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Abstract

We study empirical determinants and effects of firms' participation in the EPA's 33/50 voluntary pollution reduction program. We broaden the existing literature in three principal ways, studying (1) bi-directional links between participation in the 33/50 program and regulatory enforcement, (2) effects of implicit boycott threats, and (3) potential impacts of regulatory preemption incentives. We find evidence that firms' participation in the 33/50 program was motivated by the expectation of relaxed regulatory scrutiny, an expectation that was borne out by regulatory practice. 33/50 program participation and pollutant reductions were also prompted by a firm's likelihood of becoming a boycott target and/or being subject to environmental interest group lobbying for tighter regulatory standards.

Keywords: Voluntary environmental programs, regulatory enforcement, boycott deterrence.
JEL: Q28, K42, L51, D62

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I. Introduction

Why do private firms voluntarily over-comply with environmental regulations? For example, over 1200 firms joined the U.S. Environmental Protection Agency's (EPA) 33/50 program. In this program, firms pledged to reduce emissions of 17 key toxic pollutants beyond targets required by law. Current voluntary EPA programs include "Energy Star," which seeks to decrease carbon dioxide emissions, and the "National Environmental Performance Track," designed to encourage environmentally proactive firms through rewards and public recognition.

Economists have offered a number of theories to explain why profit-driven firms might volunteer for costly pollution reduction efforts. Arora and Gangopadhyay (AG, 1995) argue that firms want to attract a clientele of "green consumers" who are willing to pay more for goods produced in an environmentally friendly way (see also Arora and Cason (AC), 1996). Voluntary pollution reductions may also deter lobbying by environmental groups for tighter legislative or regulatory standards (Maxwell, Lyon and Hackett (MLH), 2000); spur tighter environmental standards that "raise rivals' costs" (Salop and Scheffman, 1983; Innes and Bial, 2002); and/or avoid future environmental liability.

Two additional motives for voluntary over-compliance have received relatively little attention in the literature and are of particular interest in the present study. First, a firm's participation in a voluntary pollution reduction program may spur reduced scrutiny by environmental authorities, reducing the frequency of costly environmental inspections and enforcement actions. The EPA officially claims that such rewards are not offered to program participants.[1] Nevertheless, such rewards, promised implicitly if not officially,

[1]With regard to the 33/50 program, the EPA has stated (EPA, 1992, p. 11): "Participation in the program is enforcement neutral: a company will receive no special scrutiny if it elects not to participate and receive no relief from normal enforcement attention if it does elect to participate."

may constitute an optimal spur to program participation, which in turn prompts a firm to adopt management practices that reduce its costs of pollution abatement (Maxwell and Decker, 2002).[2] Second, some firms may be under the potential threat of boycott by environmental interest groups. In order to assuage these groups -- and thereby deter costly consumer action -- a firm may participate in a voluntary pollution reduction program (Baron, 2001; Innes, 2003).

The purpose of this paper is to study (1) the empirical validity of these and other motives for participation in the EPA's 33/50 program, and (2) the related effects of program participation on both a regulated firm's pollution levels and the government's enforcement activity. In studying these issues, we seek to bridge two empirical literatures, one focusing on voluntary pollution reduction programs (e.g., AC; Khanna and Damon (KD), 1999; Videras and Alberini (VA), 2000; Anton, Deltas and Khanna, 2004) and the other investigating determinants and effects of government enforcement activities. The former literature suggests that participation in voluntary pollution reduction programs is motivated, in part, by green marketing (AC, KD, VA) and potential liability (KD, VA), with larger firms found to be more likely to participate (AC, VA). In contrast to our focus, however, this literature does not study effects of voluntary over-compliance on government enforcement and does not consider potential effects of boycott threats and incentives for regulatory preemption (MLH).[3]

A number of papers study determinants of the government's environmental enforcement activity, and its impact on pollution (e.g., Magat and Viscusi, 1990; Gray and

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[2]Maxwell and Decker (2002) show that a reduced probability of enforcement may result from a firm's adoption of abatement-cost-reducing investments, thus spurring these investments apriori. If regulators can implicitly commit (apriori) to a flexible enforcement rate -- as a function of the firm's investment -- it can be shown that welfare is necessarily enhanced by the promise of reduced enforcement when firms invest more by, for example, participating in a voluntary pollution reduction program. Miceli and Segerson (1998) also stress benefits of voluntary pollution reduction programs in lessening tensions and facilitating negotiations between enforcement agencies and polluting firms.

[3]VA consider the potential impact of prior regulatory fines on voluntary program participation, finding some evidence that such enforcement actions make participation more likely. We study the impact of regulatory inspections as well and, like KD, also model impacts on pollution. MLH study potential effects of environmental constituencies on statewide pollution aggregates; we consider effects of environmental constituencies on both 33/50 participation and pollution decisions at a firm level.

Deily, 1996; Laplante and Rilstone, 1996; Nadeau, 1997). This work provides evidence that government enforcement efforts tend to prompt pollution reductions, a conclusion for which we also find support. However, most closely related to our study are papers that focus on the government's strategic use of enforcement tools to leverage desired conduct from regulated firms. Harrington (1988) argues that the apparent paradox of low and infrequent regulatory fines for environmental violations can be explained by the targeting of enforcement resources to "bad" firms that prompts desired conduct from "good" firms, despite low penalties for "good" firms' violations.[4] Helland (1998) studies an additional basis for targeting, the extent of a firm's self-reporting of violations. Decker (2003) studies an additional reward that may be offered to "good" firms -- more rapid environmental permitting for new source construction. Both find evidence that these regulatory tools are exploited in enforcement practice. We find evidence that regulators use another instrument to target their enforcement activities: a firm's participation in voluntary pollution reduction programs.

The remainder of the paper is organized as follows. Section II provides a summary of the 33/50 program. Section III discusses hypotheses on determinants of 33/50 participation, firm pollution decisions, and government inspections that are tested in the paper. In section IV, we discuss our data and the econometric specification for our model. Section V presents our estimation results. Finally, Section VI concludes.

