ORIGINAL ARTICLE

The threat of regulatory environmental inspection: impact on plant performance

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Abstract   The expected costs of violating a regulation would typically increase if the probability of regulatory inspection increases. Thus, changes in the anticipated threat of inspection should affect firm compliance. Like environmental protection agencies in several other countries, the Norwegian agency typically emphasizes compliance with institutional requirements (e.g. firm-internal routines and auditing systems) rather than emission caps. Using a panel dataset of polluting Norwegian plants, we find that the threat of inspection significantly reduces the probability of serious violation. However, emissions are not significantly affected. We point at various reasons for the regulator to emphasize institutional requirements, but we also argue that the lack of effect on emissions encourages the agency to review the pros and cons of the common emphasis on institutional requirements over emissions.

1 Introduction

Passing anti-pollution laws is only the first step in securing compliance with direct environmental regulations. The existence of effective monitoring and enforcement policies is crucial to reach required environmental standards. Inspections are an important feature of most countries' monitoring policies. According to Becker's (1968) theory of rational crime, a firm complies if the expected (marginal) costs of complying are

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lower than the expected (marginal) penalty of violating. In the simplest framework, compliance costs, the penalty and the probability of detection are constant. Hence, after detection the firms simply pay the levied penalties and continue violating. Thus, an inspection has *no* effect on the compliance status of a firm. However, in a more realistic setting, the regulator may change the inspection probability according to certain criteria, like previous performance of the firm. Such an increase in expected inspection probability (i.e. inspection threat) will then (*ceteris paribus*) raise the expected penalty and thereby reduce violations. The present study investigates empirically whether such an increased threat of inspection improves plant performance.

Although the number of theoretical studies of monitoring and enforcement of environmental regulations is growing (see Cohen 2000 for a review), empirical contributions are scarce. While several related aspects of monitoring and enforcement policies have been studied (see e.g. Magat and Viscusi 1990; Nadeau 1997; Dasgupta et al. 2001; Helland 1998; Rousseau 2005), we are only aware of a few papers that include effects of the expected inspection probability on compliance status or emissions. Eckert (2004) finds a positive relationship between expected inspections and compliance. She considers compliance with the petroleum storage regulations in a province in Canada. Gray and Deily (1996) investigate compliance and enforcement of environmental regulations in the U.S. steel industry, and obtain similar results. Laplante and Rilstone (1996) observe plants in the pulp and paper industry in Quebec. They find a negative relationship between the probability of inspection and water emissions relative to the cap. Earnhart (2004a,b) examines discharges relative to the cap from municipal wastewater facilities in the state of Kansas. The effects of expected inspection probability on emissions are not fully clear in his studies, and when accounting for unobserved plant heterogeneity in Earnhart (2004a), increased inspection threat does not reduce emissions relative to the cap.[1] Shadbegian and Gray (2005) apply a dataset of U.S. pulp and paper, oil and steel plants, and find no negative effect of inspection threat on emissions.[2]

The present study investigates the effects of the threat of inspection on both violations and emissions to air. We use a panel data set covering inspections, violations, annual emissions and plant characteristics of about 90 polluting Norwegian manufacturing plants from 1990 to 2004. In Norway the compliance status of a firm does not relate to emissions only, but also to extensive institutional requirements set in the emission permit. For example, it is an almost universal requirement that internal routines and audit systems for environmental surveillance must be properly implemented. Such regulatory focus on institutional aspects is not a peculiarity of the Norwegian regulatory system (Nyborg and Telle 2006; Russell 1990). It is important to note that any violation of the conditions of the emission permit, including both emission caps and institutional requirements, is a violation of environmental regulations. Moreover,

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[1] Earnhart (2004a) seems to be the only one of these studies that includes regressions accounting for unobserved plant heterogeneity. Dasgupta et al. (2001) account for unobserved plant heterogeneity and tend to find a negative, but small, effect of the probability of inspection on emissions of polluters in China. Moreover, the effect is not statistically significant at the 5 percent level for all pollutants. Their setting, however, is a bit different since it is not illegal to exceed the "emission caps" in China (Dasgupta et al. 2001, p.490).

[2] We are not aware of any previous quantitative study considering effects of inspection threats on emissions or violations of environmental regulations using data from Europe.

although the regulator monitors both emission caps and institutional requirements during inspections, it seems to emphasize the institutional requirements (Nyborg and Telle 2006). Hence, there may be weaker incentives to comply with emission caps than with institutional requirements. This inspection policy might therefore result in less violation despite unchanged or even increasing emissions.

The next section includes an overview of the theoretical background. In Sect. 3 we sketch the monitoring and enforcement activities of the Norwegian Pollution Control Authority (NPCA). Our econometric approach is discussed in Sect. 4. The approach is based on two-step estimation, where, in the first step, we estimate the probability of inspection. Predictions from this first step estimation are applied as our measure of inspection threat. In the second step, we estimate the effect of the inspection threat on plant compliance and emissions. In Sect. 5 we present the data, and in Sect. 6 we present the results from the regressions and investigate the robustness of the results. We conclude in Sect. 7.

2 Theoretical background

Following the ideas of Becker (1968), a firm will comply with environmental regulations if it is profitable to do so.[3] Let C denote compliance costs, π the fine for violators and q the probability of inspection and detection. Then a (risk neutral) firm, i, will violate as long as $C_i > q_i \pi$. Assuming that q is a constant, an inspection in period t will have no influence on the future compliance status of the firm. However, if q in period t is a function of variables like whether there was an inspection or not in period $t-1$, or whether violations have been detected or suspected in period $t-1$, then an inspection in period t can influence the future compliance status of the firm. Moreover, if q is an increasing function of previously detected violations, an inspection in the present period that reveals violations will increase the inspection probability in the next period and thereby also the expected fine. Then the compliance costs of some firms may turn lower than the expected fine, making it profitable to become compliant. Hence, an increase in a firm's expected inspection probability (i.e. the inspection threat) can reduce violations.

However, how much to emit is not a dichotomous choice, like comply vs. violate. For emissions the idea of Becker implies that the plant will emit until the expected marginal benefit of higher emissions is equal to the expected marginal costs of abating. Then, if the emission caps impose actual restrictions on the plant's emissions and the penalty function is smooth and continuously increasing in excess emissions, standard theory predicts that plants will emit in excess of emission caps. Hence, increased inspection threat, which raises the expected costs of excess emissions, would reduce emissions.

In this setting one may correctly argue, however, that increased inspection threat will not necessarily raise compliance in a model where the penalty function makes a discrete jump at the cap: firms may face a penalty of zero if emissions are at the

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[3] See e.g. Heyes (1998) for an introduction to some applications of Becker's (1968) ideas in the economics of environmental regulations.

cap, but a high penalty if emissions are just above the cap. The optimal emission level may now no longer be to emit in excess of the cap, but rather just *at* the cap. In this particular case it is straightforward to show that optimal emissions may be unaffected by a change in inspection threat (cf. Fig. 1 and footnote 6).

