Do Citizen Lawsuits Affect Plants' Decision to Comply with Environmental Regulation?

An instrumental variable analysis on wastewater treatment plants' compliance with the Clean Water Act in the U.S during the period 1990-2000.

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Abstract

This study aims to contribute one piece to the puzzle on why industry plants in the U.S. choose to comply with environmental laws under seemingly low incentives i.e., few inspections and low fines. This study investigates private enforcement, specifically citizen suits, as a possible determinant of plants' compliance behavior.

Empirically, the study focuses on compliance with the Clean Water Act (CWA) among 1494 wastewater treatment facilities in the U.S. In a fixed effects 2SLS IV model, using a ten year panel with monthly data, this study finds that a plant that has been subject to private enforcement through a citizen lawsuit during the past 12 months, is up to 485% less likely to be in violation of the CWA, compared to plants that have not been sued, holding all other variables. The marginal direct deterrence effect of a citizen suit is expected to yield a reduction of 6.24 to 14.31 violations.

The result is robust to three different IV estimation methods (OLS, LPM and Probit) and three different sets of instruments. Citizen suits are instrumented with judicial instruments, exploiting that all citizen suits have to go through a federal district court. Public enforcement are instrumented with three new found instruments: EPA's enforcement budget and two instruments capturing the public demand for enforcement of environmental regulation. This study's key contribution is systematic micro-level empirical evidence on citizen suits significant causal effect in plants' compliance behavior.

Key words: Citizen lawsuits, Enforcement, Clean Water Act, Compliance, Violation, deterrence effect, EPA, environmental regulation.

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Contents

1 Introduction

Environmental pollution is a serious problem in all western countries and is of major concern in Environmental economics. One way to limit pollution is to legislate firms' allowed pollution levels. For such a mechanism to work, law enforcement is crucial.

In the United States, the Environmental Protection Agency (EPA) has the principal responsibility to enforce the environmental regulations. Since the 1990s, EPA's enforcement budget has decreased and plant inspections have significantly been reduced. According to the enforcement literature, one would expect decreased public enforcement to lead to more violations of the environmental laws. This concern has however not materialized in the U.S. An early explanation to the large compliance rate was EPA's changed enforcement strategy towards voluntary pollution reduction programs and the introduction of a self-reporting system. This has later been shown to have very limited or no impact on compliance (Delmas and Toffel, 2008; Innes and Sam, 2008).

This study investigates a potential determinant of compliance that has not been considered in previous research. Instead of focusing on public enforcement or voluntary compliance, this study investigates if private enforcement of the environmental laws can explain the apparent contradiction between decreased public enforcement and the decreasing number of violations observed in the U.S. The approach taken is to investigate the effect of private prosecution against plants in violation of the U.S. environmental regulation. Do plants' compliance behavior change after they have been sued in a federal district court by a citizen lawsuit, for instance by a non-governmental environmental group?

The working mechanism is the same as for public enforcement and and goes back to Becker (1968) and the rational agent. Law compliance by a rational agent is assumed to increase with the probability of conviction and the severity of the punishment if convicted (Becker, 1968). Penalties from citizen suits are in general larger than fines issued by EPA. This makes the expected costs for a citizen suit larger than for public enforcement, even for a low probability of an actual suit. Furthermore, although citizen suits are few, which decreases the perceived probability of conviction, citizen suits come unexpectedly with only 60 days for the plant to move to compliance before the suit goes to court, whereas EPA issues many warnings before sanctioning a plant. This makes the expected effect of an actual citizen suit larger than an actual sanction by EPA. Citizen suits are hence expected to have a larger marginal deterrence effect than public enforcement by EPA.

The study uses data from the U.S., which is also where citizen suits are the most common. The investigated sample period is 1990-2000, a period of time where public enforcement decreased and the number of citizen suits increased. The vast majority of all citizen suits are filed for water violation, therefore this study focuses on violations of the Clean Water Act (CWA). The plants chosen for investigation are municipal wastewater treatment plants since they represent more than two-thirds of major regulated entities under the CWA. The research questions of this study are:

> *Firstly, do plants change their violation behavior after they have been sued in a federal district court by a private citizen? Secondly, how does the potential effect of a citizen suit on plants' violation behavior compare to the effect of EPA enforcement?*

Citizen Suits (CSs) have never explicitly been evaluated as a possible determinant of plants' compliance behavior with environmental regulation in the U.S. Two major reasons are data scarcity and the challenge of determining causality. This study uses a unique data set obtained from a Freedom of Act request from the US Department of Justice, merged with data from EPAs Permit Compliance System database. To capture the causal effect of CSs on violations, CSs are instrumented with two judicial instruments, previously successfully used by Langpap and Shimshack (2010), exploiting that all CSs have to go through a federal district court. Public enforcement is instrumented with EPA's enforcement budget and two instruments capturing the public demand for enforcement of environmental regulation.

Evidence for a causal effect of private enforcement on plants' behavior could partly explain why plants choose to comply under seemingly low economic incentives. The study finds evidence for CSs to have a significant impact on plants' violation behavior. The result contributes an important empirical insight on a previously unexplored aspect of environmental law enforcement in the economics literature.

The remainder of the thesis is organized as follows. Section 2 presents a background on public enforcement, private enforcement and the expectations based on the theoretical framework used in this study. Section 3 presents previous research on citizen suits and known determinants of plant compliance with environmental laws. Section 4 presents the data used in this study, the economic model explaining plant violation, and the variables to estimate. Lastly, the chosen empirical estimation method is presented. Section 5 presents the main results of this study, followed by a sensitivity analysis of the results. Conclusions are provided in Section 6.

2 Background

2.1 Public enforcement

The CWA is a federal law, for which EPA has the principal enforcement responsibility. In practice, the majority of the enforcement (monitoring, inspections and sanctions) are delegated to state agencies where the facilities are located. The state agencies also issue the emission permits. Although EPA and the states share the enforcement responsibility, EPA still retains the right to take direct action where it believes that a state has failed to enforce the law. A state can also request EPA involvement in case it needs support (Bearden et al, 2011).

According to EPA's guidelines for enforcement, local state agencies are recommended to inspect all plants in the state at least once every two years for large plants and somewhat less often for smaller plants. An inspection can be long and detailed or a short visit, depending on the plant's compliance history and plant specific characteristics such as abatement technology (USEPA, 2007). Longer inspections are very costly for the plant since it requires planning and since staff are taken out of production. When a plant is found in non-compliance, EPA (or the state agency) will issue warnings to the plant in the form of phone calls and letters of notice. If the plant chooses to stay in violation, the plant may be fined during an inspection visit or sanctioned by the administrative law judges.[1]

For very serious violations of the CWA, the legal case can be determined by the federal Department of Justice for civil prosecution. EPA's guidelines however, give strong preference to prosecuting non-compliance in the administrative court over civil and criminal referrals. The reason is that the administrative court sets the lowest possible fine needed to push the plant into compliance, whereas the fines determined in the federal district court don't consider such circumstances and are higher in general. Furthermore, prosecuting in a federal district court is more costly for all parties involved (Grey and Shimshack, 2011). Because civil court prosecution for public enforcement is very rare, it will be treated as negligible in this study.

EPA (and the local state agencies) have approximately seven thousand facilities to monitor under the CWA, and about forty-one million facilities in total under all environmental regulations (Grey and Shimshack, 2011). Due to limited resources, and because monitoring and sanctions are costly, state agencies will solve the maximization problem with respect to the number of firms in compliance subject to their

[1]Factors determining the magnitude of the fine include: history of compliance, duration of non-compliance, economic benefit for the plant of non-compliance, harm to human health and environment, the plant's ability to pay a fine, fairness and consistency as well as discretionary adjustments (USEPA 2001).

budget constraint. Hence, monitoring will not be complete and all violations will not lead to sanctions. Both the number of inspections and sanctions vary between states and over time (Russel, 1990; Naysnerski and Tietenberg, 1992 and Percival and Schroeder, 2006).

In the beginning of the 1980s, the international environmental law enforcement policy community started to advocate for more voluntary pollution reduction programs and information policies and less conventional enforcement, such as plant inspections and sanctions (Mintz, 2012). The idea was that law enforcement could be made more cost-effective than conventional enforcement. Harrington (1988) suggests that the optimal enforcement strategy would be to enforce only "low cooperate" (violating) plants, and relieve the "high cooperate" plants from expensive inspections and monitoring. This enforcement strategy would give plants additional incentives to choose compliance. The United States followed this new trend, and the U.S. EPA started to reduce its traditional enforcement activities in the 1990s, and redirected towards information policies. Between 1990-1996, plant inspections were reduced by approximately 25% and the enforcement budget was reduced by approximately 15% (Gray and Shimshack, 2011). However, voluntary adaption of abatement technology and voluntary reduced emissions have shown to have very little or no effect on compliance in the U.S. (May, 2005 and Delmas and Toffel, 2008), and cannot explain the decreasing number of violations during the period.

To summarize, EPA has the principal responsibility to enforce the environmental regulations in the U.S. Due to constrained enforcement budget and enforcement being costly, public enforcement is never complete. Furthermore, public enforcement has decreased since the 1980s in favor of voluntary programs, which have been shown to have small or no effect on compliance.

