DOES SELF-REGULATION REDUCE POLLUTION? RESPONSIBLE CARE IN THE CHEMICALS INDUSTRY

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Abstract

Responsible Care (RC) is a worldwide self-regulation program, whose codes of conduct include pollution reduction beyond existing regulations. We estimate the impact of RC on plant-level pollution intensity in the US chemical manufacturing sector, correcting for self-selection using characteristics of other plants belonging to the same firm. In a panel of 1,523 firms which own 2,735 plants between 1988 and 2001, we find that plants owned by RC participating firms do not reduce their pollution intensity relative to statistically-equivalent plants owned by non-RC participating firms, either on average or for various subsets of plants. Perversely, in several specifications, the former raise their toxicity-weighted air pollution intensity by 7-9% relative to the latter, a sizable amount compared to the 6% average yearly decline of that figure for all plants. Therefore, it is premature to rely on similarly designed self-regulation programs, i.e., without third-party certification or penalties for non-performance, to reduce pollution.

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1. Industry Self-regulation: Responsible Care

Self-regulation programs, in which industry associations set codes of conduct for their members against the backdrop of mandatory government regulations, are prevalent worldwide [1]. Self-regulatory programs operate in high risk industrial sectors, including the Institute of Nuclear Power Operations, Strategies for Today Environmental Partnership, and Responsible Care in the nuclear, petroleum and chemical industries, respectively [2]. Our study asks: *did self-regulation in the US chemical manufacturing sector reduce plant-level pollution intensity?*

In response to the Bhopal accident, the American Chemical Council (ACC) created Responsible Care (RC) in 1989 and mandated participation in RC for its members.[1] We study RC for three reasons. First, RC's Pollution Prevention (PP) Code, which aims "to achieve ongoing reductions in the amount of all contaminants and pollutants released to the air, water, and land from member firm's plant" *can be quantified and thus evaluated* [3].[2] RC members are required to submit their annual self-assessment of their progress toward code implementation, signed by the firms' CEOs, to the ACC [4]. Second, RC shares key features of other prominent self-regulatory programs, including Climate Leaders, a greenhouse gas reduction program, in particular, the absence of third-party certification pre-2002 and penalties for non-performance [4, 5]. Third, despite scarce empirical evidence on its performance, policymakers have stated that RC is an effective program that should be emulated by other industries [6].

Our study improves upon Lenox and King's [7] seminal empirical assessment of RC in three ways. First, we address firms' self-selection into RC, a source of estimation bias [8,9], with two sets of instruments: (i) for multi-plant firms, we use instruments based on the exogenous

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[1] The American Chemical Manufacturers' Association was renamed the American Chemical Council.

[2] Unlike RC, self-regulatory programs in other concentrated industries are typically adopted by almost all firms in the industry at the same time period, making it difficult to identify program effects.

characteristics of *other plants* belonging to the same firm that affect the likelihood of the firm participating in RC, as these characteristics are unlikely to be related to the individual plant of interest's pollution; and (ii) for single and multi-plant firms, we use three other instruments - the share of plants in the given sub-industry participating in RC, firms' lagged RC participation, and firms' participation in the ACC before the introduction of RC – factors that are related to the costs and benefits of firms joining RC, but are less likely to affect plants' pollution directly other than through the RC program. Second, in constructing our plant-level panel database of the US chemical sector, we have been able to compile plant-level employee data from Dun & Bradstreet (D&B), which allows our study to better address, albeit imperfectly, the confounding effect of output fluctuations. Third, we examine the heterogeneous impact of RC on plants with specific characteristics because according to Dawson and Segerson's theoretical study [10], a subset of plants may reduce their pollution in self-regulatory programs, even if other plants free-ride.

Our study of 1,523 firms that own 2,735 plants, spanning 1988 to 2001, finds that on average, plants owned by RC participating firms did not reduce their pollution intensity relative to statistically equivalent plants owned by non-RC participating firms. These results are robust to multiple measures of pollution and pollution intensity and to various time periods and subsamples. Allowing for the possibility of heterogeneous program effects, we find only a small subset of plants had reductions in pollution intensity that we can attribute to the program, while a much larger subset experienced significant increases. In a few specifications, we even find that RC participants on average increased their toxicity-weighted air pollution intensity by 7-9% compared to statistically equivalent non-RC participants. This increase among RC participants is sizable when compared to the yearly decline of 6% for the average toxicity-weighted air pollution intensity of all plants in our sample between 1990 and 2001.[3] Based on our estimates, the switch from non-participation to participation in RC would wipe out approximately a year of this trend. From a policy perspective, we conclude that it would be premature to rely on self-regulation programs that mirror RC before 2002, i.e., without third party verification or enforceable penalties for non-performance, as a tool to reduce environmental risks.

2. Responsible Care – empirical evidence

The Deep Horizon and Bhopal accidents [1,11] illustrate that firms face a collective action problem in maintaining their industries' reputation as socially responsible actors. One firm's adverse actions can impose costs on other firms in the industry. Therefore, while every firm in the industry would be better off if each firm were to act in a socially responsible way, individual firms do not have the incentive to act in this manner. Proponents argue that self-regulation programs can resolve this problem, by limiting their membership to firms that commit to their codes of conduct, and thereby providing a mechanism for participants to signal that they are undertaking superior risk management processes [12]. Participants benefit from this reputation in their interactions with consumers, investors, activists and regulators [13,14]. Participation may reduce inspections by regulatory agencies [14,15] and discourage boycotts by environmental groups or pre-empt their lobbying for stricter regulations [16,17]. Dawson and Segerson's [10] theoretical study of pollution regulations show that even if other firms free-ride, a critical number of firms, will reduce their pollution in order to maintain the overall credibility of the self-regulation program. These firms would incur larger costs of stricter regulation should the self-regulation program fail. In contrast, opponents argue that these programs do not provide sufficient incentives for firms to undertake costly actions to reduce their adverse environmental

[3] The annual decline is 11.8% for RC participants and 2.6% for non-participants between 1990 and 2000.

impacts [1,18]. Givel [11], drawing on previously confidential ACC documents, argues that RC, which did not require third party certification of members' performance pre-2002 and did not impose penalties on members' failure to implement codes of conduct, was a publicity effort to stave off regulations.

The fundamental empirical question is – has RC reduced, left unaffected or raised pollution intensity? In King and Lenox [7]: (i) their GLS model, which does not correct for self-selection, finds that participants reduce their pollution at a slower rate than non-participants; and (ii) their fixed effects model, which addresses the selection issue, finds that the RC coefficient is not statistically significant. Nevertheless, it is difficult to draw conclusions from that study for two reasons. First, the direction of bias arising from self-selection is a priori unknown – the GLS model could have *understated* RC's effect on pollution reduction if firms that self-select into RC are those that face more difficulties in reducing pollution, and join in order to benefit from shared best practices.[4] Conversely, if the firms that self-select into RC are those that will reduce their pollution regardless of RC participation, the failure to correct for self-selection would overstate RC's effect on pollution reduction. Second, their statistically insignificant coefficient in the fixed effects model could have resulted from (1) imprecise estimates due to the reliance of identification on few plants that switched status and (2) attenuation bias resulting from their use of production ratio data that is at best, noisy and at worst, uninformative [19].[5]

3. Estimation Method

3.1 Method

[4] Comparison of our GMM and OLS estimates indicates that this form of self-selection is dominant during our study period.

