

Essays on the Impacts of Environmental Regulation on Firm Behavior

by

Nicholas E. Powers

A dissertation submitted in partial fulfillment
of the requirements for the degree of
Doctor of Philosophy
(Business Administration)
in The University of Michigan
2010

Doctoral Committee:

Professor Thomas P. Lyon, Chair

Professor Michael R. Moore

Associate Professor Uday Rajan

Assistant Professor Jagadeesh Sivadasan

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For Jessica

ACKNOWLEDGEMENTS

This work has benefitted greatly from discussions with Thomas Lyon, Michael Moore, Uday Rajan, and Jagadeesh Sivadasan. I am grateful for the contributions of my co-authors Allen Blackman, Thomas Lyon, Urvashi Narain, and Haitao Yin. I also am appreciative of the assistance provided by Randy Becker, Clint Carter, Wayne Gray, and Maggie Levenstein in the work performed in the Michigan Research Data Center of the U.S. Census Center for Economic Studies. I am also indebted to the Centre for Science and Environment, The Erb Institute, and numerous seminar participants at several conferences, universities, and government agencies.

The camaraderie of Nathan Wilson was extremely helpful throughout the entire doctoral process, and backing from my family and other friends was invaluable. Most importantly, I thank Jessica Lin for her patience and support.

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ABSTRACT

Essays on the Impacts of Environmental Regulation on Firm Behavior

by

Nicholas E. Powers

Chair: Thomas P. Lyon

My dissertation is composed of three papers that examine the effects of environmental policies as they relate to firms' decisions. The challenge of crafting environmental policies that achieve their environmental goals without placing an unnecessary burden on the firms that drive economic growth is becoming increasingly acute as concerns over global warming and other environmental issues continue to grow. All three papers in my dissertation examine effects of relatively new approaches to environmental regulation that attempt to accomplish this task.

In the first paper, I analyze the effects of a pollution disclosure and rating program, run by a non-governmental organization in India, on the environmental performance of the country's largest pulp and paper mills. I find that this program drove significant reductions in pollution loadings among dirty plants but not among cleaner ones, and that plants located in wealthier communities were more responsive to these ratings, as were single-plant firms. These findings shed light on the conditions under which information-based regulation may prove effective in improving industrial environmental performance.

In the second paper, I provide the most rigorous empirical analysis to date of the

effectiveness of renewable portfolio standards (RPS), which are state-level policies designed to promote investment in renewable electric generating capacity. I introduce a new measure for the stringency of an RPS that explicitly accounts for several important dimensions of RPS heterogeneity. My econometric analysis of state-level panel data suggests that RPS policies have had a significant and positive effect on in-state renewable energy development.

In my third paper, I return to the topic of disclosure, though this time the program I analyze is a government-run program in the United States. In particular, I use unique establishment-level Census micro data to examine how potential entrants' entry decisions changed in response to the creation of the Toxics Release Inventory. I find that on average, counties that were found to be among the dirtiest in the country experienced a decrease in "dirty" plant births and an even larger increase in "clean" plant births. Furthermore, the magnitude of this shift is closely related to per capita income in the affected counties.

CHAPTER I

Introduction

This dissertation is composed of three distinct papers that all examine the incidence of different environmental policies representing novel approaches to environmental regulation. The economic impacts of environmental regulation have been an important research topic since the dawn of the modern environmental policy movement in the mid-20th century, as economists have attempted to answer the question of whether the fixes to the market failures that environmental policies are intended to correct have an overall positive impact. In the last three decades, however, new forms of environmental regulation, requiring new analysis and study, have emerged. Each paper in my dissertation examines one of these new approaches; I provide a short summary of each paper below.

The first paper examines a non-traditional form of environmental regulation from a non-traditional source - a non-governmental organization. Public disclosure programs that collect and disseminate information about firms' environmental performance are increasingly popular in both developed and developing countries. Yet little is known about whether they actually improve environmental performance, particularly in the latter setting. I use detailed plant-level survey data to evaluate the impact of India's Green Rating Project (GRP) on the environmental performance of the country's largest pulp and paper plants. I find that the GRP drove significant reductions

in pollution loadings among dirty plants but not among cleaner ones. This result comports with statistical and anecdotal evaluations of similar disclosure programs. I also find that plants located in wealthier communities were more responsive to GRP ratings, as were single-plant firms.

My second paper examines a regulatory policy that is more traditional in that it is administered by state governments, but most often relies on the relatively new approach of market mechanisms to achieve its environmental goals. Several states have passed renewable portfolio standard policies in order to encourage investment in renewable energy technologies, but little is known about their effectiveness. In this paper I present the most rigorous statistical analysis of these policies to date, allowing the impact of the policies to vary with the heterogeneity in their design. I find that RPS policies do in fact lead to a statistically significant increase in renewable electricity development, but that much of the increase in renewable development can be explained by other factors. I also introduce a new measure that more accurately captures the stringency of state-level RPS policies, and demonstrate how crucial the measurement of RPS stringency is for this type of policy analysis.

My final paper also deals with a U.S. context, but returns to the question of whether public disclosure of pollution is sufficient to induce polluters to alter their behavior. The Toxics Release Inventory was the first major initiative to take a disclosure-based approach to environmental regulation and has served as the model for several other disclosure-based environmental policies. Yet the magnitude of its direct impacts, whether on improvements in environmental quality or on firm efforts to abate pollution, has not been established, due to data shortcomings and other concurrent policy changes. This proposed research project will permit better identification of some of the impacts of both the Toxics Release Inventory and the Clean Air Act Amendments of 1990 on establishment-level pollution control efforts.

