FORTHCOMING IN THE JOURNAL OF ENVIRONMENTAL ECONOMICS AND MANAGEMENT

TITLE: Private Citizen Suits and Public Enforcement: Substitutes or Complements?

RUNNING TITLE: Private and Public Environmental Enforcement

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ABSTRACT: Every major domestic environmental statute provides for citizen suits yet we know little about their implications. This paper's key contribution is systematic micro-level empirical evidence on the extent to which private environmental prosecutions crowd out, or crowd in, public monitoring and public enforcement. We use judicial instruments in an attempt to isolate the causal influence of private enforcement on public enforcement. We find that private citizen suits crowd in public monitoring but significantly crowd out public sanctions.

KEY WORDS: citizen suits, citizen enforcement, enforcement, regulation, pollution policy, crowding out

1. Introduction

Every major environmental policy in the United States provides for citizen suits. Private groups sue government agencies or individual polluters to enforce statutory requirements. Citizen prosecution has been called "perhaps the most pervasive, prominent, and continuing innovation in the modern environmental era" (Thompson [33], pg 185) and the legal literature on the subject is extensive.[1] Private suits also play substantive roles in securities, antitrust, consumer safety, and civil rights policies.

Congress's stated objective for citizen involvement in environmental regulation was to increase public enforcement by bringing attention to instances of noncompliance, lax monitoring, and lenient agency sanctions [34]. Indeed, it is not uncommon to observe incomplete enforcement in practice [13, 26, 27, 28]. Public agencies have limited budgets, respond to local economic conditions [6], and may be sensitive to regulatory capture [21, 31]. Further, the costs of sanctioning some violations may exceed the benefits of doing so.

The conventional wisdom about citizen suits is consistent with Congress's intent. For example, Hodas [15] and Adler [1] assert that private prosecutions will inevitably induce greater public enforcement (crowding in). However, this common wisdom may not be correct in practice. Basic economic intuition and a small theoretical literature suggest that public and private enforcement may be substitutes rather than complements (crowding out). In short, public regulators may deem it imprudent to allocate scarce monitoring and enforcement resources where active private intervention exists.

Despite the attention private environmental enforcement receives in the legal literature, we do not know much about its empirical implications. Most notably, no

systematic revealed evidence on the effects of private enforcement on public regulation yet exists. This study helps fill that gap. We explore the extent to which private citizen participation crowds out, or crowds in, public monitoring and enforcement. Our evidence sheds light on the private enforcement debate and assigns empirical magnitudes to important policy-relevant relationships.

The dearth of empirical studies on the relationship between private and public enforcement stems from at least two challenges. First, data on citizen prosecutions are scarce.[2] This paper uses citizen suit records from the Department of Justice, obtained with a Freedom of Information Act (FOIA) request. We combine this data with Clean Water Act (CWA) public agency enforcement and monitoring actions to construct a comprehensive dataset of public and private enforcement activity in the municipal wastewater industry, by plant and month, between 1990 and 2000. This final dataset allows us to conduct the first far-reaching quantitative analysis of enforcement crowd in/crowd out that includes direct observations on citizen suits.

The second difficulty in this type of analysis is disentangling the causal impacts of private enforcement on public enforcement. However, federal district court characteristics, such as district court judicial temperament and district court caseloads, may be plausible instruments. We construct judicial temperament variables following the political science convention and we obtain caseload data directly from the US federal judiciary. Private citizen prosecutions must go through federal district courts while the public monitoring and enforcement activities in our dataset are conducted at a state or regional administrative level. Thus, federal court characteristics may be relevant for

private enforcement decisions yet may satisfy exclusion restrictions for public enforcement decisions.

We find evidence that private enforcement *crowds in* public monitoring. This is consistent with the conventional wisdom and Congress's intent when authorizing citizen suit provisions in environmental policy. However, we find strong evidence that private enforcement *crowds out* public enforcement actions. Such crowding out stands in marked contrast to the conventional wisdom. Results are robust to several sensitivity checks.

We conclude by interpreting our results in the context of compliance with CWA discharge permits, and we find that the crowd out from penalties swamps any crowd in effect for inspections. On balance, private enforcement actions importantly enhance environmental compliance, but direct deterrence effects are significantly weakened by the net crowding out of public enforcement.

2. Background

2.1 Background: Public Enforcement

The CWA prohibits all point-source water pollutant discharges exceeding permitted levels. Under the Act, public permitting, inspection, and enforcement activities are conducted by a variety of regulatory authorities under the auspices of the National Pollution Discharge Elimination System (NPDES). The vast majority of these authorities are state environmental agencies; the rest are regional EPA offices.[3] Monthly self-monitoring reports are an important source of compliance information, and on-site regulator inspections are intended to ensure the accuracy of these self-reports. Inspections also identify maintenance issues, serve as a source of information for future permitting, and provide a means for gathering evidence to support enforcement actions.

Public enforcement actions range from informal calls, letters, and notices of violation to formal consent decrees and monetary penalties. Here we focus on officially designated formal enforcement actions, which hereafter we refer to as “sanctions”, as well as on the subset of these actions that include monetary penalties. Even for fines and other formal actions, nearly all enforcement activities are conducted through administrative proceedings by the EPA and state agencies [26, 19, 22]. Significant appeals are handled by the Environmental Appeals Board (EAB), an impartial and independent body of three administrative law judges located in Washington, DC. Appeals of EAB rulings or very serious violations requiring especially aggressive responses may be referred to civil and criminal courts, but such actions are extremely rare under the CWA.

Since local regulators have considerable discretion under the law over the existence, type, and severity of enforcement activities, public enforcement varies substantially over both space and time. Many violations are not sanctioned [26, 35], [14]). Some states frequently impose monetary fines for violations, others rarely do so. Even within states and conditional on violations, enforcement varies substantially over time.

2.2 Background: Private Enforcement

Citizen suits are authorized under all major federal environmental laws, including the Clean Air Act, the Clean Water Act, the Superfund Act, the Emergency Planning and Community Right-to-Know Act, the Resource Conservation and Recovery Act, the Endangered Species Act, and the Safe Drinking Water Act. The vast majority of citizen

suits, however, are filed for Clean Water Act violations. For example, Smith [29] reported that 88% of total environmental citizen prosecutions were for CWA violations.[4]

Congress's stated mandate for citizen actions was to spur public enforcement activity and improve environmental quality. A private suit is barred if the EPA or state regulator is "diligently prosecuting" the violator [34, 33].[5] Statutes require citizen plaintiffs to notify the federal EPA, the state authority, and the alleged violator 60 days prior to filing a suit. After this notice-of-intent period expires, the citizen suit is officially filed in a federal district court.[6] Successful suits may require the violator to pay fines to the US treasury or comply with action-based consent decrees. The victorious group may also recoup litigation expenses. This can substantially increase the defendant's costs, because reimbursements are based on market rates for private attorneys rather than the rates charged by the public-interest lawyers typically used by citizen plaintiffs.