[5] Our analysis of the data and our conversation with researchers and EPA region 1 personnel suggest that the variable does not, in practice, capture plants' annual changes in output. Moreover, the American Petroleum Institute's survey notes that its members do not have well-established methods for estimating this variable. E.g., a plant that faced a 20% reduction in output may report a production ratio value of 0.8, 8 or 80.

We estimate the impact of RC by comparing plants owned by RC participating firms with ***statistically equivalent plants*** owned by non-RC firms, i.e. both the average treatment effect and the effect conditional on plant and firm attributes. We achieve this comparison by using instrumental variables to control for firms' self-selection into the RC program. We model the firm's decision to participate in RC separately from the plant's pollution equation. This distinction helps us to motivate valid instruments i.e., variables that are related to the plant's owner's participation in RC, but do not directly affect the plant's pollution.

3.2 Model.

$$y_{ijt} = y_{ijt-1}\,\beta_y + x_{1ijt}\beta_1 + x_{2it}\beta_2 + p_{ijt}\delta_1 + p_{ijt}\,(x_{3ijt} - \bar{x}_3)\delta_2 + \mu_{ijt}$$

(Pollution: Equation 1)

Pollution in time t is affected by the characteristics of plant j owned by firm i in time t (x_{1ijt}), characteristics of parent firm i (x_{2it}), the observed participation status of the plant's parent (p_{ijt}), a subset of plant characteristics which affect the impact of RC (x_{3ijt}), and an unobserved component (μ_{ijt}). We also include lagged pollution, y_{ijt-1}, to capture persistence in plant technology. The first term ($x_{1ijt}\beta_1$) accounts for the effect of the covariates on pollution regardless of RC status. The second term ($p_{ijt}\,\delta_1$) captures the effect of RC on the average plant, while the third term ($p_{ijt}\,(x_{3ijt} - \bar{x}_3)\delta_2$) captures the impact of RC that varies by plant characteristics. We demean the x_3 variables in the third term in order to consistently estimate the effect of RC on an average plant with the δ_1 coefficient.

The unobserved component is made up of industry sub-sector and year components,[6] as well as an idiosyncratic shock, $\mu_{ijt} = \eta_{SICj} + \zeta_t + \varepsilon_{ijt}$. We restrict the shocks (ε_{ijt}) to be mean zero,

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[6] We also estimated the model using sub-industry and year interaction dummies, and we used plant dummies instead of lagged pollution to capture persistent technology. In addition we used an Arellano-Bond estimator including plant

and independent across firms, but allow them to be correlated within the same firm. In addition, we place no restrictions on the variance of the errors. Because we find evidence of heteroskedasticity, we use a GMM estimator that is more efficient than the standard IV estimator.

Firms choose to participate in RC if they believe the benefits accrued across their plants exceed the cost of membership and adhering to the goals of the program.

$$p_{it}^{*} = \sum_{(s=j\in i)} x_{1ist}\theta_1 + x_{2it}\theta_2 + z_{1it}\theta_3 + \xi_{it} \quad \text{(Firm Participation: Equation 2)}$$

p_{it}^{*} is not directly observed, but firm *i* chooses to join RC in time *t* if ($p_{it}^{*} \geq 0$). This latent variable equation can be interpreted as firms choosing to join RC if they receive positive net benefits from participation. The net benefits are a sum of the plant-specific benefits from each of a firm's plants, $\sum_{(s\in i)} x_{1ist}\theta_1$, firm-level factors that also affect pollution, x_{2it} , firm-level characteristics unrelated to pollution z_{1it}, and an idiosyncratic shock, ξ_{it}.

The estimation complication arises due to the correlation between (ξ_{it}, μ_{ijt}) as unobserved factors that affect the participation decision may also effect plant-level pollution. We examine factors that affect the likelihood of a plant being owned by a member of RC to construct excluded variables. Based on Equation 2, the likelihood of a plant *j* being a member of RC, can be written as:

$$p_{ijt} = 1[x_{1ijt}\theta_1 + \sum_{(s=-j\in i)} x_{1ist}\theta_1 + x_{2it}\theta_2 + z_{1it}\theta_3 + \xi_{it} \geq 0] \quad \text{(Plant Participation: Eq. 3)}$$

Equation 3 is not a structural equation because the decision to join RC is made at the firm level, but it motivates our instruments and it provides an estimating equation to use to generate additional instruments for the participation interactions $p_{ijt}\,(x_{3ijt} - \bar{x}_3)$.

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fixed effects and lagged dependent variables. However, in each case, we found qualitatively similar results to our OLS and GMM estimates, with the OLS results biased in the same direction.

$\sum_{(s=-j\in i)} x_{1ist}$, captures exogenous characteristics of other plants owned by the same firm, which affect the firm's cost of adhering to RC's standards, but do not directly affect pollution at the plant of interest. For example, if Dow Chemical needs to improve pollution at a plant in New Jersey due to local regulatory or political pressure, it may reduce the costliness or increase the benefit of the company joining RC and therefore may affect the likelihood of all Dow plants being in the program. However, it would not directly cause Dow to reduce pollution at a plant in Louisiana.

We control for the endogeneity of (p_{ijt}), and therefore ($p_{ijt}\ (x_{3ijt} - \bar{x}_3)$)using a GMM estimator of the pollution equation. We instrument for participation (p_{ijt}) using the instrumental variables, ($\sum_{(s=-j\in i)} x_{1sit}, z_{1it}$) described below. We could use the estimated participation probability from Equation 3 to instrument directly for observed participation. However, it would leave us with an equal number of excluded variables and endogenous covariates, restricting our use of well-defined over-identification tests. Because the validity of our instruments is crucial to our analysis of this voluntary program, we opt to directly include each of the instruments in our estimator.

In specifications where we allow the effect of RC to vary with firm characteristics, we use the Logit estimates of the probability of participation, ($\hat{p}_{ijt}$) interacted with demeaned covariates to instrument for ($p_{ijt}\ (x_{3ijt} - \bar{x}_3)$). Estimates of the participation probability are correlated with (p_{ijt}) and independent of (μ_{ijt}) by construction. Therefore, when interacted with - the demeaned covariates, they are valid instruments for ($p_{ijt}\ (x_{3ijt} - \bar{x}_3)$).

In addition, because we include lagged pollution as a potentially endogenous covariate, we include pollution two years prior as an additional instrument. This makes our estimates robust to first-order serial correlation in the errors.

