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Monitoring and enforcement of environmental regulations Lessons from a natural field experiment in Norway

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ABSTRACT

Relying on a small natural field experiment conducted by the Norwegian Environmental Protection Agency, I estimate effects of three fundamental elements of most monitoring and enforcement practices: self-reporting, audit frequency and specific deterrence. I find evidence of under-reporting of violations in firms' self-audits, as more violations are detected in on-site audits than in self-audits. Announcing the increased audit frequency has no effect on firms' compliance, but an audit raises subsequent compliance substantially.

1. Introduction

Environmental protection agencies (EPAs) in most Western countries suffer from the recent cutting of public expenses. Smaller budgets accentuate the need to identify and implement cost effective monitoring and enforcement policies that do in fact reduce violations of environmental regulations. Audits and self-reporting are core elements of agencies' monitoring and enforcement practices, and the theoretical foundation of these practices is persuasive (Becker, 1968; Stigler, 1970; Russell et al., 1986; Heyes, 2000; Polinsky and Shavell, 2007). The empirical literature of the effectiveness and efficiency of monitoring and enforcement of environmental regulations is also growing, but it lacks evidence from field experiments (Gray and Shimshack, 2010, 2011).[1]

The current paper presents results from the first natural field experiment[2] on effects of monitoring and enforcement activities of EPAs. Three fundamental elements of most monitoring and enforcement practices are investigated using random assignment of treatments; the extent of under-reporting in firms' self-audits,[3] effects of increased audit frequency and effects of audits on firms' subsequent compliance behavior.

The main finding is that violations are under-reported in self-audits compared with on-site audits. Moreover, while I find evidence of substantial reductions in firms' violations after an audit, the effect of higher audit frequency is negligible in the investigated regulatory setting. I argue that the lack of effect of higher audit frequency is related to the extensive use of warnings, which is typical for the monitoring and enforcement policy of EPAs of many Western countries (Russell, 1990; Nyborg and Telle, 2004, 2006; Rousseau, 2009). Overall, the study confirms the concern that a shift toward reliance on cheaper and softer monitoring and enforcement practices, like self-reporting and voluntary disclosure programs, could undermine compliance with environmental regulations.

The next section starts by underlining the ability of natural field experiments to provide estimates with a straightforward causal interpretation. Then theoretical and empirical support for the reliability of information from self-audits, as well as for the impact of increased

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[1] A few natural field experiments on monitoring and enforcement exist in the tax literature; see e.g. Kleven et al. (2011), Pomeranz (2011) and Slemrod et al. (2001).

[2] The treatments of this experiment were embedded in the typical monitoring and enforcement activities of the Norwegian EPA, and the firms did not know that they were part of an experiment. Following the terminology of Harrison and List (2004), the current paper thus reports results from a "natural field experiment".

[3] In the current experiment, the EPA either required the firm to fill in the audit-form and send it to the EPA ("self-audit") or an EPA official came to the firm to fill in the audit-form "on-site" ("on-site audit"); see Section 3 for details.

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audit frequency and firms' post-audit compliance behavior, is discussed. Section 3 describes the Norwegian institutional setting within which the current experiment is conducted, and outlines the experimental design. Data and estimation methods are also described in Section 3. Section 4 presents the results, and Section 5 concludes.

2. Empirical studies and theoretical background

Scholars and policymakers seem to agree that regulatory compliance hinges on the implemented monitoring and enforcement policies. Still, it remains difficult to empirically establish effects of various types of such policies. The fundamental problem is that enforcement efforts are not exogenous but typically an endogenous response by the enforcement agency to the perceived compliance behavior of the firms (Gray and Shimshack, 2010, 2011; Harrington, 1988; Helland, 1998; Kleven et al., 2011).

Within the literature on environmental regulations, scholars have applied several empirical strategies to address such endogeneity issues. More sophisticated strategies utilize variation in rules across states and time (both in reduced form models and in more structural models) and reliance on instrumental variables (e.g. Alberini and Austin, 2002; Stafford, 2003; Shimshack and Ward, 2005; Sigman, 2010). Valid instruments are, however, scarce (List, 2006), and in their recent review of enforcement of environmental regulations, Gray and Shimshack (2010, 2011) discuss the fundamental problems of using observational data in estimating effects of monitoring and enforcement activities, and they conclude by calling for field experiments to "transparently reveal the impacts of different enforcement activities" (p. 34). This is in line with previous reviews of the role of regulatory actions by EPAs in enforcing environmental regulations, pointing at the pervasive concern that results from studies using observational data are potentially biased (Cohen, 2000; Heyes, 2002; Glicksman and Earnhart, 2007).

The virtue of an experiment with random assignment is that the source of variation in monitoring and enforcement activities is exogenous. Relying on random assignment, the enforcement activity directed at a firm is independent of strategic behavior by the firm or by the agency. Thus, random assignment of enforcement activities handles the selection problem that complicates interpretations of results from studies using comparison groups that are not randomly assigned.

The present paper presents results from the first natural field experiment on effects of monitoring and enforcement policies in the domain of environmental regulations.[4] This enables us to address three fundamental elements of most enforcement policies: the reliability of information from self-audits, the effect of audits on firms' subsequent behavior and the effect of increased audit frequency.

The reliability of information in self-audits is undermined by firms' incentives to mis-report or to stay ignorant about regulations (Brehm and Hamilton, 1996). In a simple Becker (1968) framework, self-reporting is accurate *only* to the extent that it serves the firm (in expectation terms). Thus, deliberate mis-reporting is typically considered fraud and punished severely, but studies in the tax literature indicate that underreporting of tax liabilities is still common (possibly close to 20%; see Andreoni et al., 1998; Bloomquist et al., 2005; Slemrod, 2007; Internal Revenue Service, 2008; Kleven et al., 2011). In spite of this, information in self-audits appears to be believed adequately reliable by many EPAs (Pfaff and Sanchirico, 2004; Nyborg and Telle, 2006).[5]

The effect of audits on firms' subsequent behavior is an element of *specific deterrence*, which can be defined as the extent to which regulatory actions deter subsequent violations at the audited or sanctioned unit (Gray and Shimshack, 2011). In the Becker framework, there are several reasons why being audited today can improve future compliance. First, being audited today can increase expected future punishment of non-compliance. To the extent that EPAs target previously observed violators, a bad-performing firm will expect future audit and detection probability to increase. There is some empirical indication of this (e.g. Harrington, 1988; Nyborg and Telle, 2006; Rousseau, 2007), as well as some indication that EPAs provide violators with warnings, thus only sanctioning offenders that fail to comply upon detection (e.g. Russell, 1990; Nyborg and Telle, 2004, 2006; Eckert, 2004; Rousseau, 2009). In these cases, being audited increases the perceived probability of future audits and sanctions, and thereby raises the incentives for compliance.[6]

Second, being audited today can reduce (expected) future costs of compliance. Thorough audits require the firm to spend time understanding the regulation. Indeed, during audits, EPAs typically try to convey knowledge to the firm on production processes and technical solutions that are environmentally friendly. Information about regulations, including new regulations, may also be more easily available to the firm through contact with the EPA during an audit. Thus, an audit may have an "education effect" on the firm, which could lower future compliance costs and thereby raise incentives to comply.