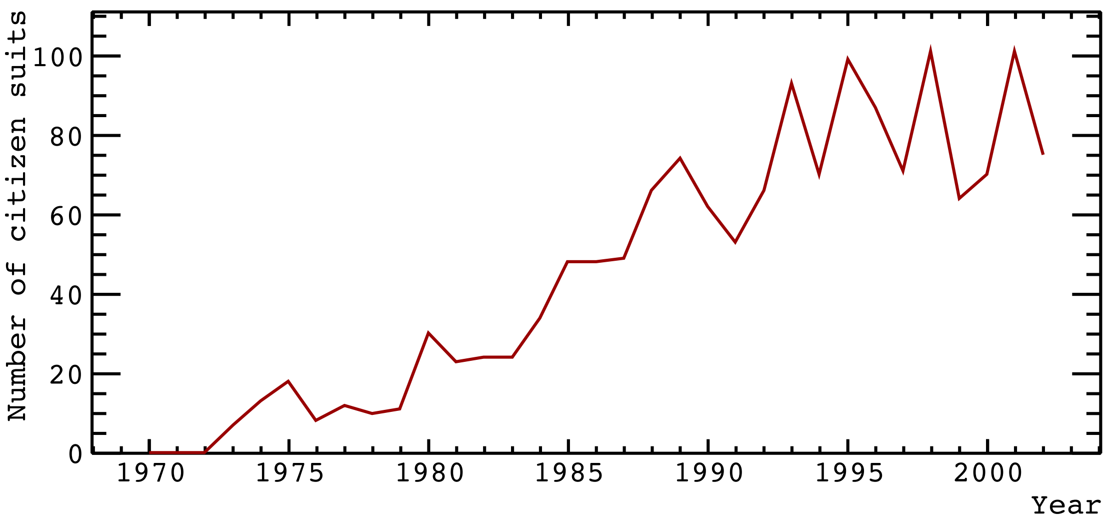
2.2 Private enforcement

As mentioned in Section 2.1, plants in violation of the CWA may not be sanctioned by EPA or the state agency in charge. In 1972, the U.S. opened up for private actors to help enforce the environmental laws. The Clean Air Act (CAA)[2] was the first federal environmental statute to include provisions for citizen enforcement, followed by the CWA the same year[3]. In 1976, also the Resource Conservation and Recovery Act (RCRA)[4] followed and the Comprehensive Environmental Response, Compensation

[2] 42 U.S.C. §7401 *et seq.*
[3] 33 U.S.C. §1251 *et seq.*
[4] 42 U.S.C. §6901 *et seq.*

and Liability Act (CERCLA, sometime referred to as the Superfund)[5] in 1980. Since then, private enforcement has become a natural part of the law enforcement in the U.S. Today, all major federal environmental laws[6] support citizen lawsuits. Since the introduction of CSs in 1972, an increasing trend of CSs is observed until the mid 1990s, where the number of CSs are stabilized with a fluctuation around 90 CSs per year, see Figure 1.

Figure 1: Development of citizen suits during 1970-2002



Note: the graph includes all citizen suits in the U.S during the period 1970-2002. The number of acts providing for citizen suits increases with time. The first three years include only the CAA and the CWA, whereas all acts are included from 1980 and onwards. The data are from May (2003).

Citizen suits are the most common in the United States but also exist in other countries. Also Australia, Malaysia, the Philippines, Colombia, Argentina and India provide a possibility of citizen suits.[7] Although in India, especially one person called M.C. Mehta has been very successful in civil suits, lawsuits by private citizens or

[5] 42 U.S.C. §9601 *et seq.*

[6] CAA, CWA, CERCLA, 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.

[7] A different version of citizen suits exists in the EU environmental law. Such a citizen suit can only be directed against a public enforcement authority that has failed to fulfill its duty, and thus not the violating agent directly. Citizen suits in the EU require less proof burden due to the "precautionary principle" in EU environmental law (McIntyre and Modedale, 1997).

environmental organizations are few compared to in the United States. Therefore it's reasonable to do this study on U.S. data.

Individuals may bring a CS in a U.S. district court against persons[8] who violate a prescribed effluent standard or limitation, often a person responsible for a facility. To initiate a CS, the plaintiff has to send a written notice to the polluter (and to the public enforcement agency). The person/plant then has 60-90 days to rectify the violation and move to compliance. In case the plant is still in violation of the act, the suit will come into force and be determined in a federal district court. If the CS is successful, the violator will have to pay substantial fines to the US treasury and may also be bound to comply with costly action-based consent (Smith, 2004).

Pursuing a lawsuit is expensive and requires judicial knowledge. Therefore most civil lawsuits come from large environmental groups, that have lawyers and the financial resources to take action.[9] Because the losing party pays the legal expenses, the prospects of conviction in court is crucial for the environmental group (Baik and Shogren, 1993). Also visibility is important; the environmental groups are more likely to initiate a lawsuit if a court case can give the organization publicity which leads to new members and donations (Langpap and Shimshack, 2010).

A CS works through the same mechanisms as public enforcement, i.e., it's costly for a plant to be targeted under non-compliance. But CSs also differ from public enforcement in several respects. Firstly, all CSs have to go through a federal district court whereas court cases initiated by EPA go through the administrative court. Penalties determined in the federal district courts are in general larger than penalties issued by the administrative court (Gray and Shimshack, 2011). Secondly, whereas public enforcement is relatively predictable — inspections are planned and many warnings are issued in case of a violation — a CS comes unexpectedly with only one formal warning before the case goes to court. This makes an actual citizen lawsuit very effective and the marginal effect of such a lawsuit is expected to be much larger than a fine issued by the administrative court. Thirdly, CSs crowd in public monitoring (Langpap and Shimshack, 2010), which makes citizen lawsuits forceful in double respect.

[8]Although unusual, individuals may also bring a citizen suit against EPA (or the state agency in charge) if EPA fails to carry out a non-discretionary duty under the law.

[9]Large environmental groups are organizations such as *Baykeepers* or *Riverkeepers*. The groups *Earthjustice* and the *Tulane Environmental Law Clinic* are both specialized on CSs.

2.3 Theoretical expectations

Becker's (1968)[10] hypothesis, that law compliance increases with the probability of conviction and the severity of the punishment if convicted, is well established both theoretically and empirically in the environmental law enforcement literature.[11] Assuming rational compliance behavior, a possible explanation for the increased compliance would be lower abatement costs which would make it more profitable to choose compliance. But lower abatement costs require investments in costly abatement technology, which is not expected to be adopted under low economic incentives. The discrepancy between economic theory about the rational agent and the decreasing number of violations, indicates that there are more costs associated with non-compliance to the environmental regulations in the United States, than previously investigated. Such costs may come from private enforcement.

CSs are expected to increase plants' cost of non-compliance substantially. Firstly because it's costly to be a defendant in a lawsuit and the lawsuit may lead to a fine. Secondly, the additional dimensions of CSs being less predictable, the fines being larger, CSs crowding in public monitoring, and because the plant's reputation may be damaged, make CSs likely to have a stronger impact on plants' compliance behavior than regular public enforcement would.

In the public enforcement literature, a general finding is that public enforcement towards a given plant has a positive effect on that plant's probability to choose compliance with environmental regulation. This effect is often referred to as "specific deterrence" or "direct effect". In addition to direct effects, "general deterrence", or "indirect deterrence" effects refer to the effect on a given plant's compliance behavior as a function of law enforcement at other plants. Public enforcement consists mainly of inspections, sanctions and fines, whereof inspections coupled with substantial fines are found to have the strongest direct and indirect effect on violations. Specific deterrence effects are expected to be effective after a plant has been sued and some time ahead. Because CSs work through the same mechanisms as public enforcement, theoretically we expect CSs to have both a direct and an indirect deterrence effect on plants' compliance behavior. In practice however, CSs are few[12] and therefore the data contain too little variation for CSs per state and year to test such a hypothesis. Hence, general deterrence of CSs is not included in this study as a possible

[10] See also Stigler, 1970 and Estlund and Wachter, 2013.

[11] For adaptations of Becker's model to environmental context, see for instance Sutinen and Anderson (1985) and Russell et al (1986). For empirical findings see for instance Magat and Viscusi (1990), Polinsky and Shavell (2000), Kagan et al (2003) and Deily and Gray (2007).

[12] In the sample investigated in this study, the average number of CSs per state and year is only 0.34.

determinant of CSs on plants' violation behavior.

This study doesn't investigate the effect of citizen suits' existence, only the behavior change in case a plant has been sued. Similarly, the possible compliance status change when a plant in violation receives a warning letter about a citizen lawsuit isn't considered. It's possible that not all warnings lead all the way to prosecution in court. To not consider this effect only has implications for the estimated results in case the plants that choose compliance after a warning, are not representative for all plants that receive a warning. One may expect larger plants to have more difficulties to adjust the pollution levels due to large production, than smaller plants. On the other hand, smaller plants may have less advanced abatement technology and may have difficulties to reduce production due to smaller margins. Either way, all equations will control for plant size. If there are other variables, however, which make some plants stay in violation after a warning, the estimated results would be biased. Although no such a bias is expected theoretically, the bias is unexplored.

A final theoretical expectation is that both private enforcement and public enforcement are reversely correlated with violations because known violators are expected to receive more enforcement. Estimating enforcement on compliance without considering endogeneity would result in a biased and inconsistent model capturing the correlation and not the causal effect, which may, if the reverse causality is stronger than the causal effect of interest, result in a negative coefficient and lead to the interpretation that more enforcement leads to more violations. However, disentangling causality may be possible by identifying factors that affect private enforcement and public enforcement but do not directly influence plants' decision to violate the CWA. Therefore, appropriate instruments might include the factors that influence citizen groups' perceived chances of lawsuit success or private enforcement costs, and for public enforcement, EPA's economic ability to enforce the CWA and the public pressure in the state for enforcement.