The estimated effect of RC on the pollution of an individual plant is:

$\left(p_{ijt}\hat{\delta}_1 \quad + p_{ijt}\left(x_{3ijt} - \bar{x}_3\right)\hat{\delta}_2\right)$. (RC impact on pollution: Equation 4)

We use the Delta Method to calculate the standard errors of these estimates and determine for which plants the program has a significant effect on pollution. The variance of the estimated effect of RC on a given plant is given by,

$$\left[1\ \left(x_{3ijt} - \bar{x}_3\right)\right]\hat{\Sigma}_{\mathrm{RC}}\left[1\ \left(x_{3ijt} - \bar{x}_3\right)\right]^T$$

where $\hat{\Sigma}_{\mathrm{RC}}$ is the variance-covariance matrix of the coefficient estimates of RC variables.

3.3 Dependent variable

We measure the impact of RC on plants' pollution and pollution intensity. We limit our analysis to chemicals whose TRI reporting requirements are consistent since 1988. We use toxicity-weighted pollution to control for variation in the toxicity of chemicals. To account for pollution intensity, we use plant-level number of employees as an imperfect proxy for output. Given the absence of publicly available output data, we check the robustness of our results to alternative normalization strategies.

3.4 Control variables

We account for factors that influence participation in RC and plants' pollution, as noted in past studies on self-regulatory and voluntary programs [20, 21]. We account for plant size, using the log of the lagged plant-level number of employees. We account for firm size, using the log of the lagged firms' employees across plants, and the number of plants owned by the firm. Larger firms may have greater financial resources to invest in pollution abatement. Larger firms may also face greater demand, and therefore, have more to gain from signaling green to their consumers [20]. We include a dummy variable for single-plant firms to capture the differences between a single-plant firms, and multi-plant firms.

We include industry-level variables at the 4-digit SIC level (SIC-4), i.e., producer price index, shipment quantity index, the Herfindahl-Hirschman index and SIC-4 dummies. The quantity and price indices are normalized to 100 within the specific SIC-4 code in 2000. The Herfindahl-Hirschman index is calculated using the value of shipments of the largest 50 firms in the SIC-4 code, as reported in the quinquennial Census of Manufacturers. Data for interceding years is linearly interpolated. We also include year dummies to control for changes in federal regulations and available technologies. We control for the neighborhood pressure on plants, measured at the census tracts where the plants are located. The measures are the median income at the census tract, the percentage of the census tract that is inside an urban area (percent urban), the percentage of the population in the tract that is white, and the percentage with less than a high school degree (low-educated).

The rationale for the following variables is analogous to that outlined for the firm-level variables described below for instruments 1-3. A plant's participation in a higher polluting sub-industry is captured by the plant's SIC pollution index. This variable is defined as the ratio of (a) average pollution intensity in the plant's SIC 28xx to (b) average pollution intensity in the entire SIC 28. We also account for plants' share of pollution that is subject to Maximum Availability Control Technology (MACT) standards. In addition, we control for plants' lagged pollution, instrumented using the plants' pollution two years prior. Plants with larger amounts of pollution, due to dependence on polluting technology may find it more difficult to abate pollution. Alternatively, with diminishing returns to abatement, such plants may have more options for abatement.

3.5 Instrumental variables

The RC membership decision is made at the firm-level and is the same for all of the firm's plants, while pollution performance is specific to each plant. This feature of the RC program allows us to construct variables that are correlated with the likelihood of a plant's owner being in RC, but that do not directly affect a plant's pollution. This identification strategy implicitly assumes that there are no spillovers across plants in pollution reducing technologies. Implementation of new technology or processes at one plant does not make it significantly less costly to reduce pollution at other plants owned by the same firm. We check the validity of this assumption using over-identification tests.

Our approach to the construction of the first three of six instruments, described below, follows one commonly used in the industrial organization literature. For example, Berry et al. [22] use functions of the characteristics and cost shifters of all other products as instruments of a product's unobserved attributes. Nevo [23] uses the average prices of the same product in other cities in the region as instruments for a product's price in a given city. The drawback of these three instruments is that they are undefined for single plant firms, which make up approximately a quarter of our sample. Therefore, we estimate the model for plants owned by multi-plant firms (the "multi-plants" sample) using all six types of instruments, and then separately for all plants (the "all-plants" sample) using only the last three instruments.

Instrumental variable 1: Firms' plant-level share of pollution subject to MACT standards

The Clean Air Act requires the EPA to set stringent technological regulations to reduce Hazardous Air Pollutant (HAP) pollution. Among TRI chemicals, 188 are HAPS. These regulations, labeled MACT standards, require new and existing chemical plants to install the technology that has been adopted by plants in the same production category that have achieved the best pollution control and the lowest pollution [24]. Plants emitting a significant amount of HAPs will have to reduce their pollution, even in the absence of RC. This mandate to reduce HAPs causes their parent firm to face less additional costs to comply with RC. Therefore, at the plant-level, the share of pollution subject to MACT standards is likely to affect participation and pollution. The share of pollution subject to MACT standards at other plants owned the same firm, will affect the likelihood of the parent joining RC, but should not directly affect the pollution of the plant of interest. Therefore, as with Instrumental Variables 2 and 3, we exclude the plant of interest when calculating the firm-level variable to ensure exogeneity.

Instrumental Variable 2: Firm's participation in more heavily polluting sub-industries (Firm's SIC's pollution index)

This variable captures the extent to which a firm operates in dirtier sub-industries. Part of the technological options for pollution abatement is specific to sub-industries. Firms that operate in more pollution-intensive sub-industries may find it more costly to reduce pollution due to their greater reliance on pollution-intensive technologies. On the contrary, if there are diminishing returns to pollution abatement, those firms may have cheaper options to reduce pollution [20].

We measure the pollution intensity of a sub-industry as the ratio of (a) the average pollution/employee of plants operating in SIC-28xx to (b) the average pollution/employee of all plants operating in SIC-28. In creating this variable for plant j, we average the sub-industry pollution intensities for all other plants belonging to the firm that owns plant j.

Instrumental Variable 3: Firm's plants' neighborhood characteristics

This set of variables measures the neighborhood pressure for firms to join RC. We use demographic measures of the urban density, percentage of the population that is white, percentage with less than a high school diploma, and the percentage living below the poverty line. We also include county-level National Ambient Air Quality Standards (NAAQS) non-attainment status. These variables capture the location specific effects that may influence both the pollution of a plant and the likelihood their parent will join RC. As in instruments 1 and 2, in creating instrument 3 for plant *j*, we average the characteristics of all plants belonging to the firm that owns plant *j*, excluding plant *j* itself.

Instrumental Variable 4: RC participation within the Sub-Industry

This instrumental variable is the likelihood of RC participation by other plants in the same sub-industry. It is calculated as the ratio of the number of plants that are RC members to the total number of plants operating in the same 4-digit SIC code in the given year, excluding the plant of interest. If other plants in the same sub-industry are members of RC, there may be pressure for that plant to join as well, or there may be features of the sub-industry that make RC appealing. Conditional on RC, this variable would be independent of pollution given that we already include SIC-4 fixed effects in the pollution equation.