II. The 33/50 Program

Started in 1991, the 33/50 program was the EPA's first formal effort to achieve voluntary pollution reductions by regulated firms. The program sought to reduce releases of seventeen toxic chemicals by a third by 1992 and by 50 percent by 1995, measured from 1988 baseline levels. The seventeen 33/50 chemicals are listed in Appendix A. Roughly seventy percent of the 33/50 chemicals (by 1988 weight of releases) were air pollutants (AC). Two of the chemicals (carbon tetrachloride and 1,1,1-trichloroethane) depleted the

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[4]See also related work by Harford and Harrington (1991) and Heyes and Rickman (1999).

stratospheric ozone layer and, hence, came under the Montreal Protocol's provisions for the phase-out of such substances; however, these two chemicals represented less than fifteen percent of total 33/50 releases (in 1988).

The EPA initiated the 33/50 program shortly after creating the Toxic Release Inventory (TRI), a database compiling information on toxic releases of all firms with ten or more employees producing one or more of 320 targeted pollutants. In early 1991, the EPA invited the 509 companies emitting the largest volume of 33/50 pollutants to participate in the program; these companies were responsible for over three-quarters of total 33/50 releases as of 1988. In July 1991, the 4534 other companies with reported 33/50 releases in 1988 were asked to participate as well. With additional enrollments through 1995, the EPA invited a total of 10,167 firms to join the 33/50 program, and 1294 firms accepted. The latter program participants accounted for 58.8 percent of 33/50 releases in 1990. In this paper, we focus exclusively on firms that were eligible for the 33/50 program in 1991.

The 33/50 program was purely voluntary and its pollution reduction targets were not enforceable. Despite the absence of apparent regulatory teeth, the EPA (1999) cites some aggregate statistics as indicators of the program's success. Among reporting firms, total 33/50 releases declined by over 52 percent between 1990 and 1996, and net 33/50 releases, excluding the two ozone-depleting compounds, declined by over 45 percent. In contrast, non-33/50 TRI releases fell by 25.3 percent over this period. Moreover, rates of 33/50 release reductions were greater for program participants (down 59.3 percent between 1990 and 1996) than for non-participants (down 42.9 percent over the same interval). However, these numbers may mask other hidden determinants of firms' pollution. For example, participating firms may have been more apt to reduce pollution, regardless of participation in the 33/50 program. One of our goals in this paper is to estimate the pollution abatement that is attributable to the 33/50 program, controlling for other relevant explanators and potential selection bias in program participation.

III. Hypotheses

Participation in the 33/50 program, although involving no enforceable commitment, required a firm to file a plan documenting how it proposed to reduce its emissions of target pollutants. Indeed, more than 82 percent of participants stipulated specific pollution reduction targets. In addition, the program was accompanied by some technical assistance to aid participants in realizing their target emission reductions (Khanna and Damon, 1999). Although the EPA, in its public statements, stressed the public recognition that participation could bring, there is little evidence that such recognition occurred in the broader public;[5] indeed, only with effort could a researcher obtain the names of program participants. However, the process of planning for emissions reductions, including possible managerial changes and environmental auditing procedures, could yield the very reductions that were the program's objective. To spur these innovations, and the participation that promoted them, the EPA may have implicitly afforded participants less scrutiny in its enforcement of pollution control laws, leading to fewer costly inspections and enforcement actions for a participating firm (Maxwell and Decker, 2002). The value of this reward is expected to have been higher for firms that otherwise anticipated greater regulatory scrutiny.

Hypothesis 1. Firms with higher rates of government inspection and enforcement action in previous periods are more likely to have participated in the 33/50 program.

Hypothesis 2. 33/50 participants should have experienced lower rates of government inspection and lower levels of pollution.

Hypothesis 3. Government inspections should have prompted pollution reductions (Harrington, 1988).

In addition to enforcement considerations, a number of theories suggest motives for participation in voluntary programs such as 33/50, and for desired pollutant reductions as well. We summarize these implications next, followed by a discussion of each.

[5] The EPA (1992) states that its "partnership programs offer recognition ... that can enhance corporate image with customers, regulators, neighbors, and the media."

Hypothesis 4. A firm was more likely to participate in the 33/50 program and to achieve pollution reductions if it:

(a) had more contact with final consumers (green marketing);

(b) was a more likely object of a consumer / environmental group boycott (boycott deterrence);

(c) was more exposed to potential liability because it was larger (with deeper pockets) and/or operated in strict liability states (liability);

(d) was in a more concentrated industry and invested more in research and development (raising rivals' costs); and

(e) had a greater incentive and ability to preempt regulation because it was a larger firm and operated in states with larger environmentalist constituencies (preempting regulation).

A firm may be able to exploit "green consumerism" to establish a market niche for goods produced in an environmentally friendly way (Arora and Gangopadhyay (AG), 1995); if present at all, such an ability is tied to a firm's proximity to consumers (AC, KD, VA). We follow KD in measuring this link using a dummy variable that takes a value of one if the firm sold a product directly to final consumers (FG for "final good").[6] AC indicate that green product differentiation incentives are also likely to be stronger in less concentrated industries. This conjecture runs counter to Hypothesis 4(d) and is tested in our analysis using a standard measure of industry concentration (HERF for Herfindahl index).

Firms may also be the potential object of consumer boycotts organized by environmental interest groups (Baron, 2001; Innes, 2003; Henriques and Sadorsky, 1996). Voluntary pollution reductions and participation in the 33/50 program may be actions that a

[6]AC argue that industry-aggregated advertising expenditures may also measure closeness to consumers; however, because this measure may be an indicator of market power as well, we adhere to the more direct measure of consumer proximity. Indeed, because of the prevalence of missing values for advertising data, even industry-aggregated, we did not use this measure in our analysis.

firm can take to deter such organized consumer action. The prospect of a boycott is greater -- and hence, more likely to motivate a firm's voluntary pollution reduction -- when the firm's products have good substitutes, are perishable, are sold publicly at a retail level, and are "visible" in the marketplace (Smith, 1990). For example, over the recent past, environmental and animal rights activists have successfully challenged large, "powerful" and visible firms such as McDonalds and Home Depot using boycott tactics (Innes, 2003).[7] To test for potential boycott threat effects in this paper, we construct a dummy variable that takes on a value of one if a firm is in an industry that was contemporaneously targeted for boycott.[8] We denote this variable BC. Because boycott threats are likely to be more accute when firms are larger polluters and/or operate in states with large environmentalist constituencies, we also consider interaction variables between BC and both the per-capita Sierra Club membership in the firm's home state (SIERRA) and a firm's 33/50 releases.