As we shall see, whether or not theory predicts inspection threat to affect emissions is important for the interpretation of our empirical results. In the following, we will therefore show that the inspection threat *does* affect optimal emissions even in a model where the penalty makes a jump at the cap:[4] assume a model where the firm cannot fully control its emissions, meaning that it may emit more (or less) than the cap by mistake. Then it can be optimal for the firm to take on additional abatement costs to reduce the probability of having to pay the high above-cap-penalty by mistake. In this case a higher inspection threat amplifies the firm's need to reduce the likelihood of having to pay the above-cap-penalty. Thus, when a firm cannot completely control its emissions, optimal emissions will fall as the inspection threat increases, even in the presence of a discrete jump in the penalty at the cap.[5]

Let $q \in (0,1]$ denote the probability of regulatory inspection, Y the emission level of the firm and s the maximum allowed emission level. Then, the penalty function, Π, can be defined as,

$$\Pi(Y) = \begin{cases} 0, & Y \leq s \\ q\pi(Y), & Y > s, \pi' > 0 \end{cases} \tag{1}$$

We allow the penalty to make a descrete jump at the cap $\left(\lim_{Y \to s^+} \pi(Y) > 0 \right)$. Let x denote abatement undertaken by the firm. The costs of such abatement can then be defined as follows,

$$C = c(x), \quad c' > 0$$

Assume that the plant cannot completely control its actual emissions, for example as employees commit errors, machinery fail or the contents of pollutants in inputs varies. In particular, for any given level of abatement the resulting emission level is to some extent random. Let V be a random variable with expectation zero and finite variance. Then we can let the emission function comprise a deterministic ($y(x)$) and a random (V) component,

$$Y = y(x) + V, \quad y' < 0 \tag{2}$$

The problem of the firm is to minimize expected overall costs,

$$\min_x \{E[\Pi] + C\}$$

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[4] As we shall see in Sect. 3, the sanctioning policy of the NPCA indicates that the penalty facing violating firms in our sample does *not* make a discrete jump at the cap.

[5] Previous theory on effects of errors on compliance and enforcement is limited, see Polinsky and Shavell (2007) for a brief overview. Our approach here is somewhat similar in spirit to the approach of Craswell and Calfee (1986). See also Segerson (1988) and Rousseau (2008).

This yields the first order condition,

$$\frac{dE[\Pi]}{dx} = -c' \tag{3}$$

Let $p(V)$ denote the probability density function of V, assumed to be continuous (and everywhere differentiable), and let v denote an outcome of V (and l and h denote the minimum and maximum value of v, possibly infinite). The derivative of the expected penalty is then (see Appendix A),

$$\frac{dE[\Pi]}{dx} = qp(s - y(x))\pi(s)y' + qy' \int_{s-y(x)}^{h} p(v)\pi'(y(x) + v)dv > 0 \tag{4}$$

The first term in (4) captures the effect of a change in x on the probability that above-zero penalties occur. The second term captures the effect of a change in x on the size of every above-zero penalty. Since both terms are negative, the overall expression is negative: The expected penalty falls when abatement increases.

Inserting (4) into (3), the first order condition (3) says that abatement should increase until the benefits of the decline in the likelihood of punishments and the decline in the size of punishments equal the marginal cost of abatement.

Now consider this situation in Fig. 1. We see that $E\Pi + C$ (the continuous and thin line) does not exhibit a discrete jump despite Π exhibiting such a jump: Taking the expectation of Π has smoothed the function. This implies that $E\Pi + C$ will eventually approach C from above as x increases. As x declines from high values towards values in the vicinity of x^* (where x^* yields emissions exactly at the cap by definition) the shape of $E\Pi + C$ will be sensitive to the curvature of the probability density function,
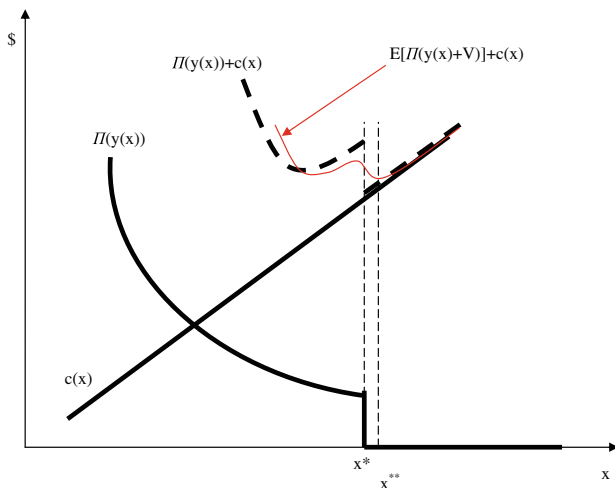

Fig. 1 Higher inspection threat increases abatement even in the presence of a discrete jump in the penalty function

and more than one local minimum may exit. Given the way $E\Pi + C$ is drawn in Fig. 1, we see that the optimal $x(x^{**})$ is above x^{*}.[6]

In addition to increasing abatement, we also see that changes in q (recall from (1) that Π is increasing in q) affect the optimal choice of x, not only for $x < x^{*}$, but also for $x \geq x^{*}$. Moreover, it can be shown that the derivative of the optimal x with respect to q is positive (see Appendix A); implying that expected emissions will drop as the probability of inspection increases.

To conclude, within this model with errors it *cannot* be maintained that an increase in the threat of inspection has *no* effect on emissions due to possible discrete jumps in the penalty function. As long as the firm cannot fully control its emission level, increased inspection threat reduces expected emissions.

3 A sketch of the Norwegian regulatory system[7]

Any emission from a manufacturing plant that harms or may harm the environment is prohibited in Norway. However, the Norwegian Pollution Control Authority (NPCA) may grant emission permits. The permits contain two types of regulations. First, it contains emission caps that specify e.g. maximum emissions per unit of time, per unit of production and/or per unit of spill water. It may also specify maximum production levels. Unfortunately, the heterogeneity of these quantitative regulations across pollutants and plants makes it very difficult to compare actual emissions with emission caps for different plants. Therefore, data indicating to what extent the plants meet their emission caps are unavailable for quantitative analysis.[8] Second, the permits contain institutional requirements, like a variety of qualitative requirements concerning institutional aspects within the plant. Thus, a violation will occur if there is a violation of at least one of these two types of regulations.

Inspections are the most important instrument in NPCA's monitoring of plant compliance. Both actual emission levels and institutional requirements, like routines and general maintenance of equipment, are subject to investigation during the same inspections. However, the NPCA seems to emphasize institutional requirements, rather than actual emissions (Nyborg and Telle 2006). As the overall purpose of the NPCA is to protect the environment, it may appear more effective to devote attention to environmentally detrimental emissions. The following seems to be the main arguments applied by regulators for focusing on institutional requirements. First, according to emission permits, emissions may legally fluctuate during a day, week, or year; therefore, to measure emissions at the time of an inspection may say little about the firm's actual compliance with the regulations. Second, emissions may be closely related to the technology used by the firm or the maintenance and condition of the abatement equipment. Third, the purpose of the inspections is not only to verify past and/or current

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[6] This result is not general as it depends on the properties of the probability density function (see Craswell and Calfee 1986). Note that x^{*} would be optimal in the case of no errors, and that this optimal x would be unaffected by inspection threat.

[7] See Nyborg and Telle (2006) for a more careful description of the practice of the NPCA.

[8] For each plant, however, emission caps are formulated, and they are subject to investigation during inspections.

violations, but also to prevent future environmental damage; and if a firm does not comply with the institutional requirements (e.g. maintaining an internal environmental audit system) this is believed to increase the probability of such future damages.