While any "person or persons having an interest which is or may be adversely affected" (33 U.S.C. § 1365(a),(g)) may file a private suit, prosecutorial success has often been a function of legal standing. Judicial interpretation has varied over time, but typically standing has only required injuries to aesthetic and environmental interests defined broadly [29]. Since 1990, most private enforcement actions are brought by local groups or local chapters of large organizations, such as Baykeepers or Riverkeepers. Key determinants for initiating private suits include the probability of success in a given court, the expected cost of litigation, and the potential value of winning to the organization itself, such as increased visibility or a boost in membership and donations, as well as reimbursement of litigation costs [19].

Citizen suits are relatively common. Naysnerski and Tietenberg [19] reported over 1,200 citizen enforcement cases between 1978 and 1987. Smith [29] identified 287 cases brought by private plaintiffs between 1995 and 2000.

Costs of private enforcement actions to sued facilities are often large. Even in cases settled out of court, legal fees, settlement terms, and consent decree remediation are significant. In cases that do go to court, supplemental costs often swamp direct penalties, which may be significant themselves [29]. For example, DOJ data indicate that direct fines in *American Canoe Association v. Greensboro (NC) North Buffalo Waste Water* were only $3,000. However, settlement terms included $129,000 in settlement costs for local conservation projects and plaintiff legal fees. Fines in *American Canoe Association v. City of Wilson (NC) Wastewater Treatment* were approximately $135,000. These fines, however, do not include required expenditures on new effluent filters, treatment modifications, and plant expansion. In *Waste Action Project v. City of Anacortes (CA)*, the settlement required the defendant to pay approximately $100,000 to non-party organizations for water monitoring and environmental projects.

3. Literature

We provide empirical evidence on the crowding out or crowding in of public monitoring and enforcement from private enforcement. In related theoretical research, Heyes and Rickman [14] predicted crowding out in the absence of regulatory dealing and crowding in when regulatory dealing is present. Langpap [16] analytically predicted that private litigation may either crowd in or crowd out public monitoring and sanctions.

The observational literature on enforcement crowd in or crowd out is sparse. Naysnerski and Tietenberg [19] explored the evolution of private enforcement, the

incentives faced by private litigators, and the potential implications of those incentives. The authors noted a negative correlation between public and private environmental enforcement actions and interpreted the correlation as evidence that private litigation occurs when public enforcement is lax. However, as the authors noted, the paper was unable to isolate causality due to highly aggregated time series data.

More broadly, this paper also relates to Earnhart [7] and Ashenmiller and Norman [4]. Earnhart [7] examined legally mandated public responses to citizen complaints in the Czech Republic. The paper primarily considered relationships between different public enforcement instruments and did not attempt to understand crowd out behavior. Ashenmiller and Norman [4] examined the impact of state-level policy changes regarding strategic lawsuits against public participation. The authors found that the passage of anti-strategic lawsuit legislation was correlated with increased public monitoring and public enforcement. The analysis used no data on private enforcement actions or lawsuits.

4. Conceptual Framework

In this section, we present a minimal conceptual framework for empirically analyzing the impacts of private enforcement on public monitoring and enforcement. The framework shares features of the more formal models in Naysnerski and Tietenberg [19] and Payne [20]. However, our context differs by emphasizing potential interactions between private enforcement actions and public monitoring and enforcement actions.

An environmental group and a public regulator influence environmental quality outcomes. The regulator moves first, by deciding whether or not to inspect a facility or sanction any given violation. When making decisions, the public regulator takes into account its beliefs about the expected level of private enforcement. If the regulator does

not pursue a violation, environmental statutes allow a private group to notify the public regulator of its intent to sue. After a 60 day waiting period, the private group may commence prosecution.

The private environmental group takes public monitoring and enforcement strategies as given and chooses the level of private enforcement to maximize its expected benefits from environmental quality, subject to a budget constraint. The resulting private enforcement decision depends on factors that determine the probability of success in court and litigation costs. Private enforcement levels also depend on endogenous public monitoring and enforcement levels.

We assume that the public regulator oversees a given sector by minimizing costs subject to an environmental quality goal for that sector.[7] The resulting regulatory activity decision depends on public monitoring and enforcement costs, public relations costs, environmental quality targets, and expectations about endogenous private enforcement levels. Note that monitoring and enforcement costs (expenses) increase with regulator activity. Public relations costs, such as Congressional oversight and negative publicity, decrease with regulator activity but increase with private enforcement activity since citizen suits call attention to lax public monitoring and sanctioning.

This simple framework suggests that private enforcement activity may crowd in or crowd out public enforcement and/or public monitoring. A natural economic hypothesis is the crowding out of public sanctions and fines. Public penalties and private enforcement are likely to be close substitutes, since they attempt to achieve the same goals. Here, public enforcement costs create incentives for private citizen suits to crowd out regulator enforcement. The intuition is that private enforcement makes it easier for

public regulators to achieve environmental quality goals within a sector without incurring their own enforcement expenses. An alternative hypothesis is the crowding in of public sanctions and fines. If private enforcement dramatically increases a regulator's public relations costs by calling significant attention to lax public oversight, incentives for crowd in may swamp the free-riding incentives for crowd out described above. The extent to which citizen suits crowd in or crowd out public fines and sanctions is an empirical question.

A second natural hypothesis stemming from the simple framework above is the crowding in of public inspections. Public inspections and private enforcement are inherently complementary, since public inspections provide evidence to support both public and private enforcement. The regulator therefore faces greater incentives to monitor when private enforcement is likely. The intuition is that private enforcement makes each inspection dollar go farther towards achieving environmental quality goals. Additionally, because public inspections and private enforcement are complements rather than substitutes, free-riding incentives for crowding out are less likely for public monitoring. An alternative hypothesis is that private enforcement crowds out public monitoring through a general crowd out of all public regulatory oversight. The extent to which citizen suits crowd in or crowd out public inspections is an empirical question.

A final implication of our conceptual framework is that private enforcement and public monitoring and enforcement are jointly determined. However, disentangling causality may be possible by identifying factors that affect private enforcement decisions but do not directly influence public regulatory choices. Therefore, appropriate

instruments might include the factors that influence citizen groups' perceived lawsuit success or private enforcement costs.

5. Data

5.1 Data: Public and Private Enforcement Actions

We analyze citizen suit litigation data from the US Department of Justice. We merge this data with public enforcement data from the EPA's Permit Compliance System. Our sample contains data on Clean Water Act enforcement, since the vast majority of private citizen suits are filed for water violations.

Our unique data on private enforcement were obtained through a Freedom of Information Act request from the U.S. Department of Justice. Citizen suit provisions in federal environmental laws require plaintiffs to submit a copy of the complaint to the Department of Justice upon filing. We therefore obtained DOJ data on all citizen prosecutions formally filed under the Clean Water Act. Details include the names of the parties to the suit, the date the suit was filed, and the relevant district court.

We also obtained public enforcement data from the EPA's Permit Compliance System (PCS) database, which documents information associated with the National Pollution Discharge Elimination System (NPDES). The PCS provides plant-level inspection dates and types, plant-specific formal enforcement action dates and types, and pollution-specific discharges. Monetary sanction data include fine amounts.