Larger firms, with deeper pockets, may voluntarily reduce pollution in order to avoid potential liability for harm caused. Such incentives will be greater in states that levy strict liability for environmental harm, as opposed to negligence liability (Alberini and Austin, 1999). We attempt to capture the liability motive for pollution reduction using a dummy variable taking a value of one if a firm's home state has a strict liability statute (STRICT).

In a concentrated industry, a firm that has developed cost-effective pollution abatement methods may wish to over-comply with government environmental standards in order to prompt tighter standards that disadvantage its rivals (Salop and Scheffman, 1983; Innes and Bial, 2002). This "raising rivals' costs" motive for voluntary pollution reductions

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[7]In 1999, McDonalds agreed to significant reforms in its supplier protocols for handling chickens after boycott actions by the animal rights group PETA (People for the Ethical Treatment of Animals); Burger King and Wendies quickly followed suit. Also in 1999, Home Depot agreed to phase out products using old growth timber and to give preference to timber certified by the Forest Stewardship Council; other major home improvement retailers, as well as home builders, have since made similar commitments.

[8]The 1992-1993 issue of the *National Boycott News* lists products subject to contemporaneous organized consumer boycott, including over 400 products made by over 100 firms. If a firm in our sample is in an industry that produces a targeted product, our boycott variable is assigned a value of one. We should point out that actual boycotts are rare. In fact, theory predicts that boycotts will generally be deterred by cooperating firms (Baron, 2001). Hence, none of the firms in our sample were actually boycotted. Rather, our boycott variable attempts to measure the potential likelihood that a firm might face a boycott threat.

is likely to be greater for firms that invest more heavily in research and development, investments that make cost-saving innovations in environmental technologies more likely. We capture these effects with variables measuring industry concentration (HERF) and firm-level R&D expenditures (R&D).

Finally, Maxwell, Lyon and Hackett (2000) argue that firms may voluntarily abate pollution in order to prevent the enactment of more costly environmental regulation. Environmental interest groups may, at a cost, lobby the government for tighter environmental regulation. By abating pollution voluntarily, firms can reduce these groups' incentive to lobby. Indeed, firms may be able to preempt lobbying by abating pollution to a lesser extent than would otherwise be compelled by a successful lobbying campaign. This motive for a firm's pollution reduction -- and participation in the 33/50 program -- is likely to be greater in states with larger environmental constituencies. In these states, the public sensitivity to a firm's pollution is likely to be greater, as are environmental groups' incentive and ability to successfully lobby the government for change. To test for these effects, we use the Sierra Club membership in a firm's home state (SIERRA).

IV. The Data and the Equations

We estimate three equations in order to explain (1) firms' participation in the 33/50 program (in 1991), (2) firms' annual emissions of 33/50 pollutants (by weight, 1989-1995), and (3) the government's (State and Federal) annual number of environmental inspections of firms' facilities (1989-1995).

Several data sources are used to estimate these equations. Financial and employment data was obtained from the Standard & Poor's Compustat database. The EPA's Office of Environmental Information Records provided data on 33/50 participation, Federal and State enforcement actions under the Clean Air Act (CAA) and the Resource Conservation and Recovery Act (RCRA) (1988-1990), and facility-level government inspections under the CAA (1988-1995).[9] The Toxic Release Inventory (TRI) provided facility-level data on 33/50 chemical releases, primary standard industrial codes (SIC), parent company names, and facility locations. Firm-level 33/50 pollutant releases and inspections were obtained by aggregating across each firm's facilities. The Sierra Club provided data on its state membership (1988, measured per capita). The Maxwell, Lyon and Hackett (2000) dataset provided information on state characteristics (1988), such as per capita state spending on clean air laws, educational status (the number of bachelors degrees per capita), the number of lawyers per capita, and indicators for whether the state had a right-to-work law or strict environmental liability.[10] The number of 1988 Superfund sites for which a firm was a potentially responsible party (PRP) was obtained from the EPA's Superfund Office. County attainment status (whether a facility's home county was designated by the EPA to be out of attainment with clean air laws) was obtained from the EPA's website (www.epa.gov/oar/oaqps/greenbk/anay.html). County population density (1990) was obtained from the U.S. Census.

Our study focuses on manufacturing firms that operated in SICs 20-39 and were invited to participate in the 33/50 program in 1991. Appendix B lists the industries associated with the included SICs. Merging the Compustat and environmental datasets for these firms gives us a sample of 496 companies. However, we limit the study to firms with three years or more of complete data over 1988-1995. Allowing for lagging, we thus have an unbalanced panel of 319 firms and 1257 facilities over the seven years, 1989-1995. We include 1989-1990 data in order to capture pre-program trends.

Tables 1 and 2 present variable definitions and descriptive statistics for our sample.

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[9]We restrict attention to CAA inspections because the 33/50 program was principally an air toxics program.

[10]We owe thanks to John Maxwell and Tom Lyon for generously providing us with their data. We also owe thanks to Chris Decker for providing invaluable advice on navigating the EPA's information services.

(1) The Participation Equation. We estimate a probit model of firms' decisions to participate (or not) in the 33/50 program.[11] To test Hypothesis 1 (the enforcement motive for participation), regressors include (i) the number of government inspections of firm facilities in 1989-1990 (INSP89-90), (ii) an indicator that takes a value of one if a firm had an enforcement action in the period 1989-1990 (ENFORCE), and (iii) the number of Superfund sites for which a firm is a potentially responsible party (PRP). Potential enforcement-driven rewards to 33/50 participation and pollution reductions are expected to have been greater for firms with more Superfund involvement, as measured by the PRP variable.