This study aims to find if plants change their violation behavior after a CS. In an attempt to disentangle causality and correlation, this study is done as an Instrumental Variable (IV) analysis, instrumenting citizen suits following Langpap and Shimshack (2010), and introducing three novel instruments for public enforcement.

3 Previous literature

Previous literature on CS effects is very sparse, and even more absent for effects on plant compliance with environmental laws. The topic of CSs is difficult to address for mainly two reasons: data availability, both for citizen suits and plant level (EPA) state enforcement, and the difficulty of determining the causal impact of CSs on compliance, mentioned above.

The topic of citizen suits is mainly investigated in the law literature where the law process of a CS, the development over time and possible future direction of CSs are assessed. For instance, Tyson (2014) discussed future possible collaboration between citizen suits' initiators and EPA. In economics, the literature has primarily focused on the efficiency of citizen suits.[13] Fadil (1985) argues for the social benefits of developing citizen suits where public enforcement is limited and market-based pollution-taxes are not a possibility. Heyes (1997) discusses the optimal tax or subsidy which would make citizen suits efficient.

Langpap and Shimshack (2010) investigate the effect of CSs on public enforcement. They obtained data on CSs from a Freedom of Information Action (FOIA). To account for endogeneity, they instrument CSs with judicial variables. They find that CSs crowd in public monitoring but crowd out public sanctions (Langpap and Shimshack, 2010).

Thus, previous research on private enforcement is limited, and no previous research exists on citizen suits direct effect on plants' violation behavior. The existing literature on plants' compliance behavior with environmental regulation has focused primarily on the relationship between public enforcement and compliance. A general finding is that voluntarily pollution reductions are rare, and that direct as well as indirect enforcement increases plants' probability to choose compliance with environmental regulation. Some examples of these findings will be more thoroughly discussed in what follows.

Vidovic and Khanna (2007) identify the new EPA enforcement strategy as the main explanation for the reduced number of violations in the U.S., and Sam (2010) finds that voluntary programs launched by EPA decreased violations in three industries, while it increased violations in two others. Also the participation rate in the voluntary programs has sometimes been emphasized as a success. However, according to the critics, firms that participated in the programs are found to the be the same firms that already had made large pollution reductions before the programs were launched. The observed effects are simply a continuation of the time trend that had started before the programs (Arora and Cason 1995 and 1996; Khanna and Damon 1999; Gamper-Rabindran 2006; Innes and Sam 2008). Innes and Sam (2008) find that plants are only motivated by voluntary programs if the programs are coupled with relaxed enforcement, which supports Harrington's argument of optimal enforcement. But since the incentive to cooperate disappears as soon as enforcement is reduced, the strategy is inoperative. Therefore, Innes and Sam (2008) conclude

[13] See for instance Naysnerski and Tietenberg (1992), Baik and Shogren (1994), and Heyes and Rickman (1999).

voluntarily programs to be complements rather than substitutes to traditional enforcement, in an optimal enforcement strategy. In addition, as also Harrington (1988) himself points out, his theory assumes that the individual plant knows to which group it's assigned at any given time, and that EPA knows the plant's likelihood to violative an environmental law, which requires information about every plant's abatement costs (Harrington, 1988). These are assumptions that are not likely to hold in reality.

Instead, legal regulation accompanied by substantial enforcement including monetary fines are what have been found to really affect plants to choose compliance.[14]

Magat and Viscusi (1990) investigate the direct deterrence effects of EPA inspections on compliance with the CWA in the pulp and paper industry. They find that a given facility's probability to violate the law is about twice as high if the facility has not been inspected in the previous quarter. Deily and Gray (2007) also examine direct deterrence effects of enforcement on compliance. In a study on 41 large steel mill plants in the U.S. between 1976 and 1989, they find that the probability that a plant would comply with the environmental law is about 33% higher if the plant has been inspected during the past two years, compared to a plant that has not been inspected. Their study also shows that firms with fewer violations received in general fewer inspections (Deily and Gray, 2007). Earnhart (2004a; 2004b) finds that enforcement actions on a given plant has a positive effect on compliance for that plant, and that monetary fines has the greatest effect. Indirect deterrence is shown to be nearly as strong for a given plant, as if the given plant is enforced directly. Monetary fine amounts are shown to have a strong indirect deterrence effect (Shimshack and Ward, 2005). Gray and Shadbegian (2007) investigate indirect deterrence further and find that the deterrence effect is limited to the state. These empirical findings illuminate two major empirical insights — public enforcement matters (through both specific and general deterrence) and reverse causality exists between public enforcement and compliance: public enforcement affects compliance behavior and violations attract more enforcement.

Gray and Shadbegian (2005) investigate whether plant characteristics can explain why some plants choose to comply whereas other plants choose not to. They use plant-level compliance data together with data on plant characteristics on pulp and paper mill plants in the U.S for the period 1979-1990. They find that plant characteristics are not significant determinants of compliance in general, however large and old plants tend to comply less with the environmental regulations (Gray and Shadbegian, 2005).

As already pointed out, reverse causality is a major issue when estimating the

[14]See for instance Magaat and Viscusi (1990), Doonan, Lanoie, and LaPlante (2005), Gray and Schadbegian (2005) and Shimshack and Ward (2005).

effect of enforcement on compliance.[15] Finding instruments for public enforcement is however difficult, and previous studies use different approaches to capture the causal effect. The dominant approach so far is to predict public enforcement in a first stage probability model.[16] In an attempt to make compliance exogenous to the model, Magat and Viscusi (1990) model the probability of compliance in a first step using past pollution, past inspections and plant and region specific characteristics. Using lags makes sense since it's not likely that today's compliance can affect monitoring that has already occurred. But as Gray and Shimshack (2011) point out, compliance decisions by the plant may be constant over time. If that's the case, then today's observations may be correlated with past observations and thus, reverse causality is still present in the model. Gray and Deily (1996) estimate compliance as a function of plant and local characteristics, and public enforcement as a function of plant characteristics. They obtain two equations from which they model the relationship between compliance and public enforcement in both directions, and find that public enforcement encourages more compliance, but also that more compliance generates less public enforcement.

To conclude, according to previous literature, conventional enforcement with "teeth" such as sanctions, including more monitoring and substantial fines, has the strongest reduction impact on violations. Both direct and indirect deterrence effects exist for public enforcement. Also plant specific effects, such as plant size, explain plants' compliance behavior. Public enforcement and private enforcement are simultaneously determined and public enforcement and compliance are causally connected in both directions. Still, there is no answer to why plants choose to comply also in the absence of strong public enforcement. No previous literature has investigated CSs as a possible determinant for this choice.

This study relates to previous research showing that, firstly, citizen suits are powerful; for instance CSs affect EPA enforcement (Langpap and Shimshack, 2010). Secondly, all cost-increasing enforcement should lead to increased incentives for plants to comply with environmental laws (Becker, 1968). By investigating citizen suits as a possible determinant of plants' violation behavior, this study contributes to the environmental economics literature on environmental law enforcement.

[15]For empirical evidence on reverse causality for public enforcement, see for instance Carson (1970), Bardach and Kagan (1982), Fenn and Veljanovski (1988) and Gray and Deily (1996).

[16]See for instance: Laplante and Rilstone (1996); Earnhart (2004a, 2004b); Stafford (2002, 2003); Sigman (2009) and Shimshack and Ward (2005, 2008).

4 Empirical strategy

The empirical strategy of this study is to identify a consistent regression model, from which conclusions can be made about the citizen suits' impact on plants' violation behavior. In an attempt to capture the causal impact of CSs and public enforcement on violations, an IV-approach is taken. CS are instrumented with judicial instruments exploiting that CSs go through a different court than lawsuits initiated by EPA. Public enforcement are instrumented with EPA's enforcement budget and two instruments capturing the public demand for enforcement of environmental regulation. The endogenous variables are predicted using the instruments and all other explanatory variables in first stage equations (one equation per endogenous variable). The predicted variables are then included in a second stage equation including all other explanatory variables, from which the main results are interpreted. The data set covers the period 1990-2000 and consists mainly of monthly EPA/state enforcement data including plant inspections, sanctions and fines, merged with plant level data on citizen suits.

This section begins with an assessment of the data sample. The next step is then to identify an economic model explaining plants' violation behavior, assuming economic rational behavior among plants. Then follows an assessment of the economic model from an econometric point of view, including the issue of endogeneity, followed by the empirical model specification and the estimation methods.

4.1 Data

The plant level data include: citizen suits and judicial instruments, public enforcement including plant inspections and monetary fines, violations and plant size. The citizen suit litigation data, including judicial instruments, is from the US Department of Justice, and the public enforcement data is from the EPAs Permit Compliance System.[17] Public enforcement instruments, county level GDP per capita, state income tax and EPA enforcement budget, are from the Bureau of Economic Analysis, Census Bureau's data base and EPA's web page, respectively. Control variables are discussed in Section 4.3. The data set consists of 1494 plants, with 197208 unique plant level observations.