5.2 Data: Sample

Our sample covers 1990-2000 and includes all "major" municipal wastewater treatment plants that report conventional water pollution discharges to the regulator in the fifteen states with citizen suits in the municipal wastewater industry.[8] While citizen suits

occur in many industries, we examine one industry for transparency and empirical tractability. We focus specifically on wastewater facilities since they represent more than two-thirds of major regulated entities under the CWA. Further, wastewater treatment plants face far more citizen suits than any other industry. No other industry had more than a handful of private prosecutions for water pollution violations over our sample period. Finally, it is straightforward to link DOJ citizen suit data to wastewater treatment plants; it is difficult to do so for other industries. Other industries often have facilities with multiple names, both at a given point in time and over time. Legal venue varies for cases involving other industries because parent companies and multiple defendants often complicate venue considerations. In practice, venue for citizen suits with wastewater treatment plant defendants is the federal district court where the plant is located.

We link public and private enforcement data, by month, by matching the names and locations of the parties to the citizen prosecution to the names and locations of municipal wastewater treatment facilities in the public enforcement database. DOJ records provided insufficient information to credibly link eleven citizen suits to the public enforcement data. We therefore omit these prosecutions from the analysis. Since the analysis includes one year lags, the final sample for the regression analysis contains 1,494 plants over the 120 months spanning 1991-2000.

5.3 Data: Summary Statistics

Table 1 displays descriptive statistics on public and private enforcement. On average, a municipal wastewater treatment plant in our sample was inspected 1.5 times per year, sanctioned 0.5 times per year, fined 0.04 times per year, and sued by a private group 0.003 times per year. Frequent inspections, common sanctions, infrequent fines,

and rare private enforcement actions are expected. Public inspection costs are low relative to the cost of levying administrative fines and very low relative to private citizen suit prosecutions in federal courts. Further, as discussed in the background section, the expected deterrent effects of public inspections, sanctions, and administrative fines are likely lower than the deterrent effects of private enforcement actions. The legal fees, settlement terms, and consent degree remediation costs associated with private case resolutions are high [29].

The average public administrative fine was approximately $11,000 and the median fine was $2,000. Relatively modest regulatory actions are consistent with the broader literature on public enforcement [13, 18, 27, 28]. Also, modest pecuniary penalties only partially reflect true deterrent effects, as political scrutiny, local media coverage, and consent decree terms are frequently significant. Sanctions may also signal the potential for steeply escalating future penalties.

Inspections and total sanctions declined over time, although non-monotonically. In contrast, the number of monetary fines and citizen suits generally increased over the sample period. There were 3,005 inspections of our 1,494 plants in 1990 and 1,705 inspections in 1996. Only 19 administrative fines were levied against major wastewater treatment facilities in our fifteen sample states in 1996, but 177 fines were imposed in 2000. Only one citizen suit was filed in 1990, but 19 suits were filed in 1996.

6. Analysis

6.1 Analysis: Variables

Our primary goal is to examine the relationship between private enforcement and public regulatory activities, and we estimate the effect of the probability of citizen suits

on government monitoring and enforcement choices. Regulatory behavior is the key outcome in our analysis. As such, the dependent variable in each of our regressions is the presence or extent of public monitoring or enforcement actions. The dependent variable may be a dummy variable set equal to one if plant *i* is inspected in period *t*. Alternatively, the dependent variable may be a dummy equal to one if plant *i* received a formal sanction in period *t*, a dummy equal to one if plant *i* received an administrative monetary penalty in period *t*, or the logarithm of plant *i*'s administrative fines in period *t*.

The key explanatory variable in each analysis represents the regulator's perceived or predicted probability of the likelihood of a citizen suit directed towards plant *i* in period *t*.[9] Perceptions, of course, are unobserved and unobservable, so our primary specifications econometrically predict citizen suit probabilities.

We exploit the panel structure of our data by using state-specific fixed effects to capture unobservable cross-state differences and several nearly constant public enforcement determinants that have been identified by the previous literature. These latter covariates include average agency budgets, average regulatory stringency, community political and environmental attributes, and average economic conditions.[10]

Other explanatory variables include year fixed effects, season fixed effects, recent state-specific regulator activity, and lagged violations. We include year dummies because budgets and enforcement priorities may vary systematically across time. Quarterly dummies account for the effect of seasonal weather on monitoring and enforcement costs and permitted effluent levels. We include public monitoring and enforcement activity directed towards other plants in *i*'s state in the last 12 months as a proxy for recent regulatory stringency. We exclude plant *i* to minimize endogeneity concerns. Finally, we

include the total number of violations incurred by plant *i* in the previous 12 months. This variable allows us to examine the impact of private enforcement actions on public monitoring and enforcement, holding recent non-compliance constant. Therefore, we estimate a ceteris paribus substitute/complement relationship. It is possible that this variable is endogenous, so we also present results for models that do not include it.[11]

6.2 Analysis: Empirical Model

For public monitoring or enforcement activities *y* directed towards plant *i* in month *t*, our most basic regression model is:

$$y_{it} = \alpha_0 + X_{it}\Gamma + \beta CS_{it} + \varepsilon_{it}, \quad (1)$$

where α_0 is a constant, *CS* represents the probability of a private enforcement action directed at *i* in period *t*, and ***X*** represents a vector of additional explanatory variables discussed above.

Disentangling the causal connections between private and public enforcement, however, is difficult. A possible concern when estimating (1) is the endogeneity of private enforcement, i.e. $Cov(CS_{it}, \varepsilon_{it}) \neq 0$ due to simultaneity or omitted variable bias. OLS coefficients may be biased in a negative direction because there may be fewer private enforcement actions when public monitoring and enforcement is frequent. Or, OLS coefficients may be biased in a positive direction because unobserved factors like local political or economic conditions drive both higher private and public enforcement.

The standard approach to control for such endogeneity is instrumental variables estimation. We use federal district court characteristics such as judicial temperament and judicial caseload as instruments Z_{it}. Citizen prosecutions must go through federal district courts, and private litigators are less likely to bring a suit against a plant if they perceive

that the district court where the suit would be tried might be particularly hostile to their case. Therefore, judicial temperament may influence the private environmental group's perceived victory probabilities. Further, a private group is less likely to pursue a prosecution if they believe that the lawsuit, and thus their limited litigation resources, may be tied up in the district court for a long time. Therefore, judicial caseload may influence perceived private litigation costs. Naysnerski and Tietenberg [19] qualitatively discuss private litigators' cost-benefit considerations in more detail, and we confirmed these conjectures through personal conversations with attorneys directly involved in citizen suit litigation.

With instruments Z_{it}, our basic regression model becomes:

$$\begin{aligned} CS_{it} &= \delta_0 + X_{it}\Pi + Z_{it}\Theta + \eta_{it} \\ y_{it} &= \kappa_0 + X_{it}\Omega + \lambda CS_{it} + \mu_{it}. \end{aligned} \tag{2}$$

When the dependent variable y_{it} is continuous, like the logarithm of plant i's fines in period t, we follow Angrist's [2] framework for a discrete endogenous regressor and estimate (2) using a fully linear two-stage least squares instrumental variables approach. When the dependent variable y_{it} is discrete, like the existence of a fine, sanction, or inspection in period t, we take two approaches. First, again following [2], we estimate an instrumental variable linear probability model using two-stage least squares. Second, we estimate the standard simultaneous bivariate probit model:

$$\begin{aligned} CS_{it}^{*} &= \delta_0 + X_{it}\Pi + Z_{it}\Theta + \eta_{it} \\ y_{it}^{*} &= \kappa_0 + X_{it}\Omega + \lambda CS_{it} + \mu_{it}, \end{aligned} \tag{3}$$

where $CS_{it} = 1$ if latent variable $CS_{it}^{*} > 0$; $CS_{it} = 0$ otherwise. $y_{it} = 1$ if latent variable $y_{it}^{*} > 0$; $y_{it} = 0$ otherwise.