CHAPTER II

Does Disclosure Reduce Pollution? Evidence from India's Green Rating Project¹

2.1 Introduction

Programs that collect and disseminate information about firms' environmental performance have been characterized as the "third wave" in environmental regulation, after command-and-control and market-based approaches (Tietenberg, 1998). Two types of national public disclosure programs have emerged over the past two decades (Dasgupta et al., 2007). So-called pollutant release transfer registries simply report emissions or discharge data without using them to rate or otherwise characterize environmental performance. More than 20 countries have set up such registries.²

The second type of national public disclosure program both reports emissions or discharge data and uses them to rate plants' environmental performance. These programs are confined to developing countries and focus mostly on conventional pollutants. Examples include Indonesia's Program for Pollution Control, Evaluation,

¹The material in this chapter represents joint work with Allen Blackman, Thomas Lyon, and Urvashi Narain.

²Countries that have at least the inception of a web-accessible pollution release transfer registry include Austria, Australia, Canada, Chile, the Czech Republic, Denmark, England, France, Germany, Hungary, Italy, Japan, Mexico, the Netherlands, Norway, Scotland, South Korea, Spain, and Sweden, and the United States (Dasgupta et al., 2007; Kerret and Gray, 2007). Like the seminal U.S. Toxic Release Inventory, most focus on toxic pollutants not covered by conventional regulations.

and Rating (PROPER), which was the first such program to appear and is the best known; India's Green Rating Project; the Philippines' EcoWatch program; China's GreenWatch program; and Vietnam's Black and Green Books initiative. These programs have been touted as a means of circumventing perhaps the most daunting obstacle to pollution control in developing countries: weak environmental regulatory institutions. Public disclosure does not necessarily require an effective enforcement capability or even a well-defined set of environmental regulations. Furthermore, the costs of the administrative activities it does require – data collection and dissemination – are declining thanks to new information technologies (Dasgupta et al. (2007)). Notwithstanding the promise and growing popularity of public disclosure, we know little about whether it actually improves environmental performance, in either industrialized or developing countries. As discussed in the next section, most evaluations to date have been anecdotal, and only a few rigorous analyses have appeared.

To help fill this gap, this paper evaluates the impact of India's Green Rating Project (GRP) on the environmental performance of the country's largest pulp and paper plants. To our knowledge, it is the first rigorous analysis of the GRP's environmental impact and only the second such evaluation of a developing country public disclosure program.³ To identify the effect of the GRP, we control for other factors that drive cross-sectional and intertemporal variations in pollutant discharges using exceptionally detailed plant-level data, including both primary survey and secondary census data.

Our analysis suggests that the GRP drove significant reductions in pollution loadings among dirty plants but not among cleaner ones. This result comports with statistical and anecdotal evaluations of similar disclosure programs (Dasgupta et al., 2007; García et al., 2007, 2009). We also find that plants in wealthier communities were more responsive to GRP ratings, as were single-plant firms.

³To the best of our knowledge, the only other developing country program that has been rigorously evaluated is the PROPER program; see García et al. (2007) and García et al. (2009) for details.

The remainder of the paper is organized as follows. Section 2 reviews the literature on public disclosure and presents a conceptual framework for our empirical analysis. Section 3 provides background information on the GRP and the Indian pulp and paper industry. Section 4 describes the empirical framework and the data used in the regression analysis. Section 5 presents our econometric results, and Section 6 concludes.

2.2 Literature Review and Conceptual Framework

This section briefly reviews the empirical literature on public disclosure programs, focusing on two questions: do they improve environmental performance, and if so, how? Then, drawing on the literature, it presents a heuristic graphical model of public disclosure to underpin the econometric analysis.

2.2.1 Does Public Disclosure Improve Environmental Performance?

Only a few papers have evaluated environmental performance rating programs like the GRP, but all have concluded that the programs have generated environmental benefits. Of these papers, to our knowledge, only two – García et al. (2007) and García et al. (2009) – present a rigorous statistical analysis. Using panel data on both participants and nonparticipants, the authors test whether Indonesias PROPER drove reductions in water pollution. They find that the program in fact spurred significant emissions reductions, particularly among plants with poor compliance records. Dasgupta et al. (2007) present a largely qualitative evaluation of performance rating programs in Indonesia, the Philippines, China, and Vietnam. In each program, the authors find that a large number of plants initially rated “noncompliant” improved to “compliant” over time (although plants rated “flagrant violators” and “compliant” tended to remain in these categories). Without additional statistical analysis, however, one cannot determine whether public disclosure or exogenous changes in

technological, regulatory, or market conditions were responsible for the apparent increase in compliance. Wang et al. (2004) provide a more detailed but still primarily anecdotal evaluation of the Chinese performance ratings program that suggests it succeeded in improving environmental performance.

Several recent papers evaluate public disclosure initiatives other than environmental performance rating programs and also find significant impacts on environmental performance. Bennear and Olmstead (2008) find that a 1996 amendment to the U.S. Safe Drinking Water Act, mandating that community drinking water systems publicly report regulatory violations, reduced the incidence of subsequent violations. Similarly, Delmas et al. (2007) find that regulations requiring U.S. electric utilities to mail bill inserts to consumers reporting the extent of their reliance on fossil fuels led to a significant decrease in fossil fuel use. And Foulon et al. (2002) find that a policy of publicly disclosing the identity of plants that are non-compliant or “of concern” spurred emissions reductions in a sample of pulp and paper plants in British Columbia.

Finally, a number of papers have examined the U.S. Toxic Release Inventory (Konar and Cohen, 1996; Khanna and Anton, 2002; Bui, 2005; Koehler and Spengler, 2007). Since the program began in 1986, total reported releases of the toxics it covers have fallen by at least 45%. However, it is not clear that public disclosure has been responsible for this decline. Data on toxic releases are not available for the period preceding the program, or for plants that fall outside the program, and as a result the usual means of estimating releases absent the program are not available (Bennear and Olmstead, 2008).⁴

⁴Moreover, several papers propose alternative explanations for the observed reductions in toxic releases, including the imposition of more stringent conventional regulation (Bui, 2005); plants practice of substituting unlisted toxics for listed ones (Greenstone, 2003); and simple underreporting of emissions (Koehler and Spengler, 2007).