Critics of the 33/50 program suggest that firms joined because their prior (1988-1990) emission reductions already placed them in near reach of the program's goals (KD). We control for this effect by including a variable measuring a firm's 33/50 pollutant reductions from 1988 to 1990 (DIFREL). In addition, we control for industry effects by including dummy variables for the seven industries most heavily represented in our sample (SICs 28, 33, 34, 35, 36, 37, and 38).

(2) The Pollution Equation. We have an unbalanced panel of 319 companies for seven years, 1989-1995, giving us a total of 1879 company-year observations. A number of econometric issues arise in this panel.

First, there may be individual firm effects. Because ours is a relatively small sample from the population of 33/50 polluters -- and we have a good deal of cross-section data -- we model individual effects as random.

Second, we wish to test for effects of participation in the 33/50 program on 33/50 releases. However, in 1991, participation and pollution were jointly determined. To avoid simultaneity bias in this year, we use predicted probabilities of participation, obtained from estimation of the participation equation, in place of actual participation decisions. For the

[11]For the probit estimation of the participation equation, we include all firms that had data in 1990, even those with fewer than three years of complete data. Hence, our sample for this equation contains six more companies than used in the other equations, for a total of 325 sample firms.

other program years, 1992 to 1995, participation decisions were pre-determined; nevertheless, there may (or may not) be sample selection bias. Specifically, if the error in the participation equation is correlated with the error in the pollution equation, then using actual participation decisions in the pollution equation leads to biased and inconsistent estimates. For example, due to attributes that we do not observe in our data, 33/50 participants may have been more likely to reduce pollution even had they not joined the program (the endogenous treatment problem identified by Heckman (1978)). We allow our data to reveal any such correlation by using actual participation decisions and constructing a selection correction (an augmented inverse Mills ratio) to remove any source of inconsistency.[12]

Because participation effects may (or may not) wane over the course of the program, we measure distinct effects for each of the program years 1991-1995. This is done by constructing five participation variables that measure the incremental effect of participation on pollution in a given year; for example, the coefficient on the 1993 participation variable measures the pollution change from 1992 onwards that is attributable to a firm's participation in the 33/50 program.[13]

Third, per Hypothesis 3, firms may make pollution decisions in view of their recent history of government inspections. We test for these effects using, as an explanatory variable, a firm's lagged inspections-per-facility (LINSPFAC).[14]

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[12]The selection correction is achieved (following Vella, 1998) by constructing the fitted regressor, IMR_{ti}, where IMR_{ti}=0 for t≤1991 and, for t≥1992,

$$IMR_{ti}=p_i[\phi(\hat{\gamma}'w_i)/\Phi(\hat{\gamma}'w_i)]+(1-p_i)[-\phi(\hat{\gamma}'w_i)/(1-\Phi(\hat{\gamma}'w_i))],$$

where p_i is the participation dummy for firm i, $\hat{\gamma}$ is the estimated parameter vector for the probit estimation of the participation equation, w_i is the firm i set of explanatory variables in the participation equation, and $\phi()(\Phi())$ are normal density (distribution) functions.

[13]Our five regressors are constructed as follows: If P_t is our participation variable for year t (taking a value of zero for all years other than t) then we construct the regressors, $P^*_\tau = \sum_{t=\tau}^{1995} P_t$ for τ=1991,...,1995. We denote these variables by PSTATUS for 1991 and D92-D95 for the other years (see Table 1).

[14]Lagging, while logically sensible, avoids any potential problem of joint determination. Because we capture scale effects on pollution by including facility numbers as a regressor, the relevant measure of inspection activity is a firm's annual inspections-per-facility.

Finally, because we use predicted regressors to obtain consistent parameter estimates, standard error estimates obtained by conventional methods are biased and inconsistent (Murphy and Topel, 1985). To obtain consistent estimates of standard errors, we perform the Murphy-Topel correction to our random-effects-adjusted (quasi-differenced) data.[15]

(3) The Inspection Equation. For this equation, we have an unbalanced panel of 1257 facilities over seven years, 1989-1995, giving us 5703 facility-year observations. Several econometric issues arise in this panel.

First, our dependent variable -- facility-level annual inspections -- takes a count data form, with discrete and predominantly small values. Second, we again wish to allow for individual random effects. To account for these properties, we assume that our dependant variable is distributed Poisson, and the individual effect is normally distributed; we estimate this model by maximum likelihood.[16] A notable advantage of our random effects specification, relative to a standard Poisson model, is that it accomodates over-dispersion.

Third, contemporaneous inspections are posited to depend upon firm performance -- pollution and 33/50 program participation -- with a lag. For example, program participation decisions were made by firms principally in the last two quarters of 1991, suggesting that any effects on annual government inspections would arise in 1992 and beyond. For these years, there is, in principle, the potential for sample selection bias with respect to 33/50 participation effects, as in the pollution equation. However, in the inspection equation, sample selection -- if an issue at all -- is expected to bias our estimates against our

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[15]We also bootstrapped our sample, following List, et al. (2003) and Fredriksson, List and Millime (2003). Specifically, we obtained 250 bootstrap samples (of 319 companies each) from our data; performed our multi-step estimation for each sample; and constructed standard error estimates for our parameters from the resulting distribution of bootstrapped parameter estimates. While we do not report standard error estimates from the bootstrap procedure, they are available from the authors and are broadly consistent with the Murphy-Topel-adjusted results that we report in Table 4 (see note 19).

[16]We also estimated a random effects Poisson with the individual effect assumed to be distibuted gamma. Qualitative results were similar (see note 20). In addition, we attempted to estimate a Hausman, Hall and Grilliches (1984) model with the dependant variable assumed to be distributed as a negative binomial and the individual effect distributed beta. However, as is common with this procedure (Cameron and Trivedi, 1998), none of our estimations converged.

hypothesized effect (Hypothesis 2 that participation lowers inspection rates). The reason (per Hypothesis 1) is that participants are expected to be those who otherwise experience higher inspection rates. Nevertheless, we test for sample selection bias by implementing Terza's 1998 two-step estimator.[17] In doing so, we find no statistical evidence for sample selection bias (with a statistically insiginficant coefficient on the augmented inverse Mills ratio). We therefore proceed under a maintained hypothesis of no selection correlation.