So, although there are formal restrictions on the emissions,[9] in practice, however, it is not obvious that sanctions should always be expected for emissions in excess of the caps since the NPCA possesses limited monitoring and enforcement resources and tends to focus on serious violations. Or putting it differently, it does not appear obvious that all emission caps are actually binding. Further, if they are not binding, the theory does not predict any effect of expected inspection frequency on emissions. On the other hand, however, if the caps were not binding, then why would the NPCA bother to incorporate them in emission permits, or require firms to self-report emissions? And why would the NPCA devote any effort to actual emissions during inspections if excess emissions are not sanctioned? While these questions make it appear unreasonable that at least excessive above-cap emissions will not meet sanctioning, we will return to the question of whether the emission caps are actually binding when interpreting and discussing the results in Sect. 7.

The inspection frequency of the NPCA follows a scheme that dictates the regular inspection frequency of groups of plants. This scheme depends on the *risk class* of the plant: When a plant is granted a permit, which is ordinarily valid for 10 years, the NPCA puts the plant in one of four risk classes; with risk class one embracing plants whose operation is considered potentially highly environmentally dangerous. The potentially least dangerous plants are placed in risk class four, etc. Since the operation's potential environmental harm is closely related to its potential to violate regulations, the risk class of the plant also carries information on its likelihood of violation. The NPCA could deviate from the inspection frequency dictated by the scheme for several reasons. First, after an inspection, the inspection officer makes an evaluation of the need for future inspections based on an overall judgment of his observations during the inspection. Normally, when non-minor violations are observed or suspected, the inspection officer recommends inspections more frequent than the scheme.[10] Second, information from the plant, the police or the public may also result in more than regular inspections.[11]

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[9] As mentioned, any emissions from plants are prohibited without a permit. Also, every permit includes a general statement that even emissions allowed by the permit ought to be reduced if practicable.

[10] This practice carries some resemblance to the approach suggested by Harrington (1988). He applies a model were the inspection frequency depends on the compliance history of the firm, and this history-dependency enables the regulator to reach a compliance target at lower costs. As the compliance rate approaches one, however, this cost reduction only appears under the unrealistic assumption that a firm violating once will face perpetual surveillance.

[11] One may find it peculiar that the current emissions of the plant are not an important determinant of NPCA's inspection frequency. The NPCA may receive reports of emissions that cause instant damage to the local environment, like emissions of very hazardous pollutants, and such reports can trigger an inspection. However, high emissions of the pollutants considered here are unlikely to be an important determinant of inspection frequency except through bad inspection evaluations. The reason is, first, that emissions of these pollutants are closely related to energy consumption. Thus, excessive and uncontrolled emissions are typically unlikely. Second, and generally, emissions of pollutants that are not very hazardous are not easily observed without an inspection. Hence, the NPCA does not know the current emissions of the plant, and it is therefore unable to base its current decision to inspect on the plant's current emissions.

NPCA has numerous ways of imposing costs on violators. Although severe criminal penalties are formally available, NPCA seldom prosecutes plants. Some coercive fines are issued annually, but few are collected since plants normally comply before the time limit specified in the decision necessary to impose coercive fines. The inspections of the NPCA do not only serve monitoring purposes, but can also be considered to carry elements of enforcement and sanctioning. The reason is, first, that the costs (which can be considerable[12]) of NPCA of performing an inspection have to be covered by the inspected plant. Second, an inspection can also involve considerable work on the plant's own staff. Hence, since the NPCA increases the inspection frequency if it observes or suspects unsatisfactory performance, this practice could reasonably be considered as sanctioning of non-compliant plants. Nyborg and Telle (2006) document that even minor violations result in the NPCA imposing sanctions costly to the firm, but these costs can be very small (like firm staff having to respond to letters from the regulator or having to pay external-consultants for providing additional documentation, etc.). Roughly, it seems like the NPCA carefully adapts the seriousness of its sanction to reflect the seriousness of the detected violation.

4 The econometric approach

When estimating the effect of the inspection threat on violations and emissions we first need an operational measure of inspection threat. To estimate this effect, we apply so-called two-step estimation. In the first step, we estimate the probability of inspection, which, according to the inspection policy of the NPCA, depends on several variables like the risk class of the firm and compliance status observed by NPCA in previous inspections. Then we use these results to predict individual probabilities of inspection, and use these predictions as our measure of inspection threat. In the second step, we estimate violation and emissions on the inspection threat. This two-step approach is similar to the approach taken by e.g. Earnhart (2004a), Eckert (2004) or Gray and Deily (1996).[13]

Consider the following first-step relation,

$$\Pr(Inspection_{i,t} = 1) = \Delta \left(\alpha + PreviousPerformance_{i,t-1}\beta + X_{i,t}\gamma + \eta_i \right) \quad (5)$$

where the variable $Inspection_{i,t}$ is a dichotomous variable set to one if plant i was inspected in period t (otherwise zero). The vector $PreviousPerformance_{i,t-l}$ represents the evaluation of the plant if inspected in the previous period (otherwise zero) and the vector X includes observable plant specific characteristics, like the risk class or the size of the plant. η captures unobserved time-invariant plant specific characteristics, like plant location, sub-industry or vulnerability of the plant's primary recipient, or time-

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[12] Nyborg and Telle (2006) report the median costs of the most frequent types of inspections of firms in risk class 1 to be NOK 120,000 (about 15,000 Euro).

[13] Such two-step estimation yields consistent estimators under standard conditions (see e.g. Cameron and Trivedi 2005; Greene 2000; Murphy and Topel 1985). Note the resemblance of this approach to traditional instrumental variable methods, applied in e.g. Shadbegian and Gray (2005).

invariant elements of plant technology, vintage, management, employee motivation and education, etc.

The parameters in (5) can be estimated consistently by OLS (with fixed effects) if the error term is uncorrelated with the right hand side variables. Since it is a priori not completely clear what variables to include in X, there is a possibility that this assumption does not hold. There is at least one reason why this concern should not be over-emphasized, though. As long as the possibly erroneously omitted variables are relatively time-invariant, they would to a large extent be controlled for by the plant specific effects (η).

Although applying OLS on (5) would produce consistent estimates under standard assumptions, such a linear probability model is not efficient and it is likely to produce predicted values of the probability of inspection that are above one or below zero. Thus, to avoid non-sense probability predictions we apply maximum likelihood estimation based on the logit model with random effects (see e.g. Baltagi 2001; Arellano and Honore 2001 or Hsiao 1992). In Sect. 6.3 we investigate whether our main results are robust to some other estimation methods and model specifications.

Consider the following second-step relations,

$$\Pr(Violation_{i,t} = 1) = \Delta \left(a + bInspectionThreat_{i,t} + X_{i,t}d + v_i \right) \quad (6a)$$

$$E_{i,t}^{p} = a + bInspectionThreat_{i,t} + X_{i,t}d + v_i + u_{i,t} \quad (6b)$$

where $E_{i,t}^{p}$ denotes emissions of pollutant p for plant i in year t. We have separated the second step relation (6) into (6a) and (6b) to make it clear that, since $Violation_{i,t}$ is dichotomous and $E_{i,t}^{p}$ is continuous, we estimate (6a) by a non-linear probability model and (6b) by a linear regression model. The threat of inspection ($InspectionThreat_{i,t}$) is operationalized as the predicted probability of inspection based on (5); and X is the set of observed plant characteristics affecting the dependent variable ($E_{i,t}^{p}$ or $Violation_{i,t}$). Again, we control for unobserved time-invariant plant specific effects (v_i). $u_{i,t}$ is the error term with mean zero.

