VOLUNTARY POLLUTION REDUCTIONS AND THE ENFORCEMENT OF ENVIRONMENTAL LAW: AN EMPIRICAL STUDY OF THE 33/50 PROGRAM

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This paper studies determinants and effects of firms' participation in the 33/50 program, a voluntary pollution reduction program (VPR) initiated by government regulators. We examine a wide range of explanations for voluntary corporate environmentalism and find evidence in support of an "enforcement theory" that predicts: (i) VPR participation is rewarded by relaxed regulatory scrutiny; (ii) the anticipation of this reward spurs firms to participate in the program; and (iii) the program rewards regulators with reduced pollution. We also find that 33/50 participation was more likely for firms operating in states with larger environmentalist constituencies.

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

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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 volunteer for costly pollution reduction efforts. Arora and Gangopadhyay (AG, 1995) argue that firms want to attract a clientele of "green consumers" 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 regulatory standards (Maxwell, Lyon and Hackett (MLH), 2000); spur tighter environmental standards that "raise rivals' costs" (Salop and Scheffman, 1983; Innes and Bial, 2002); avoid future environmental liability; and/or deter boycotts by environmental interest groups (Baron, 2001; Innes, 2006).

Another potential motive for voluntary environmentalism is to lessen the scrutiny of environmental authorities, reducing the frequency of costly environmental inspections and enforcement actions. The EPA officially claims that such rewards were not offered to 33/50 program participants.[1] Nevertheless, such rewards, promised implicitly if not officially, may represent an optimal government policy to promote participation in a voluntary pollution reduction program (VPR). The societal benefit of a VPR is to prompt participating firms to adopt management practices that reduce their costs of pollution abatement, leading ultimately to

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[1]On the 33/50 program, the EPA 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." However, in the recent “Performance Track” program, the EPA offers a number of explicit regulatory rewards to participants, including less frequent reporting requirements, more flexible air permits, and expedited reviews for water discharge permits (www.epa.gov/performancetrack).

pollution reductions (Maxwell and Decker, 2006).[2] While intuitively compelling, the empirical strength of this "enforcement theory" for VPRs has yet to be studied.

The purpose of this paper is to examine (1) the empirical validity of this enforcement-based spur to participation in the EPA's 33/50 program, among many other potential participation motives, 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 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.

In the former literature, scholars study several determinants of participation in voluntary programs (AC, KD, VA) and, in addition, effects of the 33/50 program on pollution (KD; Vidovic and Khanna (VK), 2007). They find that participation in 33/50 was motivated, in part, by green marketing and potential liability, with larger firms found to be more likely to participate (AC, KD, VA). KD find that 33/50 led to significant pollutant reductions; however, VK argue that this effect vanishes when accounting for time effects. In contrast to our focus, this literature does not study effects of voluntary over-compliance on government enforcement and does not consider potential effects of boycott threats or incentives for regulatory preemption (MLH) or liability law.[3] We thus

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[2]Maxwell and Decker (2006) show that a reduced probability of enforcement may result from a firm's adoption of abatement-cost-reducing investments, thus spurring these investments apriori. 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]In this literature, the only study that allows for any enforcement effects is VA. VA consider the potential impact of prior RCRA corrective actions on 33/50 participation, finding some evidence

examine a more complete range of possible explanations for voluntary pollution abatement efforts than in prior work and, in doing so, find evidence for enforcement, boycott deterrence, and regulatory preemption motives for voluntary efforts, but no “green marketing” incentive effects. In addition, unlike others, we study the timing of the 33/50 program’s impacts, permitting us to identify when it was effective in reducing pollution. In doing so, we consider time effects (like VK), but examine a broader array of manufacturing firms.

A second literature studies determinants of the government's environmental enforcement activity, and its impact on pollution (e.g., Magat and Viscusi, 1990; Gray and Deily, 1996; Nadeau, 1997). 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.

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that such enforcement actions make participation more likely. We study the impact of both regulatory inspections and enforcement actions and, unlike VA, also model program impacts on pollution and government enforcement activity. 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.

[4]See also related work by Harford and Harrington (1991) and Heyes and Rickman (1999). In addition, consistent with this theory, Decker (2005) finds that government inspection activity responds to reductions in reported toxic pollutant releases as well as reductions in regulated pollutant releases and a good statutory compliance history.

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.

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 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, those invited in March and July of that year.

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; participating firms may have been more apt to reduce pollution, regardless of participation in the 33/50 program.

III. HYPOTHESES

Participation in the 33/50 program, while 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). The process of planning for emissions reductions, including possible managerial changes and environmental auditing procedures, could yield the pollution reductions that were the program's objective.

Although the EPA 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, to spur 33/50 participation and

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

associated pollution abatement innovations, the EPA could have afforded participants a more cooperative / less adversarial treatment of potential infractions, with fewer costly inspections and enforcement actions – over and beyond reductions in enforcement rates due to reduced pollution (Maxwell and Decker, 2006).[6] The value of this regulatory reward to 33/50 participation 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. After joining the program, 33/50 participants experienced lower rates of government inspection, fewer enforcement actions, and lower levels of pollution.

A number of theories suggest additional motives for 33/50 participation and pollution reductions:

Hypothesis 3. 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) had a greater incentive and ability to preempt regulation because it was a larger firm and operated in states with larger environmentalist constituencies (regulatory preemption);

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

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[6]Firms may be averse to inspections and enforcement actions, not only because of their direct costs, but also because of their potential to ignite adverse public reaction in the media and financial markets (e.g., Hamilton, 1995).

(e) was in a more concentrated industry and invested more in research and development (strategic and cost effects).

For "green marketing," a firm's ability to establish a market niche for goods produced in an environmentally friendly way is tied to its proximity to consumers (AC, KD, VA); we therefore 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"). To test for incentives to deter consumer boycotts by environmental interest groups (Baron, 2001; Innes, 2006; Henriques and Sadorsky, 1996), 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.[7] We denote this variable BC.