V. Results

1) The Participation Equation. Table 3 presents selected results from estimation of the participation equation. Note that, for these and other specifications, we could not reject homoskedasticity, a premise underpinning the reported probit results.[18]

Several implications of Table 3 merit emphasis. First, larger firms with larger 33/50 releases are found to have been more likely to participate in the 33/50 program. These effects are consistent with a number of the theories / hypotheses discussed in Section III. Larger polluters are likely to have been more sensitive to any enforcement benefits of program participation; more able to preempt lobbying for tighter environmental regulations (MLH, 2000); more exposed to potential liability for environmental harm; and more exposed to potential harm from boycott threats. Notably, however, incentives for green marketing are not typically associated with larger polluters (AC), with smaller and less concentrated firms thought to enjoy greater incentives for "green" product differentiation (AG).

Second, let us turn to explanatory variables which *can* distinguish between different hypothesized motives for program participation. Statistically significant (positive)

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[17]To our knowledge, Terza's (1998) is the only known endogenous treatment correction for count data. As in our model, Terza's procedure assumes that the dependent variable is distriubted Poisson, with a random effect that is normal. However, for our purposes, a drawback of this estimator is that it assumes an observation-specific random effect, rather than the firm-specific effect that we posit in this paper.

[18]In testing for heteroskedasticity, we follow standard practice (e.g., Greene, 2000, Chapter 19) by considering a variance that is a squared exponential function of exogenous data. In our case, the exogenous data that we posit may drive any heteroskedasticity is the level of prior 33/50 releases. As reported in Table 3, the likelihood ratio statistic for the null hypothesis of homoskedasticity provides rather strong support for the null.

parameter estimates on (1) our enforcement variables (PRP, ENFORCE, INSP89-90), (2) the measures of boycott sensistivity (BC and its interaction with 33/50 releases, BC-RELEASE), and (3) per-capita Sierra Club membership (SIERRA), suggest that the potential for implicit enforcement rewards, boycott deterrence, and regulatory preemption (MLH, 2000) were important motives for 33/50 program participation.

Firms with higher levels of R&D were more likely to participate as well. However, no statistically significant link is found between industry concentration and program participation, although the estimated sign of this link is positive. While these results do not refute a "raising rivals cost" motive for program participation, they provide little evidence in its favor. More research-intensive firms may have participated because their costs of program obligations -- in lowered pollution -- were smaller; they could thereby obtain *other* program benefits (such as enforcement rewards and boycott deterrence) at lower cost.

Our results also fail to provide evidence that program participation was motivated either by the threat of future liability (with a statistically insignificant effect of strict environmental liability, STRICT) or by incentives for "green marketing" (with a statistically insignificant effect of proximity to final consumers, FG). Thus, by accounting for three other motives for program participation -- enforcement, boycott deterrence, and regulatory preemption -- we come to a strikingly different conclusion about the impact of "green marketing" incentives than does prior work (AC, KD, VA).

(2) The Pollution Equation. Table 4 presents results from estimation of the pollution equation. In all model variants, the coefficient on the augmented inverse Mills ratio is statistically significant, providing evidence for sample selection (from program participation decisions) in the predicted direction.

Several qualitative conclusions emerge from Table 4. First and most important, we find that firms' participation in the 33/50 program tends to lower pollution. These pollution reductions are statistically significant in the first two years of program operation (1991 and 1992), but persist throughout our sample period (to 1995). Second, as in prior work, we find that government inspections tend to lower firms' pollution levels. Although inspections have a direct effect on pollution that is statistically insignificant (Table 4), note that they also indirectly spur pollution reductions by promoting participation in the 33/50 program (Table 3). Third, firms may have been motivated to lower pollution in order to preempt regulation (with a statistically significant negative coefficient on SIERRA) and/or deter boycotts in states with large envrionmental constituencies (with a statistically significant negative coefficient on BC-SIERRA). Fourth, although we find little evidence that firms participated in the 33/50 program in order to "raise rivals' costs," we find some evidence that pollution reductions may have had this motive (with statistically significant negative coefficients on both firms' R&D expenditures and the measure of industry concentration, HERF). Fifth, no statistically significant link is found between pollution and either a firm's proximity to final consumers or the presence of strict environmental liability (although the estimated effect of strict liability is negative, as predicted).[19] Hence, by accounting for potential effects of enforcement activity, boycott deterrence and regulatory preemption incentives, we again find no evidence that voluntary pollution reduction activity is motivated by incentives for "green marketing."

(3) The Inspection Equation. Table 5 presents selected results from estimation of the inspections equation. We find that inspections tend to rise when a facility's prior period pollution is higher, with enforcement resources thus targeted to facilities for which inspectors can anticipate good prospects for pollutant reductions. In addition, inspection rates tend to be higher at the facilities of larger firms (with a statistically significant positive coefficient on our measure of firm size, EMP). However, most important from our estimations is the link between 33/50 program participation and government inspections. Program participation is estimated to have had only a marginal impact on inspection rates in 1992, perhaps because program-sponsored technical assistance took the form of some

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[19]When using the bootstrap procedure to estimate standard errors (see note 15), we obtain the same conclusions, with two qualifications. Using the bootstrap procedure, parameter estimates for the LRD and BC-SIERRA regressors become statistically insigificant.

short-term government oversight. However, program participants experienced statistically and quantitatively significant reductions in their inspection rates from 1993 through 1995. To help understand the quantitative significance of these effects, Table 6 presents the estimated marginal impacts of 33/50 participation on inspection rates in each of the program years, 1992-1995. We estimate that a firm's 33/50 program participation translated into a cumulative reduction of .25 inspections over the 1992-1995 period -- approximately 17 percent of the sample average inspection rate (1.5 per year). Note also that a firm's benefit of 33/50 participation, in a reduced inspection burden, tends to persist throughout the program years, 1992-1995, even though pollution reduction benefits of participation tend to wane (Table 4).[20]

