Voluntary Pollution Reductions and the Enforcement of Environmental Law: An Empirical Study of the 33/50 Program

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Abstract

This paper studies determinants and effects of firms' participation in the 33/50 program, which is a voluntary pollution reduction (VPR) program 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 that (1) VPR participation is rewarded by relaxed regulatory scrutiny, (2) the anticipation of this reward spurs firms to participate in the program, and (3) 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.

1. Introduction

Why do private firms voluntarily overcomply with environmental regulations? For example, over 1,200 firms joined the U.S. Environmental Protection Agency's (EPA's) 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, which is 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 (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

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Arora and Cason 1996). Voluntary pollution reductions may also deter lobbying by environmental groups for tighter regulatory standards (Maxwell, Lyon, and Hackett 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 (VPR) program. The societal benefit of a VPR program is to prompt participating firms to adopt management practices that reduce their costs of pollution abatement, leading ultimately to pollution reductions (Maxwell and Decker 2006).[2] While intuitively compelling, the empirical strength of this enforcement theory for VPR programs 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 VPR programs (for example, Arora and Cason 1996; Khanna and Damon 1999; Videras and Alberini 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 (Arora and Cason 1996; Khanna and Damon 1999; Videras and Alberini 2000) and effects of the 33/50 program on pollution (Khanna and Damon 1999; Vidovic and Khanna 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 (Arora and Cason 1996; Khanna and Damon 1999; Videras and Alberini 2000). Khanna and Damon

[1] With regard to the 33/50 program, the Environmental Protection Agency (EPA) stated (U.S. Environmental Protection Agency 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 (U.S. Environmental Protection Agency, National Environmental Performance Track: Regulatory and Administrative Incentives [http://www.epa.gov/performancetrack/benefits/regadmin.htm]).

[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 a priori. Segerson and Miceli (1998) also stress the benefits of voluntary pollution reduction programs in lessening tensions and facilitating negotiations between enforcement agencies and polluting firms.

(1999) find that the 33/50 program led to significant pollutant reductions; however, Vidovic and Khanna (2007) argue that this effect vanishes when accounting for time effects. In contrast to our focus, this literature does not study effects of voluntary overcompliance on government enforcement and does not consider potential effects of boycott threats or incentives for regulatory preemption (Maxwell, Lyon, and Hackett 2000) or liability law.[3] We thus examine a more complete range of possible explanations for voluntary pollution abatement efforts 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, which permits us to identify when it was effective in reducing pollution. In doing so, we consider time effects (like Vidovic and Khanna 2007) but examine a broader array of manufacturing firms.

A second literature studies determinants of the government's environmental enforcement activity and its effect on pollution (for example, 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 (see also Harford and Harrington 1991; Heyes and Rickman 1999).[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 VPR programs.

2. The 33/50 Program

Started in 1991, the 33/50 program was the EPA's first formal effort to achieve VPRs by regulated firms. The program sought to reduce releases of 17 toxic chemicals by a third by 1992 and by 50 percent by 1995, measured from 1988

[3] In this literature, the only study that allows for any enforcement effects is Videras and Alberini (2000), who consider the potential impact of prior Resource Conservation and Recovery Act corrective actions on 33/50 participation, finding some evidence that such enforcement actions make participation more likely. We study the impact of both regulatory inspections and enforcement actions and, unlike Videras and Alberini (2000), also model program impacts on pollution and government enforcement activity. Maxwell, Lyon, and Hackett (2000) 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] In addition, consistent with this theory, Decker (2005) finds that government inspection activity responds to reductions in reported toxic pollutant releases as well as to reductions in regulated pollutant releases and a good statutory compliance history.

baseline levels. (The 17 33/50 chemicals are listed in Appendix A.) Roughly 70 percent of the 33/50 chemicals (by 1988 weight of releases) were air pollutants (Arora and Cason 1996). 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 phaseout of such substances; however, these two chemicals represented less than 15 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 10 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 4,534 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 1,294 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, that is, 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 (U.S. Environmental Protection Agency 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 nonparticipants (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.

3. 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 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

1. had more contact with final consumers (green marketing),
2. was a more likely object of a consumer or environmental group boycott (boycott deterrence),
3. 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),
4. was more exposed to potential liability because it was larger (with deeper pockets) and/or operated in strict-liability states (liability), and
5. was in a more concentrated industry and invested more in research and development (strategic and cost effects).