6.3 Analysis: Constructing District Court Instrumental Variables

Our first instrument measures the judicial preferences of district court judges in a given district and year. We follow the judicial temperament literature to construct the instrument, and we use a well studied method developed by political scientists Giles, Hettinger, and Peppers [11]. Here, each district court judge is assigned a political ideology score based upon the ideology score of the appointing US President and the ideology scores of the US senators from the state in which the court is located. Ideology scores of appointing politicians have been consistently linked to federal judges' behavior [17, 23]. When the appointing president's political party affiliation differs from both US senators from the state in which the court is located, a district court judge's GHP ideology score is simply the appointing president's score. When the appointing president's political party affiliation is the same as one or more US senators from the state in which the court is located, the judge's GHP score is the score of the senator from the same party as the president (or the average of the senators' scores when both share the same party affiliation as the president). This method recognizes the important role of senators in the judicial appointment process when senatorial courtesy is present.[12]

The political ideology scores underlying our GHP metrics follow Poole and Rosenthal [25] (henceforth "PR") and Poole [24]. PR argue that congressional voting can be generally conceptualized in terms of a single ideological dimension, which they measure using a score scaled from -1 for most liberal to +1 for most conservative. These scores place the presidents and senators on a metric that is common across time and institutions, and hence are known as "common space scores." To construct our specific instrument, we first calculate the GHP score for each federal district court judge in each sample state using PR scores. Then, we compute the mean score for all the district judges

in the state where plant *i* is located for each of our sample years. More conservative court/year combinations have higher GHP scores and more liberal court/year combinations have lower GHP scores. *A priori*, we expect GHP scores to have a negative effect on the probability of private enforcement.

Judicial temperament scores should represent, or at least be highly correlated with, the idiosyncratic preferences of the judges in a district court in any time period. Scores are based upon political factors at the time of appointment, and judges are federally appointed for life. Note, however, that *our temperament instrument does not reflect overall state-level political preferences over the period of analysis*. Given that instrumental variable regressions contain state-level fixed effects, the proper interpretation of our instrument is judicial temperament relative to average state-level political leanings.

Our second instrument measures the average caseload per judge in a given district court in a given time period. Since slow moving or heavily burdened courts increase the plaintiff's opportunity costs, caseload per judge should be negatively correlated with private enforcement actions.[13] The specific instrument is constructed using caseload profile data from the Administrative Office of the US Courts. A priori, we expect caseload to have a negative effect on the probability of private enforcement.

7. Results

7.1 Results: Determinants of Citizen Suits Regressions

First stage regression results for 2SLS linear probability and bivariate probit specifications are presented in Table 2.[14] These regressions explore the relationship between the instruments and the probability of a citizen suit. Standard errors are in

parentheses. Presented F-statistics and Wald Chi-squared statistics indicate a good fit for all specifications. Predicted probabilities of citizen suits are approximately four times greater for observations with citizen suits than for observations without citizen suits.

The estimated coefficients indicate that our instruments influence the likelihood that a citizen group will pursue a given violation in the expected manner. Coefficients for GHP scores are consistently negative and significant across all specifications, indicating that citizen suits are less likely when district court judges are particularly conservative. Coefficients for court caseload, although statistically significant only for the instrumental variables model, are negative as well. This suggests that the predicted probability of a private prosecution decreases as the judicial caseload increases.

Stock-Yogo [30] F-statistics on excluded instruments in the two stage least squares regressions are 4.5 and 4.6, depending on whether violations are included or excluded. These tests statistics suggest that we may have weak instruments. Any bias from weak instruments, however, is towards zero in our regressions, so all of our reported results are conservative understatements of true effects. Further discussion and robustness tests are discussed in detail in Section 8.

7.2 Results: The Influence of Private Suit Probabilities on Public Regulation

Key second stage regression results for 2SLS linear probability and bivariate probit specifications are presented in Table 3. These regressions explore the relationship between the predicted probability of a private citizen suit and public monitoring/enforcement activity. Standard errors appear in parentheses below estimated coefficients.

Note that throughout Table 3 we report statistics for Sargan over-identification tests of exclusion restrictions in the 2SLS IV specifications. Based on the Sargan statistics, we fail to reject the null hypothesis of uncorrelated residuals and instruments at a 1% confidence level for the fine existence and fine magnitude specifications, and at a 5% confidence level for the inspections specification. While over identification tests are inherently imperfect, we do fail to reject a null of satisfactory exclusion restrictions for the majority of our key results. We do not, however, reject imperfect exclusion restrictions for the sanctions model. Implications and robustness tests are discussed in Section 8.[15]

7.21 Results: Private Suits and Public Monitoring - Substitutes or Complements?

Columns (1) – (3) of Table 3 present results for the relationship between private enforcement and public monitoring. Columns (1) and (2) show results for 2SLS IV linear probability models and column (3) presents results for a bivariate probit specification. The results in columns (1) – (3) provide suggestive but not definitive evidence for the crowding in of inspections. Private enforcement effect magnitudes are large, consistent, and statistically significant across 2SLS specifications, but we find statistically insignificant effects for the bivariate probit specification.

Interpreting the coefficient from our preferred specification in column 1, the linear probability instrumental variable regression results indicate that the probability of a regulatory inspection at a given plant increases by approximately 33 percentage points (0.3327) in response to a one percentage point (0.01) increase in the probability of a private citizen prosecution at that same plant, even after conditioning on violations. A one percentage point increase in the probability of relatively rare citizen suits is large. To

put our results in a more practical context, we run a simple experiment. Since empirically predicted probabilities of citizen suits are approximately four times greater for observations with citizen suits than for observations without citizen suits, we consider the marginal effects of a three-fold increase in the probability of a private citizen suit prosecution. This marginal effect is equivalent to approximately one additional citizen suit per state per year.[16] IV results indicate that the probability of a regulatory inspection at a given plant increases by approximately 25 percent in response to a three-fold increase in the probability of a private citizen suit at that same plant.[17]

In all specifications, estimated coefficients for other variables generally reveal expected results. Plants are more likely to receive a monitoring action if they recently violated limits. Similarly, when the number of recent monitoring actions in a state is particularly high, the probability of an inspection at any given facility increases. Seasonality, time trends, and time invariant state characteristics are also important predictors of inspection probabilities.

7.22 Results: Private Suits and Public Sanctions - Substitutes or Complements?

Columns (4) – (6) of Table 3 present results for the relationship between private enforcement and public sanctions. Columns (4) and (5) show results for 2SLS IV linear probability models and column (6) presents results for a bivariate probit specification. The results in columns (4) – (6) provide consistent evidence for the crowding out of public sanctions. Private enforcement effect magnitudes are large in magnitude and statistically significant across 2SLS and bivariate probit specifications.