In their survey of the literature, Gray and Shimshack (2011, p. 20) conclude that "environmental monitoring and enforcement activities generate substantial specific deterrence, reducing future violations at the targeted firm". They note, however, that the scope for strategic behavior, both by the firms and by the EPA, introduces concerns that results from studies using observational data could be biased.

Increased audit frequency raises the detection probability and thus the expected penalty, thereby enhancing incentives to comply with the regulation, in a Becker framework.[7] Kleven et al. (2011) conduct a natural field experiment in Denmark, and consider a treatment that increases the audit probability (for a subsample of taxpayers) from traditionally low levels to the very high levels of 50 and 100%. They do find positive effects of letters informing taxpayers of these excessive audit probabilities, but the magnitudes of the effects are modest (p. 689). Some other studies in the tax literature have also found very small or even negative effects of increased probability of audit (Johnson et al., 2010; Slemrod et al., 2001; Blumenthal et al., 2001; Coleman, 1996; Alm et al., 2009). Slemrod et al. (2001) use a natural field experiment to analyze effects on reported liabilities of a letter from the tax authority announcing increased probability of audit. They find that the treatment effects are heterogeneous with respect to income level, and, surprisingly, they conclude that "the reported tax liability of the high income treatment group *fell* sharply relative to the control group" (p. 455).

There are several studies in the environmental economics literature where the firms' perceived audit frequency is estimated and used to explore impacts on subsequent compliance behavior (e.g. Alberini and Austin, 2002; Telle, 2009), but I am not aware of any studies in this field that look at effects on regulatory compliance of a general increase

[4] Andreoni et al. (1998) survey the literature on tax compliance (Kleven et al., 2010 include a more recent survey), and they hold that the empirical support for core theoretical predictions, such as deterrence effects of audit frequencies, is limited. Though the natural field experiment by Kleven et al. (2011) represents an outstanding exception, empirical documentation in the tax literature of effects of monitoring and enforcement policies remains scant.

[5] There is, however, a debate about the effectiveness of EPA-policies relying on self-reporting (e.g. Kambhu, 1989; Malik, 1993; Heyes, 1994; Livernois and McKenna, 1999; Stafford, 2008; Innes, 1999; Brehm and Hamilton, 1996).

[6] Note that if audits have substantial spill-over effects on non-audited firms, then an audit will also reduce the violations of non-audited firms (e.g. Shimshack and Ward, 2005; Gray and Shadbegian, 2007). This will result in a downward bias in the estimate of the specific deterrence effect (since the control group has been partly treated). Moreover, if firms postpone complying till they receive a warning, we may observe a big specific deterrence effect (Nyborg and Telle, 2006).

[7] This reasoning relies on the assumption that detected violators do in fact face higher (expected) penalties. The term "general deterrence" is typically used to describe deterrence actions that are not directed at a specific unit (like the general audit frequency), but it is also used to describe the extent to which regulatory actions aimed at one unit generate spill-over effects that impact the regulatory performance of other units (Gray and Shimshack, 2011; Pomeranz, 2011; Shimshack and Ward, 2005; Gray and Shadbegian, 2007).

in the EPA's audit frequency. Given the prominent role of the detection probability in the Becker framework, it is a concern that the empirical evidence of appreciable effects of audit frequencies on regulatory compliance is so limited.

3. Setting, design, data and method

3.1. Institutional setting

The Norwegian environmental protection agency (NEPA) is responsible for monitoring and enforcing most environmentally related regulations in Norway. Polluting manufacturing facilities are required to obtain an emission permit from the NEPA and to file annual self-monitoring reports. For firms in other domains, such as trade with manufactured goods or handling of waste, compliance data are obtained from self- or on-site audits. For smaller firms, audit probabilities could be virtually zero, while bigger facilities could be audited several times a year. The firm has to cover NEPA's costs of conducting the audit, and self-reported information, adequacy and maintenance of the firm's surveillance equipment and procedures are important aspects of audits. It appears that the responsibilities and organization of the NEPA are similar to how Gray and Shimshack (2011) describe the situation in the U.S.

When a violation is detected (or suspected) the NEPA normally starts by sending the firm a warning letter, stating in what ways the firm is believed to be out of compliance, indicating the seriousness of the violations, requesting documentation that the firm is in compliance within a given deadline, and pointing out the firm's legal duty to comply with the instructions. Typically, only violators failing to respond adequately to the warning face more formal and direct sanctions. Criminal referrals are infrequent, and reserved for cases of deliberate operation outside the regulatory environment, deliberately deceiving behavior like record falsification, or cases with exceptional harm to human health (Nyborg and Telle, 2006). This seems to be similar to the practices of the U.S. EPA (Russell, 1990; Gray and Shimshack, 2011; Uhlmann, 2009).

The field experiment reported here was initiated in 2007 within one domain of the activities of the NEPA, namely regulations to secure the environment and human health from manufactured goods containing hazardous substances. The regulations cover a broad specter of hazardous substances in products like toys, electronics, textiles, furniture and hobby supplies. A few examples of these regulations include the prohibition of arsenic and chromium in impregnated timber; strict limits to the leakage of cadmium and lead from toys; the prohibition of mercury in consumer products; and the prohibition of brominated flame retardants in children's textiles that can be put in the mouth (like mittens and collars). The regulations impose a duty on the firm to ensure that it has sufficient knowledge of the regulations and that it has internal routines sufficient to actually follow the regulations. For example, firms selling electric and electronic products have a duty to accept discarded products and to inform the costumers about the duty. It must also ensure that the accepted products are recycled in line with regulations.

Before 2007 the NEPA did not systematically devote significant resources to monitoring and enforcing these regulations. However, the proportion of releases of hazardous substances from products had been rising relative to that from traditional sources of pollution such as manufacturing plants. For a number of hazardous substances, like lead, arsenic, copper or chromium, products had become the only or the chief source of national releases. These products had become an important potential threat to the natural environment and to the health of adults and children (Norwegian ministry of the environment, 2006; SFT, 2010).

Therefore, the NEPA started a program to systematically enforce these regulations for a group of firms known to import products likely to contain serious amounts of these hazardous substances. The population of firms to be monitored contained those importing solid and manufactured goods from Asian countries to Norway. Based on records from the Norwegian Directorate of Customs and Excise, the full universe of these firms was known to amount to about 2000. From 2007 NEPA devoted resources to perform between one and two hundred self-audits and up to 50 on-site audits annually toward these firms. As the resources for auditing activities were limited compared to the population of firms, and as the NEPA had limited knowledge about the regulatory performance of these firms, it was decided to assign enforcement activities randomly.[8] The experiment was conducted in line with the general principles of NEPA's monitoring and enforcement activities, which are the same across NEPA's various fields of responsibility. One key principle is targeting of units with the most substantial risk of causing harm.