2.2.2 How might public disclosure improve environmental performance?

Tietenberg (1998) identifies seven channels through which public disclosure may motivate improved environmental performance. To simplify the exposition, we group these channels into four broad categories.

- *Output market pressures.* Disclosure may affect the demand for firms' goods.
- *Input market pressures.* Disclosure may affect the demand for firms' securities and the firms' ability to hire and retain employees.
- *Judicial pressures.* Disclosure may encourage private citizens to initiate tort law actions against polluters, motivate private suits to force firms to undertake abatement, and give rise to judicial actions in countries whose constitutions guarantee citizens the right to a healthy environment (as in India).
- *Regulatory pressures.* Disclosure may build support for new pollution control legislation or better enforcement of existing legislation.

Based on the literature discussed below, we add two more mechanisms.

- *Community pressures.* Disclosure may enhance pressures that community groups and nongovernmental organizations place on polluters to cut their discharges.
- *Managerial information.* Disclosure may provide new information to managers about their plants' discharges and options for reducing them.

Empirical analysis aimed at determining which of those six mechanisms explain how various public disclosure programs have their effect is limited. To our knowledge, only a few empirical papers have examined this issue as it relates to performance rating programs, like the GRP and PROPER. Gupta and Goldar (2005) test whether GRP ratings affect the stock prices of Indian companies in the pulp and paper, chlor alkali, and automobile sectors (three of the four sectors rated by the GRP, the other

being cement, which was rated after 2005). They find that poor GRP ratings led to significant negative abnormal returns. These results suggest that GRP may have an important effect on environmental performance through capital markets. Blackman et al. (2004)'s survey of managers of plants participating in PROPER generated data suggesting that an important means by which the program spurs abatement is improving managerial information. García et al. (2009) identify characteristics of Indonesian plants that were more responsive to PROPER ratings and find that foreign-owned plants, those in more densely populated areas, and those with low initial ratings were more responsive, all other things equal.

The empirical literature on the workings of public disclosure initiatives other than performance rating programs has focused mainly on capital markets. Although this research clearly shows that public disclosure can affect stock prices (Dasgupta et al., 2001, 2006; Laplante and Lanoie, 1994), it does not establish that the changes in stock prices have, in turn, affected firms pollution control activities. However, Konar and Cohen (1996) and Khanna et al. (1998) find evidence suggesting that this can occur.

Beyond the studies cited thus far, empirical research on how public disclosure *per se* has an impact is quite limited. However, the literatures on “voluntary regulation” and “informal regulation” are relevant. The literature on voluntary regulation examines pressure to over-comply with mandatory regulations generated by regulators, markets, and courts (see Lyon and Maxwell 2002 and Khanna 2001 for reviews). For example, Segerson and Miceli (1998), Maxwell et al. (2000), and Glachant (2007) present analytical models in which firms voluntarily over-comply to preempt more stringent mandatory regulation; some empirical research supports this approach (Videras and Alberini, 2000; Innes and Sam, 2008). Arora and Gangopadhyay (1995) hypothesize that firms over-comply with environmental regulations to attract “green” consumers. Some empirical evidence also supports this proposition. Finally, empir-

ical research by Videras and Alberini (2000), Innes and Sam (2008), and Vidovic and Khanna (2007) suggests that judicial pressure can also drive voluntary over-compliance.

The literature on informal regulation focuses on pressures to abate generated by private sector agents in developing countries where state regulation is weak or effectively nonexistent (see World Bank 1999 for a review). For example, Pargal and Wheeler (1996) examine the environmental performance of plants in Indonesia at a time when regulatory enforcement was negligible (and before the PROPER program was initiated); they find that emissions were lower in communities with higher per capita income and higher levels of education, implying that such communities effectively pressure plants to abate (see also Blackman and Bannister 1998).

2.2.3 Conceptual framework

This section presents a heuristic graphical model of public disclosure to underpin the econometric analysis (see Appendix for an analytical version of the model). It draws upon the standard representation of a plants abatement decision in the environmental economics literature (see, e.g., World Bank 1999). We assume marginal abatement costs (MAC) are increasing in abatement while (private) marginal abatement benefits (MAB) are decreasing in abatement (Figure 2-1). The plant chooses the level of abatement, α^* , such that MAC equals MAB .

Drawing on the literature surveyed in Section 2, we define six channels through which public disclosure might affect the firm's abatement decision. The first five channels have to do with the costs that can be imposed on dirty firms by green consumers (g), input (capital and labor) markets (k), courts (j), regulatory authorities (r), and communities (c). The sixth channel is related to the costs of pollution abatement arising from the plant's need to acquire information about abatement technologies and its own pollutant discharge (t). We use Figure 1 to illustrate how each chan-

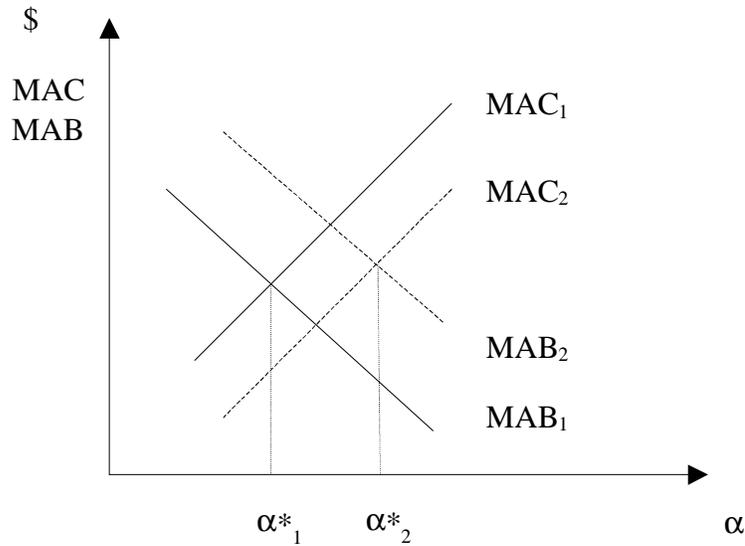


Figure 2.1: Marginal abatement cost (MAC) and marginal abatement benefit (MAB) schedules; optimal abatement level, α^*