VI. Conclusion

In this paper, we have studied why firms chose to participate in the EPA's voluntary 33/50 pollutant reduction program; effects that this program had on firms' pollution; and effects of program participation on subsequent government inspection activity. In doing so, we find empirical support for the "enforcement theory" of voluntary pollution reductions (Maxwell and Decker, 2002). Specifically, program participation involves firm investments in environmental auditing and technology that lowers their pollution abatement costs and thereby prompts pollution reductions (the pollution equation effect of program participation). In view of this benefit, environmental authorities implicitly offer regulatory rewards to program participants (the inspection equation effect of program participation) that spurs participation by those firms who have the most to gain from such regulatory rewards (the participation equation effect of prior inspections and pollutant releases). In sum, we find evidence in support of Hypotheses 1-3 presented at the outset of this study.

[20]In the Poisson model with a gamma distributed firm effect, estimated impacts of 33/50 participation are similar to those presented in Table 5, both in magnitude and statistical significance. However, impacts of some other variables are somewhat different. For example, the Sierra Club (SIERRA), boycott (BC), and right-to-work (RTW) variables do not have statistically significant effects in the Poisson-gamma model.

Our results also support the hypotheses that firms participated in the 33/50 program in order to forestall potential boycotts by environmental groups (Baron, 2001; Innes, 2003) and/or to preempt lobbying by these groups for tighter environmental regulation and enforcement (MLH, 2000). Pollutant reductions, beyond those prompted by participation in the 33/50 program, were another means by which firms sought to preempt regulation and boycotts. However, in contrast to prior work that did not account for the potential enforcement, boycott deterrence or regulatory preemption incentives found to be important here, we find no support for the hypothesis that firms participated in the 33/50 program, and/or reduced their pollution levels, in order to obtain any "green marketing" advantages -- that is, any consumer (price) premia for goods produced in an environmentally beneficial way (AC, AG).

Overall, this work lends support to the view that voluntary pollutant reduction programs -- carefully combined with regulatory / enforcement rewards for program participation -- can be useful and effective tools to reduce pollution and save government costs of overseeing firms' environmental performance. Voluntary programs may also offer firms the opportunity to signal their environmental commitment to potential political adversaries and thereby deter costly boycotts and political conflicts. As a result, even when consumer free-riding prevents firms from obtaining any "green premia" in the marketplace -- a failure that would otherwise doom voluntary pollution reduction efforts -- voluntary environmental programs can succeed.

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Appendix A: List of Chemicals Targeted by the 33/50 Program

Benzene	Lead and Compounds	Tetrachloroethylene
Cadmium and Compounds	Mercury and Compounds	Toluene
Carbon Tetrachloride	Methyl Ethyl Ketone	Trichloroethane
Chloroform	Methyl Isobutyl Ketone	Trichloroethylene
Chromium and Compounds	Methylene Chloride	Xylenes
Cyanides	Nickel and Compounds	

Source: 33/50 Program: the Final Record. EPA, March 1999.

Appendix B: SIC Codes of Manufacturing Industries

SIC CODE	INDUSTY
20	FOOD AND KINDRED PRODUCTS
21	TOBACCO MANUFACTURING
22	TEXTILE MILL PRODUCTS
23	APPAREL AND OTHER TEXTILE PRODUCTS
24	LUMBER AND WOOD PRODUCTS
25	FURNITURE AND FIXTURES
26	PAPER AND ALLIED PRODUCTS
27	PRINTING AND PUBLISHING
28	CHEMICALS AND ALLIED PRODUCTS
29	PETROLEUM AND COAL PRODUCTS
30	RUBBER AND MISC. PLASTIC PRODUCTS
31	LEATHER AND LEATHER PRODUCTS
32	STONE, CLAY, GLASS AND CONCRETE PRODUCTS
33	PRIMARY METAL INDUSTRIES
34	FABRICATED METAL PRODUCTS
35	INDUSTRIAL MACHINERY AND COMPUTER EQUIPMENT
36	ELECTRICAL EQUIPMENT AND COMPONENTS
37	TRANSPORTATION EQUIPMENT
38	MEASURING AND ANALYZING INSTRUMENTS
39	MISC. MANUFACTURING INDUSTRIES

Source: http://www.siccode.com

Table 1: Variable Definitions

RELEASE	Total firm releases of 33/50 pollutants (millions of pounds) (annual)
LRELFAC	Lagged per-facility firm releases of 33/50 pollutants
PSTATUS	Firm's participation status in 33/50 program (estimated for 1991, actual for 1992-95)
D92 - D95	Dummies that equal 1 if a firm is a 33/50 participant (note 13)
INSPECT	Number of a facility's CAA inspections (annual)
DIFREL	Change in total firm releases of 33/50 pollutants from 1988-1990
INSP89-90	Number of CAA inspections of firm facilities, 1989-90
ENFORCE	Dummy that equals 1 if firm had an enforcement action in 1989-90
LINSPFAC	Firm lagged inspections per facility (annual)
PRP	Number of Superfund sites for which a firm is a PRP, 1990
SIC28 - SIC38	Dummies for a firm's primary two-digit SIC class
LRD	Lagged firm expenditures on R&D ($millions) (annual)
LEMP	Lagged number of firm employees (1000's) (annual)
FAC	Number of firm facilities (annual)
HERF	Herfindahl index for firm's two-digit SIC class
BC	Dummy that equals one if firm operates in an SIC that was subject to contemporaneous boycott, 1992
FG	Dummy that equals one if firm produces a final good (determined by a firm's primary four-digit SIC class)
SG	Firm percentage sales growth (annual)
SIERRA	Sierra Club members per capita in firm/facility home state (annual)
STRICT	Dummy that equals one if firm's/facility's home state has a strict liability statute, 1988
RTW	Dummy that equals one if firm's/facility's home state has a right-to-work statute, 1988
SPENDAQP	State expenditures on air quality programs in the firm's/facility's home state, 1988
LAWYERS	Number of lawyers per capita in firm/facility home state, 1988
EDUC	Percentage of college degrees in firm/facility home state population, 1990
ATTAIN	Dummy that equals one if a facility's home county is out of attainment with clean air laws in any year, 1992-1995
CDENSITY	Population density of facility's home county, 1990