3.2. Experimental design

In 2007 the NEPA started a program to systematically enforce regulations meant to secure the environment and human health from hazardous substances in manufactured goods.[9] Fig. 1 provides a sketch of the experimental design which was imposed in three waves; first in 2008,[10] then in 2009 and finally in 2010. Equipped with a list of the complete universe of firms, the NEPA stratified the population according to type of firm (Consumer products, Electronics and Manufacturing products) and judgments of size and potential harm (three groups). In line with the general practice of the NEPA of targeting more environmentally risky firms, the audit-frequency was set differently across strata. *Within* each stratum, however, monitoring actions (and non-actions) were randomly assigned to firms. This procedure provided a control group (of firms not receiving treatment) for each group of firms receiving treatment, and within each stratum the random assignment procedure should ensure that the treatment and control groups are identical up to a random component. By controlling for stratum we can thus obtain estimates of the average treatment effect without much concern for the potential selection bias of most previous studies that rely on observational data. The following three variations of treatment were assigned.

First, effects of announcing the actual increase in the audit frequency were explored. At the introduction of the monitoring and enforcement activities under the regulation, the NEPA randomly selected (about half of the) firms within some strata to receive a letter announcing the renewed monitoring and enforcement activities. In the letter, the treated firms were informed that the audit frequency would increase substantially, and they were made aware of the legal obligations and possible punishment facing them.[11] As announced in the letter, within a few months, the NEPA randomly selected firms (including firms that had not received a letter) for audits. Based on the outcome of these audits (see below), we are able to compare the regulatory performance of firms that did and did not receive the notification letter. Since the notification letter did in fact convey new information about higher audit probability, those firms receiving the letter

[8] In 2007 I was asked to assist the NEPA in conducting this experiment, and since then I have participated in setting up the experiment and advising the NEPA as they conducted it.

[9] Since monitoring actions are *randomly assigned* to firms from 2007/8 on, there cannot be a correlation between these monitoring actions and firms' pre-2007-performance or receipt of monitoring and enforcement actions. This ensures that even if there were some monitoring and enforcement actions toward firms pre-2007, the error we may make by assuming that firms had not received monitoring and enforcement actions before 2007, is to introduce a downward bias in our effect estimates (since some firms in the control group might have been audited in, say, 2006).

[10] Audits of some firms were conducted in 2007, but the audits of 2007 and 2008 were treated as one wave of audits by the NEPA. Thus, in the following, I will refer to audits conducted in 2007 and 2008 as conducted in 2008. See Telle (2011) for details. For the last wave (2010) the additional audit at the bottom of Fig. 1 was not completed.

[11] Appendix A provides my translation of the letter sent to firms in the building industry (firms in other industries got similar letters, but parts of the legal basis and attachments differed).

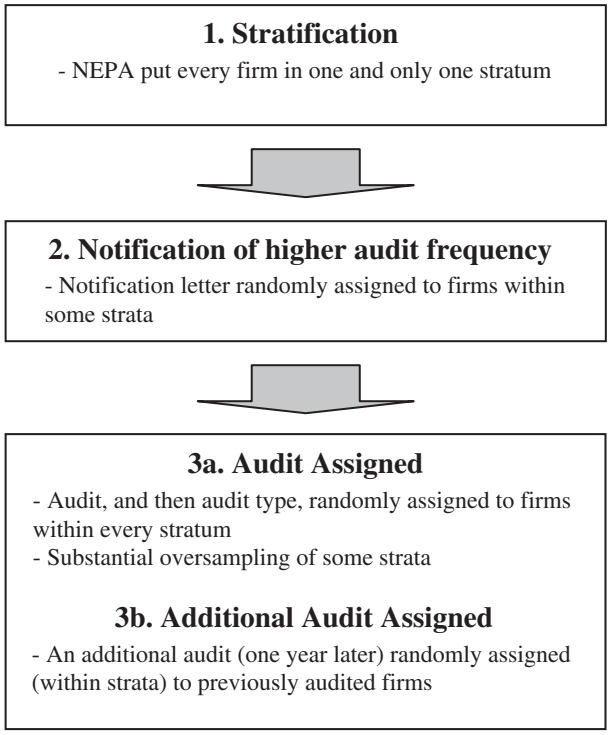

Fig. 1. Overview of the experimental design.

should expect the costs of violation to be higher than those firms not receiving the letter.[12] The experimental design therefore enables us to test effects on violations of announcing an *actual* increase in the audit frequency.

Second, the extent of under-reporting of violations in self-audits was investigated (see Fig. 1). NEPA randomly selected firms within every stratum to receive an audit (and the remaining firms received no audit), and then the selected firms were randomly assigned one of the two audit types (on-site audit or self-audit). Since audit type was randomly assigned, we would expect latent violations to be the same among firms receiving on-site audits and self-audits. Thus, any systematic differences between the detection of violations in the two audit types should reflect the ability of the audit type to expose violations to the NEPA.

Both types of audits were based on the *identical* audit form, but for the self-audit the firms filled in the form themselves and mailed it to the NEPA, while for the on-site audit, the form was filled in by a NEPA official while visiting the firm. The form contained information about the firm's knowledge and compliance with the regulation, about contents of hazardous substances in the products imported by the firm and about recycling and handling of hazardous waste.[13]

All the forms were evaluated by a NEPA official. The regulatory performance indicated by the form was captured for each firm, and based on the official's judgment he or she registered whether the firm was considered to be in violation or not. Though a violation is defined in legal terms and as a breach of any regulation, and though the forms were constructed to ease the identification of violations, it was not always easy to infer from a form whether a violation was present or not.[14] Moreover, NEPA did not register how serious the violation was, which disables us from analyzing possible heterogeneous effects of the treatments on the seriousness of the violations. The outcome measure on which we rely in this paper is based on this registration of each firm's violation.

Third, effects of audits in previous years were investigated ("specific deterrence"). To be able to measure compliance behavior of firms after an audit, a large fraction of firms that were audited in the previous year were randomly selected for a new audit (see Fig. 1). By inspecting the same firms in more than one year, we can test the effects of an audit, as well as audit type, on subsequent compliance behavior.

3.3. Data and estimation methods

Data are available from all audits conducted in 2008, 2009 and 2010. There were 1975 different firms in the overall population, and over the period NEPA conducted a total number of 534 audits. The number of audits in 2008, 2009 and 2010 was 202, 165 and 167, respectively; and the overall number of audits comprised 114 on-site audits and 420 self-audits. At least one violation was detected in 35% of the audits.