With regard to 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 (Arora and Cason 1996; Khanna and Damon 1999; Videras and Alberini 2000); we therefore follow Khanna and Damon (1999) 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

[5] The EPA (U.S. Environmental Protection Agency 1992, p. 2) states that its "partnership programs offer recognition . . . that can enhance corporate image with customers, regulators, neighbors, and the media."

[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 (see, for example, Hamilton 1995).

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.

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 (versus 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 research and development (R&D) expenditures 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, Lyon, and Hackett 2000), and R&D can directly lower costs of pollutant abatement, both of which favor 33/50 participation and pollution reduction.

4. 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, 1989–95), (3) the government's (state and federal) annual number of environmental inspections of firms' facilities (1989–95), and (4) the government's annual number of enforcement actions against firms' facilities (1989–95). Inspections and enforcement actions are important for our purposes because they can lead to potentially costly disputes between a facility or firm and government regulators. Even actions considered minor in and of themselves are

[7] *National Boycott News* (1992–93, pp. 6–13) 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 standard industrial classification code), 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.

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 are obtained from 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 (1988–95).[9] The TRI provides facility-level data on 33/50 chemical releases, primary standard industrial classification (SIC) codes, 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 (1989–95, measured per capita). The Maxwell, Lyon, and Hackett (2000) data set provides information on state characteristics (1988), including per capita state spending on clean air laws, educational status (the number of bachelor's 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 responsible party (PRP) is obtained from the EPA's Superfund Office. County unemployment rates (1989–95) and state GDP growth rates (1989–95) 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.[10] County population density (1990) is obtained from the U.S. census.

Our study focuses on manufacturing firms that operated in SIC codes 20–39 and were invited to participate in the 33/50 program in 1991. Table B1 lists the industries associated with the included SICs. Merging the Compustat and environmental data sets for these firms gives us a sample of 496 companies. Limiting attention to firms with 3 years or more of complete data over 1988–95 and allowing for lagging, we have an unbalanced panel of 319 firms and 1,257 facilities over the 7 years 1989–95. We include 1989–90 data in order to capture preprogram 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

[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 (U.S. Environmental Protection Agency, Region 9: Compliance and Enforcement [http://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 Clean Air Act (CAA) have led to remedies and penalties ranging from the very small to over $130 million (U.S. Environmental Protection Agency, Region 4: Environmental Accountability. Enforcement Actions 2007 and 2008 [http://www.epa.gov/Region4/ead/general/recent.html]).

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

[10] U.S. Environmental Protection Agency, Green Book: The Green Book Nonattainment Areas for Critical Pollutants (http://www.epa.gov/oar/oaqps/greenbk).

Table 1
Variable Definitions

Variable	Definition
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 1988–90
PART92–95	Dummies that equal one if a firm is a 33/50 participant
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 one 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 potentially responsible party, 1990
SIC28–SIC38	Dummies for a firm's primary two-digit SIC class
LRD	Lagged firm expenditures on research and development ($ millions, annual)
LEMP	Lagged number of firm employees (1,000s, 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 class 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 a facility's home state has a strict-liability statute, 1988, averaged for the firm
RTW	Dummy that equals one if a facility's home state has a right-to-work statute, 1988, averaged for the firm
SPENDAQP	State expenditures on air quality programs in a facility's home state, 1988, averaged for the firm
LAWYERS	Number of lawyers per capita in a facility's home state, 1988, averaged for the firm
EDUC	Percentage of college degrees in a 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, 1992–95
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)

Note. CAA = Clean Air Act; SIC = standard industrial classification.