Since the vast majority of all CSs are filed for water violation, this study focuses on CWA violations. A violation is defined as exceeding the permitted level of Biological Oxygen Demand (BOD). BOD is a good general pollution indicator because BOD

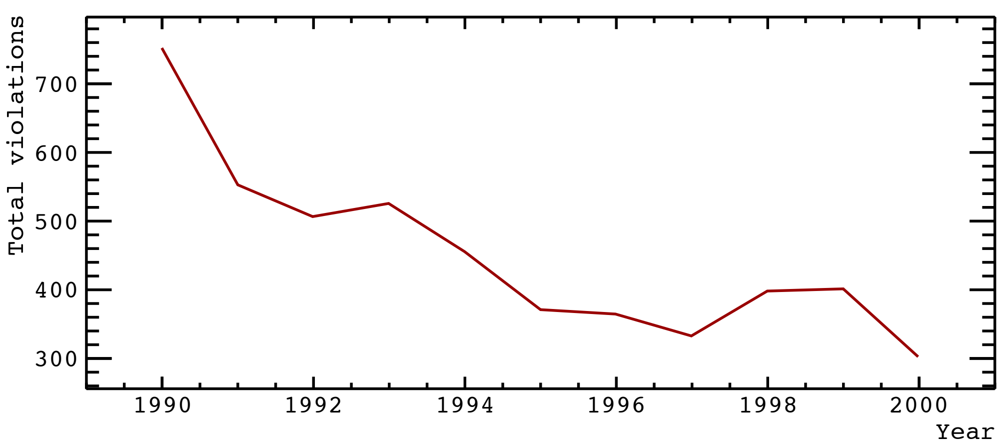
[17]The data were used in (Langpap and Shimshack, 2010), which focused on whether private citizen suits and public enforcement are substitutes or complements.

also comes with a certain proportion of other pollutants. It's also the standard measure of the organic pollutant content of water (Magat and Viscusi, 1990). This study focuses on all the "major"[18] municipal wastewater treatment plants in the U.S that report conventional water pollution discharges to the regulator in the fifteen states where the plants are located.[19] The states included in the sample are approximately evenly distributed among Democratic and Republican state governments.

Data on compliance with the CWA are in general based on the plants' own reports of their emission levels. Compliance data from small plants, with limited resources to install monitoring technology, are based solely on inspections. According to Gray and Shimshack (2011), it's considered standard to trust the self-reported data due to the harsh punishment for mis-reporting. This study will follow this standard.

The general plant behavior is to comply with the CWA. On average, a plant is in violation 0.3 times per year and only 3.9% of all plant level observations are violations. For data analysis, this implies small variation in the data which may create difficulties to perform some statistical tests. The number of violations decrease over the sample period, see Figure 2.

Figure 2: Total number of violations per year during the sample period 1990-2000



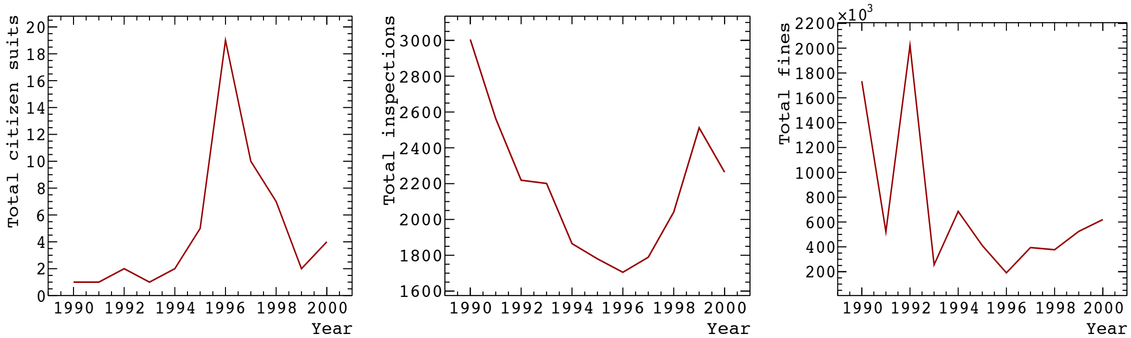
Note: The vertical axis is truncated.

[18]The term "major" is EPA's own term and includes all municipal wastewater treatment plants that discharge at least 1 million gallons per day, cause a significant impact on the receiving water body, or serve a population of at least 10,000.

[19]The fifteen states are: Alabama, Arkansas, Arizona, California, Connecticut, Georgia, Illinois, Mississippi, North Carolina, New York, Oregon, Pennsylvania, Texas, Washington, and West Virginia.

On average, plants are inspected 1.5 times per year and fined 0.04 times per year. A fine is on average approximately \$11,000. As shown in Figure 3, both the rate of EPA inspections and the magnitude of monetary fines issued by EPA, decrease during the period, whereas CSs increase during the period, neither of which monotonically.[20] In 1990, only one wastewater treatment plant was sued by a citizen for violating the CWA. The number of CSs peaked in 1996, with 19 sued plants.

Figure 3: General trends for the number of citizen suits, number of EPA inspections and the magnitude of EPA fines issued in case of violation.



(a) Total number of CSs (b) Total number of inspections (c) Total fine amount

Table 1 shows the main variables in this study, presented in numbers.

Table 1: Enforcement Descriptive Statistics: 1990-2000

	Number of instances
Plants	1494
States	15
Inspections	23,944
Public fines	684
Citizen lawsuits in federal courts	54
Violation of the CWA	6094

[20] In both Figures 4(a) and 4(b), a direction turn in the data is shown in 1996. This is in agreement with the findings in Langpap and Shimshack (2010), that citizen suits and the EPA monitoring are complements.

4.2 Economic model

In the economic model, plants' compliance behavior is a function of several factors:

$$violation_{it} = f(CS_{it}, public\ enforcement_{it}, X\beta_{it}), \quad (1)$$

where CS is self-explanatory, *public enforcement* includes plant inspections and sanctions (in case of violation), and $X\beta$ stands for all other explanatory control variables such as plants' abatement technologies, abatement costs and plant size. The subscripts indicate plant i at time t.

Plants' abatement technology and abatement costs are however unobservable and therefore not possible to include in the empirical model. Together the unobserved variables that affect a plant's violation behavior form an error term. A second obstacle with the economic model (Equation 1) is that some of the explanatory variables are expected to be endogenously determined with the dependent variable *violations*. For an overview of which variables are expected to be endogenous to the model, see Table 2. The expected signs in the table refer to the expected causal impact of the variable, and not the correlation between the variables.

Table 2: The expected causal impact of the variables affecting plant violations.

Variable	Expected sign	Endogenous
Direct deterrence variables:		
Citizen suits against plant i	−	yes
Inspections of plant i by EPA/state	−	yes
Plant i fined by EPA/state	−	yes
Indirect deterrence variables:		
Other plants inspected by EPA	−	no
Other plant fined by EPA	−	no
Additional control variable:		
Plant size	+	no

Both CSs and public enforcement are expected to affect violations negatively, since all enforcement increases the cost of non-compliance. At the same time, both environmental groups and the state agency are likely to monitor previous violators more than plants with a history of compliance. Plants that have a history of violations are also issued larger fines in case a state agency finds the plant in violation again, since the last fine was not large enough to push the plant into compliance.[21]

[21] For more factors determining the magnitude of the fine, see Section 2.1.

Therefore, the three first variables in Table 2 are expected to be reversely correlated with violations.

The general deterrence variables (other plants sanctioned and fined) are expected to be exogenous to the model, because enforcement of one plant is not based on other plants' violation status.[22]

Plant size is expected to increase plants' likelihood to be in violation because larger plants emit more pollution. The variable is not expected to be endogenously determined with compliance status since plant size is fairly stable over time.

4.3 Variables in the empirical model

The primary goal of this study is to examine the effect of a citizen suit on plants' violation behavior, specifically, if a given plant's likelihood to violate the CWA changes after the plant has been sued in a citizen lawsuit. The dependent variable in each of the main model regressions is the violations of the CWA. The dependent variable may be a count variable: the integer number of violations by a given plant during the past 12 months, or a dummy variable which equals one if the given plant is in violation at any time during the past 12 months, and zero otherwise. An IV-approach is used to overcome the endogeneity issues of citizen suits and public enforcement.

4.3.1 A short note on valid instruments

A valid instrument has to be *relevant*; being able to significantly explain variation in the instrumented variable. For the main model to be consistent, the instrument also has to pass the *exclusion restriction*; it has to be exogenous to the main model, i.e., not correlate with the error term in the main equation, and it can only affect the dependent variable in the main model through the endogenous variable, never directly. While relevance and direct correlation can be tested, exogeneity cannot. Instead, economic theory and logical reasoning become crucial. Because all unobserved variables affecting plants' compliance behavior are captured in the error term, the instruments cannot be correlated with any of the unobserved variables.

The omitted (unobserved) variables are presented below, followed by the intuition of the chosen instruments and the motivation why the instruments are exogenous to the main model. All instruments included in this study meet the relevance criterion and are only significant in the main model through the instrumented variables.

[22]The enforcement budget is flexible within its constraints, therefore CWA violations are not assumed to be substitutes, although trade-offs between plant enforcement are made on an aggregated national level due to fixed national enforcement budget.

4.3.2 Omitted variables

Recalling previous literature in Section 3 and data availability in Section 4.1, unobserved plant characteristics affecting plants' compliance behavior may be: plant's abatement technology, abatement costs and plant age. Other omitted variables may be plant governance and productivity, which in turn may be explained by: price of water, local infrastructure and geographical factors. These latter variables are to a large extent captured by state dummies and seasonal dummies, see Section 4.3.5, and are therefore not part of the error term.

4.3.3 Direct deterrence variables

As concluded in Section 4.2, all direct deterrence variables are endogenous to the model and have to be instrumented in order to estimate a consistent regression model. For citizen suits, this study follows Langpap and Shimshack (2010) and instruments citizen suits with judicial instruments. The chosen instruments for both public enforcement variables (inspections and fines) are not used in previous literature and are an important contribution of this study. Together with the instruments of CSs, the new public enforcement instruments allow for an empirical investigation of the causal effect of citizen suits on plants' violation behavior.