Instrumental Variable 5: Lagged RC participation

A plant that belongs to a firm that is an RC participant in time *(t-1)* is more likely to belong to an RC participating firm in time *t*. Persistence in a firm's participation is likely, as the cost of continuing participation may be less than the cost of a new member joining because of members may have already implemented new systems and procedures to adhere RC's standards. In addition, firms already participating in RC may find it costly to switch out of the program as it may send a negative signal to their consumers or to regulators about their conduct. The plant would change its RC status if the firm changes its RC status, if the firm merges with another company, or the plant is sold to another firm with a different RC status.

Instrumental Variable 6: Participation in ACC prior to RC

Prior to the creation of Responsible Care in October 1989, firms that were already ACC members were likely receiving a positive net benefit from the trade association aspects of the program. These benefits accrued from activities such as public relations and lobbying efforts, and shared best-practices. After RC was implemented and was a condition of membership in the ACC, the ACC did not change its mission. They simply added additional obligations and benefits. Membership then included both the benefits and costs of being a part of the trade association, along with those from the self-regulation program. Therefore, holding all else constant, firms which were members prior to RC were more likely to receive a positive net benefit from the trade association or self-regulation benefits of RC compared to firms that had not yet joined ACC.

4 Data

Our data comprise plants which are both required to report their pollution to the TRI and which report their number of employees to D&B. The self-reported TRI data is one of the few comprehensive sources of pollution data and it has been widely used [25, 26, 27]. The chemical-specific toxicity weights are from the Risk Screening Environmental Indicators model [28]. Plant-level counts of EPA inspections are from the EPA's Air Facilities System (AFS). Annual county-level non-attainment status for the criteria air pollutants under the Clean Air Act are from the EPA [29].The SIC-4 Herfindahl–Hirshman Index is from the Census Bureau, while the quantity of shipment and the producer price indices are from Bureau of Economic Analysis. Our final dataset is comprised of an unbalanced panel of 1,523 different firms and 2,735 different plants for the years 1988-2001. Our sample is likely to include the larger plants within the US chemical sector, as plants that are in our database are those that report to D&B, which tend to be the larger plants. Further, plants are required to report to the TRI only if their pollution exceeds a specific threshold [28].[7] We link a firm to all its plants that report to the TRI and that report in TRI as being in the chemical manufacturing sector (SIC-28). Therefore, for a firm which operates in both chemical and non-chemical manufacturing sectors, the firm's pollution and number of employee is that from its plants operating in SIC-28 only.

5 Data Description

RC participants have grown slightly from 126 firms (or 804 plants) in 1988 to 142 firms (or 1,199 plants) in 2001. On average RC participants are more pollution intensive (measured in toxicity-weighted air pollution) than non-RC participants, but the pollution-intensity for both cohorts declines over our sample period. Comparison of columns 3 and 4 in Table 1 indicates that RC and non-RC participants differ systematically in their characteristics. On average, plants that belong to RC participating firms emit more pollution, measured in levels or pollution intensity; are larger, measured in number of employees; and tend to belong to multi-plant firms with larger numbers of plants. RC plants operate in more polluting sub-industries, as indicated by the SIC Pollution index, and in more concentrated industries, as measured by the Herfindahl-Hirshman Index. This comparison indicates that the two cohorts are fairly different, and highlights the need for our analysis to control for these systematic differences.

6. Regression results

Our sample covers 18,850 observations of plant-years. Our analysis spans the years 1990-2001, as each year of observations requires two lagged years. Our dependent variable is air

[7] Plants operating in SIC-28 are required to report to the TRI if they: (1) had 10 or more full-time employees or the equivalent; and (3) "manufactured" or "processed" more than 25,000 pounds or "otherwise used" more than 10,000 pounds of any listed chemical during a calendar year.

pollution intensity, measured as the ratio of toxicity-weighted air pollution at a plant to the number of employees at that plant. We report robust standard errors clustered on firms.[8]

6.1 OLS

Regressing toxicity-weighted air pollution intensity on RC participation status (RC-status) using OLS, we find that RC participation is associated with **larger** pollution intensity (Table 2, columns 1-4). These results hold for the full sample and the subset of plants owned by multi-plant firms. We recover C-statistic values between 3 and 8, which are statistically significant, confirming that RC participation is endogenous, and thus calling for the IV or GMM estimator.[9] Because the Pagan-Hall test statistic indicates that the errors are heteroskedastic, we apply the GMM estimator that is more efficient than the simple IV estimator [30].[10]

6.2 Main regression results

In the participation regression, we regress the RC participation dummy on the instruments and other covariates. The coefficients for the variables that serve as instruments in the GMM estimation are reported in Table 3. The coefficients on other covariates are available on request. Table 4 and Table 5 show the results of the GMM estimation, which uses the instrumental variables to control for self-selection in estimating the impact of RC-status on pollution intensity.

The first column in Table 3 – Table 5 shows our main specification for our full sample, in which we use three instruments. The fifth column in Table 3 – Table 5 shows our main specification for the subset of plants owned by multi-plant firms, in which we use additional

[8] Our results clustering on industry-year interactions are qualitatively similar.
[9] We also test for the endogeneity of lagged pollution in the participation equation. The C-statistic for lagged pollution is also significant at the 1% level, thus motivating its treatment as endogenous.

[10] The Pagan and Hall test (1983) is robust to the presence of heteroskedasticity in other areas of the model, i.e., the participation equation.

instruments, i.e., the characteristics of other plants owned by the parent firm. In Columns 2-3 and columns 6-7, we check the robustness of our results by varying the instruments relative to our main specifications. For the purposes of comparison, in column 4 which uses the full sample, we include instruments based on characteristics of other plants owned by a given firm, even though these instruments are undefined for plants owned by single plant firms.[11] Again, for the purposes of comparison, in column 8, which analyzes the subset of plants owned by multi-plant firms, we exclude the instruments based on the characteristics of other plants owned by a given firm.

6.21 Instruments

Results of the logit regression of the RC participation dummy on the instruments and other covariates are presented in Table 3. We note that a statistically (in)significant coefficient on any one instrument does not necessarily imply that the instrument is (in)valid. First, we consider the three instruments defined for both the "all-plants" and "multi-plants" samples. The first two instruments, firms' lagged RC participation and firms' membership in ACC, show the expected signs and are statistically significant in all specifications. The third instrument, RC participation in the SIC-4 industry, show the expected signs and are statistically significant in the main specifications for both the "all-plants" and "multi-plants" samples.

Second, we consider the additional instruments defined only for the "multi-plant samples." All but two instruments are statistically significant in at least one specification. As expected, plants are more likely to participate in RC if they belong to firms with a high HAP to TRI ratio (column 6). However, they are less likely to participate if their parents own more plants

[11] In order to estimate the model with this specification, single plant firms receive the value of 0 for the additional instruments and a value of 1 for the dummy variable indication a firm owning a single plant.

located in neighborhoods that are in NAAQS non-attainment for at least one of the criteria air pollutants (column 7). The neighborhood pressure variables do not yield a consistent picture. In most specifications, firms whose plants are located in more educated neighborhoods are more likely to participate in RC (columns 5-6), but surprisingly, locations in poorer neighborhoods have the same effect (column 6).