Incentives for regulatory preemption arise when voluntary corporate environmentalism can deter environmental interest groups from lobbying for tighter environmental regulations (Maxwell, Lyon and Hackett, 2000). Because these incentives are likely to be greater in states with larger environmental constituencies (where the public sensitivity to a firm's pollution is likely to be greater, as is environmental groups' ability to successfully lobby the government for change), we control for them using the per-capita Sierra Club membership in a plant's home state (SIERRA), averaged across plants to obtain a firm-level variable.

[7] 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 or plant in our sample is in an industry that produces a targeted product (based on the firm's or plant's primary SIC classification), our boycott variable is assigned a value of one for that firm or plant. In practice, boycotts are rare, as theory predicts (Baron, 2001). In fact, none of the firms in our sample were actually boycotted. Hence, our boycott variable attempts to measure the potential likelihood that a firm might face a boycott threat.

Building on Alberini and Austin (1999), we capture the liability motive for pollution reduction using a dummy variable that takes a value of one if a plant's home state has strict (vs. negligence) environmental liability (STRICT); for a firm, this variable is constructed by averaging these zero-one values for the firm's plants.

Finally, we include measures of industry concentration (the Herfindahl index, HERF) and firm-level R&D expenditures (R&D) to control for a number of relevant forces. A research-intensive firm in a more concentrated industry is potentially more prone to voluntary environmentalism as a strategy to prompt tighter pollution standards that disadvantage the firm's rivals (Salop and Scheffman, 1983; Innes and Bial, 2002). In addition, more concentrated industries are better able to coordinate in the preemption of regulation (Maxwell, et al., 2000), and R&D can directly lower costs of pollutant abatement, both favoring 33/50 participation and pollution reduction.

IV. THE DATA

We estimate four equations in order to explain (1) firms' participation in the 33/50 program (in 1991); (2) firms' annual emissions of 33/50 pollutants (toxicity weighted, 1989-1995); (3) the government's (State and Federal) annual number of environmental inspections of firms' facilities (1989-1995); and (4) the government's annual number of enforcement actions against firms' facilities (1989-1995). Inspections and enforcement actions are important for our purposes because they can lead to potentially costly disputes between a facility/firm and government regulators. Even actions considered minor in and of themselves are notices that, if regulators are not quickly satisfied with compliance measures, can be followed by costly legal disputes, remedies and penalties.[8]

Several data sources are used to estimate these equations. Financial and employment data is obtained from the Standard & Poor's Compustat database. From the EPA's Office of Environmental Information Records, we obtain data on 33/50 participation, and facility-level government inspections, compliance status and enforcement actions under the Clean Air Act (CAA) (1988-1995).[9] The Toxic Release Inventory (TRI) provides 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, inspections, and enforcement actions are obtained by aggregating across each firm's facilities. From the Sierra Club we have data on its state membership (from 1989-1995, measured per capita). The Maxwell, Lyon and Hackett (2000) dataset provides information on state characteristics (1988), including 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. The number of 1988 Superfund sites for which a firm was a potentially

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[8] Enforcement actions can range from notices of violation to administrative orders for compliance to initiations of civil lawsuits to filing criminal charges against responsible firms and individuals (www.epa.gov/region9/enforcement). Beyond legal costs, costs to firms of remedies and penalties can be very large. For example, recent enforcement actions in EPA's Region 4 under the CAA have led to remedies and penalties ranging from the very small to over $130 million (www.epa.gov/region4/ead/general/recent).

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

responsible party (PRP) is obtained from the EPA's Superfund Office. County unemployment rates (1989-1995) and state GDP growth rates (1989-1995) are obtained from the U.S. Bureau of Labor Statistics and the Bureau of Economic Analysis (U.S. Department of Commerce), respectively. County attainment status (whether a facility's home county is designated by the EPA to be out of attainment with clean air laws) is obtained from the EPA (www.epa.gov/oar/oaqps/greenbk). County population density (1990) is 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. Limiting attention to firms with three years or more of complete data over 1988-1995, and allowing for lagging, we 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. From Table 2, we can compare attributes of 33/50 program participants to those of non-participants. Under the null of equal mean values, the calculated z statistics are asymptotically distributed as standard normals. In a statistical sense, most of the variables have significantly different means for participants than non-participants. In particular, participants were significantly larger (with higher weighted 33/50 releases and levels of employment), more research intensive (with higher levels of lagged R&D expenditure), and more likely to be in industries that were subject to boycotts.

Participants were also subject to more regulatory oversight. We use three variables to measure prior regulatory scrutiny: (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 (ENF89-90), and (iii) the number of Superfund sites for which a firm is a potentially responsible party (PRP). 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. For all three measures, Table 2 indicates that participants were subject to more prior enforcement scrutiny than non-participants, with more inspections, a higher likelihood of enforcement action, and more Superfund activity. These statistics provide some preliminary evidence for our Hypothesis 1.

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). From Table 2, we see that participants in our sample experienced significantly greater reductions in 33/50 releases prior to the program’s onset (1988-1990); however, as a proportion of 1988 releases, pre-program (1988-90) reductions in participant emissions were only 18.5 percent, substantially less than for non-participants (35.5 percent).

Did 33/50 releases fall proportionally more for participant firms vs. non-participants, from their initial pre-program (1991) level to their final post-program (1995) level? And did average annual rates of inspection / enforcement action rise less for program participants (vs. non-participants) from their pre-program (1989-91) levels to their post-program (1992-95) levels? For our sample, Table 3 reveals that participants experienced approximately a 16.5 percent greater reduction in releases from pre to post-program, half again as much as the entire release reduction experienced by non-participants. Similarly, while the average number of non-participant enforcement actions more than doubled (multiplied by over 2.57), corresponding participant enforcement numbers increased by less than fifty percent in the post-program years. And while non-participants experienced over an 8 percent rise in inspection rates from the pre to post-program years, participants experienced a 6.6 percent decline.[10] These statistics are suggestive of the participation effects that we conjecture in Hypothesis 2.