nel might affect the firm's abatement decision. First, public disclosure could either reduce or enhance consumers' demand for the firm's output, depending on whether the firm is relatively clean or dirty (g). Either effect, in turn, implies the marginal benefit to the firm of cutting discharge will be greater regardless of the actual level of abatement. For a clean firm, disclosure enhances its demand, but for a dirty firm, disclosure decreases its demand. Graphically, in either case, the MAB curve shifts up and in equilibrium the firm chooses a higher level of abatement. Similarly, depending on whether the plant is dirty or clean, public disclosure could either raise or lower costs imposed by regulators (r), input markets (k), communities (c), and courts (j). Once again, regardless of whether the plant is clean or dirty, each of these effects shifts the MAB curve up and results in a higher equilibrium level of abatement. Finally, public disclosure could reduce the cost to firms of acquiring information about abatement (t), lowering the marginal cost of abatement at every level of abatement. In this case, graphically, the MAC curve shifts down, and the end result is a higher level of abatement.

2.3 Background

2.3.1 Green Rating Project

The Centre for Science and Environment (CSE), one of India's best-known and most influential environmental nongovernmental organizations, began work on the GRP in 1997. According to CSE background materials, the program was urgently needed to shore up India's weak environmental regulatory institutions and was inspired by the Council on Economic Priorities, a now-defunct U.S. nongovernmental organization that provided investors with annual ratings of the environmental performance of U.S. companies.

To date, the GRP has rated the environmental performance of large plants in four pollution-intensive industrial sectors: pulp and paper, chlor-alkali, cement, and automobiles. Plants in the pulp and paper sector have been rated twice (once in 1999 and again in 2004); plants in the other three sectors have been rated just once. In each rating, plants are assigned a numerical score from 0 to 100 and are awarded symbolic leaves depending on their score: five leaves for scores of 75 and above, four for 50-74, three for 35-49, two for 25-34, one for 15-24, and none for 14 and below. The GRP scores are based on an evaluation of the plant's life-cycle environmental impacts, from the sourcing and processing of raw materials to the manufacture, use, and disposal of products. The exceptionally detailed data needed to conduct this cradle-to-grave analysis are collected from questionnaires administered to participating plants, along with secondary data provided by local environmental regulatory institutions and other sources. Both the questionnaires used to collect the data and the methodology used to analyze them were designed by a panel of leading technical experts in each rated sector. In addition, to ensure objectivity and transparency, the entire GRP program is supervised by a panel comprising high-level representatives of industry, government, the judiciary, academia, and nongovernmental organizations. Lastly, self-reported

data from the firms are carefully checked by GRP inspectors and compared with the secondary data.

In addition to informing the public about plants environmental performance, the GRP also informs plants about their pollution and pollution abatement options. The program uses the primary and secondary data it collects to construct a detailed environmental profile of each plant and sends it to the facility for review before releasing the ratings to the public. The program also publishes specific recommendations for improving environmental performance in each sector. Finally, the ratings are released at a high-profile public event by leading public figures. For example, some ratings were released by the late Dr. K.R. Narayanan, India's former president.

2.3.2 Pulp and Paper Sector

Pulp and paper is a notoriously dirty industry worldwide. The environmental performance of mills in North America and Scandinavia, which tend to be much larger and more modern than their developing-country counterparts, has improved considerably over the past few decades. Indian mills lag behind, with per unit measures of industrial pollution 5 to 10 times higher than those of Western plants (Centre for Science and Environment, 2004).

Making paper involves four main steps, all of which generate water pollution: raw material processing, pulping, bleaching, and papermaking.⁵ To reduce pollution loadings, plants undertake both pollution control and prevention. All the plants rated by the GRP have wastewater treatment plants. In addition, to prevent pollution, mills have eliminated particularly dirty inputs, adopted good housekeeping measures, improved chemical recovery systems, and modified the pulping process.

As previously mentioned, the pulp and paper sector is the only sector that has been rated twice. As a result, survey data on environmental impact indicators and

⁵See Centre for Science and Environment (2004) or Schumacher and Sathaye (1999) for more detail.

other relevant variables are available for several years before and after the first rating. This allows us to construct a counterfactual – that is, an estimate of what pollution would have been absent the program – needed to identify the impact of the GRP. Because the necessary data on environmental indicators and firm behavior are not available after the second rating, we can analyze the effectiveness of only the first rating.

The first rating included all 28 plants in the pulp and paper industry with a production capacity exceeding 100 tons per day in fiscal year 1998⁶ (India’s fiscal year ends on March 31; all years referred to in this paper and accompanying figures are fiscal years). Collectively, these plants were responsible for 59% of pulp and paper production in India. They were contacted for the survey in January 1998, toward the end of fiscal year 1998, and asked to provide data (that they normally track and record anyway) for 1996 through 1998. The ratings for these plants were released by Dr. Manmohan Singh, India’s current prime minister, on July 18, 1999. All major Indian daily newspapers covered the event. Of these 28 plants, 22 were rated a second time in 2004 and responded to questionnaires for 1999-2003.⁷ Again, the data were reported retroactively. For the remainder of the paper we will refer to years 1996 to 1998 as pre-disclosure and 1999 to 2003 as post-disclosure.

2.3.3 Trends in pollution indicators

Average annual discharge data for 1996 to 2003, as shown in Figures 2.2 and 2.3, suggest that the environmental performance of particularly dirty plants – those that received one leaf in the 1999 rating – did in fact improve significantly during 1999, the year of the first GRP rating. Figure 2.2 shows trends in chemical oxygen demand (COD), and Figure 2.3 shows trends in total suspended solids (TSS) – two common

⁶By way of comparison, the average plant in the United States has a capacity of nearly 600 tons/day (CSE 2004). GRP’s size criteria excluded more than 500 smaller plants.