6.22 Validity tests for instruments

While no tests can positively determine that an instrument is valid, we run a number of tests to check if they are conclusively invalid. The first condition for valid instruments is that they are not correlated with the error term in the second stage. Based on the p-value of the Hansen-J statistics for over-identification (Table 4), we fail to reject the null that instruments are jointly valid.

The second condition for valid instruments is that they are correlated with the endogenous regressors. To examine their relevance, we conduct tests based on the relationship between the instruments and RC-status. We consider whether the instruments are "weak enough to imperil inference," i.e., if the bias in coefficients from the IV estimates exceeds a specific percent bias in the coefficients from the OLS estimates [31]. In Table 4, we report the Stock and Yogo [31] compilation of critical values at which the bias in coefficients from the IV estimates exceeds 5% or 20% the bias in the coefficients from the OLS estimates [32].[12] First, we report a test of under-identification. Our reported Kleibergen and Paap LM statistic exceeds the Stock and Yogo critical values even at the 5% relative bias; thus, we fail to reject the null that the instruments are correlated with the endogenous regressors. Second, we report a test of weak

[12] Baum et al. (2007) suggest the use of this comparison even though the Stock and Yogo (2005) compilation is based on the assumption that the errors are independent and identically distributed (i.i.d). They note that no studies test weak instruments when the errors are not i.i.d.

instruments. Our Kleibergen and Paap Wald statistic easily exceeds the Stock and Yogo [31] critical values; thus we fail to reject the null that the instruments are strongly correlated with the endogenous regressors in any of the specifications.

6.23 RC impact on pollution

The results from the GMM estimation show that plants owned by firms participating in RC did not reduce their pollution intensity relative to statistically equivalent plants owned by non-RC participating firms (Table 5). The coefficient on the RC status is positive, but not statistically significant at conventional levels. These results hold for the full sample of all plants (column 1-4) and the subset of plants owned by multi-plant firms (column 5-8), both for the main specifications and the specifications in which we varied the instruments.

Our estimates indicate that we cannot rule out that the program effect is not statistically different from zero. Nevertheless, it is still useful to consider the magnitude of the bounds of these estimates. With a 95% confidence interval, we estimate that the program effect lies between the end-points [-0.02, 0.12]. Even taking the lower-bound estimate, i.e. the most favorable towards finding a reduction in pollution, we report with 95% confidence that plants owned by RC participants reduce their pollution by no more than 2% relative to statistically-equivalent plants owned by non-RC participants. In other words, even in the most favorable estimate towards finding a reduction, the magnitude of the reduction is still small. Interestingly, in the most favorable case against finding a reduction in pollution, the upper-bound estimate indicates that plants owned by RC participants increase their pollution by 12% relative to the non RC participants. Such an increase is large relative to the yearly 6% reduction of pollution among all plants in our sample between 1988 and 2001.

Comparing the results from the OLS (Table 2 columns 2 and 4) and GMM (Table 5 columns 1 and 5) regressions, we find that the coefficients from OLS (β_{OLS}= 0.2) and GMM (β_{GMM}=0.03 or 0.05) are both positive, β_{OLS} is statistically significant, and the magnitude of β_{OLS} exceeds β_{GMM}.[13] This could be explained by firms which face more difficulties in reducing pollution self-selecting into RC to signal green. Firms may use RC as a substitute for reducing pollution, but controlling for selection, the increase due to participation is less pronounced.

6.25 Participation in RC

We analyze the impact of marginal changes in covariates on the probability of participation in RC (Table 6). We calculate the changes in the likelihood of participation using coefficients estimated in the base participation regression (Tables 3) and holding the covariates at their mean values. The probability of participation in RC estimated at the sample means is 13% for all plants and 54% for plants owned by multi-plant firms.

Institutional aspects of the RC program exert a strong influence on plant's participation. Based on the results from the full sample of all plants, participation in RC in the previous period raises the probability of participation by 90%, while plants' ownership by a firm that was a member of the ACC prior to RC raises participation by 34%.[14] Sub-industry specific factors also influence RC participation. Switching a plant from a sub-industry with mean participation in RC to a sub-industry one standard deviation above the mean, increases the plant's likelihood of participation by 20%.

[13] In an alternative specification that includes plant fixed effects and excludes lagged pollution, β_{GMM} = 0.11 and still statistically insignificant and β_{OLS} =0.13. We use the Arellano-Bond estimator for a specification that includes both plant fixed effects and lagged pollution, using the first stage predicted RC probability to correct for the endogeneity. The coefficient on RC remains statistically insignificant.

[14] For a plant at the sample mean, being in RC the previous period increases the probability of participation by 90% from 1.7% to 91%. For a plant at the sample mean, being in ACC in 1988 and 1989 the previous period increase the probability of participation by 34% from 7% to 40%. Figures are rounded.

Firm size exerts a sizeable influence on plant participation. A one standard deviation increase in the number of plants owned by the firm raises RC participation by 20%. Ownership by a firm whose logged mean number of employees per plant is one standard deviation more than the average raises participation by 7%. “Environmental justice” concerns have been voiced suggesting that plants in poorer or less educated neighborhoods may be less likely to join self-regulatory programs due to lack of public pressure. However, we find that neighborhood characteristics exert only a weak influence on participation and their effects appear contradictory. In the full sample, plants in poorer neighborhoods are more likely to participate (+2%), while among multi-plant firms, those firms whose plants are located in less educated neighborhoods are less likely to participate (-6%).

6.31 Robustness checks: RC effects over time

To check whether the impact of RC has changed over its existence, we specify models which allow the program effects to vary over time. In these specifications which omit the RC participation dummy, the coefficient on the interaction variable between the RC participation dummy and the year(s) dummy provides a comparison of the pollution intensity from RC participants and non-RC participants for the given time period. As seen in Table 7, in the specification with blocks of years, RC participants increased their pollution intensity by about 7-9% relative to statistically equivalent non-RC participants. It is noteworthy that we do not find statistically significant negative effects in any of the time periods.