Interpreting the coefficients from our preferred specification in column 4, the linear probability instrumental variable regressions results indicate that the probability of

a regulatory sanction at a given plant decreases by approximately 14 percentage points (0.1383) when the probability of a private citizen prosecution at that same plant increases by one percentage point (0.01), even after conditioning on violations. The linear probability regression results imply that the probability of a regulatory sanction at a given plant decreases by approximately 28 percent in response to a three-fold increase in the probability of a private citizen suit at that same plant.[18]

7.23 Results: Private Suits and Public Fines - Substitutes or Complements?

Columns (7) – (9) of Table 3 present results for the relationship between private enforcement and the existence of public fines. Columns (7) and (8) show results for 2SLS IV linear probability models and column (9) presents results for a bivariate probit specification. The results in columns (7) – (9) provide consistent evidence for the crowding out of public fines. Private enforcement effect magnitudes are large in magnitude and statistically significant across 2SLS specifications and bivariate probit specifications.

Interpreting the coefficients from our preferred specification in column 7, the linear probability instrumental variable regressions results indicate that the probability of a regulatory fine at a given plant decreases by approximately 3.4 percentage points (0.0335) when the probability of a private citizen prosecution at that same plant increases by one percentage point (0.01), even after conditioning on violations. The linear probability regression results imply that the probability of a regulatory fine at a given plant decreases by as much as 86 percent in response to a three-fold increase in the probability of a private citizen suit at that same plant.[19]

Columns (10) - (11) in Table 3 present results for the relationship between private enforcement and the magnitude of public fines. We once again find strong evidence that private citizen prosecutions crowd out total fines. Interpreting the coefficients of the preferred specification in column 10, the linear probability instrumental variable regression results indicate that the total predicted fine magnitude at a given plant in a given month decreases by approximately 29.5 percent (0.2946) when the probability of a private citizen prosecution at that same plant increases by one percentage point (0.01), even after conditioning on violations. In practical terms, linear probability results indicate that the total public fine magnitude at a given plant decreases by approximately 2.7 percent in response to a three-fold increase in the probability of a private citizen suit at that same plant.[20]

8. Sensitivity

8.1 Sensitivity: Weak Instruments

As we note above, test statistics suggest we may have weak instruments. However, any second stage bias from weak instruments is towards OLS estimates, which in our case is always towards zero. If weak instrument bias is important in our context, all of our key results are understated.

Nevertheless, we believe the magnitude of such bias may be small on average for three reasons. First, we repeated the analysis with a limited information maximum likelihood (LIML) approach. LIML estimators are asymptotically equivalent to 2SLS, but less biased in finite samples with weak instruments [3]. Results from this sensitivity experiment were similar in sign, magnitude, and significance to standard results. Second, we repeated the analysis with a just-identified IV approach using judicial temperament as

the sole instrument. This method is median-unbiased and consequently less susceptible to weak instrument concerns [3]. Results from this sensitivity experiment were similar in sign, magnitude, and significance to standard results. Third, we repeated the analysis with reduced form regressions of public actions on the excluded instruments and the other explanatory variables. Reduced form OLS results are unbiased, and estimated coefficients on the excluded instruments Z are proportional to the behavioral relationships of interest [3, 5]. This last sensitivity experiment supported all of our causal interpretations; reduced form coefficients were jointly statistically significant, with the expected signs. Results for all sensitivity experiments related to the strength of our instruments are presented in Table 4.

8.2 Sensitivity: Potential Violations of Exclusion Restrictions

It is possible for EPA enforcement cases to end up in civil district courts. Appeals of Environmental Appeals Board (EAB) rulings or extremely serious violations requiring especially aggressive responses may be referred to civil courts. Consequently, it is possible that district court characteristics might directly influence public enforcement choices. In this case, residuals from our public enforcement equations and our instruments may be correlated and our exclusion restrictions may not be met exactly.

Quantifying and even signing the bias from imperfectly satisfied exclusion restrictions is difficult in our context. However, we believe the magnitude of such bias may be small on average. As noted in Table Section 7.23, Sargan's over-identification tests fail to reject a null hypothesis of uncorrelated residuals and instruments for our fine existence and fine magnitude specifications at all standard levels of significance. At a 5% significance level, we also fail to reject for the inspections specification. Moreover, our

results for fines and sanctions are broadly robust to bivariate probit specifications which do not require excluded instruments for identification. In addition, states typically implement CWA enforcement. Administrative penalties are strongly prioritized over civil and criminal referrals and hence such referrals are extremely rare in CWA settings. We also repeated our analysis including an additional variable representing EPA court victory percentages.[21] With this control, the influence of court characteristics on public actions via EPA beliefs about their own victory probabilities in public courts should be removed from the residual of the equation and our exclusion restrictions should be more completely met. Results were very close to presented results. Finally, we repeated the analysis with an additional instrument unrelated to court characteristics: private citizen suits levied against other industries in the state.[22] Second stage results with the new instrument set are qualitatively similar to presented results for all fine and inspection specifications. Results for the sensitivity experiments related to our exclusion restrictions are presented in Table 5.

8.3 Sensitivity: Robustness to Specification

Results are robust to alternative specifications of temporal change. Our analyses combine state-specific fixed effects with overall year dummies. Replacing year dummies with linear time trends does not importantly alter the results. Signs and significance are similar, but magnitudes are typically larger with the less flexible linear time trends. Key point estimates are also robust to the inclusion of state-specific time trends, although statistical significance is typically stronger in presented results.

Results are robust to alternative control variable specifications. Our analyses use state-level fixed effects since the regulator is the decision maker. However, including

plant-level fixed effects in place of state-level fixed effects yields results similar in sign, magnitude, and significance to the presented results. Our analyses condition on past conventional water pollution violations so that estimates more closely reflect the ceteris paribus effect of private enforcement on public decisions, holding non-compliance constant. However, as shown in Table 3, including or omitting recent violation variables does not significantly change results.

9. Discussion

To place our empirical findings in a compliance context, we consider the impacts of public and private enforcement on CWA discharge permit violations at the plant level. To do so, we follow the related public enforcement literature and regress conventional water pollution violations on lagged enforcement actions [18, 27, 28]. Plants face an uncertain regulatory environment, so they assess the threat of private and public enforcement based upon the recent behavior of private and public enforcement organizations. Recent actions directed at any plant in a state impact every plant's perceived threat of actions in that state.

For the interpretation exercise, our dependent variable is a dummy indicating a biochemical oxygen demand average concentration violation for plant *i* in month *t*.[23] Our key explanatory variables are the number of private prosecutions levied against municipal wastewater treatment facilities in plant *i*'s state during the 12 months prior to *t*, the number of public fines levied against municipal wastewater treatment facilities in plant *i*'s state during the 12 months prior to *t*, and the number of public inspections at municipal wastewater treatment facilities in plant *i*'s state during the 12 months prior to *t*. Other explanatory variables include seasonality terms and year dummies.