⁷Of the six plants that were rated in the first period but not the second, five were permanently closed and one was temporarily closed after the first rating.

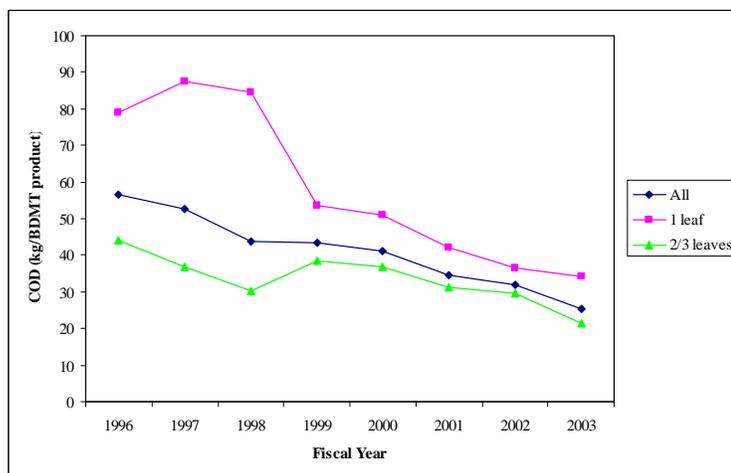


Figure 2.2: Annual unweighted average discharges of chemical oxygen demand (COD) from 22 pulp and paper plants participating in India's Green Rating Project, by performance rating

measures of water pollution.⁸ Both pollutants declined dramatically after 1998 for plants that received a one-leaf rating, but not for plants with higher ratings.

The fact that the most dramatic reductions in discharges took place in 1999, the year that the ratings were released, suggests that the rating prompted improvements in the environmental performance of poorly performing plants. These reductions, however, could have been caused by contemporaneous changes in technological, market, and regulatory conditions that influence the plants pollution loadings. For example, they could have been caused by reductions in the relative price of cleaner inputs that happened to coincide with the release of the GRP ratings. To identify the impact of the GRP, we develop an econometric model that can control for such confounding factors.

⁸COD is a measure of the amount of oxygen needed to fully oxidize the organic compounds in water to carbon dioxide; it is a linear function of the chemical composition of the organic pollutants in a sample of water. TSS is even more straightforward: it is the dry weight of particles suspended in a sample of water and, like COD, is typically expressed in mg/L. To properly scale for water use, we convert both measures and express them as kg/bdmt of product. This is done by multiplying first by a constant, then by the amount of water used to produce a bone dry metric ton of product.

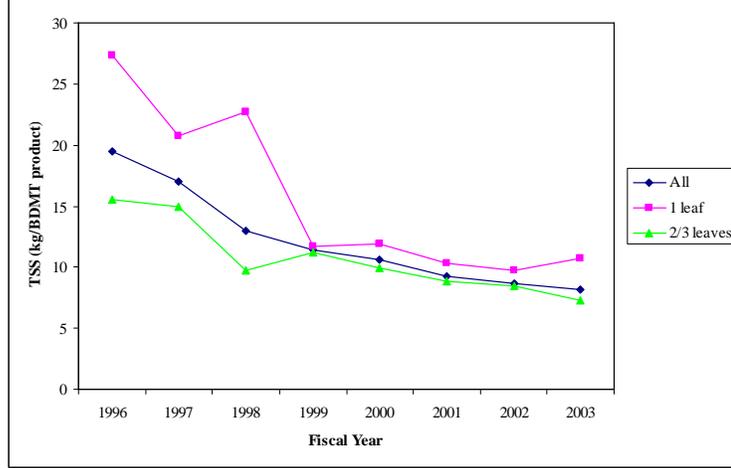


Figure 2.3: Annual unweighted average discharges of total suspended solids (TSS) from 22 pulp and paper plants participating in India’s Green Rating Project, by performance rating

2.4 Empirical framework

The first goal of our empirical analysis is to determine whether the drops in COD and TSS levels depicted in Figures 2 and 3 were caused by the GRP and not by confounding factors. In addition, if we find that the GRP was, in fact, responsible, we seek to identify the channels through which it had this effect.

2.4.1 Econometric model

2.4.1.1 GRPs impact

To isolate the impact of the GRP, we run separate fixed-effects ordinary least squares (OLS) panel-data models for COD and TSS of the form

$$y_{it} = \alpha_i + \beta_1 TREND + \beta_2 POSTGRP_t + \beta_3 TREND * ONELEAF_i + \beta_4 POSTGRP_t * ONELEAF_i + \beta_5 X_{it} + u_{it} \quad (2.1)$$

where a_i is the plant fixed effect, $TREND$ is a linear time trend, $POSTGRP_t$ is a dummy variable that takes the value one for the post-disclosure period (1999-2003)

and zero otherwise, $ONELEAF_i$ is a dummy variable for plants that received a one-leaf rating, and X_{it} is a vector of confounding factors including plant characteristics, input prices, and measures of regulatory pressure (Table 1).⁹ $TREND$ allows us to control for exogenous improvements in technology that could affect pollution levels prior to and after the information disclosure.¹⁰ $TREND * ONELEAF$ allows exogenous improvements in technology (both before and after the disclosure) to differ between dirtier plants and relatively clean plants. $POSTGRP_t$ is included to capture the impact of the ratings on firm behavior. Finally, $POSTGRP_t * ONELEAF$ allows the effect of $POSTGRP_t$ to differ between dirty and clean plants.¹¹ To allow for heteroskedasticity across plants and arbitrary serial correlation within each plant, we report standard errors that are clustered by plant (Wooldridge, 2002).¹²

⁹Although we expect the error terms in the COD and TSS regressions to be correlated, we do not use a seemingly unrelated regression (SUR) approach because there is no reason, a priori, to omit any explanatory variables from either of the two regressions. When the same explanatory variables appear in both regressions, SUR is equivalent to OLS (Greene, 2002) and yields no efficiency gain. The only advantage of SOLS in this case is that it allows for joint hypothesis testing across the two regressions. The disadvantage is that SOLS routines in standard statistical packages do not allow for clustered standard errors. SOLS results (available from the authors upon request) comport with the OLS results presented in Tables 2 and 3: although the individual coefficients of interest are not always significant, they have the same sign, and testing the joint significance of these coefficients in both equations rejects the null hypothesis that they are jointly zero.