6.32 Other robustness checks

Our dependent variable on pollution intensity is the ratio of toxicity-weighted air pollution to the number of employees. This specification could lead to misleading conclusions if plants respond to RC by choosing a production process that is less labor intensive, but that does not raise pollution per unit of production. Should larger plants increase their output at a faster rate than labor, our denominator for large plants may be too small, resulting in too large a measure of pollution intensity. Given that RC participants typically have larger plants, this mis-measurement of pollution intensity could bias our estimates of the impact of RC. To address this concern, we use two alternative dependent variables: the toxicity-weighted level of pollution and pollution divided by the squared number of employees. Results using these alternative dependent variables, available from authors, provide no evidence that RC reduced pollution.[15]

6.33 Heterogeneous program effects

We test Dawson and Segerson's [10] hypothesis that sub-groups of plants have incentives reduce their pollution intensity, even if other plants free-ride. Our results in Table 8, however, indicate that program effects do not vary significantly based on most plant or firm characteristics, including measures of plant or firm size. Environmental justice concerns that RC firms whose plants are located in poor neighborhoods would be less inclined to reduce pollution are not supported by our results (column 4). Nevertheless, we do find that the effect of RC on pollution intensity is decreasing with the lagged pollution of plants (columns 5-7). Cleaner plants are more likely to engage in a greater degree of green-washing, i.e., participating in the RC program that signals green but failing to reduce their pollution intensity. In contrast, additional pressure from participation or shared best practices in the program may cause the dirtier plants that participate in RC to partially reduce their pollution intensity. Next we apply Equation 4 to estimate the impact of RC on plants' pollution intensity, and use the Delta Method to estimate

[15] Results for other specification checks are also available on request. We restrict our analyses to three different samples of plants: continuous reporters for pollution, continuous reporters for counts of employees, and plants whose owners' RC status changes. We examine both pounds and toxicity-weighted pollution released into various media: water, land, offsite and all media. We include the inspection variable to ensure our estimated coefficient on the RC participation dummy does not erroneously capture regulatory effects. In no case did we find that participation leads to a statistically significant reduction in pollution intensity.

the standard errors of these estimates. At a 90% confidence level, we estimate RC significantly increased pollution intensity for 7,178 plant-year observations, of which 2,459 belong to RC participants. We estimate that RC reduced pollution intensity in 5,845 plant-years out of 18,850, but the effect was only significant for 373 observations, of which 77 were RC participants. These figures indicate that RC significantly reduced pollution intensity for approximately 1% of participating plants.

As a point of reference, we consider a counterfactual in which the 884 plants that did not participate in RC in 1999, decided to join the program and adjusted their performance in the same manner as we estimated the actual participants did. In this case, we estimate with 90% confidence that 377 would have increased their pollution intensity under the program, while only 17 would have decreased their pollution intensity.[16] While we find that RC only led to decreased pollution intensity for an extremely small number of the plants, these results suggest that there may be offsetting forces at work. Participation in a "program that signals green" may be a substitute for improving performance leading to increased pollution intensity. Shared best practices, on the other hand, may help the dirtiest plants to reduce their pollution intensity. We cannot separately identify these effects, though our results indicate that if both are at work, the substitution effect is much greater.

7. Conclusion

We conclude that participation in RC ***does not*** reduce a plant's pollution intensity. In most specifications, we find that plants owned by RC participating firms do not reduce their pollution intensity relative to statistically equivalent plants owned by non-RC participating firms.

[16] There may be additional equilibrium effects from adding plants to the program, as the collective benefits may be reduced by the addition of firms which are likely to be free-riders. These additional free-riding firms would reduce the incentive for any of the participants to improve performance.

In a few specifications, we even find that RC participants increase their toxicity-weighted air pollution intensity by 7-9% relative to statistically equivalent non-RC participants. This increase is sizable when compared to the yearly decline of 6% for the average toxicity-weighted air pollution intensity of all plants in our sample between 1990 and 2001. Based on our estimates, the switch from non-participation to participation in RC would roughly wipe out at least one year of this trend.

There are three caveats to our results. First, our study would understate RC's impact on reducing pollution intensity if non-participants reduced their pollution intensity in response to RC [33, 34]. For example, RC could have prompted innovations in pollution abatement technology and this technological spillover could have reduced the pollution abatement costs and thus the pollution intensity for all plants. Nevertheless, the likelihood that these spillover effects would be larger among RC members that share best practices mitigates this concern. Second, we examine only one of RC's codes i.e. Pollution Prevention, and leave the evaluation of other codes to future work. Because RC was created in response to Bhopal, plant managers may have prioritized RC's codes, such as Process Safety [3], that are more closely related to the prevention of industrial accidents. Third, we examine the RC regime prior to their requirement of third party verification of plants' conformance with the RC14000 technical standards [4].

Our results that RC failed to reduce pollution intensity are consistent with the findings that voluntary programs to reduce greenhouse gas pollution have failed to reduce utilities' carbon intensity and plants' pollution [18, 35]. Furthermore, our results from several specifications that RC participants *increase* their pollution intensity relative to statistically equivalent non-participants are consistent with results from the 33-50 program in the US chemical and metal industries [27] and from the ISO 14001 program in Quebec's pulp and paper industry [36]. Our study is not designed to uncover why RC led to these paradoxical results, but our results are consistent with regulators potentially reducing their scrutiny of participants in voluntary programs [14, 37] or with firms offsetting their behavior that signal green, i.e., joining RC, with negative behavior i.e., raising pollution intensity [38]. From a policy perspective, we conclude that it would be premature to rely fully on self-regulation programs which mirror RC before 2002, i.e. without third party verification or enforceable penalties for non-performance, as a tool to reduce pollution. Future work should test the role of third party certification, required in the RC program in 2002, on the effectiveness of self-regulation in reducing pollution.

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Table 1: Summary statistics: Means for various subsets of plants

	[1]	[2]	[3]	[4]		[5]	[6]	
	All Plants					Plants owned by		
			RC	Non-RC		Multi-plant	Single-plant	
	Mean	Std. Dev.	Participants	Participants		Firms	Firms	
RC participation dummy	0.37	0.48	1	0	**	0.53	0.04	**
ACC Membership in 1988 and 1989	0.36	0.47	0.89	0.05	**	0.52	0.05	**
Number of employees	162	416	264	102	**	204	76	**
Number of plants owned by firm	8.9	11.5	16.7	4.3	**	12.7	1.0	**
Single-plant firm dummy	0.33	0.47	0.03	0.50	**	0	1	**
Toxicity weighted air pollution	$3x10^7$	$6x10^8$	$7x10^7$	$1x10^7$	**	$5x10^7$	$8x10^6$	**
Toxicity weighted air pollution/employees	$4x10^5$	$4x10^6$	$6x10^5$	$2x10^5$	**	$5x10^5$	$2x10^5$	**
Toxicity-weighted HAP/TRI	0.85	0.30	0.86	0.85	*	0.84	0.87	**
Plant's neighborhood's characteristics								
% white	0.76	0.29	0.77	0.75	**	0.763	0.756	**
% low education	0.33	0.17	0.33	0.33		0.326	0.330	*
% poor	0.16	0.15	0.16	0.16		0.16	0.15	*
% urban	0.74	0.39	0.68	0.78	**	0.72	0.78	**
County Non-attainment dummy	0.64	0.48	0.61	0.66	**	0.62	0.69	**
SIC Index	4.62	26.64	7.44	2.96	**	5.51	2.77	**
Value of Shipment Index	95	12	93	95	**	95	94	
Producer Price Index	92	9	93	92	*	93	91	**
Herfindahl-Hirschman Index	675	540	730	643	**	712	599	**