V. ECONOMETRICS

(1) The Participation Equation. We estimate a Probit model of firms' decisions to participate (or not) in the 33/50 program in 1991, using lagged cross-section explanatory data.[11] 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. To estimate the impact of 33/50 on firms' chemical releases, we have an unbalanced panel of 319 companies for seven years, 1989-1995, giving us a total of

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10 Although inspection *rates* fell for participants over the program years, the average annual *number* of inspections rose for both participants and non-participants – and by approximately the same amount. Hence, participant facilities experiencing multiple inspections apparently did not enjoy significantly fewer inspections overall. A likely reason is that this simple calculation fails to control for effects of firm and facility size (among other variables) on inspection numbers. With participant firms much larger on average than non-participant firms, and increased inspection activity in post-program (Clinton Administration) years targeted more at the larger firms, we expect to see a larger increase in inspection numbers for participant (vs. non-participant) firms in the absence of the 33/50 program.

11 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.

1879 company-year observations. We control for enforcement effects using a firm's lagged inspections-per-facility (LINSPFAC), and account for time effects by including year dummies.[12]

On the econometrics, there are several issues. First, we consider both fixed and random effect specifications, and present a Hausman test for the alternatives. In the random effects model, we include our key industry dummies. In the fixed effects specification, we construct robust standard errors that (as with random effects) are clustered by facility.

Second, we wish to test for effects of participation in the 33/50 program on 33/50 releases. As program participation occurred late in 1991, we model participation effects only from 1992 onwards. Although participation decisions were pre-determined in these years, there may nevertheless be sample selection bias; 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)). If, as a result, the error in the participation equation is correlated with the error in the pollution equation, then using actual participation decisions in the pollution equation, without including a selection correction, leads to biased and inconsistent estimates. We allow our data to reveal such correlation by using actual participation decisions and constructing a selection correction (an augmented inverse Mills ratio, IMR).[13] Resulting coefficient estimates are consistent (Vella, 1998).

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[12]We considered two alternative measures to capture time effects, a time (year) variable and time (year) dummies for all but one year of our sample. In all of our pollution equations, constraint tests reject the year variable restriction in favor of the time dummies, with p-values less than 0.001. Hence, we present results using year dummies.

[13]The selection correction is achieved (following Vella, 1998) by constructing the fitted regressor, IMR_{ti}, where $IMR_{ti}=0$ for $t\leq1991$ and, for $t\geq1992$,

Because participation effects may (or may not) wane over the course of the program, we measure distinct effects for each of the program years 1992-1995. This is done by constructing four 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 1993 onwards that is attributable to a firm's 33/50 participation.[14]

Finally, because we use a predicted regressor (the augmented IMR) to obtain consistent parameter estimates, standard error estimates obtained by conventional methods are inconsistent (Murphy and Topel, 1985). To obtain consistent estimates of standard errors, we perform the Murphy-Topel correction.

(3) The Inspection and Enforcement Action Equations. For these equations, we have an unbalanced panel of 1257 facilities over seven years, 1989-1995, giving us 5703 facility-year observations. Here we include additional explanatory variables known to be relevant for enforcement activity (see, for example, Deily and Gray, 1991; Gray and Deily, 1996; Decker,

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

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 (from our "full" Model III of Table 4 below), w_i is the firm i set of explanatory variables in the participation equation, and $\phi()(\Phi())$ are normal density (distribution) functions.

[14]Our four 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=1992,...,1995. We denote these variables by PART92-PART95 (see Table 1).

2005; Stafford, 2002). In particular, for the county in which a facility operates there is the time-varying attainment status (NONATTAIN, a dummy variable that equals one if the EPA deems the county to be out of attainment with clean air laws), population density (CDENSITY), unemployment rate (URATE), and growth in gross state product (GSPG). In addition, a facility's prior compliance status (LOUTCOMP, the number of times in a given year that the EPA deems the facility to be out of compliance) can affect government enforcement activity; to avoid the potential for joint endogeneity, we lag this variable two years. Similarly, a facility's lagged enforcement actions (LENFORCE) can affect the government's inspection strategy; conversely, a facility's lagged number of inspections (LINSPECT) can affect the subsequent probability (and number) of enforcement actions.[15] Time effects are incorporated with a time (year) variable.[16]

Again a number of issues arise on the econometrics. First, inspections take a count data form, with discrete and predominantly small values, and a large proportion of observations that are zeroes and ones. We therefore consider both count (Poisson) and binary (Probit) models, each estimated by maximum likelihood. With enforcement actions, 97 percent of the observations are zeroes and ones, and we therefore restrict attention to the Probit model.[17] In the Probit models, the

[15] We are indebted to the referee for suggesting many of these regressors.

[16] In all cases, we test the linear restrictions implied by a year variable specification, vis-à-vis time (year) dummies, and do not reject the year variable model at any reasonable level of significance (with p-values of the test statistic between .19 and .92).

[17] We estimated a variety of Poisson models for enforcement actions as well, obtaining qualitatively similar results. For both equations, we also attempted to estimate zero-inflated count (Poisson) models with normal random effects. These estimations failed to converge in most cases,

dependant variable takes a value of one whenever a facility received at least one inspection (enforcement action) in a given year. Second, in addition to including fixed industry dummies, we allow for individual effects that are assumed to be random and normally distributed.[18]

Third, contemporaneous inspections and enforcement actions are posited to depend upon firm performance – pollution and 33/50 program participation – with a lag. There is nevertheless the potential for sample selection bias with respect to 33/50 participation effects, as in the pollution equation. For the Poisson model, we test for selection effects by implementing Terza's 1998 two-step estimator.[19] For our binary models, we test for selection correlation using a

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and when they did, Vuong statistics were small (.25 and .35), failing to support the zero-inflated-Poisson model.