Table 2: Descriptive Statistics

	Participants		Non-participants	
Variable	Mean	Standard Deviation	Mean	Standard Deviation
DIFREL	-0.1881	0.6243	-0.0576	0.1833
RELEASE	0.8284	1.5340	0.1044	0.1722
LEMP	34.4284	71.4741	5.0099	7.1058
HERF	0.4481	0.1443	0.4939	0.1633
PRP	5.4061	9.7499	1.0875	2.2301
ENFORCE	0.4242	0.4957	0.1000	0.3009
INSP89-90	13.4545	19.9592	2.6000	4.7731
SIERRA	2.3322	1.2042	2.8645	1.9492
STRICT	0.8061	0.3966	0.7625	0.4269
BC	0.3818	0.4873	0.2500	0.4344
FG	0.6606	0.4749	0.6250	0.4856
LRD	211.7544	549.1934	18.3815	46.8655
RTW	0.1818	0.3869	0.2000	0.4013
SPENDAQP	1.3196	0.7372	1.3411	0.7383
LAWYERS	3.1955	1.0193	3.2358	1.0209
EDUC	20.9642	3.8932	21.0144	3.7414
SIC 28	0.2121	0.4101	0.1250	0.3318
SIC 33	0.0970	0.2968	0.0563	0.2311
SIC 34	0.0545	0.2278	0.1063	0.3091
SIC 35	0.1576	0.3655	0.1875	0.3915
SIC 36	0.1273	0.3343	0.1438	0.3519
SIC 37	0.1515	0.3596	0.0625	0.2428
SIC 38	0.0545	0.2278	0.1313	0.3387

Notes: Mean and standard deviation of variables used in the Probit models. Descriptive statistics for time varying variables are obtained using 1990 data.

Table 3: Probit Estimation of the Participation Equation

Hypothesis tested	Variable	Model I		Model II		Model III	
		estimate	t-ratio	estimate	t-ratio	estimate	t-ratio
Free-riding	DIFREL	0.528	1.459	0.470	0.948	0.475	0.962
	PRP	0.042*	1.690	-0.127	-1.514	-0.124	-1.458
Enforcement	PRP^2			0.013	1.805	0.0127*	1.779
effects	ENFORCE	0.603**	2.362	0.567**	2.181	0.557**	2.136
	INSP89-90	0.033**	2.142	0.028*	1.684	0.0273*	1.661
Raising rivals	HERF	1.685	1.425	1.404	1.165	1.489	1.236
costs	LRD	0.004**	2.080	0.0052**	3.059	0.0044**	2.249
Preemption of	SIERRA	0.562*	1.690	0.561*	1.656	0.569*	1.681
regulation	SIERRA^2	-0.093**	-2.418	-0.09**	-2.275	-0.090**	-2.271
Liability	STRICT	0.088	0.278	-0.102	-0.304	-0.003	-0.009
effects	STRICT-LEMP			0.0221*	1.770	0.005	0.226
Boycott	BC	0.538	0.948	1.174*	1.787	1.089*	1.651
deterrence	BC-RELEASE	1.3284*	1.755				
	BC-SIERRA			-0.078	-0.542	-0.073	-0.516
Green marketing	FG	0.245	0.668	0.335	0.886	0.345	0.910
	LEMP	0.0261**	2.200			0.020	0.843
	RELEASE			1.159**	2.252	1.062**	2.031
Firm-specific	RELEASE^2			-0.119	-1.515	-0.111	-1.293
effects and state	RTW	-0.017	-0.061	-0.046	-0.168	-0.055	-0.197
characteristics	SPENDAQP	0.035	0.211	0.022	0.126	0.020	0.116
	LAWYERS	-0.250	-1.112	-0.285	-1.260	-0.303	-1.324
	EDUC	0.034	0.573	0.049	0.814	0.049	0.810
	SIC 28	1.761**	2.418	1.839**	2.475	1.906**	2.546
	SIC 33	1.691**	2.164	1.559**	1.944	1.603**	1.986
Industry fixed	SIC 34	0.651	1.125	0.718	1.214	0.751	1.264
effects	SIC 35	0.9991*	1.785	0.9704*	1.704	1.0189*	1.777
	SIC 36	0.592	1.272	0.280	0.623	0.396	0.840
	SIC 37	0.396	0.745	0.181	0.352	0.295	0.553
	SIC 38	0.911	1.516	1.054*	1.709	1.069*	1.726
	CONSTANT	-3.538**	-2.904	-3.546**	-2.867	-3.678**	-2.945
Log-likelihood value			-136.86		-134.34		-133.93
H_o: all slopes equal zero	Test statistic {p-value}	176.74	{0.00}	181.78	{0.00}	182.60	{0.00}
Test of heteroscedasticity	Test statistic $\{\chi^2_{0.05}(1)\}$	2.12	{3.84}	0.04	{3.84}	0.48	{3.84}
Percent correctly classified			0.80		0.79		0.79
Number of observations			325		325		325

Notes: The dependant variable is the 33/50 program participation dummy. The dataset is a cross-section of 325 firms, with time-varying variables measured as of 1990. The hypothesis that all the slope coefficients are jointly insignificant is rejected. The likelihood ratio test of heteroscedasticity (Harvey 1976) due to firm differences in aggregate 33/50 releases fails to reject the null of homoscedasticity at the 5% level for all models. Squared variables are denoted by an addition of "^2" to the variable and interactions variables are denoted with hyphens. ** Statistically significant at the 5% level or better (two-tail). * Statistically significant at the 10% level.

Table 4: Random Effects Estimation of the Pollution Equation.