Notes: Means significantly different at the 5% ** and 10% level *

Table 2: OLS regression of pollution-intensity on RC participation dummy and other covariates

	[1]		[2]		[3]		[4]	
	All plants				Plants owned by multi-plant firms			
	Exclude Full set of Covariates		Include Full set of Covariates		Exclude Full set of Covariates		Include Full set of Covariates	
RC participation dummy	0.13	**	0.24	**	0.12	**	0.22	**
	(0.02)		(0.06)		(0.02)		(0.07)	
Full set of covariates	excl		incl		excl		incl	
Year dummies	incl		incl		incl		incl	
SIC dummies	excl		incl		excl		incl	
Observations	18,850		18,850		12,705		12,705	
R-squared	0.8		0.8		0.8		0.8	
Test-statistics								
Pagan-Hall general test statistic	109	**	251	**	95	**	196	**
C statistic (orthogonality of RC participation)	6	**	8	**	3	*	6	**
C statistic (orthogonality of lagged pollution)	113	**	120	**	61	**	68	**

Notes: Statistically significant at the 5% ** and 10% level *

Table 3: Logit regression of RC-participation dummy on covariates

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
	All plants				Only Plants Owned by Multi-Plant Firms			
	Main spec	Subset of instruments			Main spec	Subset of instruments		
ACC membership in 1988	2.5 **		4.2 **	2.5 **	2.4 **	4.2 **	2.5 **	2.5 **
and 1989 dummy	(0.1)		(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
RC participation dummy	6.5 **	7.6 **		6.5 **	6.4 **		6.4 **	6.4 **
dummy (t-1)	(0.2)	(0.2)		(0.2)	(0.2)		(0.2)	(0.2)
SIC-4 % RC	6.7 **	3.0	-2.1	6.7 **	6.8 **	-1.6	6.8 **	6.9 **
	(2.2)	(2.1)	(1.3)	(2.2)	(2.2)	(1.4)	(2.2)	(2.2)
Firm's HAP/TRI				0.1	9×10^{-6}	0.6 **	0.01	
				(0.2)	(0.2)	(0.1)	(0.2)	
Firm's SIC pollution index				0.001	-2×10^{-4}	-0.002	5×10^{-4}	
				(0.003)	(0.003)	(0.002)	(0.003)	
Firms' plants' average neighborhood pressure								
% white				-0.1	-0.1	0.2	0.01	
				(0.5)	(0.5)	(0.3)	(0.5)	
% low education				-1.4 *	-1.4 *	-1.4 **		
				(0.8)	(0.8)	(0.5)		
% poverty				0.7	0.4	2.0 **	-0.6	
				(1.1)	(1.1)	(0.6)	(0.9)	
% urban				-0.5 *	-0.4	-0.9 **		
				(0.3)	(0.3)	(0.2)		
Non-attainment county				-0.3	-0.3	-0.1	-0.5 **	
dummy				(0.2)	(0.2)	(0.1)	(0.2)	

Notes: Only results for covariates that serve as instruments in the GMM regression are shown above. Results for other covariates are available upon request. Statistically significant at the 5% ** and 10 % * level

Table 4: GMM regression of pollution-intensity on RC participation dummy and other covariates & test-statistics

	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
		All plants			Only Plants Owned by Multi-Plant Firms			
	Main spec	Subset of instruments			Main spec	Subset of instruments		
RC-status	0.05	0.05	0.05	0.05	0.03	0.03	0.03	0.02
(i.e. impact of RC on pollution)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.03)	(0.04)
<u>Test - statistics</u>								
Under-ID: Kleibergen-Paap	192	179	120	194	135	92	135	134
LM rk statistics								
Weak-ID: Kleibergen-Paap	2025	1812	62	761	726	21	871	1903
Wald rk statistics								
Stock-Yogo Critical Values								
5% Relative Bias	11	13	13	19	19	19	18	11
20% Relative Bias	6	6	6	6	6	6	6	6
Hansen J statistic	0.1	0.03	0.04	7	7	7	6	0.04
p-value for Hansen J stat	1	0.9	0.9	0.6	0.6	0.5	0.6	1
Emissions equation: R-squared	0.8	0.8	0.8	0.8	0.8	0.8	0.8	0.8
Observations	18,850	18,850	18,850	18,850	12,705	12,705	12,705	12,705

Notes: Statistically significant at the 5% ** and 10 % * level

Table 5: GMM regression of pollution-intensity on RC participation dummy and other covariates

	[1]		[2]		[3]		[4]		[5]		[6]		[7]		[8]	
	All plants								Only Plants Owned by Multi-Plant Firms							
	Main spec		Subset of instruments						Main spec		Subset of instruments					
RC-status	0.05		0.05		0.05		0.05		0.03		0.03		0.03		0.02	
	(0.03)		(0.03)		(0.04)		(0.03)		(0.03)		(0.05)		(0.03)		(0.04)	
Plant's pollution	0.9	**	0.9	**	0.9	**	0.9	**	0.9	**	0.9	**	0.9	**	0.9	**
(t-1)	(0.004)		(0.004)		(0.004)		(0.004)		(0.005)		(0.005)		(0.005)		(0.005)	
Plant's HAP/TRI	-0.01		-0.01		-0.01		-0.01		0.03		0.03		0.03		0.02	
	(0.04)		(0.04)		(0.04)		(0.04)		(0.05)		(0.05)		(0.05)		(0.05)	
Plant's SIC	0.0002		0.0002		0.0002		0.0003		0.0006		0.0006		0.0006		0.0003	
pollution index	(0.0005)		(0.0005)		(0.0005)		(0.0005)		(0.0004)		(0.0004)		(0.0004)		(0.0004)	
No firm-owned	-0.003	**	-0.003	**	-0.003	**	-0.003	**	-0.003	**	-0.003	**	-0.003	**	-0.003	**
plants	(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)	
Log (firm's no.	0.005		0.005		0.005		0.006		0.008		0.008		0.008		0.006	
employees)	(0.009)		(0.009)		(0.009)		(0.008)		(0.009)		(0.009)		(0.009)		(0.009)	
Log (plant's no.	0.02	**	0.02	**	0.02	**	0.02	**	0.03	**	0.03	**	0.03	**	0.03	**
employees t-1)	(0.008)		(0.008)		(0.008)		(0.007)		(0.008)		(0.008)		(0.008)		(0.008)	
Single Plant	0.02		0.02		0.02		0.02									
dummy	(0.037)		(0.037)		(0.037)		(0.036)									