[18]On theoretical grounds, random effects are indicated because ours is a relatively small sample from the overall population of 33/50 polluters. In addition, on practical grounds, fixed effects are problematic here. With fixed effects, the Poisson model imposes the constraint that mean equals variance; random effects, however, accommodate over-dispersion. In our Poisson model, we test for over-dispersion and reject the mean-equal-variance constraint. For binary choice (Probit) models, fixed effects models are known to be unworkable (Greene, 2000) and, as a result, could not be estimated with our data. A well-known alternative count model that accommodates over-dispersion, even with fixed effects, is that of Hausman, Hall and Griliches (1984) wherein the dependant variable is assumed to be distributed as a negative binomial and the individual effect is distributed beta. We attempted to estimate the negative binomial model as well; however, as is common with this procedure (Cameron and Trivedi, 1998), none of our estimations converged.

[19]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 distributed

bivariate Probit estimator (with 33/50 participation). In all cases, we find no statistical evidence for cross-equation correlation.[20] Although we lag 33/50 releases, there is also the potential for their endogeneity; however, in statistical tests, we do not reject the null of exogeneity in any of the models.[21] We therefore proceed under the maintained hypothesis that lagged releases and lagged participation regressors are exogenous.

V. RESULTS

1) The Participation Equation. Table 4 presents selected results from estimation of the participation equation. We present three models.[22] The first is a parsimonious specification,

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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.

[20] In the Terza and bivariate Probit estimations of the inspections equation, test statistics for the null of no selection correlation are constructed using fitted values for lagged releases and have p-values of .57 and .15, respectively (for our Table 6 models). For the enforcement equation, the corresponding p-value for the Probit model is .83.

[21] The Hausman test is a joint test of exogeneity and instrument quality. Our key identifying instrument for lagged releases is twice lagged R&D. As this instrument is highly correlated with lagged releases (in the sense of Bound, et al., 1995), we can reasonably interpret the Hausman statistic as a test of exogeneity. For the Poisson and Probit inspections models reported below, the test statistics (p values) are 1.27 (.26) and less than .0001 (.99); for the enforcement action model, the corresponding statistic is 2.69 (.11).

[22]In all three models, we test for heteroskedasticity, following standard practice (Greene, 2000, Chapter 19; Harvey, 1976) by considering a variance that is an exponential function of squared

including only the enforcement measures (INSP89-90, ENF89-90, PRP) and lagged (1990) releases. The second adds the series of other correlates in our data, and the third (our most exhaustive) adds squares of particularly important regressors (in order to capture non-linearities) and several interactions. Because strict liability is more likely to be effective in a litigation-intensive state with more lawyers and/or on larger firms with deeper pockets, we include the interactions STRICT-LAWYERS and STRICT-LEMP. In addition, a number of variables may substitute or complement one another in motivating 33/50 participation, including initial reductions in releases (DIFREL) and levels of lagged R&D (LRD), and levels of initial releases (RELEASE) and "preemption" forces (SIERRA); we therefore include the interactions LRD-DIFREL and SIERRA-RELEASE. Because boycott threats are more likely to arise against larger firms (Innes, 2006), we interact BC with each firm's number of employees, BC-LEMP. Finally, we interact FG and RELEASE as green marketing motives can be either more acute for large firms with large releases or, alternately, for small firms seeking to identify a niche.

Our estimations reveal statistically significant (positive) effects of all enforcement variables (PRP, ENF89-90, INSP89-90) in all of our specifications.[23] Adding correlates does not

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exogenous data (in our case, firm employment LEMP). In Model I, we do not reject homoskedasticity and thus present Probit results under this premise. In Models II and III, we reject homoskedasticity and therefore report heteroskedasticity-corrected estimation results.

[23] To test Hypothesis 1, we use a two year (rather than one year) history of regulatory actions in an attempt to capture a more complete picture of a firm's recent enforcement experience (as done also in Decker (2005), for example). We experimented with a variable measuring the count of enforcement actions to which firms were exposed over the pre-program period 1989-90. With this added variable, ENF89-90 remains a significant explanator of participation, while the number of

appear to weaken these effects, which are substantial. For example, having a prior (1989-90) enforcement action is estimated to increase the likelihood of 33/50 participation by 12 percent in Model III (24 percent of the average rate of participation). We thus find evidence in favor of Hypothesis 1 (the enforcement motive for participation).

In addition, we find that firms operating in states with larger per-capita Sierra Club memberships were more likely to join the 33/50 program, although this effect declines as our SIERRA variable and 33/50 releases get larger. Evaluated at sample means, the estimated marginal impact of SIERRA on the participation index estimated in Table 4 (accounting for its effect on level, squared and interaction variables) is .245 in Model II and .143 in Model III. Stated differently, a one percent (of sample mean) increase in SIERRA is estimated to increase the participation rate by 2.4 percent in Model II and 1.4 percent in Model III. We thus find some support for regulatory preemption (Hypothesis 3(c) of MLH) as a motive for 33/50 participation.

Our measure of boycott sensitivity (BC) also has a positive impact on participation, one that is statistically significant in our "exhaustive" Model III but not in Model II. Similarly, our measure of prior (1988-90) release reductions (DIFREL) has a positive impact on participation, but a statistically significant one only in Model III. This positive effect implies that firms which have smaller prior reductions in releases were more likely to participate in the 33/50 program, perhaps because they had more to gain from programmatic technical assistance; hence, we do not find evidence for "free-riding" as a motive for participation. Finally, we do not find evidence that program participation was spurred by either liability law (with insignificant coefficients on STRICT and STRICT-LEMP) or incentives for "green marketing" (with statistically insignificant

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prior actions has a statistically insignificant coefficient. Hence, our data suggests that having had a prior enforcement action was an important driver of 33/50 participation, but the number of actions was not.

effects of proximity to final consumers, FG, and its interaction with pollutant releases, FG-RELEASE).