Our dependent variable is discrete and approximately 28 percent of self-reported pollution observations are missing, so we run a probit model with sample selection on observables. To minimize concerns about potential bias caused by endogeneity of private prosecutions and public inspections and fines, we use Chamberlain's conditional random effects (CRE) probit model. Plant-level CREs achieve the same intuitive outcome as plant-level fixed effects (which are not consistent in probit models) by conditioning on the sample averages of the variables of most theoretical relevance [27]. Here we condition on plant-level averages of citizen prosecutions, public fines, public inspections, and violations; the subsequent random effects specification is consistent even if private or public regulators are particularly likely to target facilities based upon average enforcement and compliance history.

Interpretation exercise results are presented in Table 6. Bootstrapped standard errors appear in parentheses.[24] Results indicate that the estimated impact of private enforcement actions, public fines, and public inspections are all negative and strongly significant. These direct deterrence effects are also practically large, especially for the marginal enforcement actions. Marginal effects implied by Table 6 are -0.00592, -0.00061, and -0.00002 for private citizen suits, public fines, and public inspections, respectively. The background violation probability is 0.038. Therefore, the direct deterrence effects of private and public activity on subsequent violations can be easily calculated. Violations fall by approximately 15.6 percent following the marginal private citizen suit, 1.61 percent following the marginal public fine, and 0.06 percent following the marginal public inspection.

Indirect deterrence effects from the crowding out and crowding in demonstrated

in this paper's main analysis are also significant. Recall that one additional citizen suit per state per year is approximately equivalent to a three-fold increase in the probability of a citizen suit. Recall also that our key results in Table 3 indicate that a three-fold increase in the probability of a citizen suit is associated with an 86 percent decrease in fines and a 25 percent increase in inspections. The average number of fines per state per year is 4.2 and the average number of inspections per state per year is 145. One additional citizen suit per year would have an indirect deterrence effect through fines of $-0.86 \times 4.2 \times -0.00061 = 0.0022$. Given a background violation probability of 0.038, aggregate violations in a state rise by approximately 5.8 percent for each observed private enforcement action in the year following the suit due to the *crowding out of fines*. One additional citizen suit per year would have an indirect deterrence effect through inspections of $0.25 \times 145 \times -0.00002 = -0.00073$. Aggregate violations in a state fall by approximately 1.9 percent for each observed private enforcement action in the year following the suit due to the *crowding in of inspections*. On net, the indirect effect of crowding in and crowding out is a 3.9 percent increase in aggregate violations or a 25 percent decrease in the direct deterrence effects of private enforcement actions.

We note several caveats to the interpretation exercise. First, we only observe discharges for one conventional pollutant, BOD. Second, we do not observe discharges for approximately 30 percent of plant month observations. While we attempt to correct for missing data with a Heckman procedure, we are unable to obtain data on several variables that may influence selection. Third, for tractability we explore only a subset of the full slate of monitoring and enforcement instruments. Fourth, it is possible that enforcement targeting may bias the results of the interpretation exercise. Included plant-

level conditional random effects should mitigate bias if facilities are targeted based upon their average environmental behavior, but time variant targeting may bias results. Such bias from reverse causality due to targeting, however, is towards zero.

We also note two caveats for external validity of the broader empirical exercise. First, due to data limitations, we are unable to reliably and systematically match private and public enforcement data for industries beyond the wastewater treatment sector. External validity of our results therefore requires that public regulators systematically respond similarly to conditions in the wastewater treatment industry and to conditions in other industries. We have no reason a priori to doubt this assumption, and Earnhart's [8,9] results for both regulator behavior and facility polluting behavior in the US wastewater treatment industry were consistent with regulator behavior and facility polluting behavior in the other major US industries explored in the public enforcement literature. Second, our analysis focuses on water pollution, since most citizen suits target this medium. The extent to which our results apply to air, toxics, and other environmental settings remains an interesting direction for future research.

10. Concluding Remarks

Our analysis permits the first comprehensive micro-level quantitative assessment of the effects of private enforcement. We find that private enforcement actions have substantial direct deterrence effects on environmental compliance. Our key result, however, indicates that these direct deterrence effects are weakened by the net crowd out of public enforcement. Our evidence suggests that citizen involvement does not increase public enforcement by bringing attention to instances of noncompliance and lax enforcement, as Congress and the legal literature regularly assume. To the contrary, our

results indicate that citizen involvement decreases overall public enforcement. On net, the deterrence effects of private enforcement are approximately 25 percent lower due to indirect crowding out effects.

Acknowledgements

We thank the Department of Justice for timely compliance with our citizen suits FOIA request and the EPA for providing PCS data. We are extremely grateful to Michael Giles and Josh Fischman for judicial temperament data and assistance. A journal co-editor, two anonymous referees, Brett Baden, Tim Beatty, Wayne Gray, Wolfram Schlenker, Mike Ward, and numerous seminar participants provided helpful comments. Shimshack thanks the Erb Institute at the University of Michigan for space and support.

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Footnotes

1. Notable legal studies of citizen suits include [32, 31, 15, 33, 1, 29].

2. To our knowledge, only a handful of other studies contain comprehensive data on actual citizen suits. Environmental Law Institute [10], Greve [12], and Smith [29] used such data to explore the scale of citizen involvement, the characteristics of citizen prosecutors, and the characteristics of defendants. These studies did not assess the empirical consequences of the suits themselves.

3. States have the option to oversee compliance. EPA regional offices step in for states which decline this option.

4. The preponderance of CWA suits is due to the fact that extensive self-reporting under the statute allows private groups to readily assess a specific facility's CWA compliance.

5. For Clean Water Act cases, state or federal administrative actions are sufficient for "diligent prosecution" but other enforcement actions, such as notices of violation or memoranda of understanding, are not. An agency decision not to prosecute, no matter how well founded, will not bar a citizen suit [22].

6. U.S. District Courts are the trial courts (courts of original jurisdiction) for the federal court system. There are ninety-four federal judicial districts, including at least one district in each state, the District of Columbia and Puerto Rico. Districts do not cross state lines. Most federal environmental law cases are heard in federal district courts.

7. Regulators have many industries and pollutants to inspect and sanction, and states do not typically have fixed budgets for water enforcement and monitoring within a sector. Our data show significant variation in monitoring and enforcement levels across both time and space. The enforcement cost minimization assumption is plausible in practice for a given sector, and other sector-specific objective functions motivate similar empirical specifications.

8. Plants are classified as “major” by the EPA if their discharge is at least 1 million gallons per day, they cause a significant impact on the receiving water body, or they serve a population of at least 10,000. Major plants are the source of most water pollution discharges. Our 15 states are AL, AR, AZ, CA, CT, GA, IL, MS, NC, NY, OR, PA, TX, WA, and WV. Results are generally robust to including states with private enforcement activity in other industries but no citizen suits in the wastewater treatment industry.

9. It would be interesting to also examine the expected penalties from citizen suits. However, we are unable to do so due to data limitations on court-imposed penalties. Our current approach captures the baseline effect of an increased probability of the *average* private enforcement action on regulatory behavior.