Whether the firm is in violation or not, as indicated by the NEPA when the audit form is evaluated, serves as the outcome variable (see previous sub-section for details). I use several estimation methods to evaluate whether the outcome of the firms in the treatment group differs from the outcome of the firms in the control group. Results from non-parametric Wilcoxon–Mann–Whitney tests are reported in the note of subsequent tables, and the tables include estimation results from ordinary least squares (OLS) and logistic (logit) regression models. Unless otherwise indicated, control variables are not included in the models, and standard errors account for heteroskedasticity and the fact that residuals for the same firm are not independent over time (clustering). If effects are very heterogeneous across subsamples, estimates of the mean average treatment effect will depend on the number of observations in the various sampling strata. To explore the relevance of this, tables include the effect estimates with flexible control for the stratification variables (i.e. dummies for year and strata).[15] The year dummies also control flexibly for any trends over time.

Since firms are legally obliged to conduct audits as instructed by the NEPA, sample attrition (which is common in surveys) should not occur. However, a number of firms that were in the population lists were no longer in operation or had stopped importing relevant goods (NEPA took various actions to verify this information, including drawing on information in other administrative registries). There were also a few cases where firms simply did not submit the self-audit as instructed by the NEPA. According to standard procedures at NEPA, these firms were followed up in different ways, including considerations to impose coercive fines. Such procedures can take time, which implies that results for a few such firms may not be in the dataset. However, some of the firms that did not submit the

[12] As always there are some caveats; see footnote 6 and the concluding section. Say, for example, that those firms receiving the announcement letter circulated it to firms in the control group, then our effect estimate would be downward biased. Moreover, the firms may erroneously believe that the audit frequency would in fact *not* go up, or they may think that the expected costs of non-compliance were unaffected by the higher audit frequency.

[13] Appendix B provides my translation of the form used for toys and consumer products. Not all firms got the exact same form, as forms were adapted to the regulatory context, but firms in the same stratum got the identical form.

[14] The NEPA took some effort to "blind" the official who evaluated the forms: the forms were identical across audit type and there was no indication in the forms of previous enforcement activities directed toward the firm. Still, the staff at NEPA working on this comprises only a handful of officials, so the evaluator may her/himself have been handling the form or firm before evaluating the form. Nevertheless, since the form was about 8 pages and there were a couple of hundred forms each year, it does not seem very likely that evaluation practices correlated with the treatment is a serious concern.

[15] We will see that controlling for the stratification variables hardly moves the estimates of the effects. In line with the literature, I thus rely on traditional (i.e. unweighted) estimation methods (DuMouchel and Duncan, 1983; Dickens, 1990; Lee and Solon, 2011).

Table 1
Baseline characteristics across treatment and control groups.

Random assignment of	Baseline characteristic	Obs	Treated (mean)	Control (mean)	Difference	Difference with dummies for year and strata included
On-site audit (vs. self-audit)	Weight of goods (10^6 kg)	528	42.4	28.7	13.7 (15.5)	3.6 (16.0)
	Value of goods (10^6 NOK[a])	528	50.6	26.8	23.8* (9.9)	14.0 (9.9)
	Number of employees	466	171	108	62.9 (46.0)	38.6 (49.2)
	Age of employees	466	40.1	40.0	–0.93 (0.73)	–0.51 (0.78)
	Fraction of employees men	466	0.56	0.59	–0.03 (0.03)	0.01 (0.03)
	Violatingin t-1[b]	189	0.35	0.33	0.02 (0.09)	0.03 (0.10)
Audit in t-1 (vs. no audit in t-1)	Weight of goods (10^6 kg)	528	39.6	27.1	12.5+ (6.7)	0.14 (13.5)
	Value of goods (10^6 NOK[a])	528	35.0	30.1	5.0 (5.6)	–7.1 (8.6)
	Number of employees	466	129	117	11.0 (25.0)	– 60.0 (39.0)
	Age of employees	466	40.9	40.8	0.11 (0.56)	1.45+ (0.81)
	Fraction of employees men	466	0.58	0.59	–0.01 (0.02)	–0.00 (0.03)
Got announcement of higher audit frequency (vs. did not get it)	Weight of goods (10^6 kg)	322	25.6	25.4	0.20 (11.4)	17.4 (13.0)
	Value of goods (10^6 NOK[a])	322	31.0	30.9	0.09 (8.8)	–2.5 (9.2)
	Number of employees	291	131	108	23.3 (37.4)	33.3 (50.0)
	Age of employees	291	41.7	41.1	0.57 (0.80)	0.07 (1.0)
	Fraction of employees men	291	0.58	0.56	0.02 (0.03)	0.01 (0.03)
Audit (vs. no audit)	Weight of goods (10^6 kg)	5,178	31.6	7.3	24.3* (6.3)	8.8 (6.8)
	Value of goods (10^6 NOK[a])	5,178	31.8	6.1	25.8* (4.8)	5.2 (3.9)
	Number of employees	4,157	122	56	65.4* (23.1)	9.8 (15.0)
	Age of employees	4,157	40.8	42.5	–1.6* (0.48)	0.16 (0.49)
	Fraction of employees men	4,157	0.59	0.62	–0.03 (0.02)	0.01 (0.02)
	Violating in t-1[b]	338	0.35	0.34	0.01 (0.05)	–0.01 (0.06)

Note: Differences in the last two columns obtained from ordinary least square regression (OLS); first without control variables, and then with controls for the stratification variables (dummies for years and strata). Robust standard errors in parentheses account for heteroskedasticity and non-independence of residuals for the same firm over time.
[+] Indicates significance at the 10% level.
[*] Indicates significance at the 5% level.
[a] One NOK is about 0.15 U.S. dollar.
[b] Only 338 firms were audited in $t-1$ (logit estimations yield the same results).

self-audit were audited in the following year, enabling us to test whether the performance of these firms in the subsequent year differs from the performance of firms that were not selected to be audited in the previous year. There were 31 firms that did not submit the audit in year $t-1$, but that did so in year t. Formal tests reveal that their likelihood of violation in t is similar to the likelihood of violation for firms that were not (selected to be) audited in $t-1$, and the difference is not statistically significant at any conventional level. This indicates that there is not much reason for concerns that attrition seriously biases the main results.

To check that the random assignment procedure led to balance between treatment and control groups, we would like to confirm that observable pre-treatment variables are not systematically different across the treatment and control groups. A measure of the overall sales (in value and weight) of the firms at the outset was available in our dataset from NEPA, and additional baseline characteristics of the firms (2007) are merged onto our dataset from a database maintained by Statistics Norway (*FD-trygd*), relying on an official firm identifier available in all public registries in Norway. As a consequence of NEPA's intentional oversampling from strata with more environmentally risky firms, we see from Table 1 (bottom 6 lines) that firms that received an audit differ from firms that did not receive an audit — the audited firms are for example bigger (value and weight of goods; number of employees). However, the random assignment of treatments within each stratum should ensure that once we control for stratum, the audited and non-audited firms should not differ on baseline characteristics. This is also what we observe in Table 1, where none of the differences across audited and non-audited firms remain statistically significant at the 5% level when we control for the stratification variables. We also note that when we consider the subset of firms that were audited in $t-1$, violations in $t-1$ were similarly distributed across firms drawn for audit or not in t (last line of Table 1).