(2) The Pollution Equation. Table 5 presents results from estimation of the pollution equation. We present two representative models, one each with random effects and fixed effects.[24] For all model variants, the coefficient on the augmented inverse Mills ratio (calculated using fitted values from our Model III participation estimation, per note 13) is statistically significant, indicating sample selection from program participation decisions in the expected direction.

We find that firms' participation in the 33/50 program lowered their pollution, reaffirming Khanna and Damon's (KD, 1999) findings. These pollution reductions are statistically significant in the first year of program operation (1992), but persist throughout our sample period (to 1995). Moreover, these estimated effects are robust to a variety of model specifications, to alternative estimation methods, and to alternative measures of releases (toxicity-weighted and unweighted). And they are large in magnitude. Based on Table 5's fixed effects estimates, cumulative reductions in releases that can be attributed to 33/50 participation amount to over 45 percent of the participants' average prior (1990) emissions. These reductions are much larger than found by KD, who estimate 33/50-induced release reductions of less than 19 percent. However, we find that between 70 and 85 percent of the 33/50 program's entire (1992-95) effect on pollution was achieved in the program's first year.

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[24] We estimated a wide variety of additional models and obtained similar results for the key variables of interest. These estimations (available upon request) include parsimonious versions of the presented models, models with an aggregated participation effect and/or added interaction variables BC-SIERRA and FG-EMPL (both statistically insignificant), and models using unweighted (vs. toxicity weighted) 33/50 releases, all with both random effects and fixed effects.

Our results also indicate that firms were motivated to lower pollution in order to preempt regulation (with a statistically significant negative coefficient on SIERRA). Although these impacts diminish with higher values of SIERRA, the estimated marginal effects of SIERRA (evaluated at its sample mean) are negative, yielding release elasticities of approximately -.26 to -.32. In addition, a strict (vs. negligence) liability statute is estimated to spur pollutant reductions of between 25 and 30 percent. In contrast, we find no significant direct effects of boycott sensitivity (BC) or a firm's proximity to final consumers (FG, our "green marketing" proxy) on 33/50 releases. The threat of boycott nevertheless has an indirect effect – spurring pollution reductions by inducing 33/50 participation (Table 4). Finally, as expected, firms that invest more in research and/or are in more concentrated industries are estimated to have lower 33/50 emissions.

(3) Inspections and Enforcement Actions. Table 6 presents results from the inspections and enforcement actions equations.[25] Table 7 presents estimated percentage marginal effects of 33/50 participation on inspection and enforcement rates in each of the program years, 1992-1995.

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 short-term government oversight. However, program participants experienced statistically and quantitatively significant reductions in their inspection rates from 1993 through 1995 (as indicated by the statistically significant negative coefficient on PART93). From the Poisson model, for example, we estimate that a firm's 33/50 program participation translated into a 37 percent

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[25] We estimated a variety of other (more parsimonious) models for both equations. Results are available upon request and are broadly consistent across the models.

cumulative reduction in a facility's inspections by 1995.[26] These impacts are robust to alternative models and estimation methods, and do not appear to weaken with the addition of correlates.

We find significant negative effects of 33/50 participation on enforcement actions as well. Participation effects are significant at the program's inception (1992) and its end (1995); in the intervening years (1993 and 1994), we estimate that participants enjoyed much smaller reductions in enforcement rates (Table 7). Overall, a firm's 33/50 participation is estimated to spur a 44 percent cumulative reduction in the likelihood of enforcement action by 1995.

Our results also indicate that larger firms (with higher LEMP) tend to be inspected more, but have fewer enforcement actions, most likely because heightened inspection oversight promotes greater compliance, thus vitiating the need for enforcement action. Due to similar logic, state air quality spending spurs more inspections but, perhaps due to improved compliance, fewer enforcement actions.

In addition, we find that prior enforcement actions spur subsequent (follow-up) inspections and that prior inspection activity is positively associated with subsequent enforcement actions, likely because inspections are one key precursor (among others) for the identification of

[26] Marginal effects in Table 7 only account for the direct impact of 33/50 participation on inspections / enforcement actions. For inspections, there is an additional indirect effect, with participation lowering releases (Table 5), which in turn lowers inspections (Table 6). The approximate cumulative indirect effect of 33/50 participation is to reduce inspections by a further 6.4 percent by 1995 (based on the fixed effects model of Table 5 and the Poisson inspection model of Table 6). For enforcement actions, we find no indirect impact of participation, with a coefficient on lagged releases (LRELFAC) that is statistically insignificant.

infractions.[27] Other precursors for enforcement actions can be local community and environmental group reports. Environmental groups can also apply political pressure for government action against facilities that they target. Due to either or both of these effects, we find that larger local environmental constituencies (as measured by SIERRA) have a positive effect on enforcement actions. Inspection rates are estimated to rise with boycott sensitivity (BC), but fall with the Sierra Club measure. Hence, it appears that general environmental group influence in a community substitutes for government inspections in promoting environmental objectives, but that government authorities respond to the visibility of potential boycott targets by inspecting with greater frequency.[28]

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[27] However, enforcement actions need not derive from inspections. For example, community groups can alert authorities to infractions. Hence, we find in our data that the proportion of time facilities experience enforcement actions when they have no inspections (.0656) is almost the same (and not significantly different statistically) as when they have inspections (.0861). We therefore do not treat a facility's enforcement action as a selection from the sample of facilities that are inspected in a given year.

[28] One might conjecture that environmentalism may spur pressure on government agencies for more inspections; our results suggest, in contrast, that government agencies recognize the salutary effects of environmentalism on firm performance and therefore reduce their inspection rates when there is more environmentalist pressure. Similar logic may explain why our "right to work" variable has a significant positive effect on inspection rates. Specifically, right to work states are likely to be pro-business, with constituencies that may impose little community pressure for environmental performance; government authorities may compensate for this lack of community pressure by exercising more regulatory oversight.