¹⁰The choice of a linear trend term, as opposed to other functional forms, is based on preliminary regressions using pre-disclosure data. In these regressions, available from the authors upon request, the addition of a quadratic term adds little explanatory power.

¹¹We also experimented with the possibility that the disclosure program induced a persistent increase in the rate of environmental progress (which would be consistent with a change in the slope of the trend term) rather than in a one-time shift of the equilibrium path (consistent with a one-time decrease in the intercept term). The most obvious way to identify such an effect would be to include an additional trend term that takes only positive values after disclosure. However, given the sample size and a high degree of multi-collinearity between the post-disclosure trend term and the original trend term, this renders identification difficult. Furthermore, regression results (available from the authors upon request) suggest that the one-time drop is more consistent with the data than is a change in the trend. Accordingly, we proceed without the post-disclosure trend variable.

¹²Although the inclusion of fixed plant effects allows for heterogeneity in the form of a mean plant-specific unobservable component, we still need to allow the variance to differ across plants. Also, although it is reasonable to assume independence of the error terms across plants, we cannot, a priori, rule out persistence in the error term within a plant.

Table 2.1: Variables in econometric analysis and descriptive statistics

Variable	Description	Mean	S.D.	Min.	Max.
<i>COD</i>	Natural log of chemical oxygen demand (kg/bdmt)	3.37	0.9	0.04	5.43
<i>TSS</i>	Natural log of total suspended solids (kg/bdmt)	2.1	1.07	-2.74	4.08
<i>POSTGRP</i>	1999 or later (0/1)	0.72	0.45	0	1
<i>ONELEAF</i>	Received one leaf in 1999 grp rating (0/1)	0.31	0.47	0	1
<i>TREND</i>	Linear time trend term (1996 = 1, 1997 = 2, 2003 = 8)	4.74	2.23	0	8
<i>SCALE</i>	Natural log of output (kg/bdmt)	11.19	0.55	9.84	12.21
<i>FINALGOOD</i>	Percentage of output that is final (vs. intermediate) product	0.62	0.35	0	1
<i>PCTBAMBOO</i>	Percentage of fiber inputs from bamboo	0.29	0.34	0	1
<i>PCTGRASSES</i>	Percentage of fiber inputs from grasses	0.02	0.08	0	0.56
<i>PCTRECYCL</i>	Percentage of fiber inputs from recycled materials	0.12	0.22	0	1
<i>PCTPULP</i>	Percentage of fiber inputs from market pulp	0.1	0.22	0	1
<i>FORSHARE</i>	Percentage of sales derived from exports	0.05	0.05	0	0.2
<i>PRICE_WAGES</i>	Inflation-adjusted national avg. daily wages for nonagricultural unskilled workers	0.35	0.02	0.32	0.39
<i>PRICE_CL</i>	Inflation-adjusted national avg. price of chlorine	1.01	0.12	0.83	1.25
<i>PRICE_NAOH</i>	Inflation-adjusted national avg. price of sodium hydroxide	0.83	0.13	0.68	1.08
<i>PRICE_WOOD</i>	Inflation-adjusted national avg. price of wood	1.17	0.15	0.96	1.41
<i>PERMIT</i>	Water effluent permit (0/1)	0.87	0.34	0	1
<i>EFFRIVER</i>	Percentage effluents discharged into a river (vs. land, etc.)	0.68	0.42	0	1
<i>WEALTH</i>	Percentage households in subdistrict that own moped	0.41	0.2	0.12	0.79
<i>SINGLE</i>	Stand alone plant (0/1)	0.45	0.51	0	1

2.4.1.2 GRP channels

To analyze the channels through which the GRP potentially affects firm behavior, we run separate models for *COD* and *TSS* of the form

$$\begin{aligned}
y_{it} = & \alpha_i + \beta_1 TREND + \beta_2 POSTGRP_t + \beta_3 TREND * ONELEAF_i + \\
& \beta_4 POSTGRP_t * ONELEAF_i + \beta_5 X_{it} + \beta_6 TREND * Z_i + \\
& \beta_7 POSTGRP_t * Z_i + u_{it}
\end{aligned} \tag{2.2}$$

where Z_i is a vector of time-invariant plant and community variables, such as whether the plant is part of a conglomerate and the level of community wealth. This specification allows us to determine whether plants with certain characteristics and in certain communities responded differently to the program. This in turn sheds light on the channels through which GRP has an effect. Note that because these community and plant variables are time-invariant, we cannot identify their effect on pollution intensity independent of the plant fixed effect. This does not pose a major problem, however, since unlike Pargal and Wheeler (1996), we are not interested in the coefficients on these variables per se. Rather, we are interested in determining whether

these characteristics have any additional effect once the disclosure program changes the institutional landscape.

2.4.2 Data

As discussed above, most of our data come from GRP surveys. In addition, we used data from the 2001 Indian census to construct proxies for several community characteristics; data from Prowess, an on-line business database, to construct company-level (as opposed to plant-level) variables and a regional energy price index; and finally, data from Indiastat, an on-line database of Indian economic statistics, to construct price indices for a number of inputs.¹³ The independent and dependent variables used in the regressions are described below. See Table 1 for summary statistics.