Table 2
Descriptive Statistics

Variable	Participants Average	Participants SD	Nonparticipants Average	Nonparticipants SD	Difference-of-Means *z*-Statistic
DIFREL	−.1881	.6243	−.0576	.1833	−2.5731*
RELEASE	.8968	1.9382	.116	.1935	5.1482**
LEMP	34.4284	71.4741	5.0099	7.1058	5.2603**
HERF	.4481	.1443	.4939	.1633	−2.6762**
PRP	5.4061	9.7499	1.0875	2.2301	5.5421**
ENF89–90	.4242	.4957	.1	.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)	.3818	.4873	.2500	.4344	2.5766**
FINAL GOOD (FG)	.6606	.4749	.6250	.4856	.6679
STRICT	.7588	.3117	.7768	.3836	−.4634
LRD	211.7544	549.1934	18.3815	46.8655	4.5059**
RTW	.2984	.3131	.2589	.3972	.9936
SPENDAQP	1.1798	.5806	1.2274	.6524	−.6940
LAWYERS	2.8539	.758	3.2358	1.0209	−3.8197**
EDUC	19.9476	2.8079	20.3976	3.4283	−1.2923
SIC 28	.2121	.4101	.125	.3318	2.1079*
SIC 33	.097	.2968	.0563	.2311	1.3817
SIC 34	.0545	.2278	.1063	.3091	−1.7156+
SIC 35	.1576	.3655	.1875	.3915	−.7112
SIC 36	.1273	.3343	.1438	.3519	−.4331
SIC 37	−.1881	.6243	−.0576	.1833	−2.5731*
SIC 38	.8968	1.9382	.116	.1935	5.1482**
N	165		160		

Note. Means and standard deviations of variables used in the probit models are reported. Descriptive statistics for time-varying variables are obtained using 1990 data. The difference-of-means *z*-statistic is asymptotically distributed standard normal.
+ Statistically significant at the 10% level.
* Statistically significant at the 5% level.
** Statistically significant at the 1% level.

with those of nonparticipants. In a statistical sense, most of the variables have significantly different means for participants than for nonparticipants. 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: (1) the number of government inspections of firm facilities in 1989–90 (INSP89–90), (2) an indicator that takes a value of one if a firm had an enforcement action in the period 1989–90 (ENF89–90), and (3) the number of Superfund sites for which a firm is a PRP. Enforcement-driven rewards for 33/50 participation and pollution reductions are expected to have been greater for firms with more Superfund involvement, as measured by PRP. For all three measures, Table 2 indicates that participants were subject to more prior enforcement scrutiny than were nonparticipants, with more inspections, a higher likelihood of enforcement action, and more Superfund activity. These statistics provide some preliminary evidence for hypothesis 1.

Critics of the 33/50 program suggest that firms joined because their prior (1988–90) emission reductions already placed them in near reach of the program's goals (Khanna and Damon 1999). 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 (1988–90); however, as a proportion of 1988 releases, preprogram (1988–90) reductions in participant emissions were only 18.5 percent, which is substantially less than those of nonparticipants (35.5 percent).

Did 33/50 releases fall proportionally more for participant firms than for nonparticipants, from their initial preprogram (1991) level to their final postprogram (1995) level? And did average annual rates of inspection and enforcement action rise less for program participants from their preprogram (1989–91) levels to their postprogram (1992–95) levels? For our sample, Table 3 reveals that participants experienced approximately a 16.5 percent greater reduction in releases from pre- to postprogram, half again as much as the entire release reduction experienced by nonparticipants. Similarly, while the average number of nonparticipant enforcement actions more than doubled (multiplied by over 2.57), corresponding participant enforcement numbers increased by less than 50 percent in the postprogram years. And while nonparticipants experienced an increase in inspection rates of more than 8 percent from the pre- to postprogram years, participants experienced a 6.6 percent decline.[11] These statistics are suggestive of the participation effects that we conjecture in hypothesis 2.

5. Econometrics

5.1. 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.[12] We control for industry effects by including dummy variables for the seven industries most heavily represented in our sample (SIC codes 28, 33, 34, 35, 36, 37, and 38).

[11] Although inspection rates fell for participants over the program years, the average annual number of inspections rose for both participants and nonparticipants—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 being much larger on average than nonparticipant firms, and increased inspection activity in postprogram (Clinton administration) years targeted more at the larger firms, we expect to see a larger increase in inspection numbers for participant (versus nonparticipant) firms in the absence of the 33/50 program.

[12] We include all firms that had data in 1990, even those with fewer than 3 years of complete data. Hence, our sample for this equation contains six more companies than were used in the other equations, for a total of 325 sample firms.