Instruments for *Citizen lawsuits* Whether an environmental group will sue a plant depends on the costs associated with prosecution and the probability of conviction. The two following instruments capture both these factors, exploiting the fact that all civil lawsuits have to go through the federal district courts.[23]

The *GHP scores* (named after its originators: Giles, Hettinger and Peppers) provide a measure of the ideological preferences of judges of the U.S. Courts of Appeals in a given district and year, on a liberal-conservative scale. The intuition is that conservative judges tend to vote in favor for the sued plant whereas less conservative judges tend to vote for the environment's best. The preferences are converted to scores on a scale of ideology which goes from negative one to positive one, where the latter (former) is the most (least) conservative, following Poole and Rosenthal (1997) and Poole's (1998) estimation method.[24] The GHP score is based on the appointing US President and the ideology scores of the US senators from the state in which the court is located. In case the US senators and the appointing President's political party affiliation differ, the GHP score is equivalent to the appointing President's

[23]This is unlike public lawsuits which are generally handled in the administrative court.

[24]The GHP scores used in this study are estimated by Langpap and Shimshack (2010).

score.[25] As Langpap and Shimshack (2010) point out, this instrument recognizes the important role of senators in the judicial appointment process when senatorial courtesy is present. Since more liberal court/year combinations have lower GHP score than more conservative, the victory probabilities may be perceived as lower for higher GHP scores, and thus the GHP score is expected to have a negative impact on the number of CSs at a given time.

The second instrument is the *average caseload per judge in a given district court in a given time period*. A higher caseload implies longer processing time in court and in turn, a more costly process as well as longer time for which the suer's money is locked into the prosecution process, i.e., a higher caseload per judge increases the plaintiff's opportunity costs. Therefore higher caseload is expected to have a negative effect on civil lawsuits.[26]

To conclude, the two judicial instruments capture the probability to succeed with a citizen suit in court, and the cost for the suer to prosecute — both of which two essential factors for the environmental group in the decision whether it will pursue a lawsuit. Since the two instruments are derived from political climate, no correlation between them and the omitted variables mentioned in Section 4.3.2 is expected, and hence the instruments are expected to be exogenous to the main model.

Instruments for *Inspections by EPA or state agency* Two instruments for *inspections* are found. The first instrument for inspections is the *EPA pollution enforcement budget*. According to EPA's own guideline, all plants should receive an inspection at least every second year, more often if possible. Because they are labor intensive, inspections are costly and make a substantial share of the enforcement budget. The EPA budget devoted to enforcing environmental regulations of pollution is therefore a crucial determinant for how many inspections can be made in a given state. Hence, inspections are expected to increase with the EPA pollution enforcement budget. The budget is on federal level, whereas omitted variables are local factors. Therefore no correlation is expected between the budget variable and the unobserved omitted variables. To avoid feed-back effects between the EPA budget and violations, the budget variable is yearly. This way, any temporary regional changes in number of violations will not directly affect the budget variable. In case of a national trend change in the number of violations, the causality is ensured with a 12 months lag of the budget variable. Inspections are expected to increase with the EPA budget.

[25] Previous literature links appointing politicians to federal judges' behavior (Lyles, 1996 and Pinello, 1999).

[26] The caseload variable used in this study is estimated by Langpap and Shimshack (2010).

The second instrument for inspections is the *General tax rate of private income in the state where the plant is located*. The intuition is twofold. Firstly, a higher tax rate indicates a stronger public demand for governmental services, including enforcement of the CWA.[27] Secondly, a higher tax rate generates more revenue to the local government, enabling more inspections. One may also consider the share of democrats in the local government as a proxy for public demand of enforcement for environmental laws. In this study, the tax rate is preferable over the share of democrats because the variable is continuous and therefore carries more variation. Recalling that the omitted variables are plant specific characteristics, there are very few reasons to expect the income state tax rate to be directly correlated with the main model's error term. The tax rate variable is lagged 12 months, because the money used for inspections come from last years' tax revenue.

Instrument for *Fines issued by the EPA or state agency* Instrumenting the magnitude of fines issued by the EPA is challenging, since to a large extent the fine is determined by the plant's violation history and ability to pay the fine.[28] Ability to pay a fine is likely to be correlated with other plant specific characteristics for the plant, and therefore the instrument cannot be plant specific in order to pass the exogeneity criterion. A natural approach would be to predict today's fine by previous fines. This modeling approach is however unsatisfactory as a plant's ability to pay often is constant over time, and thus endogeneity would remain in the model.

Sanctioning a plant or issuing a fine is costly for the state agency because it's labor intensive and may also be time consuming to determine the fine amount for the specific plant. Sanctioning a plant may also be unpopular among local citizens if the sanction leads to local unemployment. Therefore, further enforcement in case of violations needs local support (or pressure). Assuming that environment health is a normal good, the demand for cleaner environment should increase as income rises in a developed country[29] and therefore, income per capita is expected to be positively correlated with the fines issued against plants in violation of the CWA. The *income per capita where the plant is located* is used as an instrument for *fine*. Income per capita is not likely to correlate with any of the possible omitted variables such as abatement technology and local industry governance of the plant, because the omitted variables are mainly plant characteristics whereas income level is derived from variables such as innovations and international trade, and short run

[27]There is a common view that, for low income tax states, other values may be given priority over the environment, such as protecting the free market and the "personal freedom".

[28]See section 2.1 for a full description on how the magnitude of the fine is determined.

[29]This reasoning doesn't refer to a certain level of GDP or the Environmental Kuznet Curve.

fluctuations depends on factors such as employment rate and the business cycle state.

4.3.4 Indirect deterrence variables

We know from previous literature that general deterrence, or indirect enforcement, is almost as effective as direct enforcement when it comes to influencing plants' compliance behavior to environmental regulation.

CS of other plants in the state where the plant is located As mentioned in the introduction, an indirect deterrence effect of CSs is theoretically expected. Because the number of CSs is small, the data don't allow for such an empirical investigation. For the same reason, the effect of neglecting the variable is expected to be very small.

Public enforcement of other plants in the state where the plant is located Two indirect deterrence variables are included in the model: *sanctions of other plant during the past 12 months in the state where the plant is located* and *total amount of fines during the past 12 months of other plants*. The variables are expected to be exogenous to the model because states do not typically have fixed budgets specifically for water enforcement and monitoring, but rather for environmental monitoring as a whole (Langpap and Shimshack, 2010). Thus, there is no direct plant level trade-off expected — public enforcement of one wastewater facility is not dependent on public enforcement of another. The data also show significant variation in public monitoring and enforcement levels across both time and space.

4.3.5 Additional control variables

Plant size As learned from previous literature, plant size is expected to matter for compliance status, larger plants emit more pollution and are therefore more likely to exceed their permitted amount of BOD. Plant size is measured as water outflow from the plant.

As mentioned in Section 4.2, the variable is expected to be exogenous to the model because a plant's size is often constant over a long period of time and therefore not sensitive to temporary changes in other plants' compliance status. Hence no reverse causality is expected. Secondly, correlation with any of the omitted variables is not likely since abatement technology, water price, local infrastructure and geographical factors may vary across plant sizes. Plant size is more likely to be dependent on the

the demand for water cleaning in the area where the plant is located, a parameter which is not expected to affect plants' compliance behavior directly.

Time, Seasonality and State effect From Section 4.1, we know that plant inspections decline, whereas CSs increase, over the sample period 1990-2000. Thus, controlling for time effects is warranted. Inspections declined non-monotonically and therefore time dummies are preferred over a linear time trend. Year dummies are chosen over month dummies because enforcement, as well as the influential environmental groups, have budgets according to fiscal year.

Streams carry more water in winter and spring which makes discharged pollution more easily diluted during these seasons. Also, the amount of treated water may vary with season. Seasonal dummies are included to account for these factors.

Public enforcement is formed, planned and implemented at the state level. State dummies are included to allow for cross-state differences in enforcement activity.

Since some of the unobserved effects occur at the plant level, one may wish to include plant level dummies. Plant level dummies are however not included in the study for two main reasons. Firstly, we want to control for enforcement and enforcement is determined on the state level. Secondly, none of the instruments is expected to correlate with the omitted variables. Together, this makes plant dummies excessive. Furthermore, recalling that the variation in data is limited by few CSs, plant dummies would reduce the variation even further, but without benefits.

4.4 Empirical model specification

The 2SLS-model consists of two parts: a first stage equation and a second stage equation. In the first stage equation, the instruments are regressed on the endogenous variable. The prediction of the endogenous variable is then included in the second stage as an exogenous variable, together with the rest of the explanatory variables. In this study, there are three first stage equations because there are three endogenous variables. The standard IV-approach is to estimate the 2SLS-model in one step using a preprogrammed method in econometric software such as Stata. If the sample sizes between the first stage and the second stage differ, the estimation is based on the observations that are shared for the first and the second stage estimation. This is unfortunate if the sample sizes differ substantially between the estimations, since valuable information will be lost in such cases. In the sample of this study, the first stage estimations contain more observations than in the main model estimation (the second stage) does.[30] Because citizen lawsuits are few, it's preferable to keep as much data as ever possible. Therefore, the 2SLS estimations are estimated manually in two steps. Although this approach makes better use of the data, which leads us closer to the true parameter values, the standard calculation of the second stage's standard errors are incorrect because it ignores the error that comes from using a predicted, rather than an observed variable for estimation (the predicted rather than observed endogenous variable). In an attempt to correct for the incorrect standard errors, boot strapped standard errors are estimated in the second stages.[31] As a sensitivity check of the standard errors, the second stages are also estimated with the simultaneous 2SLS estimation method. These results are discussed in Section 5.3 on sensitivity analysis, but are not part of the main results in Section 5.