The coefficient of main interest in (6) is b, which captures the effect of the threat of inspection on violation or emissions of the plant. Since we assume that $Inspection$-$Threat_{i,t}$ is consistently estimated from the first step, applying OLS (with fixed effects) on (6) would result in a consistent estimate of b provided that the right hand side variables are uncorrelated with the error term. Again, however, we apply maximum likelihood estimation based on the logit model with random effects when the response variable is whether the plant is in violation or not (6a) (see e.g. Baltagi 2001; Arellano and Honore 2001 or Hsiao 1992). When the response variable is emissions (6b), we apply GLS with random effects (see e.g. Greene 2000, Ch. 14, or Earnhart 2004a). In Sect. 6.3 we investigate whether our main results are robust to other estimation methods and model specifications.

There are some concerns regarding this two-step econometric strategy. First, the previous studies seem to disregard unobserved plant specific effects in the first step, cf. η in (5). This strong assumption enables application of a standard logit or probit model where the probability of inspection can be predicted straightforwardly. We, however, allow for unobserved plant heterogeneity in the first step: The unobserved

plant specific effects are represented by random effects in the logit model (see e.g. Baltagi 2001).[14] As these unobserved effects cannot be predicted, we need to make assumptions about these effects to predict the probability of inspection needed in (6). In our main approach we assume that these effects are zero. Thus, it is not clear that our approach is preferable to the approach of previous studies. In Sect. 6.3, we therefore investigate whether our main results are similar under both of these assumptions regarding unobserved plant heterogeneity in (5). There we also investigate the importance of these heterogeneity assumptions by checking whether our main results are maintained in an OLS estimation with fixed effects. Applying the OLS with fixed effects enables predictions of the inspection probability without having to impose the assumption that there is no unobserved plant heterogeneity when estimating or when predicting.[15] Moreover, in Appendix B we outline an alternative econometric specification where we can identify the effect of the threat of inspection on the outcome variable ($Violation_{i,t}$ or $E_{i,t}^{p}$) using the estimate of β from (5). This specification does not require predictions of the probability of inspection from (5), and we thereby circumvent the problem of retrieving predictions of the unobserved plant heterogeneity. As shown in the appendix, applying this econometric specification yields the same main qualitative results as those evolving from the econometric strategy presented in the main body of the text.

Second, it is important for the credibility of our identification strategy that the vector *PreviousPerformance* in (5) is correctly excluded from (6);[16] i.e., except for the effect through changes in inspection probability, *PreviousPerformance* must *not* be a direct determinant of violation or emissions in (6). Judging from the inspection strategy of the NPCA, it does not seem likely that being inspected in previous period, and the outcome of this inspection, should be an important determinant of the plant's current outcome—except for the effect that this inspection has on the probability of future inspection (captured in (5)). On the other hand, one might imagine that the plant could, e.g., have learned how to improve performance (or mislead the regulator) if recently inspected. Such a learning effect, however, appears much more likely for smaller plants with low-skilled management than for the big, technologically skilled, professionally managed and mostly internationally competing manufacturing plants considered here. Nevertheless, we investigate this empirically. If *PreviousPerformance* were incorrectly excluded from (6), this would introduce an omitted variable bias. Thus, if these variables were incorrectly omitted from the second stage model, we would expect our estimate of *b* to be sensitive to inclusions of these variables. We investigate this empirically in Sect. 6.3.

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[14] As discussed by Arellano and Honore (2001), little is known about the properties of the estimators of nonlinear panel models with lagged dependent variables. Thus, in Sect. 6.3 we also investigate robustness of the results to models without plant specific effects.

[15] As mentioned above, although consistency is secured under standard assumptions, there are several drawbacks of OLS in this setting.

[16] If OLS were applied in the first and second stage estimations, not excluding an element of *PreviousPerformance* in (6) would unable identification of *b* since *InspectionThreat* would be a linear combination of the other right hand side variables. If non-linear procedures are applied, *b* could still be (weakly) identified by functional form.

Another concern is that *PreviousPerformance* must be an important determinant of NPCA's inspection frequency.[17] This can be evaluated by testing the significance of the estimate of β in (5). Even under standard conditions, which are sufficient to render consistent the estimate of b in (6), applying a standard estimation procedure would provide incorrect estimates of the standard errors since $InspectionThreat_{i,t}$ is predicted (Murphy and Topel 1985). We correct the standard errors as described by Greene (2000) and Hardin (2002).

5 Data and dataset issues

To analyze what determines the inspection frequency of plants, and the effect of inspection threat on plant performance, we use a plant level panel data set with annual observations for 1990–2004. The dataset consists of plants holding emission permits and belonging to the following four industries: chemicals, basic metals, pulp and paper and other non-metallic minerals (NACE-codes 24, 27, 21 and 26 respectively). Hence, the sample is not representative for Norwegian manufacturing plants: The (potentially) most polluting plants and industries are over-represented.

Statistics Norway and the NPCA publish emission data for the manufacturing industries in Norway; see e.g. Flugsrud et al. (2000). From this inventory, we apply plant specific annual emissions for 1990–2004 of greenhouse gases, acids, nmvoc-equivalents (ozone precursors) and particles. The emission data initially originates from plants' self reports. However, the quality of the reported data is carefully investigated, e.g. by comparing the figures with data on energy or input consumption originating from census data. When inconsistencies are observed, officers at the NPCA or the plant are normally consulted, and the figure most consistent with the energy or input data may be chosen. This procedure secures that the plant incentives to under-report emissions is unlikely to seriously bias the data.[18] In the analysis, we normalize emissions by production (in fixed prices), and hereafter refer to emissions relative to production of greenhouse gases, acids, particles and nmvoc-equivalents as, respectively, *GreenhouseGases*, *Acids, Particles* and *Nmvoc*.

The NPCA keeps a database including every one of its regulatory inspections of these plants. From this source, which includes various aspects of each inspection, like date and the officer's evaluation, we construct two annual variables for the period 1990–2004.

First, we construct an indicator of the plant's compliance status in year t. Data on the compliance status of plants is inherently difficult to obtain. Eckert (2004) uses violations observed in inspections. Magat and Viscusi (1990) get data on emissions in excess of the permitted levels from plants' self-reports, and this seems to be the case for Gray and Deily (1996) too. Regardless of the data source being self-reports or inspections, firms can have incentives to misreport or to conceal violations; see

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[17] Or, in the IV-terminology, the instruments must be *relevant*. In the same terminology, the previous paragraph discussed whether the instruments are *valid*.

[18] If reliable data is not available, emissions may be set to missing. Hence, observations of emissions are missing for pollutants or plants in some years.

theoretical studies by e.g. Heyes (1994) or Kambhu (1989). Thus, all studies in this field, including the present, have no choice but to rely on data on reported or detected violations, rather than data on actual violations.[19]

As mentioned in Sect. 3 above, data on whether emissions are in excess of permitted levels is unavailable in Norway, and therefore we follow the approach of Eckert (2004) and rely on violations detected in inspections. This has the advantage of circumventing the potential problem arising from plant incentives to underreport violations in self-reports or surveys. This has the disadvantage, however, of restricting the observations of violations to plants actually inspected. This selection problem involves an important concern: Our estimated effect of threat of inspection on violation may *only* be valid for plants with high probability of inspection. This argument would have greater impact, however, if we could indicate some reasonable and important causes for the threat of inspection to affect outcome differently for these two groups of plants (Wooldridge 2002, Ch. 17). Nevertheless, we should be cautious in generalizing the results based on the violation variable to plants with low threat of inspection.