		Model I		Model II		Model III	
Hypothesis tested	Variable	estimate	t-ratio	estimate	t-ratio	estimate	t-ratio
	PRP	-0.019	-0.494	-0.0206	-0.600	-0.0221	-0.635
Enforcement	PRP^2	0.001	1.483	0.0013*	1.681	0.0014*	1.682
effects	LINSPFAC	-0.020	-1.311	-0.0224	-1.585	-0.0225	-1.591
Raising rivals	HERF	-0.455*	-1.886	-0.4072*	-1.866	-0.395*	-1.830
costs	LRD	-0.0010**	-6.620	-0.0011**	-8.175	-0.0011**	-8.110
Preemption of	SIERRA	-0.066*	-1.808	-0.0836**	-2.362	-0.0805**	-2.276
regulation and	BC	0.154	0.330			0.148	0.339
boycott deterrence	BC-SIERRA	-0.114*	-1.652	-0.0998	-1.561	-0.1082*	-1.672
Green marketing	FG	-0.098	-0.317	-0.1050	-0.359		
	PSTATUS	-0.179**	-2.845	-0.1636**	-2.862	-0.1644**	-2.865
	D92	-0.125**	-2.503	-0.1373**	-2.963	-0.1377**	-2.974
Effects of the	D93	-0.029	-0.695				
33/50 program	D94	-0.033	-0.817				
	D95	-0.055	-1.017				
Liability effects	STRICT	-0.32677	-0.804	-0.346	-0.960	-0.340704	-0.939
	LAWYERS	-0.27417	-1.015	-0.2837	-1.176	-0.278	-1.145
	LEMP	0.0266**	7.370	0.0269**	8.377	0.0270**	8.345
	LEMP^2	-.00003**	-6.971	-.000032**	-7.482	-.000032**	-7.486
Firm-specific	FAC	0.013	0.266	0.016	0.380	0.016	0.376
effects and state	FAC^2	0.001	0.429	0.001	0.515	0.001	0.514
characteristics	SG	.000096	0.772	.000096	0.778	.000096	0.779
	RTW	-0.25136	-0.969	-0.2518	-1.030	-0.2537	-1.033
	EDUC	0.074	1.033	0.077	1.221	0.076	1.185
	SPENDAQP	0.035	0.226	0.046	0.321	0.045	0.315
	SIC 28	-0.101	-0.243	-0.1429	-0.394	-0.0210	-0.051
	SIC 33	-0.127	-0.198	-0.175	-0.329	-0.018	-0.033
Industry fixed	SIC 34	-0.240	-0.490	-0.2816	-0.749	-0.1705	-0.393
effects	SIC 35	-0.2206	-0.505	-0.2641	-0.862	-0.2162	-0.541
	SIC 36	0.005	0.014	0.088	0.294	0.009	0.028
	SIC 37	0.256	0.636	0.313	0.772	0.235	0.623
	SIC 38	-0.042	-0.077	-0.065	-0.147	0.082	0.186
Self-selection bias	IMR	0.148**	3.145	0.1195**	2.973	0.12008**	2.971
	TIME	3.6E-05	0.044	-0.0083	-1.095	-0.0080	-1.055
	CONSTANT	0.135	0.135	0.927	1.010	0.750	0.836
	No. obs		1879		1879		1879
	R^2		0.3572		0.3549		0.3549
LM test of OLS vs. RE	Test statistic $\{\chi^2_{0.05}(1)\}$	2665.26	{3.84}	2644.71	{3.84}	2640.31	{3.84}

Note: The dependant variable is RELEASE. The Breush-Pagan LM test of OLS vs. Random Effects rejects the null of OLS. ** Statistically significant at the 5% level or better. * Statistically significant at the 10% level.

Table 5: Random Effects Poisson Estimates of the Inspections Equation

	Variable	Model I estimate	Model I t-ratio	Model II estimate	Model II t-ratio
	D92	-0.0480	-0.524		
Effects of the 33/50 program	D93	-0.2895**	-2. 807	-0.1793**	-2.230
	D94	0.0934	0.889		
	D95	-0.2619*	-2.470		
	SIERRA	-0.1628**	-3.917	-0.1801**	-4.526
	BC	0.3086	1.515	0.5282**	2.827
	ATTAIN	-0.1844	-1.583	-0.0892	-0.820
	CDENSITY	0.0019	0.698	-0.0008	-0.293
	LRELFAC	0.00012**	2.086	0.0002**	3.296
	EMP	0.0033**	6.615	0.0025**	4.575
Firm-specific and County characteristics	SPENDAQP	0.5973**	5.972	0.6290**	6.807
	RTW	0.2785**	2.268	0.1975*	1.639
	EDUC	0.0042	0.136	0.0013	0.047
	STRICT	-0.1409	-1.109	-0.1106	-0.942
	LAWYERS	-0.0967	-0.856	-0.0960	-0.941
	SIC 28	-0.6679**	-3.352	-0.4923**	-2.887
	SIC 33	-0.8669**	-4.059	-0.6948**	-3.718
Industry fixed effects	SIC 34	-1.0863**	-5.100	-0.7484**	-4.033
	SIC 35	-0.7271**	-3.440	-0.5534**	-3.084
	SIC 36	-1.3818**	-6.212	-1.4104**	-6.475
	SIC 37	-0.8024**	-4.784	-0.9492**	-5.739
	SIC 38	-1.0381**	-3.147	-0.7565**	-2.574
	CONSTANT	-9.4063**	-3.146	-6.5026**	-2.823
	TIME	0.0865**	2.678	0.0537**	2.184
	Number of Observations		5703		5703
	Log-likelihood		-4026.99		-4031.17

Note: The dependant variable is INSPECT. **Statistically significant at the 5% level or better. *Statistically significant at the 10% level.

Table 6: Marginal Effects of 33/50 Program Participation on Government Inspections

Year	Marginal effect	t-ratio
1992	-0.0348	-0.852
1993	-0.1698**	-3.436
1994	-0.1255**	-2.269
1995	-0.2469**	-3.436

Note: The marginal effects represent estimates of the 33/50 program's impact on government inspections in each program-year from 1992 to 1995, using model I in table 5. **Statistically significant at the 5% level or better. *Statistically significant at the 10% level.