Table 3

Coarse Statistics

	Participants			Nonparticipants			Difference-of-Means
	Average	SD	N	Average	SD	N	z-Statistic
Proportional change in releases, 1991–95[a]	−.504	.425	118	−.339	.612	94	−2.218*
Increase in annual number of inspections, 1989–91 to 1992–95[b]	.032	.833	855	.033	.481	223	−.023
Increase in annual number of enforcement actions, 1989–91 to 1992–95[b]	.042	.569	855	.142	.725	223	−1.924[+]
Average rate of inspection, 1989–91[c]	.323	.219	3,223	.254	.190	834	4.022**
Average rate of inspection, 1992–95[c]	.302	.211	3,234	.282	.202	813	1.135
Change in average rate of inspection, 1989–91 to 1992–95[c]	−.022			.027			−1.985*
Average rate of enforcement action, 1989–91[c]	.052	.050	3,223	.061	.057	834	−1.03
Average rate of enforcement action, 1992–95[c]	.075	.070	3,234	.106	.095	813	−2.61**
Change in average rate of enforcement action, 1989–91 to 1992–95[c]	.023			.045			−1.47

[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 1,257 facilities because we do not have 1989–91 and 1992–95 data for all facilities. If a facility has at least 1 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 or 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 (nonparticipant) data, θ_{it} represents the average group i inspection or enforcement rate in period t, and N_{it} represents the corresponding number of observations. Similarly, for differences between (pre- and postprogram) 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 the period 1989–91 (1992–95).

[+] Statistically significant at the 10% level.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

5.2. The Pollution Equation

To estimate the impact of the 33/50 program on firms' chemical releases, we have an unbalanced panel of 319 companies for 7 years, 1989–95, which gives us a total of 1,879 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.[13]

With regard to the econometrics, there are several issues. First, we consider both fixed- and random-effects 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. Because program participation occurred late in 1991, we model participation effects only from 1992 onward. Although participation decisions were predetermined in these years, there may nevertheless be sample selection bias; because of 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]).[14] Resulting coefficient estimates are consistent (Vella 1998).

Because participation effects may wane over the course of the program, we measure distinct effects for each of the program years 1992–95. 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

[13] We considered two alternative measures to capture time effects, a time (year) variable and time (year) dummies for all but 1 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 .001. Hence, we present results using year dummies.

[14] The selection correction is achieved (following Vella 1998) by constructing the fitted regressor, IMR_{ti}, where $\mathrm{IMR}_{ti} = 0$ for $t \leq 1991$ and, for $t \geq 1992$,

$$\mathrm{IMR}_{ti} = p_i\left[\frac{\phi(\hat{\gamma}' w_i)}{\Phi(\hat{\gamma}' w_i)}\right] + (1 - p_i)\left\{\frac{-\phi(\hat{\gamma}' w_i)}{[1 - \Phi(\hat{\gamma}' w_i)]}\right\},$$

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 3 in Table 4), w_i is the firm i set of explanatory variables in the participation equation, and $\phi(\hat{\gamma}' w_i)$ $[\Phi(\hat{\gamma}' w_i)]$ are normal density (distribution) functions.

1993 participation variable measures the pollution change from 1993 onward that is attributable to a firm's 33/50 participation.[15]

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.

5.3. The Inspection and Enforcement Action Equations

For these equations, we have an unbalanced panel of 1,257 facilities over 7 years, 1989–95, giving us 5,703 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 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 2 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.[16] Time effects are incorporated with a time (year) variable.[17]

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.[18] In the probit models, the dependent variable takes a value of one whenever a facility received at least one inspection (enforcement action) in a given year. Second, in addition

[15] 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 $\tau = 1992, \ldots, 1995$. We denote these variables by PART92–PART95 (see Table 1).

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

[17] 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).

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

to including fixed industry dummies, we allow for individual effects that are assumed to be random and normally distributed.[19]

Third, contemporaneous inspections and enforcement actions are posited to depend on 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 the two-step estimator of Terza (1998).[20] For our binary models, we test for selection correlation using a bivariate probit estimator (with 33/50 participation). In all cases, we find no statistical evidence for cross-equation correlation.[21] 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.[22] We therefore proceed under the maintained hypothesis that lagged releases and lagged participation regressors are exogenous.

6. Results

6.1. The Participation Equation

Table 4 presents selected results from estimation of the participation equation. We present three models.[23] The first is a parsimonious specification, including

[19] 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 overdispersion. In our Poisson model, we test for overdispersion and reject the constraint that mean equals variance. 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 overdispersion, even with fixed effects, is that of Hausman, Hall, and Griliches (1984), wherein the dependent 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.

[20] 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 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.