Another impact of the violation variable only being observed if the plant is inspected, is that we cannot carry out the first and second stage of the two-stage estimation procedure on one identical dataset; since there would be no variation in the dependent variable of the first stage regression as all plants would have been inspected in period t. Thus, we utilize all information in the data by performing the first stage regression on all available observations (both plants inspected and not inspected in period t). In the second stage, only a sub-set of observations utilized in the first stage, i.e. the observations of actually inspected plants, can be applied. This feature of the dataset disables application of estimators that cannot be based on actual two-step estimations.[20]

NPCA's post-inspection evaluations contain a separate category for serious violation(s). In the present study we set the annual violation variable to one if a serious violation was detected at least once during year t. This variable is hereafter referred to as *Violation*.

Second, indicators of previous performance, i.e. the *PreviousPerformance*-vector in (5), are constructed. An indicator of bad performance (*BadPerformance*) is set to one if the evaluation of the last inspection in year t states that serious violations were observed and/or that the plant should be inspected more frequent than what is dictated by the inspection scheme, either because non-minor violations are observed or because violations are suspected.[21] Moreover, an indicator of normal performance (*NormalPerformance*) is set to one if the evaluation of the last inspection in year t states that there is little need for more frequent inspection than what is dictated by the inspection scheme; typically because non-minor violations are neither observed nor suspected. Finally, we include an indicator that the evaluation of the

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[19] Throughout the paper we employ the colloquial expression "probability/threat of violation" instead of the more precise "probability/threat of detected violation."

[20] This implies that estimators often interpreted in a two-stage setting, like IV-estimators, but that are in fact one-step estimators, are not applicable.

[21] After an inspection the NPCA routinely notifies the inspected plant of detected violations. The standard wording in these notifying letters depends on the observed compliance status. Formally, these letters have no legal implications, they only *remind* the plant of obligations and penalties.

last inspection in year t is missing (*PerformanceMissing*). Hence, this set of dummies (*BadPerformance, NormalPerformacne, PerformanceMissing*) tends to serve as indicators of NPCA's expectations of future violations. Along with the risk class of the plant (included as dummies) these variables should capture much of NPCA's expectations about future violations of institutional requirements and above-cap emissions.

Unfortunately, firm-level data on sanctioning is not available. However, we are not aware that the size or frequency of formal sanctioning has changed radically during the period studied in this paper, and therefore it does not appear very unreasonable that year dummies, along with industry dummies or time-invariant plant specific effects, could be an adequate way to control for possible changes in sanctioning. Nevertheless, we return to this data limitation when discussing our results in the concluding section.

Census data on manufacturing plants are available from Statistics Norway. This extensive database includes a variety of annual plant specific data; in particular, we use employment (*Employees*), investments, production and industry.

The production variable that we apply is current value of production deflated to 1992. A proxy for capital stock is created as follows. First current values of gross investment are deflated to 1983 (using a price index for investments in manufacturing industries). Then the capital stock for 1983 is set to the mean of gross investments in 1983–1985 (or earliest available years) divided by the depreciation rate (set to 0.08, cf. Todsen 1997). Finally, deflating the capital stock of the previous year and adding gross investments yields the capital stocks at the beginning of the present year. This variable is referred to as *Capital*.

Combining non-missing observations from these sources yield an unbalanced panel of 1,359 observations over the period 1990–2004. The number of plants varies from a minimum of 88 in 1994 to a maximum of 93 in 2001. Eighty-three plants are present in all years, while 14 enter and 7 exit over the period. This dataset is used for the inspection regression (5), where the number of observations is reduced to 1238 since some explanatory variables are lagged one period. For reasons explained above, subsamples of the dataset applied in the first stage regression are used for the violation and emission regressions (6). Table 1 provides summary statistics for the variables in the dataset.

6 Results

6.1 Probability of inspection (fist step estimation)

As mentioned, the NPCA has an inspection frequency scheme depending mainly on the risk class of the plant (*RiskClass2$_i$*, *RiskClass3$_i$*; excluding firms that are in *RiskClass1$_i$*), as well as the previously detected performance of the plant (*Bad Performance$_{i,t-1}$*, *NormalPerformance$_{i,t-1}$*, *PerformanceMissing$_{i,t-1}$*; excluding non-inspected group). In addition, we control for observed plant heterogeneity and changes over time by including *Employees$_{i,t-1}$*, *Capital$_{i,t}$*, industry-dummies (Pulp and paper excluded) and year-dummies (1991 excluded). Moreover, the included random effects account for unobserved time-invariant plant heterogeneity. The dependent

Table 1 Variables and summary statistics for 1991–2004

Variable name	Obs	Mean	St. dev.
Inspection	1238	0.55	
NormalPerformance	1238	0.45	
BadPerformance	1238	0.05	
PerformanceMissing	1238	0.05	
Capital	1238	338.47	472.19
Employees	1238	260.33	225.36
Pulp and paper industry	1238	0.11	
Chemicals industry	1238	0.34	
Non-metallic minerals industry	1238	0.18	
Basic metals industry	1238	0.37	
Risk class 1	1238	0.56	
Risk class 2	1238	0.23	
Risk class 3	1238	0.21	
Risk class 4	1238	0.00	
Violation	680	0.04	
GreenhouseGases	938	0.31	0.43
Acids	853	0.05	0.07
Nmvoc	945	1.25	2.00
Particles	1062	0.39	0.84

variable, $Inspection_{i,t}$, is one if the plant was inspected at least once in year t; zero otherwise.

The results of the maximum likelihood estimation of this logit model with random effects are presented in the first column of Table 2. In accordance with the inspection scheme of the NPCA, the probability of an inspection is significantly[22] lower for plants in risk class 2 and 3 than for plants in risk class 1. Again as expected, if the plant performed well in the last inspection of the previous year ($NormalPerformance_{i,t-1}$), the probability of inspection in the subsequent year decreases. A test of the hypothesis that the year dummies are jointly insignificant can be rejected (chi-square test), and the estimated year dummies indicate that the probability of inspection increases at the very beginning of the period and then starts to decline. The probability of an inspection increases with *Employees* and *Capital*, but *Employees* and *Capital* are only jointly significant. Basically, these results are as one would expect given the inspection policy of the NPCA discussed above.

6.2 Effect on performance of inspection threat (second step estimation)

We now turn to the main issue of the paper: Does the inspection threat affect the compliance status and emissions of the plant? As mentioned, the NPCA's focus on institutional requirements may impact plant incentives so that compliance can improve despite unchanged emissions.

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[22] Unless otherwise stated, we refer to an estimate as significant if the *p*-value is below .05. All estimations are performed using STATA9.