4.4.1 IV-estimation methods: OLS, LPM and Probit

All first stages are estimated with OLS and robust standard errors. The second stage regressions are estimated with three different estimation methods: OLS, Linear Probability Model (LPM) and a Probit Model. The three estimation methods are selected as the main estimation methods for this study. One may however consider many more estimation methods, depending on the purpose of the estimation. Although not presented in the main results, the count violation variable (number of violations), will also be assessed in a count variable model, and a Logit model will be estimated for comparison with the Probit estimation, as a robustness check.[32]

OLS In the OLS model, the dependent variable *violations* is a count variable: the number of violations during the past 12 months, which makes the OLS model account

[30] There are more missing observations in the dependent variable in the second stage, than for the dependent variables in the first stages. The second stage dependent variable isn't needed in the first stage estimation.

[31] Hsu et al (1986) show the effectiveness of the bootstrap bias reduction method on 2SLS estimates using Monte Carlo studies. The effectiveness of boot strapped standard errors in IV-analyses has since then been confirmed many times, for instance by Chau (2014).

[32] A Poisson model will be estimated and, in case of overdispersion, a Negative binomial regression will also be estimated, and discussed in Section 5.3.1. In this study, the Probit model is used, but also the Logit may be considered for the same estimation purposes. The Logit model has the same properties as the Probit model, except for the underlying distribution, where the Probit has a normal distribution and the Logit has a logistic distribution. The two models generate very similar results, only the logistic has slightly flatter tails, i.e., the Probit curve approaches the axes more quickly than the Logit curve. The Probit model is chosen due to the Probit estimation being compatible with IV-estimation in pre-programmed softwares, which is used in the robustness check in Section 5.3.1.

for the additional effect of being sued more than one time during the past 12 months. The great benefit of the OLS model is its transparent estimation method and familiar interpretation of the coefficients as marginal effects.

LPM Since most plants sued during the past 12 months are only sued once, the violation variable is transformed into a binary variable in the next model to avoid bias from extreme plants with many lawsuits. Using the same estimation procedure as above on a binary dependent variable regression yields the LPM: a binary dependent model of the probability of a "successful" outcome, that is, Y=1 if the plant is in violation of the law, Y=0 otherwise. In the LPM model, the parameter estimate of a given variable is the change in the probability of success when changing that variable, holding all other factors fixed. As for any LPM model, the dependent variable may be predicted outside of the (0,1) range due to the underlying assumption of a Bernoulli distribution, hence a binary response model is also warranted.

Probit The Probit model is also a binary dependent model which models the probability of a "successful" outcome. But unlike LPM, which assumes strict linearity between the parameters, the Probit model has a standard normal cumulative distribution function which maps probabilities between 0 and 1. The distribution ensures predicted probabilities to always be within the probability range 0–1. Unlike in the LPM, the Probit coefficients cannot directly be interpreted as the marginal effect, because the coefficient is the partial effect conditional on all other explanatory variables. The standard interpretation method is to evaluate the marginal effect at the mean. However, setting each independent variable to its mean does not guarantee evaluation at the steepest part of the curve. In fact this only occurs if the dependent variable has a success probability of approximately 50%. From Section 4.1, we know that the variable *violations* is heavily skewed towards compliance, which makes this evaluation method inappropriate. Instead the alternative method "average marginal effects" will be estimated and interpreted.[33] In terms of marginal effects, the LPM and Probit are largely equivalent and should give similar results.

4.4.2 2SLS equations

Now follow the specifications of the three first stages; one first stage for each instrumented variable: citizen lawsuits, plant inspections and fine amount.

[33] One may also evaluate marginal effects at other percentiles. Due to space limitations, that will not be included in this study, but a calculation of such marginal effects are available upon request.

First stage equations:

$$CS_{it} = \alpha_0 + X_{it}\Pi + Z_{it}\theta + \zeta_{it} \tag{2}$$

$$IN_{it} = \beta_0 + X_{it}\Pi + Z_{it}\theta + \nu_{it} \tag{3}$$

$$FI_{it} = \gamma_0 + X_{it}\Pi + Z_{it}\theta + \eta_{it} \tag{4}$$

where CS_{it} is the number of citizen suits a plant has received during the past 12 months, IN_{it} is the number of inspections a plant has received by EPA (or state agency) during the past 12 months and FI_{it} is the magnitude of fines a plant has been issued by EPA (or state agency) during the past 12 months. α_0, β_0 and γ_0 are constants. The variable Z is the set of instrument,[34] and X represents a vector of all the additional explanatory variables discussed in Section 4.3. The subscripts indicate plant i at time t. The errors terms ζ_{it}, ν_{it} and η_{it} are unique for each first stage equation. The predicted dependent variables from the first stages are as a second step included in the main equation as explanatory variables.

The second stage equations: In the second stage, the predicted new variables obtained from Equations 2–4 are included in the second stage equation, Equation 5, together with all the other explanatory variables.

$$VI_{it} = \kappa_0 + CS_{it}\lambda_1 + IN_{it}\lambda_2 + FI_{it}\lambda_3 + X_{it}\Omega + \epsilon_{it}, \tag{5}$$

where VI is the violation variable and κ_0 is a constant. Again, the subscripts indicate plant i at time t. ϵ is an error term, estimated with boot strapped standard errors.

5 Empirical results

5.1 First stage determinants

Before presenting the main results from Equation 5, the determinants of the instrumented variables from Equation 2–4 are presented and discussed.

[34]The set of instrument used in a first stage is sometimes referred to as the "Excluded instruments"; the exogenous variables excluded from the main equation that are used as instruments for the endogenous variables in the first stage.

From the instruments, discussed in Section 4.3.3, three instrument sets are created. They form two just-identified models and one overidentified model. A just-identified model is a specification with as many instruments as endogenous variables, whereas the overidentified model contains more instruments than endogenous variables. Because bias increases with the number of instruments, a just-identified model has as little bias as possible. If the instruments are not weak, a just-identified model is median-unbiased (Angrist and Pischke, 2009). An overidentified model on the other hand may increase the explanation power of the instrumented variable, which may lead to a more precise second stage estimation. Furthermore, an overidentified model allows for testing of the exclusion restriction, although the test is not perfect. The overidentification test will be explained in section 5.3.2, together with a discussion on the test results.

The three chosen sets of instruments, Model 1, 2 and 3, are presented in Table 3. The top row shows the endogenous variables. The variables underneath are the corresponding instruments which constitute the instrument sets for Models 1–3.

Table 3: The instruments used for citizen suits, EPA inspections and EPA fines, respectively, in the three models considered.

	Citizen lawsuits	EPA inspections	EPA fines
Instrument set in Model 1:	GHP	Tax Rate	Income
Instrument set in Model 2:	GHP	EPA budget	Income
Instrument set in Model 3:	GHP and Caseload	Tax Rate and EPA budget	Income

The results from the first stage estimations are presented in Table 4. Each endogenous variable is regressed on its instrument set and on all the other exogenous variables from the main equation. The squared cells also help to indicate which instrument that constitutes the model. The coefficients are OLS estimates with robust standard errors.

Table 4: First stage OLS results: Determinants of citizen suits, public monitoring and enforcement regressions.

	Model 1			Model 2			Model 3		
Dependent variable in the first stage:	Citizen lawsuits	Inspections	Fines	Citizen lawsuits	Inspections	Fines	Citizen lawsuits	Inspections	Fines
Instrument for citizen lawsuits									
GHP	$\boxed{\text{-0.03***}}$	-0.55***	9637.29***	$\boxed{\text{-0.04***}}$	-4.85***	-1.40**	$\boxed{\text{-0.03***}}$	-6.35***	9564.12***
	(6.36)	(19.44)	(7.43)	(7.18)	(48.46)	-1.40**	(6.20)	(48.55)	(7.41)
Caseload	–	–	–	–	–	–	$\boxed{\text{-0.02***}}$	0.10	1531.07***
(Case per judge x 1000)							(5.56)	(1.63)	(4.14)
Instrument for inspections									
Tax rate	-0.000***	$\boxed{\text{0.002***}}$	-0.000***	–	–	–	-0.000***	$\boxed{\text{0.020***}}$	-14.610**
	(3.77)	(19.01)	(5.81)				(3.55)	(37.81)	(2.29)
EPA budget	–	–	–	0.000***	$\boxed{\text{0.003***}}$	-3.041**	-0.000	$\boxed{\text{0.002***}}$	1.997***
				(3.81)	(4.09)	(1.97)	(0.56)	(33.29)	(2.96)
Instruments for fines									
Income	0.000***	-0.000***	$\boxed{\text{0.129***}}$	0.000***	0.000***	$\boxed{\text{0.128***}}$	0.000***	-0.000***	$\boxed{\text{0.129***}}$
	(3.97)	(8.21)	(4.11)	(3.83)	(18.01)	(4.15)	(3.98)	(18.71)	(4.11)
Controls									
Flow	0.000	0.001***	60.71***	-0.000	0.008***	60.75***	-0.000	0.008***	-0.080***
	(0.85)	(14.31)	(5.97)	(0.91)	(32.97)	(5.97)	(0.86)	(32.95)	(0.17)
Other sanctions	0.000***	0.000**	2.829***	0.000***	-0.000	-2.784***	0.000***	-0.000***	-2.751***
	(12.13)	(2.22)	(3.18)	(13.59)	(0.41)	(3.14)	(11.80)	(4.72)	(3.14)
Other fines	-0.000***	0.000	-0.000***	-0.000***	0.000***	-0.003***	-0.000***	0.000***	60.724***
	(3.90)	(6.30)	(5.58)	(2.68)	(7.46)	(5.55)	(3.65)	(7.05))	(5.97)
Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-test of instruments	274.90	18.50	18.57	834.96	19.89	19.12	706.18	14.95	26.37
Prob > F	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
Nummer of observ.	105385	105385	105385	109988	109988	109988	105385	105385	105385
Adjusted R-squared	0.0084	0.0765	0.0171	0.0094	0.3603	0.0170	0.0087	0.3810	0.0171

Note: The first stage estimates are the first (out of two steps) in the 2SLS IV estimation. T-statistics are shown in parentheses. Superscripts *, ** and *** indicate statistical significance at the 10%, 5% and 1% rejection levels, respectively.