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 enforcement activity. In doing so, we find empirical support for the "enforcement theory" of voluntary pollution reductions (Maxwell and Decker, 2006). 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 and enforcement equation effects 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 1-2 presented at the outset of this study.

Our results thus reaffirm Khanna and Damon's (1999) conclusion that the 33/50 program spurred pollutant reductions, while accounting for time and other effects omitted there. However, we estimate that the size of these impacts was much larger than found by KD, and that they occurred primarily in the first year of the 33/50 program's operation. Relative to the literature, we also identify new impacts of the 33/50 program, estimating that participation reduced rates of environmental inspection and enforcement action by cumulative percentages of 26 and 44 percent, respectively. And by accounting for a broader range of economic phenomena than prior work, our estimations document new economic forces driving 33/50 participation, including incentives to forestall potential boycotts by environmental groups (Baron, 2001; Innes, 2006) and/or to preempt lobbying by these groups for tighter environmental regulation and enforcement (MLH, 2000). However, contrary to earlier studies that did not account for these forces (e.g., KD, AC, VA), we do not find support for the hypothesis that firms participated in the 33/50 program, and/or reduced their pollution levels, in order to obtain "green marketing" advantages – that is, 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 convey 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	Tetrachlorethylene
Cadmium and Compounds	Mercury and Compounds	Toluene
Carbon Tetrachloride	Methyl Ethyl Ketone	Trichloroethane
Chloroform	Methyl Isobutyl Ketone	Trichloroethylene
Chromiun 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	Industry
20	Foods 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: www.siccode.com

TABLE 1
VARIABLE DEFINITIONS

RELEASE	Total firm releases of 33/50 pollutants (toxicity-weighted millions of pounds) (annual)
LRELFAC	Lagged facility releases of 33/50 pollutants (toxicity-weighted)
DIFREL	Change in total firm releases of 33/50 pollutants from 1988-1990
PART 92 - 95	Dummies that equal one if a firm is a 33/50 participant (note 15)
INSPECT	Number of a facility's CAA inspections (annual)
LINSPECT	Lagged number of a facility's CAA inspections (annual)
LINSPFAC	Lagged number of a firm's CAA inspections per facility (annual)
INSP89-90	Number of CAA inspections of a firm's facilities, 1989-90
ENFORCE	Dummy that equals one if a facility is subject to a CAA enforcement action (annual)
LENFORCE	2-year lagged number of CAA enforcement actions, by facility (annual)
ENF89-90	Dummy that equals 1 if firm had a CAA enforcement action in 1989-90
LOUTCOMP	2-year lagged number of CAA out-of-compliance citations, by 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 SIC class)
SG	Firm percentage sales growth (annual)
SIERRA	Sierra Club members per capita in facility's home state (annual), averaged across facilities for the firm
STRICT	Dummy that equals one if facility's home state has a strict liability statute, 1988, averaged for the firm
RTW	Dummy that equals one if facility's home state has a right-to-work statute, 1988, averaged for the firm
SPENDAQP	State expenditures on air quality programs in the facility's home state, 1988, averaged for the firm
LAWYERS	Number of lawyers per capita in facility's home state, 1988, averaged for the firm
EDUC	Percentage of college degrees in facility's home state population, 1990, averaged for the firm
NONATTAIN	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 a facility's home county, 1990
GSPG	Gross State Product growth in a facility's home state (annual)
URATE	County unemployment rate in a facility's home county (annual)

TABLE 2

DESCRIPTIVE STATISTICS

Variable	Participants		Non-participants		Difference of means z-statistic
	Average	Standard deviation	Average	Standard deviatio n	
DIFREL	-0.1881	0.6243	-0.0576	0.1833	-2.5731**
RELEASE	0.8968	1.9382	0.116	0.1935	5.1482***
LEMP	34.4284	71.4741	5.0099	7.1058	5.2603***
HERF	0.4481	0.1443	0.4939	0.1633	-2.6762***
PRP	5.4061	9.7499	1.0875	2.2301	5.5421***
ENF89-90	0.4242	0.4957	0.1	0.3009	7.1515***
INSP89-90	13.4545	19.9592	2.6	4.7731	6.7884***
SIERRA	2.2982	1.065	2.5442	1.7208	-1.5441
BOYCOTT (BC)	0.3818	0.4873	0.2500	0.4344	2.5766***
FINAL GOOD (FG)	0.6606	0.4749	0.6250	0.4856	0.6679
STRICT	0.7588	0.3117	0.7768	0.3836	-0.4634
LRD	211.7544	549.1934	18.3815	46.8655	4.5059***
RTW	0.2984	0.3131	0.2589	0.3972	0.9936
SPENDAQP	1.1798	0.5806	1.2274	0.6524	-0.6940
LAWYERS	2.8539	0.758	3.2358	1.0209	-3.8197***
EDUC	19.9476	2.8079	20.3976	3.4283	-1.2923
SIC 28	0.2121	0.4101	0.125	0.3318	2.1079**
SIC 33	0.097	0.2968	0.0563	0.2311	1.3817
SIC 34	0.0545	0.2278	0.1063	0.3091	-1.7156*
SIC 35	0.1576	0.3655	0.1875	0.3915	-0.7112
SIC 36	0.1273	0.3343	0.1438	0.3519	-0.4331
SIC 37	-0.1881	0.6243	-0.0576	0.1833	-2.5731**
SIC 38	0.8968	1.9382	0.116	0.1935	5.1482***
Number of observations	165		160		

NOTES: Mean and standard deviation of variables used in the Probit models. Descriptive statistics for time varying variables are obtained using 1990 data. The difference of means z-statistic is asymptotically distributed standard normal. *, **, *** denotes statistical significance at the ten, five, and one percent levels, respectively.