2.4.2.1 Environmental performance

Because all 22 plants in the sample have effluent treatment plants, *COD* and *TSS* measurements reflect posttreatment or end-of-pipe quantities. In India, as in most countries, regulatory standards for these pollutants are specified in milligrams per liter of effluent. In India, however, where water is inexpensive, the use of this metric creates an incentive for plants to dilute their liquid discharges. To control for this effect, we measure *COD* and *TSS* in kilograms (kg) per bone-dry metric ton (bdmt) of pulp and paper product. Our resulting measure, kg/bdmt, gives water pollution per unit of product produced. Finally, because we are interested in the responses of both clean and dirty plants, we use logged values of both dependent variables so that our coefficient estimates measure the relative change in the dependent variable for a given absolute change in the values of the respective explanatory variables.

¹³For information on Prowess, see <http://www.cmie.com/database/?service=database-products.htm>, and for information on Indiastat, see <http://www.indiastat.com>.

2.4.2.2 Confounding factors

We use a broad set of regressors to control for factors other than public disclosure that affect pollution loadings, including variables related to plant characteristics, input prices, and interactions with regulators. Unless otherwise indicated, these data were derived from GRP survey data. Among the plant characteristics, *SCALE* is the log of the amount of final product produced by a given plant in a given year (measured in bone dry metric tons because moisture content can vary between different classes of products). *FINALGOOD* is the proportion of a plants output that represents final products (such as writing and tissue paper) used directly by consumers. This variable is meant to proxy for consumer pressure for environmental quality. *PCTBAMBOO*, *PCTGRASSES*, *PCTRECYCL*, and *PCTPULP* measure the proportion of fiber inputs derived from bamboo, grasses, recyclables, and market pulp, respectively (wood is the omitted category, and agroresidues are also dropped, because these are collinear with plant fixed effects). *FORSHARE* is the share of sales derived from exports, a firm-level variable derived from the Prowess database. This variable is meant to capture pressure for improved environmental performance generated by foreign investors and consumers.

Among the input price variables, *PRICE_NAOH* and *PRICE_CL* are inflation-adjusted country-wide price indices for sodium hydroxide and chlorine, the major chemicals used in the pulping and bleaching processes. *PRICE_WOOD* is an inflation-adjusted national price index for wood. This serves as a proxy for fiber input prices.¹⁴ Finally, *PRICE_WAGES* is an inflation-adjusted national price index for average daily wage rates for nonagricultural, unskilled workers.

¹⁴We were unable to find sufficient data to construct similar indices for any of the other fiber inputs bamboo, grasses, agro-residues, recycled paper, and market pulp. However, these prices will generally be related to wood prices in India where the fiber inputs are substitutable Centre for Science and Environment (2004). Also we do not include a variable measuring water costs because the price industrial users pay for water is minute and there was no variation over time in the water prices for period in question.

Two variables are used to proxy for changes in regulatory pressure. *PERMIT* is a dummy variable that takes the value one if the plant has been granted a water effluent permit by its state pollution control board and zero otherwise. *EFFRIVER* is the percentage of the plant’s effluent discharged into a river (versus on land or in the sea) in a given year. Effluent discharged into a river must, by law, be cleaner, so it is important to control for shifts in the discharge destinations of effluents.

2.4.2.3 Channels

We use two variables to establish possible channels of influence, *WEALTH* and *SINGLE*. We experimented with several other variables that could conceivably be related to the level of plant response.¹⁵ However, to keep the exposition manageable, we present (in Section 5.2) only those models in which interaction terms are significant. *WEALTH* is the percentage of households in the plants subdistrict (similar to a U.S. county) that own mopeds.¹⁶ *SINGLE* is a dummy variable that takes the value one if the plant is the unique establishment owned by the company, and zero otherwise. This variable serves as a proxy for organizational differences that could affect the way different plants respond to the GRP ratings. Any plant for which *SINGLE* equals zero is either part of a larger pulp and paper company or part of a diversified conglomerate.

¹⁵These include *LITERACY*, the percentage of the population that is literate in the municipality where the plant is located; *CASTE*, the percentage of the municipal population that belongs to a scheduled caste or scheduled tribe; *AGLABOR*, the proportion of the workers in the municipality who are either cultivators or agricultural laborers; *URBANPCT*, the proportion of the population in the subdistrict who live in municipalities that are classified as urban; and *COMPTOWN*, the percentage of nonagricultural laborers in a subdistrict who work for the plant. We also investigated whether interaction terms involving variables that proxy for other channels were significant. These include *ENFWATER*, a dummy variable that equals 1 if the plant had been fined or faced some other enforcement action from the regional pollution control board for water pollution prior to disclosure; *COMWATER*, a dummy variable that equals 1 if the plant had been the subject of registered complaints about water pollution prior to disclosure; and *GOV*, a dummy variable that equals 1 for government-owned plants.

¹⁶The census data provide alternative measures of wealth, all involving the percentage of households owning a particular asset (or employing banking services). These measures are all highly correlated, and our regression results using other wealth measure are qualitatively identical.

2.5 Results

2.5.1 GRPs Impact

As previously noted, Figures 2.2 and 2.3 suggest not only that *COD* and *TSS* levels are lower after disclosure but also that reductions were greatest among the worst-performing plants. The first objective of our empirical analysis is simply to test whether these results are statistically significant. Two specifications for each dependent variable are presented in Table 2.2; Model 1 (for *COD*) and Model 2 (for *TSS*) include only regressors related to the disclosure program: *TREND*, *POSTGRP*, and *ONELEAF*, along with interaction terms for *ONELEAF*. Models 3 and 4 include all time-varying covariates as well.

In Models 2, 3, and 4, the *POSTGRP* dummy is not significant. In Model 1 it is positive and significant, although this significance disappears when all covariates are included in Model 3. We conclude that the 1999 GRP rating did not have a significant pollution-reducing impact on the average plant, including both good and poor environmental performers.

The interaction terms in Table 2.2 – *ONELEAF * TREND* and *ONELEAF * POSTGRP* – allow us to test a second hypothesis suggested by Figures 2.2 and 2.3: following the first rating, one-leaf plants improved their environmental performance more than two- and three-leaf plants. The negative and statistically significant coefficients on *ONELEAF * POSTGRP* in all four models in Table 2.2 suggest that this hypothesis is correct.