The first thing to note is that all instruments have their expected signs, and that all first stages' intended instruments (the coefficients within squares) are significant at the 1%-rejection level. This indicates that the instruments work as intended.

One potential problem with any IV method is a weak performance by the first stage; lower explanatory power in the first stage generates a less efficient 2SLS estimation. Here, the first-stage adjusted R-squareds vary between 0.0084 and 0.3810, where the former is considered as relatively weak and the latter as relatively strong, in a 2SLS first stage context. The lowest (adjusted) R-squared values are found in the regressions with CS as the dependent variable, and the strongest (adjusted) R-squared are found for the regressions with inspections as the dependent variable. Comparing the (adjusted) R squared for the just-identified models and the overidentificated model indicates that adding more instruments doesn't increase the explanatory power of the first stage estimation in this sample.

A rule of thumb in IV-analysis is that the instruments should have an F-statistic of at least 10 in order to ensure that the maximum bias in the IV estimator is less than 10% (Staiger and Stock, 1997). In this study, all F-statistics are larger than 10 and sometimes well beyond, see Table 4. Thus, despite low adjusted R-squared values for some of the first stage regressions, the large F-test statistics suggest that none of the instrument sets is weak, but rather strong.

5.2 Main results: second stage estimations

Here the main variable of interest is interpreted, the effect of CSs on plant violations. Also the effect of public direct and indirect enforcement and the control variables are interpreted. The first column in Table 5 contains all the explanatory variables. The first row indicates which estimation method is used, and the second row indicates which instrument set (forming Model 1–3) is used.

Table 5 shows a robust result for all variables across the three models (instrument sets) as well as over the three estimation methods. Citizen suits have a strong negative effect on plant's likelihood to be in violation, as well as a strong marginal reduction effect on violations. As expected, all public enforcement variables have a negative impact on violations, the plant size variable is negative and the fixed effects dummies are jointly significant.[35]

[35] Joint significance (F-statistics) for fixed effect dummy groups for Model 1–3, as well as all three estimation methods, are available upon request.

Table 5: Second stage regression results: the impact of citizen suits and public enforcement on plant-level violations.

Dependent variable: violation	OLS			LPM			Probit		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Direct Deterrence									
Instr. CS	-14.31***	-9.59***	-6.24**	-1.62**	-4.85***	-1.40***	-1.58***	-5.19***	-1.43**
	(-5.69)	(-2.91)	(3.95)	(2.74)	(4.20)	(2.32)	(2.34)	(4.12)	(2.64)
Instr. Inspections	-0.999***	-0.100***	-0.127***	-0.191***	-0.024**	-0.019***	- 0.205***	-0.026**	-0.020***
	(5.62)	(2.83)	(7.21)	(3.36)	(1.91)	(3.82)	(3.95)	(1.95)	(3.44)
Instr. Fines	-0.497***	-0.632***	-0.632***	-0.009***	-0.005	-0.004***	- 0.010***	-0.008	-0.001***
(x 1000)	(6.54)	(8.32)	(8.32)	(2.70)	(1.42)	(8.32)	(4.89)	(1.08)	(4.01)
Indirect deterrence									
Recent sanctions	-0.96	-3.31***	-3.31***	-0.85***	-0.12	-0.10***	-0.86***	-0.02	-0.10***
on others (x 1000)	(0.99)	(3.37)	(3.35)	(3.57)	(0.34)	(4.05)	(3.51)	(0.49)	(3.35)
Recent fines	-0.252***	-0.175**	-0.235***	-0.03***	-0.001	-0.026**	-0.04***	0.0078	-0.035***
on others (x 1000000)	(5.95)	(5.81)	(5.81)	(2.70)	(0.08)	(2.44)	(2.27)	(0.78)	(5.81)
Other controls									
Flow	0.003***	0.003***	0.004***	0.0000	0.001***	0.004	0.0000	0.0006***	0.0000
	(4.53)	(6.30)	(6.30)	(0.30)	(4.09)	(6.30)	(0.44)	(3.98)	(0.17)
Season FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yess
State FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Nummer of observ.	97732	97732	97732	97732	97732	97732	97732	97732	97732

Note: T-statistics are shown in parentheses. For the Probit estimations, average marginal effects are presented in the table. Superscripts *, ** and *** indicate statistical significance at the 10%, 5% and 1% rejection levels, respectively. The fixed effect dummy coefficients are not presented due to space constraints. Each dummy group is jointly significant.

The magnitude of the variables vary over Model 1–3, suggesting that the choice of instruments matter.[36] Therefore, one should interpret the exact magnitudes of the coefficients with caution. Instead of focusing on a specific number, the estimate

[36] When fines are regressed on violations as a binary variable (LPM and Probit), fines become insignificant in Model 2. In general, Model 2 generates more insignificant variables than the other models do. The instrument that makes Model 2 unique compared to the Model 1 and 3, is the EPA enforcement budget instrument. The instrument performs well in the first stages as well as in the sensitivity analysis (disregarding the Sargan test, presented in Section 5.3.2), however the instrument is more aggregated than any other instruments in this study (U.S. federal level and yearly). Reducing the variation in the dependent variable increases the risk for perfect prediction as well as insignificant coefficients, which is likely to be the explanation why Model 2 drops in significance in the LMP and the Probit estimations.

ranges within an estimation method are discussed here. The OLS estimations suggest that, a plant that has been sued by a private citizen during the past 12 months is expected to violate the law 6.24 to 14.31 times less than it otherwise would. Correspondingly, the LPM models suggest that the marginal probability to violate the CWA decreases by 140–485%. The average marginal effect estimated with the Probit model shows that one citizen suit is expected to decrease the number of violations by 1.43–5.19 violations. In the OLS and Probit estimates, Models 1 and 2 are significant at the 1%-rejection level and Model 3 at the 5%-rejection level, while in the LPM estimate, it is Model 1 which is significant at the 5%-rejection level and the other two at the 1%-rejection level. The marginal effects estimated with LPM and Probit are very similar for Model 1–3, as expected.

All OLS marginal effects are larger for Model 1–3, compared to the estimates in the LPM and the Probit estimations. The OLS estimate is the only method in which violations in the last 12 months is represented by a count variable (and not a binary). It is indeed expected that the marginal effects of the explanatory variables is more easily discernible once a trend in number of violations is allowed in the data. In general, Model 2 is less significant in the limited response model.

The effect of CSs is considerably larger than for the public enforcement variables.[37] Both sanctions of other plants and the magnitude of the fines other plants have to pay when found in violation, significantly reduce the likelihood of violations. The finding confirms previous research that an indirect deterrence effect exists, however the effect found in this study is smaller than previously research suggests.[38] All public enforcement (direct as well as indirect) variables are shown to be somewhat less strong than suggested in previous studies.[39] Because plants that are often in

[37]Compare the marginal effect: 6.24 to 14.31 violations/citizen suits and 0.10 to 0.999 violations/EPA inspection. The LPM model suggests a plant inspection during the past 12 months reduces the probability of a violation by 2-19%. The violation marginal reduction per $1000 fine is 0.497 to 0.632 in the OLS estimations. Increasing the fine by $1000 yields a violation likelihood reduction of 0.9% in Model 1, and 0.4% in Model 3, both significant at the 1%-rejection level in the LPM estimations.

[38]Gray and Shadbegian (2007) find that direct and indirect enforcement have about the same deterrence effect for a given plant, and that fines have the largest deterrence effect. This study indicates however that direct enforcement has considerably larger impact on plant violations than indirect enforcement.

[39]Magat and Viscusi (1990) find that a given facilitys probability to violate the law is about twice as high if the facility has not been inspected in the previous quarter, whereas this study finds a likelihood reduction of 19% at most, if the plant has been inspected during the past 12 months. The difference may partly be explained by the different time intervals investigated; it's reasonable for the effect of an inspection to be stronger closer to the inspection date. But also Deily and Gray (2007) find a strong deterrence effect of plant inspections: they find that the probability that a

violation are at risk of being sued by a private citizen, ignoring CSs will most likely bias the estimated effect of public enforcement upwards.

5.3 Sensitivity analysis

5.3.1 Model specification

The estimated results suggest that citizen suits have a strong negative effect on plant violations, and an effect of public enforcement in line with previous literature is found. This section will examine the robustness of the results.