TABLE 3
COARSE STATISTICS

	Participants			Non-Participants			Difference of Means
	Avg.	Std. Dev.	N	Avg.	Std. Dev.	N	z-Statistic
Proportional Change in Releases, 1991-95 [a]	-.504	.425	118	-.339	.612	94	-2.218**
Increase in Annual No. of Inspections, 1989-91 to 1992-95 [b]	.032	.833	855	.033	.481	223	-.023
Increase in Annual No. of Enf. Actions, '89-91 to '92-95[b]	.042	.569	855	.142	.725	223	-1.924*
Average Rate of Inspection, 1989-91[c]	.323	.219	3223	.254	.190	834	4.022***
Average Rate of Inspection, 1992-95 [c]	.302	.211	3234	.282	.202	813	1.135
Change in Average Rate of Inspection, 1989-91 to 1992-95[c]	-.022			.027			-1.985**
Average Rate of Enf. Action, 1989-91[c]	.052	.050	3223	.061	.057	834	-1.03
Average Rate of Enf. Action, 1992-95[c]	.075	.070	3234	.106	.095	813	-2.61***
Change in Average Rate of Enf. Action, 1989-91 to 1992-95 [c]	.023			.045			-1.47

NOTES: *,**,*** denotes significance at 10 percent, 5 percent, and 1 percent levels (two-sided).

[a] Firm-level data. Total observations are fewer than our entire sample of 325 firms because we do not have 1991 and 1995 data for all firms.

[b] Facility-level data. Total observations are fewer than our entire sample of 1257 facilities because we do not have 1989-91 and 1992-95 data for all facilities. If a facility has at least one year of data in each of 1989-91 and 1992-95, it is included in the above calculations, with annual averages calculated over the number of years for which that facility has data in each time period.

[c] Facility-year data. Facility i is given a one in year t if it receives at least one inspection /enforcement action during the year. Averages are taken over the resulting binary variables over all relevant facility-years. For differences of means from proportions data, we calculate the asymptotically standard normal z-statistic, $z = (\theta_{1t}-\theta_{2t})/\{\sum_{i=1}^{2} \theta_{it}(1-\theta_{it})/N_{it}\}^{.5}$ for time period t, where i=1(2) represents participant (non-participant) data, θ_{1t} represents the average group i inspection / enforcement rate in period t, and N_{it} represents the corresponding number of observations. Similarly, for differences between (pre and post program) differences of means (changes), we calculate the z-statistic,

$$z = (\theta_{1b}-\theta_{1a})- (\theta_{2b}-\theta_{2a})/\{ \sum_{i=1}^{2} \sum_{t=a}^{b} \theta_{it}(1-\theta_{it})/N_{it}\}^{.5},$$ where t=a(b) represents period 1989-91 (1992-95).

TABLE 4

THE PARTICIPATION EQUATION

Hypothesis tested	Variable	Model I Estimate	Model I t-value	Model II Estimate	Model II t-value	Model III Estimate	Model III t-value
	ENF89-90	0.468	2.040 **	0.581	1.950 *	0.584	1.880 *
Enforcement effects	INSP89-90	0.027	2.610 ***	0.030	2.090 **	0.037	2.580 ***
	PRP	0.075	3.950 ***	0.051	1.990 **	-0.133	-1.520
	PRP^2					0.015	2.290 **
Prior release reductions,	DIFREL			0.541	1.170	1.134	1.880 *
R&D, and concentration	LRD			0.004	1.750 *	0.004	1.680 *
effects	LRD-DIFREL					-0.009	-1.720 *
	HERF			1.928	1.220	1.736	1.040
	SIERRA			0.719	1.940 *	0.910	2.360 **
Regulatory Preemption	SIERRA^2			-0.098	-2.210 **	-0.119	-2.580 ***
	SIERRA-RELEASE					-0.374	-2.370 **
	STRICT			-0.258	-0.670	-0.346	-0.830
Liability effects	STRICT-LEMP					-0.004	-0.190
	STRICT-LAWYERS					0.096	0.850
Boycott deterrence	BC			1.003	1.480	1.263	1.660 *
	BC-LEMP					-0.008	-0.410
	FG			0.237	0.590	0.373	0.860
Green marketing	FG-RELEASE					0.177	0.220
	RELEASE	1.274	3.970 ***	0.979	2.210 **	1.663	2.150 **
	RELEASE^2					-0.064	-0.210
	LEMP			0.030	2.490 **	0.039	1.670 *
Firm-specific effects	EDUC			-0.013	-0.170	-0.044	-0.550
	LAWYERS			-0.167	-0.590	-0.092	-0.310
	RTW			-0.017	-0.040	0.079	0.200
	SPENDAQP			-0.025	-0.110	-0.045	-0.190
	SIC28	0.629	2.080 **	2.302	2.490 **	2.507	2.520 **
	SIC33	0.262	0.670	1.928	1.940 *	2.062	1.940
	SIC34	-0.165	-0.410	0.695	0.940	0.775	0.980
Industry fixed effects	SIC35	0.407	1.350	1.326	1.710 *	1.372	1.660 *
	SIC36	0.602	1.910 *	0.477	0.900	0.362	0.670
	SIC37	0.530	1.390	0.423	0.720	0.312	0.530
	SIC38	0.109	0.300	1.060	1.460	1.248	1.610
	Constant	-1.100	-4.340 ***	-3.575	-2.220 **	-3.641	-2.090 **
	No of observations		325		325		325
	LogL		-157.61		-139.17		-135.06
	Chi-square {p-value}	135.25	{0.00}	172.12	{0.00}	180.35	{0.00}

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. Squared variables are denoted by the addition of "^2" to the variable and interactions are denoted with hyphens. *** (**,*) denotes statistical significance at the 1% (5%, 10%) level or better (two-tail).