10. Plant-level fixed effects are another way to exploit the panel structure of the data, but state-level fixed effects seem more appropriate a priori since the regulator is the decision maker. As discussed in a later sensitivity section, results are robust to including plant-level fixed effects.

11. The lagged violation variable could be endogenous if plants with more violations on average may be targeted for greater public and private enforcement. However, dropping this variable does not meaningfully affect our results. Further, results are robust to plant-level fixed effects models that are robust to this type of correlation.

12. Giles, Hettinger, and Peppers [11] subject their measure to convergent and construction validation and find that it is highly correlated to the party of the appointing president, a conventional political science measure of judicial temperament. Further, GHP scores are significantly related to judges’ rulings and more completely explain judicial ruling than the simple presidential party metric.

13. CWA citizen suits represent an extremely small portion of federal courts' caseloads, so we believe caseloads are unlikely to be meaningfully endogenous.

14. Since bivariate probit regressions are simultaneous, the first and second stage labels are artificial. Labels correspond to those that would be used in the 2SLS context.

15. OLS coefficients are typically small and sometimes of the opposite sign of the instrumental variables and bivariate probit results. However, as discussed above, private citizen suit enforcement is very likely endogenous and there are strong reasons to suspect significantly biased OLS results. Durbin chi-2 tests and Wu-Hausman F tests reject the exogeneity of private enforcement at the 1 percent level for all regressions.

16. There are approximately 0.33 citizen suits per state per year, so a 3 fold increase in the probability of a citizen suit in a state can be interpreted as approximately one additional citizen suit per state per year.

17. On average, a three-fold increase in the probability of a citizen suit implies increasing the probability of a citizen suit at plant *i* in period *t* from a 0.0003 background probability to 0.0012. The 0.0009 increase multiplied by the IV linear probability coefficient of 33.27 yields a 0.0299 increase in the probability of an inspection. This is an approximately 25% increase over the background inspection probability of 0.12.

18. The change in the probability of a citizen suit translates into a 0.0009 increase. Multiplying this increase by the IV linear probability coefficient of -13.83 yields a 0.0124 decrease in the probability of a sanction, or approximately a 28% decrease over the background sanction probability of 0.044.

19. The IV linear probability coefficient of -3.35 multiplied by the 0.0009 change in the probability of a citizen suit at plant *i* in period *t* yields a 0.0030 decrease in the

probability of a fine, or approximately an 86% decrease over the background fine probability of 0.0035.

20. The change in the probability of a citizen suit times the IV linear probability coefficient of -29.47 yields a 0.027 percent decrease in the net size of fines.

21. EPA court victory percentages variables constructed from Syracuse University's Transactional Records Access Clearinghouse (TRAC), which is available online at: http://trac.syr.edu/ .

22. This additional instrument is positive and significant, as expected, in the first-stage.

23. Biochemical Oxygen Demand (BOD) is a measure of the organic content of discharges. It is the most common conventional water pollution measure. A violation occurs when the concentration exceeds permitted levels.

24. Standard errors are bootstrapped to account for the use of the predicted value of the Mills ratio obtained from the first-stage sample selection model.

Table 1. Enforcement Summary Statistics: 1990-2000

# Plants	1,494
# States	15
# Inspections	23,944
# Public Sanctions	8,721
# Public Fines	684
# Citizen Suits in Federal Courts	54

Table 2. Determinants of Citizen Suit Regressions

	IV LP Model: 2SLS	Bivariate Probit Regresssions		
Regressand		Inspections	Sanctions	Fines
Federal District Court GHP	-0.0026**	-2.203*	-2.131*	-2.310**
Judicial Temperament Scores	(0.0011)	(1.216)	(1.161)	(1.054)
Federal District Court Caseload	-0.0013**	-1.057	-1.628	-1.077
(Cases per judge x 1000)	(0.00067)	(0.872)	(1.034)	(0.799)
Inspections 1-12 months ago on	-0.00053	-0.383	-0.398	-0.813
other plants in state (x 1000)	(0.00052)	(0.919)	(0.939)	(1.109)
Fines 1-12 months ago on other	-0.00381	-10.696	-12.148	-12.462
plants in state (x 1000)	(0.00519)	(13.192)	(15.300)	(10.297)
Sanctions 1-12 months ago on	0.00105	0.975	0.782	1.232
other plants in state (x 1000)	(0.00076)	(1.693)	(1.758)	(1.501)
Violations 1-12 months ago	0.0001	0.0433	0.0504*	0.0861**
	(0.00004)	(0.0274)	(0.0305)	(0.0313)
Season Dummies	Yes	Yes	Yes	Yes
Year Dummies	Yes	Yes	Yes	Yes
State-Level Fixed Effects	Yes	Yes	Yes	Yes
Observations	179280	179280	179280	163920
F-Statistic	3.87	-	-	-
Prob > F	0.000	-	-	-
Wald chi-2	-	11710	7906	1558
Prob > chi-2	-	0.000	0.000	0.000
Stock-Yogo F-Statistic	4.54	-	-	-
Prob > F	0.011	-	-	-

Superscripts * and ** indicate statistical significance at the 10% and 5% levels, respectively. The dependent variable in each regression is the existence of a private citizen suit at plant i in month t. The citizen suit determinant equation from the fine bivariate probit estimation contains only 163920 observations from 1366 plants in 14 states over 120 months since there were no administrative fines in CA dataset.

Table 3. Determinants of Public Monitoring and Enforcement Regressions

	Dependent Var: Inspections			Dependent Var: Sanctions			Dependent Var: Fine Existence			Dependent Var: Log(fine amount)	
Regressand	2SLS IVLP (1)	2SLS IVLP (2)	Bivar. Probit (3)	2SLS IVLP (4)	2SLS IVLP (5)	Bivar. Probit (6)	2SLS IVLP (7)	2SLS IVLP (8)	Bivar. Probit (9)	2SLS IVLP (10)	2SLS IVLP (11)
Prob. of a Citizen Suit	33.271** (12.545)	33.167** (12.509)	-1.815 (1.767)	-13.832** (5.958)	-13.560** (5.904)	-2.493** (0.443)	-3.353** (1.596)	-3.330** (1.591)	-1.847** (0.240)	-29.466** (13.295)	-29.278** (13.250)
Recent Insps on others (x1000)	0.657** (0.019)	0.658** (0.019)	2.216** (0.043)	0.002 (0.009)	0.005 (0.009)	0.300** (0.065)	-0.003 (0.002)	-0.003 (0.002)	0.274 (0.235)	-0.018 (0.020)	-0.014 (0.020)
Recent Fines on others (x1000)	0.085 (0.211)	0.095 (0.209)	-0.087 (0.507)	0.015 (0.100)	0.075 (0.099)	2.827** (0.661)	0.667** (0.027)	0.675** (0.027)	9.184** (1.192)	4.839** (0.223)	4.899** (0.222)
Recent Sanctions others (x1000)	-0.079** (0.032)	-0.079** (0.032)	0.823** (0.093)	0.343** (0.015)	0.345** (0.015)	1.740** (0.087)	0.003 (0.004)	0.003 (0.004)	1.329** (0.345)	0.026 (0.034)	0.028 (0.034)
Recent violations	0.004** (0.002)	- -	0.032** (0.003)	0.022** (0.001)	- -	0.136** (0.004)	0.003** (0.000)	- -	0.116** (0.007)	0.022** (0.002)	- -
Season FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State Fes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179280	179280	179280	179280	179280	179280	179280	179280	163920	179280	179280
Sargan chi2	3.711	3.601	-	6.461	8.564	-	0.001	0.020	-	0.004	0.050
Prob > chi-2	0.054	0.058	-	0.011	0.003	-	0.978	0.889	-	0.950	0.823

Superscripts * and ** indicate statistical significance at the 10% and 5% levels, respectively.