Notes: Statistically significant at the 5% ** and 10 % * level

Table 5 (continued): GMM regression of pollution-intensity on RC participation dummy and other covariates																
	[1]		[2]		[3]		[4]		[5]		[6]		[7]		[8]	
	All plants								Only Plants Owned by Multi-Plant Firms							
	Main spec		Subset of instruments						Main spec		Subset of instruments					
Plant's neighborhood characteristics																
% white	0.05		0.05		0.05		0.05		0.01		0.01		0.02		0.01	
	(0.04)		(0.04)		(0.04)		(0.04)		(0.05)		(0.05)		(0.05)		(0.05)	
% low	0.04		0.04		0.04		0.04		-0.06		-0.06		-0.06		-0.07	
education	(0.06)		(0.06)		(0.06)		(0.06)		(0.08)		(0.08)		(0.08)		(0.08)	
% poverty	0.1	**	0.1	**	0.1	**	0.1	**	0.2	**	0.2	**	0.2	**	0.2	*
	(0.07)		(0.07)		(0.07)		(0.07)		(0.10)		(0.10)		(0.10)		(0.10)	
% urban	-0.07	**	-0.07	**	-0.07	**	-0.07	**	-0.07	**	-0.07	**	-0.07	**	-0.06	**
	(0.02)		(0.02)		(0.02)		(0.02)		(0.03)		(0.03)		(0.03)		(0.03)	
Non-attainment	-0.02		-0.02		-0.02		-0.02		-0.02		-0.02		-0.03		-0.02	
county dummy	(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)		(0.02)	
Shipment	0.0003		0.0003		0.0003		0.0001		-0.0006		-0.0006		-0.0006		-0.0003	
quantity index	(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)		(0.001)	
Producer	-0.004	*	-0.004	*	-0.004	*	-0.004	*	-0.003		-0.003		-0.002		-0.003	
Price Index	(0.002)		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)		(0.002)	
Herfindahl	-6×10^{-5}		-6×10^{-5}		-6×10^{-5}		-8×10^{-5}		-4×10^{-5}		-4×10^{-5}		-3×10^{-5}		-9×10^{-7}	
index	$(-7x10^{-5})$		$(-7x10^{-5})$		$(-7x10^{-5})$		$(-7x10^{-5})$		$(-9x10^{-5})$		$(-9x10^{-5})$		$(-9x10^{-5})$		$(-9x10^{-5})$	
SiC dummies	incl		incl		incl		incl		incl		incl		incl		incl	
Year dummies	incl		incl		incl		incl		incl		incl		incl		incl	
Observations	18,850		18,850		18,850		18,850		12,705		12,705		12,705		12,705	
Notes: Statistically significant at the 5% ** and 10 % * level																

Table 6: Impacts on the probability of participation in RC from marginal increases in covariates

	All Plants							Only Plants Owned by Multi-plant Firms						
Variable	d(Pr RC) dx		Std. Err.	Variable Mean	Variable Std. Dev.	ΔPr(RC) due to 1 Std Dev Increase		d(Pr RC) dx		Std. Err.	Variable Mean	Variable Std. Dev.	ΔPr(RC) due to 1 Std Dev Increase	
†ACC membership in 1988 & 1989	0.4	**	0.02	0.4	0.5			0.5	**	0.02	0.5	0.5		
†RC participation dummy (t-1)	0.9	**	0.009	0.3	0.5			0.9	**	0.006	0.5	0.5		
SIC-4 % RC	0.8	**	0.2	0.3	0.2	0.2	**	1.7	**	0.5	0.3	0.2	0.3	**
Firm's HAP/TRI								-0.02		0.05	0.7	0.5	-0.01	
Firm's SIC pollution index								0.0004		0.001	4.9	15.3	-0.01	
Firms' plants' average neighborhood pressure														
% white								0.02		0.1	0.8	0.4	0.004	
% low education								-0.4	**	0.2	0.3	0.2	-0.06	**
% poverty								0.06		0.2	0.2	0.1	0.01	
% urban								-0.1	**	0.06	0.7	0.4	-0.04	**
Non-attainment county dummy								-0.08		0.05	0.6	0.4	-0.03	
Plant's pollution (t-1)	0.003		0.002	8.2	3.3	0.01		0.004		0.005	8.6	3.3	0.01	
Plant's HAP/TRI (t-1)	0.02		0.02	0.9	0.3	0.01		0.08	*	0.05	0.8	0.3	0.02	*
Plant's SIC pollution index	0.0005	**	0.0002	4.5	27	0.01	**	0.0009	*	0.0005	5.4	27	0.02	*
†Single-plant firm dummy	-0.04	*	0.02	0.3	0.5									
No. of firm-owned plants	0.01	**	0.001	9	12	0.2	**	0.02	**	0.002	13	12	0.2	**
Log (firm's mean employees)	0.02	**	0.01	2.9	2.4	0.07	**	0.08	**	0.01	4.3	2.4	0.2	**
Log (plant's mean employees (t-1))	0.02	**	0.01	4.1	1.3	0.02	**	0.03	**	0.01	4.3	1.3	0.04	**

Notes: The probability of RC participation with values of covariates set at the sample mean is 0.13 for all plants and 0.54 for plants owned by multi-plant firms. † Impact on participation a discrete change from 0 to 1. Statistically significant at the 5% level ** and 10% level *
Results are reported for only a subset of covariates

Table 7: GMM regression of pollution-intensity on RC participation dummy interacted with a dummy for multiple years

[1]			[2]			[3]		
Main spec.			6 year blocks			3 year blocks		
RC	0.05		RC x	0.07	*	RC x	0.09	*
	(0.03)		I(yr<='95)	(0.04)		I(yr='90-92)	(0.05)	
			RC x	0.05		RC x	0.05	
			I(yr>'95)	(0.04)		I(yr='93-95)	(0.05)	
						RC x	0.02	
						I(yr='96-98)	(0.04)	
						RC x	0.09	*
						I(yr='99-01)	(0.05)	

Notes: The RC dummy is omitted in columns 2-3. Thus, the coefficient on "RC x years dummy" provides a comparison of the pollution-intensity from RC participants and statistically equivalent non-RC participants within a time period. Statistically significant at the 5% ** and 10% * level, respectively.

Table 8: GMM regression of pollution-intensity on RC participation - Heterogenous program effects

	[1]	[2]	[3]	[4]	[5]		[6]		[7]	
RC	0.04	0.04	0.05	0.05	0.04		0.02		0.04	
	(0.05)	(0.04)	(0.03)	(0.03)	(0.04)		(0.05)		(0.03)	
RC x					-0.02	**	-0.02	**	-0.02	**
pollution (t-1)					(0.01)		(0.01)		(0.01)	
RC x	-0.0007						-0.002			
no plants	(0.003)						(0.003)			
RC x dummy for	0.2	0.2					0.2			
single-plant Firm	(0.1)	(0.1)					-0.1			
RC x firm's #		-0.001								
employees (t-1)		(0.02)								
RC x plant's #			0.02							
employees (t-1)			(0.02)							
RC x plant's #									-0.004	
years in RC									(0.008)	
RC x firm's #									0.0004	
years in RC									(0.008)	
RC x % poor in plant's				0.1						
neighborhood				(0.1)						
RC x % urban in plant's				-0.03						
neighborhood				(0.04)						

Notes: Statistically significant at the 5% ** and 10% * level, respectively.