Table 2 Main estimation results

Dep. Var:	First stage	Second stage				
	Inspection	Violation	GreenhouseGases	Acids	Nmvoc	Particles
NormalPerformance$_{i,t-1}$	−1.273 (7.21)**					
BadPerformance$_{i,t-1}$	−0.442 (1.28)					
PerformanceMissing$_{i,t-1}$	−0.812 (2.50)**					
InspectionThreat$_{i,t}$		−7.430 (2.57)**				
Capital$_{i,t}$	0.000 (1.14)	−0.001 (1.05)	0.028 (0.77)	0.015 (1.67)*	0.128 (0.70)	0.227 (1.41)
Employees$_{i,t-1}$	0.001 (1.46)	0.004 (1.97)**	0.000 (0.01)	−0.000 (0.05)	0.000 (0.32)	0.000 (0.05)
Chemicals$_i$	−0.510 (1.56)	−2.101 (1.71)*	−0.000 (0.68)	−0.000 (4.73)**	−0.001 (1.30)	−0.001 (2.47)**
Non-metallic minerals$_i$	−0.610 (1.60)	−1.564 (0.90)	0.293 (2.38)**	0.055 (2.62)**	1.452 (0.67)	0.439 (1.96)**
Basic metals$_i$	−0.308 (0.99)	−1.682 (1.53)	0.893 (5.67)**	0.067 (2.65)**	2.675 (0.63)	0.645 (2.71)**
Risk class 2$_i$	−1.265 (5.04)**	−2.999 (1.63)	0.290 (2.41)**	0.044 (2.26)**	0.955 (0.64)	0.415 (2.02)**
Risk class 3$_i$	−1.999 (7.33)**	−3.777 (1.94)*	−0.518 (4.21)**	−0.054 (2.54)**	−1.889 (0.70)	−0.524 (2.83)**
Yr1991			−0.130 (0.96)	0.022 (0.57)	−1.052 (0.45)	−0.300 (1.43)
Yr1992	0.178 (0.50)	0.076 (0.05)	0.033 (1.63)	−0.010 (2.06)**	−0.154 (1.51)	−0.081 (0.89)
Yr1993	0.002 (0.01)	−19.649 (0.00)	0.042 (2.07)**	−0.009 (1.76)*	−0.140 (1.36)	0.006 (0.06)
Yr1994	0.707 (1.91)*	1.494 (1.03)	0.025 (1.21)	−0.018 (3.66)**	−0.267 (2.54)**	−0.075 (0.81)
Yr1995	0.444 (1.22)	0.469 (0.28)	0.012 (0.59)	−0.019 (3.96)**	−0.319 (3.10)**	−0.064 (0.71)
Yr1996	0.472 (1.32)	1.052 (0.69)	−0.000 (0.01)	−0.026 (5.27)**	−0.461 (4.49)**	−0.012 (0.13)
Yr1997	0.261 (0.73)	1.550 (1.10)	0.005 (0.24)	−0.027 (5.47)**	−0.422 (4.10)**	−0.064 (0.71)
Yr1998	−0.530 (1.50)	−20.224 (0.00)	−0.008 (0.40)	−0.030 (6.04)**	−0.444 (4.23)**	−0.089 (0.95)
Yr1999	−1.331 (3.71)**	0.541 (0.32)	0.020 (0.90)	−0.029 (5.35)**	−0.427 (3.75)**	−0.170 (1.68)*
Yr2000	−0.629 (1.75)*	0.821 (0.55)	0.024 (1.16)	−0.030 (6.06)**	−0.380 (3.58)**	−0.155 (1.66)*
Yr2001	−0.717 (2.03)**	1.128 (0.73)	0.018 (0.84)	−0.034 (6.62)**	−0.464 (4.30)**	−0.190 (1.99)**
Yr2002	−1.024 (2.88)**	0.286 (0.16)	0.019 (0.85)	−0.033 (6.08)**	−0.425 (3.77)**	−0.186 (1.88)*
Yr2003	−0.797 (2.24)**	−0.170 (0.10)	0.005 (0.24)	−0.037 (6.99)**	−0.522 (4.70)**	−0.245 (2.50)**
Yr2004	−1.657 (4.43)**	−0.373 (0.20)	−0.000 (0.01)	−0.033 (5.61)**	−0.614 (4.99)**	−0.219 (2.03)**
Constant	2.028 (4.54)**	1.508 (0.54)	0.048 (0.45)	0.050 (2.62)**	0.839 (1.25)	0.278 (1.12)
Observations (i, t)	1238 (94, 14)	680 (94, 14)	938 (72, 14)	853 (65, 14)	945 (72, 14)	1062 (83, 14)
R^2			0.38	0.33	0.23	0.12

(*) and (**) indicate significance at the 10 and 5 percent level, respectively. Absolute value of z statistics in parentheses. A test of the hypothesis that NormalPerformance, BadPerformance and PerformanceMissing are jointly zero can be rejected ($\chi^2(3) = 53$, p-value <0.000)

In relation (6) our main interest is how *InspectionThreat* affects the response variable. We also control for observed plant heterogeneity and changes over time by including $RiskClass2_i$, $RiskClass3_i$ (excluding $RiskClass1_i$ group), $Employees_{i,t-1}$, $Capital_{i,t}$, industry-dummies (pulp and paper excluded) and year-dummies (1991 excluded). Moreover, the included random effects account for unobserved time-invariant plant heterogeneity, like plant location or sub-industry, or time-invariant elements of plant technology, vintage, management, employee motivation and education, etc. The dependent variable is either the dichotomous variable indicating violation (*Violation*), or the emissions relative to production of the four pollutants (*GreenhouseGases*, *Acids, Particles* and *Nmvoc*).

The results of the maximum likelihood estimation (logit with random effects) of the violation relation (6a) are presented in the second column of Table 2.[23] While the capital stock (*Capital*) of the plant does not appear an important determinant of violation, the likelihood of violation increases with the labor stock (*Employees*). Although not statistically significant, violations do also tend to be less likely for plants in risk classes 2 and 3 than for plants in risk class 1, and tend to be more likely in the Pulp and paper industry compared to the three other industries. There is no clear pattern over time.

Our main interest, the effect of the inspection threat on violation (b), is, as expected, negative. It is also statistically significant. The estimated coefficient of -7.43 corresponds to a mean marginal effect of -0.176. The elasticity of violation with respect to threat of inspection is about -3. Thus, a one percent increase in the threat of inspection yields three percent lower likelihood of violation, which seems a substantial effect. This result indicates that the inspection policy of the NPCA improves compliance, which can be taken to represent a reassuring evaluation of the performance of the inspection policy of the NPCA.

We now turn to the results of the estimation of the effect of the inspection threat on emissions. The results of the GLS estimation (with random effects) of the emission relations (6b) are presented in columns 3 to 6 of Table 2. Emissions are generally lower for plants in risk classes 2 and 3 than for plants in risk class 1 and tend to decline with *Employees*. Emissions are also generally lower in the Pulp and paper industry, compared to the other three industries. Maybe except for *GreenhouseGases*, there is a tendency for emissions to decline over time.

Contrary to expectation, the inspection threat has a positive effect on emissions of all pollutants. However, as the estimated effect is not statistically significant for any of the pollutants, we conclude that the inspection threat does not affect the emissions of the plants. Hence, the result does not confirm the a priori expectation that the inspection policy of NPCA lowers emissions. Before we discuss possible interpretations, reasons and implications of this in the next section, we consider robustness.

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[23] As indicated in Sect. 4, standard errors of the second step estimations in the paper are corrected for the fact that the threat of inspection is a prediction from the first step estimation.

6.3 Some investigations of robustness

Here we investigate the robustness of our two main results, i.e., first, that the threat of inspection reduces the likelihood of violation, but, second, that this threat does not reduce emissions. As mentioned, there are some important concerns regarding our identification strategy; first, the *NormalPerformance*, *BadPerformance* and *PerformanceMissing* variables must be clearly significant determinants of NPCA's inspection frequency (cf. Staiger and Stock 1997 for a discussion of problems related to weak instruments). From the test of the joint significance of *NormalPerformance*$_{i,t-1}$, *BadPerformance*$_{i,t-1}$ and *PerformanceMissing*$_{i,t-1}$ reported in Table 2, it is clear that this is indeed the case.