[21] 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.

[22] The Hausman test is a joint test of exogeneity and instrument quality. Our key identifying instrument for lagged releases is twice-lagged research and development. As this instrument is highly correlated with lagged releases (in the sense of Bound, Jaeger, and Baker 1995), we can reasonably interpret the Hausman statistic as a test of exogeneity. For the Poisson and probit inspections models reported, 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).

[23] In all three models, we test for heteroskedasticity, following standard practice (Greene 2000, chap. 19; Harvey 1976) by considering a variance that is an exponential function of squared exogenous data (in our case, firm employment, or LEMP). In model 1, we do not reject homoskedasticity and thus present probit results under this premise. In models 2 and 3, we reject homoskedasticity and therefore report heteroskedasticity-corrected estimation results.

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 nonlinearities) 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), 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 because green-marketing motives can be more acute either for large firms with large releases or, alternatively, 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.[24] Adding correlates does not 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 3 (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 membership 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 2 and .143 in model 3. Stated differently, a 1 percent (of sample mean) increase in SIERRA is estimated to increase the participation rate by 2.4 percent in model 2 and 1.4 percent in model 3. We thus find some support for regulatory preemption (hypothesis 3[c] of Maxwell, Lyon, and Hackett 2000) 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 3 but not in model 2. Similarly, our measure of prior (1988–90) release reductions (DIFREL) has a positive effect on participation but a statistically significant one

[24] To test hypothesis 1, we use a 2-year (rather than 1-year) history of regulatory actions in an attempt to capture a more complete picture of a firm's recent enforcement experience (as was done in Decker [2005], for example). We experimented with a variable measuring the count of enforcement actions to which firms were exposed over the preprogram period 1989–90. With this added variable, ENF89–90 remains a significant explanator of participation, while the number of prior actions has a statistically insignificant coefficient. Hence, our data suggest that having had a prior enforcement action was an important driver of 33/50 participation but that the number of actions was not.

Table 4
The Participation Equation

Hypothesis and Variable	Model 1 Estimate	Model 1 *t*-Value	Model 2 Estimate	Model 2 *t*-Value	Model 3 Estimate	Model 3 *t*-Value
Enforcement effects:						
ENF89–90	.468	2.040*	.581	1.950$^+$	.584	1.880$^+$
INSP89–90	.027	2.610**	.030	2.090*	.037	2.580**
PRP	.075	3.950**	.051	1.990*	−.133	−1.520
PRP^2					.015	2.290*
Prior release reductions, R&D, and concentration effects:						
DIFREL			.541	1.170	1.134	1.880$^+$
LRD			.004	1.750$^+$	.004	1.680$^+$
LRD × DIFREL					−.009	−1.720$^+$
HERF			1.928	1.220	1.736	1.040
Regulatory preemption:						
SIERRA			.719	1.940$^+$	.910	2.360*
SIERRA^2			−.098	−2.210*	−.119	−2.580**
SIERRA × RELEASE					−.374	−2.370*
Liability effects:						
STRICT			−.258	−.670	−.346	−.830
STRICT × LEMP					−.004	−.190
STRICT × LAWYERS					.096	.850
Boycott deterrence:						
BC			1.003	1.480	1.263	1.660$^+$
BC × LEMP					−.008	−.410
Green marketing:						
FG			.237	.590	.373	.860
FG × RELEASE					.177	.220

Firm-specific effects:						
RELEASE	1.274	3.970**	.979	2.210*	1.663	2.150*
$RELEASE^2$					−.064	−.210
LEMP			.030	2.490*	.039	1.670⁺
EDUC			−.013	−.170	−.044	−.550
LAWYERS			−.167	−.590	−.092	−.310
RTW			−.017	−.040	.079	.200
SPENDAQP			−.025	−.110	−.045	−.190
Industry fixed effects:						
SIC28	.629	2.080*	2.302	2.490*	2.507	2.520*
SIC33	.262	.670	1.928	1.940⁺	2.062	1.940⁺
SIC34	−.165	−.410	.695	.940	.775	.980
SIC35	.407	1.350	1.326	1.710⁺	1.372	1.660⁺
SIC36	.602	1.910⁺	.477	.900	.362	.670
SIC37	.530	1.390	.423	.720	.312	.530
SIC38	.109	.300	1.060	1.460	1.248	1.610
Constant	−1.100	−4.340**	−3.575	−2.220*	−3.641	−2.090*
log L	−157.61		−139.17		−135.06	
χ^2	135.25		172.12		180.35	
	(.00)		(.00)		(.00)	

Note. The dependent variable is the 33/50 program participation dummy. The data set is a cross-section of 325 firms, with time-varying variables measured as of 1990. Values in parentheses are p-values. $N = 325$.