Since the outcome variable is only available for firms audited in t, hypotheses can only be tested across the treatment and control groups of the upper three blocks of Table 1 (on-site vs. self audit; audit in $t-1$ vs. no audit in $t-1$; and got announcement letter vs. did not get it). In these three blocks we see that the observed characteristics of the firms are not statistically different across the groups (except in one test where we do not control for the stratification variables). Overall, this suggests that the assignment of the treatments was performed in a way unlikely to introduce selection bias.

4. Results

The data reveal evidence of extensive under-reporting of violations in self-audits. Consider the results reported in the first row of Table 2. In on-site audits, 54% of the firms had at least one violation, while the corresponding figure for self-audits is 24 percentage points lower. Or put differently, on-site audits detect violations in 80% $(0.24/(0.54-0.24))$ more of the firms than do self-audits. This result thus implies excessive non-disclosure of information unfavorable to the self-auditing firm. The estimated effect is statistically significant and robust to controlling for

Table 2
Under-reporting. Less detection of violations in self-audits than in on-site audits.

Estimation method	Effect estimate	Robust standard error	Dummies for years and strata included
OLS	−0.24*	0.05	No
OLS	−0.28*	0.06	Yes
Logit	0.36* [−0.24]	0.08	No
Logit[a]	0.29* [−0.30]	0.07	Yes

Note: Estimated incremental effect on detection of violation in self-audits compared with on-site audits. The dependent variable is a dummy set to one if at least one violation is detected in the audit (zero otherwise). Mean of dependent variable in the reference group (on-site audits) is 0.54. Marginal effects from ordinary least square regression (OLS) and odds ratios from logit estimation. The marginal effect inferred from the logit estimate in brackets. Dummies for the stratification variables (audit years and strata) included in the model if indicated (but estimates not reported). N = 534. Robust standard errors account for heteroskedasticity and non-independence of residuals for the same firm over time. A non-parametric Wilcoxon–Mann–Whitney test shows that the hypothesis that data from self-audits and on-site audits are from populations with the same distribution can be rejected (z = 4.6, p = 0.00).

* Indicates significance at the 5% level.

[a] N = 521 in this model because of no variation in dependent variable within a few strata.

Table 3
Specific deterrence. Less violation in t for firms audited in $t-1$ than for firms *not* audited in $t-1$.

Estimation method	Effect estimate	Robust standard error	Dummies for years and strata included
OLS	−0.15*	0.04	No
OLS	−0.14*	0.06	Yes
Logit	0.49* [−0.15]	0.09	No
Logit[a]	0.51* [−0.15]	0.13	Yes

Note: Estimated incremental effect on detection of violation (in t) in firms audited in $t-1$ compared with firms not audited in $t-1$. The dependent variable is a dummy set to one if at least one violation is detected in the audit (zero otherwise). Mean of dependent variable in the reference group (firms not audited in $t-1$) is 0.41. Marginal effects from ordinary least square regression (OLS) and odds ratios from logit estimation. The marginal effect inferred from the logit estimate in brackets. Dummies for the stratification variables (audit years and strata) included in the model if indicated (but estimates not reported). N = 534 (firms are assumed not audited prior to 2008; excluding observations from the first year reduces the number of observations to 332 but point estimates remain very similar). Robust standard errors account for heteroskedasticity and non-independence of residuals for the same firm over time. A non-parametric Wilcoxon–Mann–Whitney test shows that the hypothesis that data from firms audited and not audited in $t-1$ are from populations with the same distribution can be rejected (z = 3.6, p = 0.00).

* Indicates significance at the 5% level.

[a] N = 521 in this model because of no variation in dependent variable within a few strata.

the stratification variables[16]; see Table 2. This appears to be clearer evidence of evasive behavior than found in the tax literature, where Kleven et al. (2011) report that tax evasion (of liabilities), as detected in audits, is in the magnitude of 2.8% in Denmark to 4% in the U.S.

The data provide evidence of specific deterrence effects, as the effects of an audit on subsequent regulatory performance are substantial. Consider the results reported in the first row of Table 3. For firms that were not audited in the previous year, violations were detected in 41% of the audited firms, while the violation rate for firms audited in the past year was 15 percentage points lower. This implies that an audit reduces the likelihood of non-compliance in the next year by 37% (0.15/0.41). The estimated effect is statistically

[16] A subset of 189 firms was also audited in $t-1$, which enables me to add a control variable for violations in $t-1$. The estimated effect on this subset of firms is very similar to the estimate for the overall sample, and it remains almost identical when controlling for violations in $t-1$. The estimate on the overall sample is also robust to excluding firms in the 1st and 99th percentiles of the distribution on weight and value of goods (see Table 1), and this robustness holds for the estimates in the subsequent tables too. Moreover, if I include dummies for years only (i.e. not the dummies for strata), the estimate is very similar to the one from the model without any controls. This too holds for the estimates in the subsequent tables.

Table 4
Specific deterrence. Differences by type of audit.

Estimation method	Type of previous audit(s)	Effect estimate	Robust standard error	Control variables included
OLS[a]	On-site audit $t-1$	−0.28*	0.05	None
	Self-audit $t-1$	−0.12*	0.04	
Logit[b]	On-site audit $t-1$	0.22* [−0.26]	0.09	None
	Self-audit $t-1$	0.58* [−0.12]	0.12	
OLS[c]	Any audit $t-1$ and $t-2$	−0.23*	0.06	None
Logit[c]	Any audit $t-1$ and $t-2$	0.32* [−0.23]	0.11	None
OLS	Any audit $t-1$	−0.13*	0.04	Audit type in t
Logit	Any audit $t-1$	0.54* [−0.13]	0.10	Audit type in t

Note: Estimated incremental effect on detection of violation (in t) by the audit type received by firms that were audited in $t-1$ compared with firms not audited in $t-1$. The dependent variable is a dummy set to one if at least one violation is detected in the audit (zero otherwise). Mean of dependent variable in the reference group (firms not audited in $t-1$) is 0.41. Marginal effects from ordinary least square regression (OLS) and odds ratios from the logit estimation. The marginal effect inferred from the logit estimate in brackets. N = 534. Robust standard errors account for heteroskedasticity and non-independence of residuals for the same firm over time.

* Indicates significance at the 5% level.

[a] Estimated in one single model including the given two covariates (omitted reference group is firms not audited in $t-1$) and an intercept. A test that the effect is the same for on-site and self-audits can be rejected (F = 5.8, p = 0.02).

[b] Estimated in one single model including the given two covariates (omitted reference group is firms not audited in $t-1$) and an intercept. A test that the effect is the same for on-site and self-audits can be rejected ($\chi^2 = 4.2$, p = 0.04).

[c] Here the reference group consists of firms neither audited in $t-1$ *nor* in $t-2$. Mean of dependent variable in the reference group is 0.42. N = 380.

significant and robust to controlling for the stratification variables; see Table 3.

In the first two rows of Table 4, I explore differences in effects over the two types of audits. As one might expect, the specific deterrence effect is considerably higher for *on-site audits* in $t-1$ (28 percentage points in OLS model) than for *self-audits* in $t-1$ (12 percentage points). This means that an audit reduces the likelihood of violation the next year by 29 (self-audit) to 68% (on-site audit).