Second, these variables must not be incorrectly excluded from relation (6). If they were incorrectly excluded, the estimated effect of inspection threat (b) on the response variable in (6) would be biased. Such a misspecification can be considered an omitted variable problem, and, thus, if these variables were incorrectly omitted from the second stage model, we would expect our estimate of b in (6) to be sensitive to inclusion of these variables. In Table 3 we report the results of including these variables, and a related one, in the second stage regressions. All variables included in the second stage regressions reported in Table 2 are also included here, but only the estimates of b and of the variables relevant for the current discussion on the exclusion restrictions are reported in the table. From Table 3 we see that including in the second stage a dummy being one if the plant was inspected in the previous period (otherwise zero) increases the estimate of b in the violation relation somewhat. However, this dummy variable is not significant. Moreover, we see that the estimate of the effect of inspection threat is almost unaffected by including *NormalPerformance*$_{i,t-1}$, *BadPerformance*$_{i,t-1}$ and *PerformanceMissing*$_{i,t-1}$. Also, none of these three variables are individually significant, and a test of the joint significance of them also fails. Summing up, there appears to be little support for a concern that our exclusion restrictions are very implausible.

Unobserved plant heterogeneity is controlled for in all regressions reported in Table 2. Results reported in Table 4 investigate whether our two main results are robust to model specifications not accounting for plant specific effects. At the top of Table 4 we report the estimated effect of threat of inspection on violations or emissions (b) when the first stage model accounts for random effects, while such effects are not accounted for in the second stage regression. Then Table 4 reports the results when the first stage model does not account for plant specific effects, while such effects are, or are not, accounted for in the second stage model. The general impression is that our two main results are not particularly sensitive to these changes in model specification. Finally, we see that our two main results also hold when applying a probit model with random effects, as well as in OLS models with and without fixed effects.

7 Concluding discussion

We have investigated the effect of inspection threat (i.e. predicted probability of inspection) on the likelihood of violation and emissions using data of about 90 Norwegian manufacturing plants from 1990 to 2004. The regression analysis reveals that a higher

Table 3 Robustness to inclusion of the variables originally excluded from the second stage regressions

	Dependent variable									
	Violation	Greenhouse Gases	Acids	Nmvoc	Particles	Violation	Greenhouse Gases	Acids	Nmvoc	Particles
$InspectionThreat_{i,t}$	−7.157	0.133	0.059	0.845	1.282	−10.868	0.103	0.027	0.348	0.45
	(0.85)	(1.07)	(1.82)*	(1.23)	(2.22)**	(1.91)*	(1.27)	(1.40)	(0.84)	(1.22)
Additional variables included in second stage regression										
$NormalPerformance_{i,t-1}$	−0.132	0.031	0.012	0.188	0.28					
	(0.06)	(0.71)	(1.41)	(1.12)	(0.76)					
$BadPerformance_{i,t-1}$	−22.278	0.016	0.003	0.044	0.078					
	(0.00)	(1.17)	(1.36)	(0.35)	(1.15)					
$PerformanceMissing_{i,t-1}$	0.326	0.028	0.009	0.139	0.14					
	(0.18)	(0.89)	(0.58)	(0.91)	(1.89)*					
$Inspected_{i,t-1}$						−1.019	0.021	0.004	0.061	0.062
						(0.69)	(1.03)	(0.73)	(0.60)	(0.67)
Test of joint significance of the included variables (chi2-stat)	0.17	1.41	2.25	1.35	4.09					

(*) and (**) indicate significance at the 10 and 5 percent level, respectively. Absolute value of z-statistics in parentheses. All controls included in second stage regressions reported in Table 2 are also included here, but they are not reported

Table 4 Robustness of estimated effect of threat of inspection (*b*) to accounting or not for plant heterogeneity, as well as to estimation method

Regression model	First stage	Second stage	Dependent variable				
			Violation	GreenhouseGases	Acids	Nmvoc	Particles
	Logit with random effects	Logit without plant specific effects	−6.54 (2.57)**				
		OLS without plant specific effects		−0.10 (1.01)	0.00 (0.24)	−0.19 (0.37)	0.15 (0.71)
	Logit without plant specific effects	Logit with random effects	−9.08 (2.58)**				
		GLS with random effects		0.04 (0.80)	0.02 (1.73)*	0.17 (0.74)	0.27 (1.38)
		OLS with fixed effects		0.04 (0.81)	0.02 (1.73)*	0.16 (0.75)	0.26 (1.36)
		Logit without plant specific effects	−7.92 (2.58)**				
		OLS without plant specific effects		−0.13 (1.07)	0.00 (0.18)	−0.27 (0.44)	0.16 (0.61)
	Probit with random effects	Probit with random effects	−3.76 (2.79)**				
		GLS with random effects		0.03 (0.71)	0.02 (1.61)	0.12 (0.63)	0.22 (1.32)
	OLS with fixed effects	OLS with fixed effects	−0.19 (2.75)**	0.01 (0.43)	0.01 (1.21)	0.05 (0.19)	0.13 (0.83)
	OLS without plant specific effects	OLS without plant specific effects	−0.24 (2.66)**	−0.18 (1.45)	0.01 (0.33)	−0.25 (0.39)	0.15 (0.53)

(*) and (**) indicate significance at the 10 and 5 percent level, respectively. Absolute value of *z*-statistics in parentheses. All controls included in second stage regressions reported in Table 2 are also included here, but they are not reported

inspection threat significantly reduces the likelihood of violation. The estimated effect of inspection threat on the likelihood of violation appears substantial, as a one percent increase in the inspection threat is estimated to yield a three percent reduction in the likelihood of violation. However, we do not estimate an expected negative effect of inspection threat on emissions; in general the threat of inspection has a positive but insignificant effect on emissions. Thus, we find no empirical support for the hypothesis that the inspection policy of the NPCA reduces emissions.[24]

It is often argued that unlike criminal activities, violations of environmental regulations may typically be inadvertent rather than willful, especially after initial compliance has been achieved (see e.g. Harrington 1988, p. 32). Violations of environmental regulations can result from a combination of several stochastic events, such as variation in input quality or breakdown of abatement equipment combined with weak firm-internal routines and auditing systems, rather than deliberate acts. Still, firms have choices of maintenance, operation and surveillance of abatement equipment, and these choices can strongly affect the likelihood and magnitude of violations. It thus appears reasonable that firm dilligence would be greater the greater the costs of violations. Moreover, if violations are closely related to the implemented equipment of the firm, it seems important to monitor, regulate and enforce the installation, operation, maintenance and firm-internal auditing of this equipment. Thus, regulators may have ample reason for the common practice of emphasizing compliance with institutional requirements (e.g. operation and maintenance of abatement equipment, firm-internal routines, auditing systems) rather than emission caps.