⁺ Statistically significant at the 10% level, two-tailed tests.

* Statistically significant at the 5% level, two-tailed tests.

** Statistically significant at the 1% level, two-tailed tests.

only in model 3. This positive effect implies that firms that 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 effects of proximity to final consumers, FG, and its interaction with pollutant releases, FG × RELEASE).

6.2. The Pollution Equation

Table 5 presents results from estimation of the pollution equation. We present two representative models, one with random effects and one with fixed effects.[25] For all model variants, the coefficient on the augmented IMR (calculated using fitted values from our model 3 participation estimation, per note 14) 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 releases, reaffirming Khanna and Damon's (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. On the basis of 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 those found by Khanna and Damon (1999), 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.

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 effects 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-liability (versus negligence) 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-

[25] We estimated a wide variety of additional models and obtained similar results for the key variables of interest. These estimations (available on 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 (versus toxicity weighted) 33/50 releases, all with random effects and fixed effects.

Table 5
The Pollution Equation

Variable	Random Effects Estimate	Random Effects t-Value	Fixed Effects Estimate	Fixed Effects t-Value
PRP	.038**	2.780		
LINSPFAC	−.011	−.800	−.017	−.979
FAC	.052**	8.189	.053**	4.908
HERF	−.600*	−2.403	−.633*	−2.216
LRD	−.001**	−9.259	−.001+	−1.697
SIERRA	−.114+	−1.856	−.120+	−1.650
$SIERRA^2$	.012	1.404	.011	1.038
BC	−.270	−.671		
FG	−.005	−.013		
PART92	−.374**	−5.282	−.348**	−3.396
PART93	−.015	−.220	−.008	−.110
PART94	.017	.233	.014	.173
PART95	−.072	−.983	−.065	−.712
LEMP	.026**	11.188	.026*	2.527
$LEMP^2$	.000**	−8.328	.000**	−2.593
STRICT	−.131+	−1.666	−.151	−1.122
SG	.000	.641	.000	.562
IMR	.221**	4.178	.200*	2.498
Constant	.348	.694	.330**	27.507
F-test of OLS versus FE (p-value)			32.33 (.000)	
LM test of OLS versus RE (p-value)	1534.08 (.000)			
Test of RE versus FE (p-value)	49.25 (.0026)			
R^2	.29		.86	

Note. The dependent variable is RELEASE. The Breusch-Pagan Lagrange multiplier (LM) test of ordinary least squares (OLS) versus random effects (RE) [$\chi^2(1)$] rejects the null of OLS. The F-test of OLS versus fixed effects (FE) rejects the null of OLS. The Hausman test favors FE over RE. The RE model also includes the cross-section variables RTW, SPENDAQP, LAWYERS, and EDUC and dummies for standard industrial classification codes 28 and 33–38. For the FE model, we report robust standard errors and an R^2 that excludes impacts of the FEs. Both models include year dummies. Values in parentheses are p-values.

+ Statistically significant at the 10% level or better.

* Statistically significant at the 5% level or better.

** Statistically significant at the 1% level or better.

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.

6.3. Inspections and Enforcement Actions

Table 6 presents results from the inspections and enforcement actions equations.[26] Table 7 presents estimated marginal effects of 33/50 participation on inspection and enforcement rates in each of the program years, 1992–95.