In the middle two rows of Table 4, I investigate the impact of being audited several times. We might expect the effect of an additional audit to remain positive, though lower than the effect of the first audit (Gray and Jones, 1991). There is some indication of this, as the effect of being audited in both of the two preceding years is 23 percentage points while the effect of an audit in the preceding year was larger than half of that (15 percentage points; see Table 3). In the last two rows of Table 4, I have controlled for the audit type in t, and we see that this hardly affects the point estimate (see Table 3).

The data provide *no* evidence that announcing higher audit frequency improves compliance behavior. Consider the results reported in the first row of Table 5. For firms not receiving the notification letter, violations were detected in 33% of the audits, while the corresponding figure in the treatment group (receiving the notification letter) is 8 percentage points *higher*. This implies more – and not less – violations among the firms that are informed about the higher audit frequency, but the point estimate is small, especially when controlling for the stratification variables, and it is not statistically significant. We may also note that the imprecision of the estimate implies that conventional confidence intervals would cover expected negative effects of non-trivial magnitudes.

Though recent natural field experiments in the tax literature have found effects of increased audit frequency (Slemrod et al., 2001; Pomeranz, 2011; Kleven et al., 2011), the effects found by Kleven et al. (2011) are modest given the extraordinary high audit probabilities (50 and 100%). Moreover, Slemrod et al. (2001) found *negative* effects for the high income treatment group. Overall, it seems like effects of higher audit probability could differ substantially across types of firms and depend on characteristics of the monitoring and enforcement regime.

Table 5
General audit frequency. Not less violations for firms that received an announcement of higher audit frequency.

Estimation method	Effect estimate	Robust standard error	Control variables included
OLS	0.08	0.05	None
OLS	0.02	0.06	Dummies for years and strata
Logit	1.43 [0.08]	0.31	None
Logit[a]	1.12 [0.03]	0.30	Dummies for years and strata
Logit	1.51 [0.09]	0.34	Audit type in *t*

Note: Estimated incremental effect on detection of violation in firms that had received the letter announcing higher audit frequency compared with firms that had not received the letter. The dependent variable is a dummy set to one if at least one violation is detected in the audit (zero otherwise). Mean of dependent variable in the reference group (firms that did not receive the announcement letter) is 0.33. Marginal effects from ordinary least square regression (OLS) and odds ratios from logit estimation. The marginal effect inferred from the logit estimate in brackets. Dummies for the stratification variables (audit years and strata) included in the model if indicated (but estimates not reported). N = 327. Robust standard errors account for heteroskedasticity and non-independence of residuals for the same firm over time. * indicates significance at the 5% level. A non-parametric Wilcoxon–Mann–Whitney test shows that the hypothesis that data for firms receiving and not receiving announcement letter are from populations with the same distribution cannot be rejected ($z = 1.5$, $p = 0.12$).

[a] N = 310 in this model because of no variation in dependent variable within a few strata.

5. Concluding remarks

The current paper presents results from the first natural field experiment on effects of monitoring and enforcement activities of Environmental Protection Agencies (EPAs). The random assignment of monitoring actions to firms handles the selection issues that have concerned scholars conducting studies on observational data. Gray and Shimshack (2011) survey the monitoring and enforcement literature from North America, and conclude that previous studies have tended to find strong effects of general and specific deterrence. The results of the current paper confirm specific deterrence effects, as the effects of an audit on subsequent regulatory compliance are substantial. There is little indication, however, of appreciable effects from the notification of increased audit frequency. Moreover, I provide evidence of excessive under-reporting in self-audits, which raises serious concerns that a shift toward reliance on self-reporting could reduce compliance with environmental regulations.

Despite the methodological reliability of the present effect estimates, interpreting the results in terms of policy implications remains difficult. One caveat relates to the external validity of the findings from a small experiment in one domain of the responsibilities of the Norwegian EPA. Though I have argued that the core elements of regulatory practices are very similar across EPAs in Western countries,[17] there is an obvious need to confirm this by conducting natural field experiments in more countries and regulatory domains.

Moreover, and as always, the current findings might be a result of peculiarities in the applied treatments. For example, we cannot rule out entirely that the lack of an effect from announcing higher audit frequency is due to firms misperceiving the notification letter. They might, for example, erroneously have believed that the audit frequency would in fact *not* go up. We would also underestimate the effect if the firms that received the notification letter circulated it to firms in the control group. Such spill-over of the treatment to the control group might be more plausible for environmental regulations, where industry groups and trade organizations frequently discuss and publicize changes in firms' regulatory conditions, than for individual taxpayers. However, even in the tax literature the estimated effects of increased audit frequency on violations are very modest (Kleven et al., 2011), or even negative (Slemrod et al., 2001). We should therefore be somewhat careful in using legitimate methodological concerns of the current study to dismiss the finding of no reduction in violations from announcing higher audit frequency. In any case, given the prominent role of the audit frequency in practice and its persuasive theoretical foundation, it is concerning that it appears hard to come up with convincing empirical evidence that the audit frequency reduces violations appreciably.

Another caveat relates to the actions typically undertaken by EPAs once a violation is detected. It seems plausible that a credible threat of sanctioning by the EPA is necessary for a detection of a violation to improve future performance. EPAs in several countries are, however, known to just issue warnings when violations are detected, only escalating to harsher sanctions if the firm fails to cooperate upon detection (Russell, 1990; Nyborg and Telle, 2004, 2006; Rousseau, 2009). If firms expect to get a second chance upon detection of a violation, then they lack incentives to comply before violations are detected. If so, it is not surprising that we find no effects of the letters notifying higher audit frequency. Indeed, under such a regime, firms will have incentives to *i)* stay uninformed about the regulation, and *ii)* cooperate with the EPA to end the violation only after it becomes detected. What we would then observe in the data would be no effect of the higher audit frequency and substantial specific deterrence effects. Though such a regime has its proponents (Braithwaite, 2002), it is incapable of deterring violations that are not detected. This makes it fundamentally at odds with theories of optimal enforcement, where the agency can minimize overall violations (i.e. the overall violations of audited *and* non-audited firms) by allocating resources from expensive monitoring activities toward severe sanctioning of detected violators (Becker, 1968; Heyes, 2000; Polinsky and Shavell, 2007).