TABLE 5
THE POLLUTION EQUATION

	Random Effects			Fixed Effects		
Variable	Estimate	t-value		Estimate	t-value	
PRPS	0.038	2.780	***			
LINSPFAC	-0.011	-0.800		-0.017	-0.979	
FAC	0.052	8.189	***	0.053	4.908	***
HERF	-0.600	-2.403	**	-0.633	-2.216	**
LRD	-0.001	-9.259	***	-0.001	-1.697	*
SIERRA	-0.114	-1.856	*	-0.120	-1.650	*
SIERRA^2	0.012	1.404		0.011	1.038	
BC	-0.270	-0.671				
FG	-0.005	-0.013				
PART 92	-0.374	-5.282	***	-0.348	-3.396	***
PART 93	-0.015	-0.220		-0.008	-0.110	
PART 94	0.017	0.233		0.014	0.173	
PART 95	-0.072	-0.983		-0.065	-0.712	
LEMP	0.026	11.188	***	0.026	2.527	**
LEMP^2	0.000	-8.328	***	0.000	-2.593	***
STRICT	-0.131	-1.666	*	-0.151	-1.122	
SG	0.000	0.641		0.000	0.562	
IMR	0.221	4.178	***	0.200	2.498	**
Constant	0.348	0.694		0.330	27.507	***
F-test of OLS vs. FE (p-value)				32.33 (0.000)		
LM test of OLS vs, RE (p-value)	1534.08 (0.000)					
Test of RE vs. FE (p-value)	49.25 (0.0026)					
R^2	0.29			0.86		

NOTE: The dependant variable is RELEASE. The Breush-Pagan LM test of OLS vs. Random Effects ($\chi^2(1)$) rejects the null of OLS. The F test of OLS vs. Fixed Effects rejects the null of OLS. The Hausman test favors Fixed Effects over Random Effects. ** Statistically significant at the 5% level or better. * Statistically significant at the 10% level. Both models include year dummies. The Random Effects model also includes the cross-section variables RTW, SPENDAQP, LAWYERS, EDUC, and dummies for SICs 28 and 33-38. For the Fixed Effects model, we report robust standard errors and an R^2 that excludes impacts of the fixed effects.

TABLE 6
THE ISPECTION AND ENFORCEMENT ACTION EQUATIONS

Variable	Inspection Equation						Enforcement Equation		
	Random Effects Poisson			Random Effects Probit			Random Effects Probit		
	Estimate	t-value		Estimate	t-value		Estimate	t-value	
Constant	-8.185	-2.733	***	-12.202	-4.454	***	-22.864	-5.763	***
YEAR	0.073	2.257	**	0.125	4.274	***	0.231	5.474	***
PART 92	0.019	0.183		-0.019	-0.172		-0.341	-2.067	**
PART 93	-0.320	-3.028	***	-0.411	-3.570	***	0.139	0.794	
PART 94	0.106	0.924		-0.002	-0.018		0.019	0.110	
PART 95	-0.271	-2.437	**	-0.103	-0.859		-0.301	-1.807	*
SIERRA	-0.180	-4.268	***	-0.239	-4.697	***	0.111	1.756	*
BC	0.392	1.996	**	0.538	1.860	*	0.392	1.206	
NONATTAIN	-0.171	-1.471		-0.115	-0.722		0.289	1.814	*
CDENSITY	6.94E-06	0.234		4.92E-05	1.135		4.97E-05	1.419	
LRELFAC	1.88E-04	3.752	***	4.93E-04	2.708	***	-7.78E-05	-0.241	
LEMP	0.002	3.779	***	0.004	3.742	***	-0.003	-2.032	**
SPENDAQP	0.659	6.835	***	0.637	4.541	***	-0.329	-2.319	**
RTW	0.330	2.703	***	0.199	1.064		-0.289	-1.460	
EDUC	-0.002	-0.076		0.033	0.797		-0.006	-0.116	
STRICT	-0.163	-1.301		-0.020	-0.108		-0.382	-1.888	*
LAWYERS	-0.045	-0.406		-0.276	-1.759	*	-0.093	-0.465	
URATE	0.002	0.096		-0.004	-0.151		-0.048	-1.457	
GSPG	-0.009	-0.508		-0.027	-1.490		-0.010	-0.325	
LOUTCOMP	0.014	0.389		-0.022	-0.622		0.233	24.656	***
LENFORCE	0.080	2.090	**	0.056	1.258				
LINSPECT							0.101	2.162	**
No. of obs.	5703			5703			5703		
Log L	-4035.82			-2362.38			-1084.38		

NOTES: The dependant variables are INSPECT and ENFORCE. All models include industry dummies (for SICs 28 and 33-38). A linear restrictions (LM) test favors the time variable (year) to year dummies in all models. "*", "**", and "***" indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

TABLE 7
PERCENTAGE MARGINAL EFFECTS OF 33/50 PARTICIPATION ON FACILITY INSPECTIONS AND ENFORCEMENT ACTIONS BY PROGRAM YEAR (1992-1995)

Year	Percentage Marginal Effects for the Inspection Equation				Percentage Marginal Effects for the Enforcement Action Equation	
	Random Effects Poisson		Randon Effects Probit		Random Effects Probit	
	Marginal Effect	t-value	Marginal Effect	t-value	Marginal Effect	t-value
1992	2.5%	0.237	-0.9%	-0.172	-31.21%**	-2.041
1993	-26.3%***	-2.788	-20.9%***	-3.773	-18.46%	-1.291
1994	-16.4%	-1.339	-21.0%***	-3.116	-16.74%	-1.014
1995	-37.0%***	-2.910	-26.1%***	-3.503	-44.26%**	-2.355

NOTES: For the Probit models, the percentage marginal effect represents the estimated impact of 33/50 participation on the probability of government inspection and enforcement action for each program year, as a percentage of sample average inspection / enforcement rates (using sample mean values for exogenous variables to evaluate the marginal effect). For the Poisson model, the reported marginal effects are the estimated percentage impact of 33/50 participation on inspection numbers in each program year. All effects are calculated for the models of Table 6. "**" and "***" indicate statistical significance at the 5% and 1% levels, respectively.