Table 4. Weak Instrument Sensitivity Experiments

Regressand	Dependent Var: Inspections			Dependent Var: Sanctions			Dependent Var: Fine Existence			Dep. Var: Log(fine amount)		
	LIML IVLP	Just Identified IVLP	Reduced Form OLS	LIML IVLP	Just Identified IVLP	Reduced Form OLS	LIML IVLP	Just Identified IVLP	Reduced Form OLS	LIML IVLP	Just Identified IVLP	Reduced Form OLS
Prob. of a Citizen suit	47.874**	53.291**	-	-25.973**	-26.378**	-	-3.353**	-3.316	-	-29.479**	-30.151*	-
	(20.349)	(24.234)	-	(12.904)	(12.385)	-	(1.597)	(2.062)	-	(13.301)	(17.482)	-
GHP Judicial temperament scores	-	-	-0.136**	-	-	0.067**	-	-	0.009**	-	-	0.078**
	-	-	(0.020)	-	-	(0.013)	-	-	(0.004)	-	-	(0.030)
Caseload (Cases per judge x 1000)	-	-	-0.005	-	-	-0.006	-	-	0.004*	-	-	0.037**
	-	-	(0.012)	-	-	(0.008)	-	-	(0.002)	-	-	(0.018)
Recent Insps on others (x1000)	0.660**	0.661**	0.633**	-0.0003	-0.0003	0.013**	-0.003	-0.003	-0.001	-0.018	-0.018	-0.002
	(0.025)	(0.028)	(0.009)	(0.014)	(0.014)	(0.006)	(0.002)	(0.002)	(0.002)	(0.020)	(0.020)	(0.014)
Recent Fines on others (x1000)	0.184	0.221	-0.028	-0.068	-0.071	0.059	0.667**	0.667**	0.680**	4.839**	4.835**	4.951**
	(0.295)	(0.331)	(0.093)	(0.169)	(0.169)	(0.060)	(0.027)	(0.028)	(0.018)	(0.223)	(0.239)	(0.140)
Recent Sanctions on others (x1000)	-0.096**	-0.102**	-0.040**	0.357**	0.357**	0.326**	0.003	0.003	-0.0004	0.026	0.026	-0.006
	(0.045)	(0.051)	(0.014)	(0.026)	(0.026)	(0.009)	(0.004)	(0.004)	(0.003)	(0.034)	(0.034)	(0.021)
Recent violations	0.003	0.003	0.006**	0.023**	0.023**	0.021**	0.003**	0.003**	0.003**	0.022**	0.022**	0.020**
	(0.002)	(0.003)	(0.001)	(0.001)	(0.001)	(0.0004)	(0.0002)	(0.0002)	(0.0001)	(0.002)	(0.002)	(0.001)
Season FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179280	179280	179280	179280	179280	179280	179280	179280	179280	179280	179280	179280
Stock-Yogo F	4.54	5.39	-	4.54	5.39	-	4.54	5.39	-	4.54	5.39	-
Prob > F	0.011	0.020	-	0.011	0.020	-	0.011	0.020	-	0.011	0.020	-
F for GHP Scores, Caseload = 0	-	-	24.05	-	-	14.51	-	-	4.33	-	-	5.40
Prob > F	-	-	0.000	-	-	0.000	-	-	0.013	-	-	0.005

Superscripts * and ** indicate statistical significance at the 10% and 5% levels, respectively.

Table 5. Exclusion Restriction Sensitivity Experiments

Regressand	Dependent Var: Inspections		Dependent Var: Sanctions		Dependent Var: Fine Existence		Dep. Var: Log(fine amount)	
	2SLS IVLP with EPA Court Success Control	2SLS IVLP with Additional Instrument	2SLS IVLP with EPA Court Success Control	2SLS IVLP with Additional Instrument	2SLS IVLP with EPA Court Success Control	2SLS IVLP with Additional Instrument	2SLS IVLP with EPA Court Success Control	2SLS IVLP with Additional Instrument
Prob. of a Citizen Suit	32.911**	32.590**	-14.283**	-3.430	-3.077**	-1.700*	-27.404**	-13.685*
	(12.235)	(9.543)	(5.974)	(3.071)	(1.508)	(0.985)	(12.587)	(7.784)
Recent Insps on others (x1000)	0.653**	0.657**	0.003	0.003	-0.002	-0.003	-0.012	-0.015
	(0.019)	(0.018)	(0.009)	(0.006)	(0.002)	(0.002)	(0.019)	0.015
Recent Fines on others (x1000)	0.071	0.080	0.015	0.086	0.671**	0.679**	4.867**	4.947**
	(0.208)	(0.200)	(0.102)	(0.065)	(0.026)	(0.021)	(0.214)	(0.163)
Recent Sanctions on others (x1000)	-0.077**	-0.079**	0.343**	0.331**	0.003	0.001	0.021	0.008
	(0.032)	(0.030)	(0.015)	(0.010)	(0.004)	(0.003)	(0.032)	(0.025)
Recent violations	0.004**	0.004**	0.022**	0.021**	0.003**	0.003**	0.022**	0.021**
	(0.002)	(0.002)	(0.001)	(0.001)	(0.0002)	(0.0002)	(0.002)	(0.001)
EPA Court Success rate	0.011**	-	-0.003	-	-0.002**	-	-0.014**	-
	(0.005)	-	(0.003)	-	(0.001)	-	(0.005)	-
Season FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	179280	179280	179280	179280	179280	179280	179280	
Stock-Yogo F	4.70	5.07	4.70	5.07	4.70	5.07	4.70	5.07
Prob > F	0.009	0.002	0.009	0.002	0.009	0.002	0.009	0.002
Sargan chi2	2.510	3.839	5.522	31.569	0.288	4.181	0.162	6.107
Prob > chi-2	0.113	0.147	0.019	0.000	0.591	0.124	0.687	0.047

Superscripts * and ** indicate statistical significance at the 10% and 5% levels, respectively.

Table 6. The Impact of Private and Public Enforcement on Plant-Level Violations

Variable Description	Probit Model with Sample Selection
Citizen suits 1-12 months ago in state	-0.1661**
	(0.0684)
Fines 1-12 months ago in state	-0.0171**
	(0.0084)
Inspections 1-12 months ago in state	-0.0006**
	(0.0003)
Season FEs	Yes
Year FEs	Yes
Plant-level CREs	Yes
Observations	114161
Wald chi-2	741.84
Prob > chi-2	0.000

Superscript * and ** indicate statistical significance at the 10% and 5% levels. The dependent variable is a dummy for the presence of a violation at plant i in month t.