The results from the current experiment, along with surveys documenting that firms rate the activities of EPAs as a crucial source of pressure for environmentally friendly performance (e.g. Khanna and Antons, 2002; May, 2005; Short and Toffel, 2010), suggest that rigorous monitoring and enforcement can be a more effective tool to detect and combat evasive behavior than self-audits or voluntary disclosure programs (see e.g. Khanna, 2001; Foulon et al., 2002). But self-audits and voluntary disclosure programs are typically less expensive than on-site audits, so it could be worth looking for ways of balancing the inexpensiveness of self-audits and the reliability of the on-site audits. Kleven et al. (2011) suggest that requiring a third-party to approve the reliability of self-audits can reduce violations. EPAs may thus consider relying on third parties, like the firm's accountant or external consultants, to verify the content of self-audits. It is not obvious, however, that such third-party verification is more cost-effective than on-site audits. It might be, for example, that the transfer of information and knowledge from the EPA to the firm during an on-site audit is a cost-effective way of improving compliance with complex environmental regulations. It might also be that the violations detected in on-site audits are qualitatively different from violations detectable in self-audits, and to get some idea about possible welfare effects of EPAs' activities, attempts to measure the seriousness of each violation are important.

As a complement, EPAs could try to also make other firms in the sales-chain act similar to third-parties or whistle-blowers by collecting comparable information from them. As demonstrated by Pomeranz (2011), paper trails, which make information regarding compliance behavior available from different sources, could facilitate monitoring and enforcement. Attempts to raise the expected penalty – in particular the sanctioning of detected violators – of evasive self-reporting of violations, may also provide an additional complement worthy of further exploration by EPAs.

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[17] Gray and Shimshack (2011, p. 2) state that the regulatory regimes in the U.S. are "broadly similar to those in many other developed countries."

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Appendix A. My translation of the letter notifying about increased audit frequency

Statens forurensningstilsyn
Norwegian Pollution Control Authority

To [Firm]

Statens forurensningstilsyn
Postboks 8100 Dep, 0032 Oslo
Besøksadresse: Strømsveien 96

Telefon: 22 57 34 00
Telefaks: 22 67 67 06
E-post: postmottak@sft.no
Internett: www.sft.no

Date: 30.08.2007
Our ref.: 2007/665
Your ref.:
Public Servant: May-Anita Dolmseth Hoel, telephone: 22 57 35 08
mdh (a) sft.no

Notification of audit of firms importing/selling construction materials

Background

Previous surveys conducted by The Norwegian Environmental Protection Agency(NEPA) show that products that are not in accordance with the prescriptions of the Norwegian regulations are imported and sold. The violations include content of illegal substances or substances that are harmful to human health or that disturb the environment.

Firms importing and selling products are themselves responsible for ensuring that the products they import and sell are in accordance with the Norwegian regulations. The firm is required to:

- know and comply with the prescriptions laid out in the regulation of the products that the firm imports/sells
- have sufficient knowledge about the chemical contents of the products that the firm imports/sells
- have necessary and sufficient routines to ensure compliance with ballpoints 1 and 2 (internal control)

Audit

NEPA will now increase the focus on monitoring these products and audit more importing firms in the period 2007-2009. The audits will *inter alia*include the following:

- Internal control
- Illegal and strictly regulated substances and products
- Duties according to the Product Control Act (requirements of due care, disclosure duties, duty to notify)

It may be pertinent to look at chemical-documentations for some products (for example contracts, product-description and analysis-documentation) and possibly collect products for further analyses.

This letter is sent to many firms and all recipients will be considered for a self audit or an on-site audit. NEPA may report serious violations to the police.

Relevant regulations

- Act on control with products and consumer services (Product Control Act), see especially § 3 requirements of due care, § 5 disclosure duties, § 6b duty to notify
- Regulation on systematic Environment-, Health-and Safety-work in firms (Internal Control Regulation) §§ 4-5
- Regulation on restrictions in use of health-and environment-damaging chemicals and other products (Product Regulation)

For more information about relevant regulations, see attachment 1.

Relevant web-pages
For EHS-regulations in your industry, see www.regelhjelp.no
Relevant acts and regulations are also here www.lovdata.no
For more information on substances in products, see www.miljostatus.no

Fee
Products are one of many sources for diffusion of environmental hazardous pollutants. When auditing the firm's internal control, NEPAcan issue a fee, cf. the Pollution Regulation, § 39-7. NEPAhereby notify that a fee will be issued to the firm in case of an on-site audit. The size of the fee will depend on the firm's magnitude (fee size 3, 4 or 5).

An overview of regulations related to the Anti-Pollution Act and the Product Control Act is available on Internet under the following address: http://www.sft.no under regulations. The regulation on issuing of fees to the state for NEPA's treatment of applications, monitoring and auditing can be found in this overview under pollution regulation. A paper copy of the regulation is available from NEPA upon request.

With regards,

Cecilie Kristiansen (b.a.)
Head of Section

May-Anita Dolmseth Hoel,
Senior Engineer

Attachemnt: Information sheet
Copy: Handels-og Servicenæringens Hovedorganisasjon, Postboks 2900 Solli, 0230 Oslo

Appendix B. My translation of an audit form

Statens forurensningstilsyn
Norwegian Pollution Control Authority

SELF AUDIT FORM 2009
TOYS AND CONSUMER PRODUCTS

Fill in form and send to: **Deadline: May 20. 2009**

Statens forurensningstilsyn co/N.N

Postboks 8100 Dep, 0032 Oslo
Case nr: **2009/435**
E-mail: postmottak@sft.no

Fax: 22 67 67 06

1. Information about the firm	
Name of firm:	
Firm identifier/ Plant identifier:	Date of filling in the form:
Address:	Telephone:
Post code. and city:	E-mail:
Contact person:	Signature:

2. Import of goods–general	Yes	No
Are you importing one or more of following products: Toys for children		
a) Toys of plastic or rubber (soft, hard, slimy-toys)	☐	☐
b) Painted wooden toys	☐	☐
c) Metal toys	☐	☐
d) Cap pistols/-guns	☐	☐
e) 17th of May toys/blow-toys/gas-bugles	☐	☐
f) Products filled with liquids	☐	☐
g) Electric and electronic toys (Note answer point 7 too)	☐	☐
Te xtiles		
a) Outerwear for children (mittens, winter suits, snowsuits)	☐	☐
b) Skin-contact children's clothes (underwear, socks, panty hoses, pajamas)	☐	☐
c) Outwear for adults(all-weather jackets, sports jackets, reflector vests/ jackets)	☐	☐
d) Baby prams, buggies, bike-wagons, snow-pulks (textiles within)	☐	☐
e) Furniture textiles, curtains, rugs, carpets	☐	☐
Jewelry/bijouteri for children or adults		
a) Necklaces, rings, earrings, bracelets etc.	☐	☐
Electric and electronic products (EE-products)		
a) Toys	☐	☐
b) Sportsgoods(like treadmills, head lamps, exercise watches)	☐	☐
c) Audio books etc.	☐	☐
d) Cellular phones	☐	☐
Other products		
a) Furniture	☐	☐
b) Riding hats	☐	☐
c) Shoes, boots, gloves, other leather products	☐	☐
Are you importing other products than those listed above?	☐	☐
(if yes, please fill in what products) 1. ____ 2. ____ 3. ____ 4. ____ 5. ____		
▪ Not importing any products ☐		
Use point 10 for possible more information about products.		

Please provide your three largest suppliers for each group of products (toys, textiles, jewelry, EE-products and other products):

Product group	Supplier/address
Toys for children	
Textiles	
Jewelry/bijouteri	
EE-products	
Other products	

Use point 9 for possible more information about suppliers.

