 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 3. 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 same state, same industry	-2.429** (0.836)	-3.360** (1.132)	-2.638* (0.866)	-2.429** (1.143)	-3.360** (1.574)	-2.638* (1.277)	-2.429** (1.079)	-3.360** (1.412)	-2.638* (1.155)
Fines on others 13-24 months ago same state, same industry	-2.311** (0.696)	-2.955** (1.150)	-2.215** (0.745)	-2.311* (1.356)	-2.955* (1.719)	-2.215 (1.370)	-2.311* (1.195)	-2.955* (1.580)	-2.215* (1.176)
Fines on others 1-12 months ago same state, different industry	-0.974 (0.715)	-0.452 (0.309)	-0.761 (0.640)	-0.974 (0.825)	-0.452 (0.488)	-0.761 (0.597)	-0.974 (0.786)	-0.452 (0.447)	-0.761 (0.568)
Fines on others 13-24 months ago same state, different industry	1.032 (1.018)	1.408 (1.053)	1.115 (0.969)	1.032 (0.872)	1.408 (1.214)	1.115 (0.908)	1.032 (0.823)	1.408 (1.116)	1.115 (0.855)
Fines on others 1-12 months ago different state, same industry	0.905* (0.458)	0.859** (0.377)	0.843* (0.507)	0.905* (0.588)	0.859** (0.506)	0.843* (0.574)	0.905* (0.555)	0.859** (0.473)	0.843* (0.528)
Fines on others 13-24 months ago different state, same industry	0.557 (0.651)	0.421 (0.382)	0.734 (0.772)	0.557 (0.581)	0.421 (0.420)	0.734 (0.718)	0.557 (0.543)	0.421 (0.388)	0.734 (0.658)
Facility-specific trends	YES	NO	YES	YES	NO	YES	YES	NO	YES
State-by-year fixed effects	NO	YES	NO	NO	YES	NO	NO	YES	NO
Industry-by-year 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), lagged own inspections and fines, 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 with facility fixed effects but without facility-specific trends are fully consistent, but omitted in the interest of space.

Appendix Table 4. SENSITIVITY TO DEFINING INDUSTRY AT THE 2-DIGIT SIC CODE LEVEL

	(1)	(2)	(3)	(4)
Fines on others 1-12 months ago same state, same industry	-2.822** (0.554)	-2.341** (0.540)	-2.543** (0.778)	-2.581** (0.601)
Fines on others 13-24 months ago same state, same industry	2.246** (0.342)	-1.201** (0.224)	-1.575** (0.123)	-1.313** (0.252)
Fines on others 1-12 months ago same state, different industry	-0.623* (0.420)	-0.715* (0.426)	-0.131 (0.346)	-0.483 (0.486)
Fines on others 13-24 months ago same state, different industry	1.335 (0.937)	1.277 (0.970)	1.597 (1.154)	1.525 (0.934)
Fines on others 1-12 months ago different state, same industry	0.126 (0.158)	0.191 (0.185)	0.307* (0.204)	0.001 (0.196)
Fines on others 13-24 months ago different state, same industry	0.233 (0.241)	0.207 (0.265)	0.232 (0.232)	0.180 (0.331)
Facility-specific trends	NO	YES	NO	YES
State-by-year fixed effects	NO	NO	YES	NO
Industry-by-year 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), lagged own inspections and fines, 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 5. SENSITIVITY TO INCLUDING OR OMITTING SPECIFIC
DETERRENCE MEASURES (with specific deterrence coefficients reported)**

	(1)	(2)	(3)	(4)	(5)	(6)
Fines on others 1-12 months ago same state, same industry	-2.429** (0.947)	-2.420** (0.957)	-3.360** (1.443)	-3.321** (1.401)	-2.638** (0.997)	-2.621** (1.003)
Fines on others 13-24 months ago same state, same industry	-2.311** (1.027)	-2.286** (1.006)	-2.955** (0.888)	-2.904** (0.842)	-2.215** (0.937)	-2.180** (0.918)
Fines on others 1-12 months ago same state, different industry	-0.974** (0.269)	-0.984** (0.275)	-0.452* (0.310)	-0.409* (0.299)	-0.761** (0.373)	-0.776** (0.376)
Fines on others 13-24 months ago same state, different industry	1.032 (0.890)	1.020 (0.873)	1.408 (1.135)	1.471 (1.219)	1.115 (0.822)	1.103 (0.815)
Fines on others 1-12 months ago different state, same industry	0.905* (0.598)	0.901* (0.593)	0.859* (0.556)	0.844* (0.545)	0.843** (0.417)	0.842** (0.415)
Fines on others 13-24 months ago different state, same industry	0.557 (0.497)	0.551 (0.492)	0.421 (0.369)	0.401 (0.360)	0.734* (0.484)	0.734* (0.485)
Dummy for own fine 1-12 months ago	-0.364 (3.153)		-0.539 (2.869)		-0.622 (3.191)	
Dummy for own fine 13-24 months ago	-1.261 (3.170)		-0.851 (2.675)		-1.131 (3.080)	
Dummy for own inspection 1-12 months ago	-1.028 (1.127)		-1.078 (1.010)		-1.069 (1.238)	
Dummy for own inspection 13-24 months ago	-0.797 (0.866)		-1.150 (0.759)		-0.804 (0.958)	
Facility-specific trends	YES	YES	NO	NO	YES	YES
State-by-year fixed effects	NO	NO	YES	YES	NO	NO
Industry-by-year fixed effects	NO	NO	NO	NO	YES	YES
Observations	40,210	40,210	40,210	40,210	40,210	40,210
Number of facilities	415	415	415	415	415	415

NOTES: All specifications include industry-by-month producer price index (PPI), lagged own inspections and fines, 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. Baseline specifications with facility fixed effects but without facility-specific trends are fully consistent, but omitted in the interest of space.