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

Table 6
The Inspection and Enforcement Action Equations

	Inspection Equation				Enforcement Equation: Probit	
	Poisson		Probit			
Variable	Estimate	*t*-Value	Estimate	*t*-Value	Estimate	*t*-Value
Constant	−8.185**	−2.733	−12.202**	−4.454	−22.864**	−5.763
YEAR	.073*	2.257	.125**	4.274	.231**	5.474
PART92	.019	.183	−.019	−.172	−.341*	−2.067
PART93	−.320**	−3.028	−.411**	−3.570	.139	.794
PART94	.106	.924	−.002	−.018	.019	.110
PART95	−.271*	−2.437	−.103	−.859	−.301+	−1.807
SIERRA	−.180**	−4.268**	−.239	−4.697	.111+	1.756
BC	.392*	1.996	.538+	1.860	.392	1.206
NONATTAIN	−.171	−1.471	−.115	−.722	.289+	1.814
CDENSITY	6.94E−06	.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	−.241
LEMP	.002**	3.779	.004**	3.742	−.003*	−2.032
SPENDAQP	.659**	6.835	.637**	4.541	−.329*	−2.319
RTW	.330**	2.703	.199	1.064	−.289	−1.460
EDUC	−.002	−.076	.033	.797	−.006	−.116
STRICT	−.163	−1.301	−.020	−.108	−.382+	−1.888
LAWYERS	−.045	−.406	−.276+	−1.759	−.093	−.465
URATE	.002	.096	−.004	−.151	−.048	−1.457
GSPG	−.009	−.508	−.027	−1.490	−.010	−.325
LOUTCOMP	.014	.389	−.022	−.622	.233**	24.656
LENFORCE	.080*	2.090	.056	1.258		
LINSPECT					.101*	2.162
log *L*	−4,035.82		−2,362.38		−1,084.38	

Note. The dependent variables are INSPECT and ENFORCE. All models include industry dummies (for standard industrial classification codes 28 and 33–38). A linear restrictions (Lagrange multiplier) test favors the time variable (year) to year dummies in all models. $N = 5{,}703$.
+ Statistically significant at the 10% level.
* Statistically significant at the 5% level.
** Statistically significant at the 1% level.

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 cumulative reduction in a facility's inspections by 1995.[27] These effects 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

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

Table 7

Percentage Marginal Effects of Participation in the 33/50 Program on Facility Inspections and Enforcement Actions by Program Year

	Inspection Equation				Enforcement Equation: Probit	
	Poisson		Probit			
Year	Marginal Effect	*t*-Value	Marginal Effect	*t*-Value	Marginal Effect	*t*-Value
1992	2.5	.237	−.9	−.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

Note. For the probit models, the percentage marginal effect represents the estimated effect of participation in the 33/50 program on the probability of government inspection and enforcement action for each program year, as a percentage of sample average inspection and 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 effect of participation in the 33/50 program on inspection numbers in each program year. All effects are calculated for the models of Table 6.

* Statistically significant at the 5% level.

** Statistically significant at the 1% level.

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 values for 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. Using similar logic, state air quality spending spurs more inspections but, perhaps because of 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 infractions.[28] 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. Because of 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,

[28] 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 in which facilities experience enforcement actions when they have no inspections (.0656) is almost the same (and not significantly different statistically) as the proportion of time in which 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.

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.[29]

7. 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 VPRs (Maxwell and Decker 2006). Specifically, program participation involves firm investments in environmental auditing and technology that lower 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 spur participation by those firms that 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 and 2, which were 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 effects was much larger than that found by Khanna and Damon (1999) and that they occurred primarily in the first year of the 33/50 program's operation. Relative to the literature, our study also identifies new effects 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 did, 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 (Maxwell, Lyon, and Hackett 2000). However, contrary to earlier studies that did not account for these forces (for example, Khanna and Damon 1999; Arora and Cason 1996; Videras and Alberini 2000), we do not find support for the hypothesis that firms participated in the

[29] 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.

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 (Arora and Cason 1996; Arora and Gangopadhyay 1995).

Overall, this work lends support to the view that VPR programs, carefully combined with regulatory and 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 VPR efforts—voluntary environmental programs can succeed.

Appendix A

Chemicals Targeted by the 33/50 Program

This list is compiled from U.S. Environmental Protection Agency (1999).

Benzene
Cadmium and compounds
Carbon tetrachloride
Chloroform
Chromiun and compounds
Cyanides
Lead and compounds
Mercury and compounds
Methyl ethyl ketone
Methyl isobutyl ketone
Methylene chloride
Nickel and compounds
Tetrachlorethylene
Toluene
Trichloroethane
Trichloroethylene
Xylenes

Appendix B

Table B1

Standard Industrial Classification (SIC) Codes of Manufacturing Industries

SIC	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 miscellaneous 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	Miscellaneous manufacturing industries

Source. SICCODE.com (http://www.siccode.com).

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