As always, the empirical results are contingent on the applied model specifications, the institutional settings, the actual dataset, etc. Given the regulatory policy of the NPCA, the lack of effect on emissions may not be very surprising. Like environmental protection agencies elsewhere (Nyborg and Telle 2006; Russell 1990; Rousseau 2007), the NPCA puts emphasis on institutional requirements rather than emissions. Doing so, the NPCA may provide incentives for the plants to put more effort in improving performance in these areas rather than in reducing emissions. Hence, from the monitoring and enforcement activity of the NPCA, the plants may learn that there are none or very lenient sanctioning of excess emissions of some pollutants.[25] This *might* indicate that it is not appropriate, as we have done here, to assume that the regulations of emissions impose actual restrictions on plant emissions. If the emissions caps are superfluous in the sense that they are set higher than the emissions the plant would have had in the case of no regulations, then there is no reason to expect

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[24] Not finding an effect of inspection threat on emissions is not unambiguously in conflict with previous studies. Most studies find a negative relationship between emissions relative to the cap and previous inspection or the probability of inspection (Laplante and Rilstone 1996; Magat and Viscusi 1990), but these do not account for unobserved plant heterogeneity. When Dasgupta et al. (2001) and Earnhart (2004a) control for plant specific effects, their results are not so clear. In Dasgupta et al. (2001) the effect of the probability of inspection is not significant at the 5 percent level for all pollutants, and Earnhart (2004a) does not find that higher probability of inspection significantly reduces emissions (relative to cap). Moreover, Shadbegian and Gray (2005) investigate the effect of inspection threat on emissions (relative to production) of several pollutants, and they do, like us, not find a negative effect.

[25] Another possibility is that inspection threat increases *reported* emissions, while it may still reduce actual emissions. If so, we would estimate a positive effect of inspection threat on (reported) emissions, although actual emissions might in fact have remained unchanged or even declined.

increased inspection threat to reduce emissions. The possibility that emissions caps are not binding can only be ruled out by documenting that marginal abatement costs are positive;[26] which is a challenging task left for future research.

The dataset is not as comprehensive as one could wish for, and data on more plants and more industries would be important for making more general assessments. In particular, data on the sanctioning of individual plants is not available. Like us Shadbegian and Gray (2005) do not find a negative effect of inspection threat on emissions. However, they do, as one would expect, find a negative effect of enforcement activities directed towards each plant (penalties, notice of violation, etc.). Thus, and since various monitoring and enforcement activities are likely to impact firm behavior differently (see also e.g. Rousseau 2007), having data that does distinguish between inspection threat and such direct sanctioning activity would enable an important check of the robustness of the results of the present analysis. Despite these caveats, we think the present study can provide an interesting point of departure for regulatory agencies, as well as researchers, when discussing various aspects of inspection policies, in particular the common policy-emphasis on institutional requirements.

Appendix A

The expected penalty can be written as,

$$E[\Pi] = \int_{l}^{h} p(v)\Pi(Y)dv = \int_{l}^{s-y(x)} p(v)\Pi(Y)dv + \int_{s-y(x)}^{h} p(v)\Pi(Y)dv$$

Here we note that the prior integral is zero since $\Pi(Y)$ is zero (follows from (1) and (2)). Thus, we have,

$$E[\Pi] = q \int_{s-y(x)}^{h} p(v)\pi(y(x) + v)dv$$

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[26] Although data on marginal abatement costs is unavailable, firms' expenditures on environmental protection appear to be non-negligible. In 2002, Statistics Norway asked a sample of firms in manufacturing industries about their environmental protection expenditures (see Hass 2004). All sub-industries report positive costs, and firms in pulp and paper and non-metallic minerals report environmental expenditures of about 2 percent of total input costs (incl. personnel costs), and end-of-pipe-investments of 11–22 percent of overall gross investments.

The derivative of the expected penalty with respect to x is then (using Leibnitz' formula) given by (4).[27]

We now show that the derivative of the optimal x with respect to q is positive. Inserting (4) into (3), the first order condition (3) becomes,

$$qp(s - y(x))\pi(s) y' + qy' \int_{s-y(x)}^{h} p(v)\pi'(y(x) + v)\, dv = -c'$$

Differentiating this first order condition with respect to x and q yields,

$$\frac{dx}{dq} = \frac{-y'\left[\pi(s)p(s - y(x)) + \int_{s-y(x)}^{h} p(v)\pi'dv\right]}{\frac{d^2 E[\Pi]}{dx^2} + c''}$$

By noting that the second order condition for the minimization problem is $\frac{d^2 E[\Pi]}{dx^2} + c'' > 0$, it is clear that the overall expression is positive.

Appendix B

Here we outline an alternative econometric specification where we can identify the effect of the threat of inspection on violations and emissions without having to use predictions of the probability of inspection from (5). Thus, we can allow for unobserved plant heterogeneity and simultaneously circumvent the problem of retrieving predictions of the unobserved plant heterogeneity. To do this, we operationalize the threat of inspection as the log-odds ratio of the probability of inspection.

The logit models of relation (5) and (6a) with random effects can be written as follows

$$\Pr(Inspection_{i,t} = 1) = \frac{1}{1 + \exp\left(-\alpha - PreviousPerformance_{i,t-1}\beta - X_{i,t}\gamma - \eta_i\right)} \tag{A1}$$

$$\Pr(Violation_{i,t} = 1) = \frac{1}{1 + \exp\left(-a - I_{i,t}b - X_{i,t}c - v_i\right)} \tag{A2}$$

where $I_{i,t}$ captures the firm's anticipated inspection threat. In the main body of the text we have assumed that firms respond to the predicted probability of inspection, but our qualitative results would be unaffected by applying any alternative measure that is monotonically increasing in this predicted probability. The odds ratio measures the probability of inspection relative to the probability of no inspection. The log-odds

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[27] We allow for a slight misuse of notation as $\pi(s)$ now denotes $\pi(s + e)$, where e is a positive and infinitesimal number.

Table A1 Main results of the estimated effect of threat of inspection on emissions and the probability of violation applying the alternative econometric specification

	Dependent variable				
	Violation	*GreenhouseGases*	*Acids*	*Nmvoc*	*Particles*
$PreviousPerformance_{i,t-1}\hat{\beta}$	−1.42	0.00	0.00	0.01	0.03
	(2.73)**	(0.49)	(1.21)	(0.32)	(0.81)
Observations (i, t)	680	938	853	945	1062
	(94, 14)	(72, 14)	(65, 14)	(72, 14)	(83, 14)

(*) and (**) indicate significance at the 10 and 5 percent level, respectively. Absolute value of z-statistics in parentheses. Standard errors are not corrected for the fact that β has been estimated in the first step. All controls included in second stage estimations reported in Table 2 are also included in the estimations in this appendix, but they are not reported

ratio is monotonically increasing in the probability of inspection, and applying it is convenient from a formal point of view. Following from (A1) the log-odds ratio is

$$I_{i,t} = \ln \frac{\Pr(Inspection_{i,t} = 1)}{1 - \Pr(Inspection_{i,t} = 1)} = \alpha + PreviousPerformance_{i,t-1}\beta + X_{i,t}\gamma + \eta_i$$

Inserting this into (A2) yields

$$\Pr(Violation_{i,t} = 1) = \frac{1}{1 + \exp\left(-\hat{a} - bPreviousPerformance_{i,t-1}\beta - X_{i,t}\hat{c} - \hat{v}_i\right)} \quad (A3)$$

where

$$\hat{a} = a + \alpha b$$
$$\hat{c} = c + b\gamma$$
$$\hat{v}_i = v_i + b\eta_i$$

We can now identify the effect of the threat of inspection on the probability of violation (*b*) by obtaining (an estimate of) β from (an estimation on) (A1) and inserting it into (A3). The effect of the threat of inspection on emissions can be obtained analogously.

The logit model with random effects is applied in the first stage estimation, as well as in the second stage estimation when the response variable is *Violation*. GLS with random effects is applied for the second stage emissions regressions. Table A1 contains the main results from these second step estimations, and we see that our main qualitative results are the same as the ones reported in the main body of the text.

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