In an attempt to rectify for incorrect standard errors in the two-step 2SLS approach, boot-strapped standard errors are used in the second stage estimations. Although this remedy is supported in the literature and is shown to be asymptotically unbiased, the sample is not infinitely large and incorrect standard errors may be present. Therefore, the one-step (OLS) 2SLS approach is estimated as a robustness check. The standard errors in the two-step estimation are allowed to show some deviations from the one-step estimation due to the different sample sizes assessed. The one-step estimations show slightly lower significance than obtained with the boot-strapped standard errors shown in Table 5 for Model 1–2. The CS variable is insignificant in the third model using the one-step approach.[40] This difference may be an indication of incorrect standard errors but may also be explained by the reduced data sample, which the variable CS is especially sensitive to.

As a robustness check of the choice of estimation methods, two additional models are estimated: the Logit model, in order to compare the results from the probit estimations, and a count model as a complement to the OLS estimates, for which the dependent variable is a count variable. Estimating the models with Logit gives similar results as with the Probit model presented in Table 5.[41] Estimating the models with Poisson estimation indicates overdispersion and therefore a Negative Binomial (NB)-regression is estimated.[42] The result shows no major differences in

plant would comply with the environmental law is about 33% higher if the plant has been inspected during the past two years. That is a longer period of time investigated compared to this study, still the estimated deterrence effect is larger.

[40] CS has a marginal deterrence effect of –7.77 and is significant on the 5% rejection level in Model 1. In Model 2, the marginal effect of CSs is –8.72 and is significant on the 10% rejection level.

[41] In the Logit estimation (with boot-strapped errors), CS has an average marginal effect of -1.51 in Model 1, -5.09 in Model 2, and -1.32 in Model 3, significant at the 5% rejection-level in Model 1 and 3, and at the 1%-rejection level in Model 2.

[42] Overdispersion refers to greater variability (statistical dispersion) than the expected mean in a given data sample. Under such circumstances, the Negative Binomial NB-regression is preferable

significance compared to the OLS estimates.[43]

As a final robustness check, the time dummies are replaced by time trends in case month trends exist in the data. The results with time trend are consistent with the results in Table 5 (only some coefficients are larger in magnitude), suggesting that year dummies are warranted.

5.3.2 Potential violations of the exclusion restriction

As previously mentioned, exogeneity of the instruments cannot be tested. With an overidentificated model, it's however possible to conduct an overidentification test, among which, the "Sargan test" is standard. It exploits that the coefficients should not differ significantly when they are regressed separately on the endogenous variable.[44] The test is however not perfect, since it's impossible to estimate unobserved effects in practice. In this study, Model 3 is an overidentified model.

The null hypothesis of exogenous instruments is rejected on the 1% rejection level,[45] suggesting that we cannot reject the possibility that one or more instruments are endogenous to the model. It is possible that plants are affected by the general local demand for a clean environment. If they are, then the GDP per income variable and the state income tax variable, which are both believed to increase with people's demand for a cleaner environment, may be correlated with omitted factors that determine plant pollutions and, in turn, violations of the CWA. These scenarios assume that plants are sensitive to the local demand for a certain level of clean environment. However, according to Becker (1968), as well as the empirical findings on plant compliance, plants are not sensitive to private enforcement without penalties.

Furthermore, if the state income tax is correlated with the state corporation tax level, and the corporation tax affects the plant's profitability which in turn affects the choice of abatement technology — which is expected to be a relevant omitted variable for plant violations — then state income tax would fail the exclusion

over the Poisson regression. The likelihood test is chi-square distributed and has a chi-square test statistic of $6.8 \cdot 10^4$, which corresponds to a p-value of 0.000.

[43]In the NB-reg estimation, the CS variable have the following coefficients –29.03, –7.11 and –16.55, for Model 1–3 respectively. All estimates are significant on the 1%-rejection level.

[44]Since the 2SLS is estimated manually in two steps in this study, also the Sargan test (which is predefined in most econometric software) has to be computed manually. It is calculated by first estimating the second stage equation and obtaining the 2SLS residuals. The R-squared obtained from this regressions is multiplied by the number of observations and is chi-square distributed; $nR^2 \sim \chi^2_q$. Under the null hypothesis, all residuals are uncorrelated with the instruments. If the null is rejected, the test suggests that at least one of the instruments fail the exclusion restriction. For a full description of the test method, see Sargan (1958).

[45]For all three estimation methods, the Sargan χ^2 is 9.77 and Prob $> \chi^2$is 0.008***.

restriction. This assumes that profitability would determine abatement technology and not, as we would expect according to the literature, the net gain of implementing the technology.

IV estimations with slight violations of the exogeneity assumption can cause severe bias in IV coefficients. The bias is amplified if the instruments are weak (Bound, Jaeger, and Baker, 1995). Therefore it's important to remember that bias may be present, yet presumably small, when interpreting the coefficients in Table 5. Noteworthily, the Sargan test is an imperfect test and the possible bias is not aggravated, since the instruments are strong.

6 Conclusions

By investigating citizen suits as a possible determinant of plants' compliance behavior in a 10 year monthly panel data set, together with public enforcement variables, this study aims to answer two questions.

The first aim of this study was to investigate whether plants change their behavior after they have been sued in a federal district court by private citizen. The empirical results suggest that this is indeed the case. Plants that have experienced a private citizen suit during the past 12 months, are five times less likely to be in violation with the CWA. Although citizen lawsuits are relatively few compared to inspections and sanctions by EPA, the marginal deterrence effect of a CS is large and is likely to be the explanation to the seemingly irrational behavior among plants to comply under weak incentives. In fact, the perceived cost of non-compliance may never have been reduced when public enforcement started to decrease in the 1990s, since CSs started to increase during the same time. This study offers one explanation: that the incentives in fact might be higher than anticipated, given the possibility of a CS.

The second aim of this study, was to compare plants' behavior change after they have been sued by a private citizen, to the plant behavior change after they have received public enforcement. In line with previous literature, public enforcement is shown to have both a direct and indirect deterrence effect on violating the CWA, however the probability reduction for a plant to be in violation of the CWA after a citizen suit is substantially larger compared to when a plant has been subject to public enforcement. Furthermore, by acknowledging the effect of CSs, this study suggests that the deterrence effect of EPA inspections and the marginal effect of EPA fines are smaller than previously found.

In order to capture the causal effect of citizen suits on violations, this study takes a (fixed effects 2SLS) IV-approach, with in total five instruments. The result is robust to three different IV estimation methods (OLS, LPM and Probit) and three different sets of instruments. Citizen suits are instrumented with judicial instruments recognized in previous literature, exploiting that all citizen suits have to go through a federal district court. Public enforcement are instrumented with EPA's enforcement budget and two instruments capturing the public demand for enforcement of environmental regulation. The three instruments for direct public enforcement have never been used before, and are a contribution of this study. All instruments behave as expected and show strong first stages. Furthermore, all instrumented variables behave as expected.

The large deterrence effects of CSs is a remarkable result in magnitude and significance. Although the effect of a CS was expected to be larger than for public enforcement, a five-time reduction in the likelihood to violate the CWA after a CS during the past 12 months, is a very strong deterrence effect. This result opens up for possibilities, how should this information be transformed to policy recommendations?

Already in 1985, Fadil (1985) argues for the potential of CSs, and how CSs can be a solution when public enforcement is lacking and no prospects of increased enforcement budget is in sight. Pollution and lacking environmental law enforcement, are however problems facing more countries than the U.S., where this study is conducted. Furthermore, since a government may not always act in the public's interest, the possibility for the people to pursue a lawsuit may be important from a democratic point of view.[46] It might thus seem natural to suggest adapting a system for CSs to other contexts.

Recalling the unique features of CSs, compared to public enforcement, that CSs are: unpredictable, very costly for the plant, and crowding in public monitoring, call for some thought whether CSs are desired to take a larger part in a society, before a promotion of its large marginal effect. EPA's enforcement objective is to push noncompliance plants into compliance. Private enforcers on the other hand, will claim for the largest possible fine. Therefore, CSs may lead to market distortions. Also, private enforcers may choose plants arbitrarily, as well as making mountains out of molehills. If plants would be subject to more private enforcement, plants would have to spend substantial time and money in defense, an inefficiency which would lead to additional market distortions.

As opposed to the U.S., Sweden has chosen the "ombudsman" way to go in order

[46] The empirical results found in this study may theoretically be generalized to other environmental laws in the U.S., although water violations are easier to prove for an environmental group than for instance air pollution violations, owing to quick dissolution with large quantities of air. The results may also be generalized as a possible outcome for other countries, given that they have strong environmental groups, or other powerful agents, that are able to prosecute against violators.

to take charge of private interests to enforce the environmental laws. Instead of bringing a citizen suit directly towards the polluter, the private citizen turns to the ombudsman to make a complaint, and then the ombudsman prosecutes against the violator. To assess which way is better, more democratic and more economically efficient, more studies have to be done on the effects of citizen suits. This study has contributed one new piece to the puzzle – a citizen lawsuit directed towards a plant in violation is found to have a large direct deterrence effect on future violation. But what about the perceived marginal cost of CSs' existence? What are the indirect deterrence effects of CSs? This study shows the potential in further exploring the impact of citizen suits.

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A Appendix

A.1 Data Sources

Table 6: Data sources

Variables included in the data analysis:	**Data source:**
Citizen suits, GHP score, case load of judges	Langpap and Shimshack, 2010 (US Department of Justice)
Plant inspections, monetary fines, violations, plant size	Langpap and Shimshack, 2010 (EPAs Permit Compliance System)
Income per capita	Bureau of Economic Analysis
State-local tax burden on private income	Census Bureau's data base providing historical statistics on State Tax Collections
EPA enforcement budget	EPA's webpage, see historical data