3. Internal control in the firm	Yes	Partly	No
Are you familiar with the requirements in the Internal control regulation?	☐	☐	☐
Is internal control implemented in the firm?	☐	☐	☐
Do you have a person in the firm who is responsible for internal control and follow -up of the regulations of products?	☐	☐	☐
Are you familiar with the regulations for import and trade of consumer products, toys, textiles, jewelry/bijouteri, EE-productsand other products, given in point 2?	☐	☐	☐
The Productcontrol Act §3 requires duty of due care for those importing or trading products. Are you familiar with the duty of due care?	☐	☐	☐
Do you have routines to ensure compliance with the duty of due care?	☐	☐	☐

Appendix B (*continued*)

3. Internal control in the firm	Yes	Partly	No
The Productcontrol Act § 6b requires that you notify the authorities about hazardous or illegal products on the market. Are you aware of this notification duty?	☐	☐	☐
Do you have routines to ensure compliance with the notification duty?	☐	☐	☐
Anyone has the right to receive information from the importer and trader about a product's hazard to health and environment, according to the Product control Act § 10. Are you aware of this information duty?	☐	☐	☐
Do you have routines to ensure compliance with the information duty?	☐	☐	☐
Have you ever detected illegal products?	☐		☐
What do you do if you detect that you are importing illegal products? ▪ Treat it as a deviation ▪ Notify supplier/producer ▪ Notify authorities ▪ Withdraw the product from the market	☐ ☐ ☐ ☐		☐ ☐ ☐ ☐
In what way do you ensure that the products you import do not contain prohibited substances that are hazardous to the health and/or environment? ▪ Requirement at contract signing ▪ Get oral confirmation from supplier/producer ▪ Get written confirmation from supplier/producer ▪ Certificates or proofs of analyses follow the products ▪ Send products for analysis (spot tests) No routines ☐ Use point 10 for possible more comments.	☐ ☐ ☐ ☐ ☐		☐ ☐ ☐ ☐ ☐
If you do not import any products within the product groups listed in point 2, you can proceed directly to point 10 and give a brief summary of the firm.			

4. Toys – elaborative questions (see also point 7)	Yes	Partly	No
Are you importing toys ?	☐		☐ If no, go to point 5
Are you familiar with the regulation about safety of toys (the toys-regulation) with associated standards ?	☐	☐	☐
Are you familiar with the limits for leakage of inter alia cadmium and lead from toys (given in standard NS-EN 71-3 to the toys regulation)?	☐	☐	☐
Are you familiar with that six phthalates are illegal in toys and products for small children (product regulation § 3-12)?	☐	☐	☐
Are you familiar with the limits for organic solvents in toys (given in standard NS-EN 71-9 to the toys regulation)?	☐	☐	☐
Toys like cap pistols and guns, 17th May bugles, children's books with music and all battery powered toys generate noise. Are you aware of the noise limits for toys?	☐	☐	☐
Can you document that the toys you import are in accordance with the requirements of the regulations?	☐	☐	☐
Are you aware that it is possible to eco-label toys (Nordic eco-labels Swan or EU-flower)?	☐	☐	☐
Are you aware of Rapex, a system for rapid exchange of notification about hazardous products between EU/EEC-countries, and that these notifications are available on www.sft.no?	☐	☐	☐

5. Textiles – elaborative questions	Yes	Partly	No
Are you importing textiles?	☐		☐ If no, go to point 6
Are you familiar with the prohibition of phthalates in children's textiles that can be put in the mouth (mittens and collars of outwear) (product regulation § 3-12)?	☐	☐	☐
Are you aware that formaldehyde and some other types of azo dyes are prohibited in textiles in contact with the skin (product regulation § 3-4)?	☐	☐	☐

Appendix B (*continued*)

Are you familiar with the prohibition of PFOS and PFOS-related compounds in textiles (product regulation§ 2-23)?	☐	☐	☐
Are you familiar with the prohibition of the brominated flame retardants penta-, octa- og deca- BDE in textiles, like for example furniture textiles and curtains (product regulation § 2-20)?	☐	☐	☐
Can you document that the textiles you import are in accordance with the requirements of the regulation?	☐	☐	☐
6. Jewelry/bijouteri – elaborative questions	Yes	Partly	No
Are you importing jewelry/bijouteri?	☐		☐ If no, go to point 7
Are you familiar with the regulation of nickel in jewelry /bijouteri (product regulation § 3-5)?	☐	☐	☐
Mercury can be present in jewelry. Are you familiar with the prohibition of mercury in consumer products (product regulation § 2-6)?	☐	☐	☐
Can you document that jewelry/bijouteri that you import are in accordance with the requirements of the regulation?	☐	☐	☐
7. Electric and electronic products – elaborativ questionse	Yes	Partly	No
Are you importing EE-products?	☐		☐ If no, go to point 8
Are you aware that leakage of nickel from cellular phones is regulated (product regulation § 3-5)?	☐	☐	☐
Are you aware that six substances are prohibited in electric and electronic products (product regulation § 3-18, known as EU's RoHS-directive)?	☐	☐	☐
Can you document that the EE-produktene you import are in accordance with the requirements of the regulations?	☐	☐	☐
Are you aware that all EE-products ought to be marked with the crossed waste container? 	☐	☐	☐
Are you checking that the EE-products you import are correctly marked?	☐	☐	☐
Are you aware that all trashed products that use electric currents or batteries ought to be returned as hazardous waste?	☐	☐	☐
Do you have a system to receive trashed products that use electric currents or batteries?	☐	☐	☐
Do you inform in writings about the system to return for EE-waste on your internet sites, in other information material and at you traders?	☐	☐	☐
Are you members of an approved receiver of EE-waste? If yes, what firm?	☐		☐
8. Other products	Yes		No
Serious allergic reactions from contact with the substance dimethyl fumarate have been reported from consumers in Europe over the last year. Are you familiar with that furniture, riding helmets, shoes and boots produced in Asia, can contain the mold inhibition agent dimethyl fumarate?	☐		☐
9. REACH	Yes		No
Are you aware that EU's new regulation of chemicals REACH also includes some products? Duties in accordance with article 7.1 in REACH concern products that contain a chemical substance that is intended to escape (e.g. scent eraser). If the content of the substance in the product exceed 1 tonn pr importer per year, the substance ought to be registered at The European Chemical bureau ECHA.	☐		☐
Are you importing products from countries outside of EU/EEC, like Asia or USA, which are covered by REACH?	☐		☐
10. Other information to points 1-9			
You are welcome to provide more elaborate information about the products you import, about your supplier or about other conditions.			
11. Possible comments to the self-audit			
▪ Were the questions in the self-audit understandable? Yes ☐ No ☐ In the case of new self-audits, do you wish to receive and fill in the form electronically, i.e. one-mail or Internet? Yes ☐ No ☐ ▪ Possible comments to this self-audit:			

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