The important question that follows from this pair of findings is whether the net effect of GRP on one-leaf plants was significant. A simple Wald test of the sum of the coefficients on *POSTGRP* and *ONELEAF * POSTGRP* allows us to test this hypothesis. The results, reported in the bottom row of Table 2.2, suggest that GRP did drive reductions in *TSS* by one-leaf plants. The net effect for *COD* was not

Table 2.2: OLS regression results: Green Ratings Project impact

Model number	1	2	3	4
Dependent variable	<i>COD</i>	<i>TSS</i>	<i>COD</i>	<i>TSS</i>
<i>TREND</i>	-0.136** [0.025]	-0.12** [0.026]	-0.124** [0.039]	-0.154** [0.032]
<i>POSTGRP</i>	0.308** [0.107]	0.021 [0.109]	0.201 [0.129]	0.087 [0.111]
<i>ONELEAF * TREND</i>	0.043 [0.038]	0.064 [0.043]	0.014 [0.033]	0.066 [0.046]
<i>ONELEAF * POSTGRP</i>	-0.446** [0.142]	-0.445** [0.154]	-0.411* [0.185]	-0.527* [0.215]
<i>SCALE</i>			0.051 [0.229]	-0.274+ [0.146]
<i>FINALGOOD</i>			-0.453 [0.296]	-0.242 [0.260]
<i>PCTBAMBOO</i>			-0.529 [0.357]	-0.336 [0.344]
<i>PCTGRASSES</i>			0.545 [1.247]	-0.906 [1.377]
<i>PCTRECYCL</i>			-0.492 [0.511]	-0.927* [0.369]
<i>PCTPULP</i>			-2.081 [1.524]	0.736 [0.911]
<i>PRICE_WAGES</i>			1.648 [1.262]	4.033* [1.769]
<i>FORSHARE</i>			-0.473 [0.639]	-2.252** [0.727]
<i>PRICE_NAOH</i>			0.028 [0.527]	-0.874* [0.361]
<i>PRICE_CL</i>			-0.191 [0.220]	-0.489+ [0.238]
<i>PERMIT</i>			-0.061 [0.088]	-0.133+ [0.077]
<i>EFFRIVER</i>			-4.399** [1.444]	-4.551** [0.790]
<i>PRICE_WOOD</i>			-0.186 [0.503]	-1.093** [0.363]
Constant	3.85** [0.066]	2.695** [0.080]	6.89+ [3.321]	10.577** [1.981]
Observations	155	153	154	152
R-squared	0.49	0.58	0.56	0.64
Wald statistic	-1.46	-3.9	-1.1	-2.09

+ significant at 10%; * significant at 5%; ** significant at 1%

Clustered standard errors in brackets; all regressions include plant fixed effects

significant, however.

In all regressions *TREND* is negative and significant, suggesting that exogenous technological improvements in the pulp and paper industry, independent of the GRP, are also responsible for declines in pollution loads. The results for the other covariates unrelated to the GRP rating shown in columns 3 and 4 are largely consistent with stylized facts about the determinants of water pollution in the pulp and paper industry. In Model 4 (for *TSS*), *SCALE*, *PCTRECYCL*, *FORSHARE*, *PRICE_NAOH*, *PRICE_CL*, *PERMIT*, *EFFRIVER*, and *PRICE_WOOD* are negative and significant, and *PRICE_WAGES* is positive and significant. In Model 3 (for *COD*), *EFFRIVER* is negative and significant. These results comport with the conventional wisdom that pollution abatement entails economies of scale (*SCALE*); pollution loadings are decreasing in the share of non-wood inputs (*PCTRECYCL*); plants that export are subject to more pressure to improve their environmental performance (*FORSHARE*); pollution loadings are decreasing in the price of polluting inputs (*PRICE_NAOH*, *PRICE_CL*, and *PRICE_WOOD*); regulatory pressure spurs abatement (*EFFRIVER* and *PERMIT*); and labor and pollution abatement are substitutes in production (*PRICE_WAGES*). It is encouraging that coefficients on these regressors have the same sign, if not significance, in Models 3 and 4.

To assess the economic significance of the estimates, we analyze the effect of disclosure on a hypothetical plant that received one leaf in the first rating and had mean values for all other covariates, using the estimation results from Models 3 and 4. With GRP disclosure, such a plant's *COD* discharges would decrease by 63% between 1996 and 2003. However, absent disclosure, the plant's *COD* discharges would have decreased by only 54%. The effect of the disclosure program is stronger when we focus on *TSS*. The plant's emissions would decrease 65% with the disclosure program but only 46% without it.

Finally, it is important to note that of the six pulp and paper plants that partici-

pated only in the first round of the GRP, five – those ranked 5th, 17th, and 24th-26th of the 28 plants evaluated in the first round went out of business before the second round was initiated. In at least two cases, environmental protests were an important reason.¹⁷ Although we do not have enough observations to formally model these closures (using a Heckman selection approach), they suggest that, if anything, our econometric results probably understate the impact of the 1999 ratings.

2.5.2 GRP channels

The regressions presented in Table 2.3 explore whether certain types of plants were more responsive to GRP disclosure. We use an identification strategy similar to that employed to test whether one-leaf plants were more responsive: we create variables that interact various time-invariant plant or community characteristics with *POSTGRP*.¹⁸ In addition, we control for the possibility that these characteristics affect the slope of the trend term using a second set of variables that interact the time-invariant plant or community characteristics with *TREND*. Instead of including all interaction terms in a single model, we include pairs of interaction terms corresponding to a single characteristic (e.g., *TREND * WEALTH* and *POSTGRP * WEALTH*) in separate models.

There are three reasons: several of the interaction variables formed are highly collinear (in part because all take a value of zero for the three pre-disclosure years); including several sets of these interaction variables simultaneously poses a degrees-of-freedom problem; and we are more interested in identifying channels by which disclosure has an effect than in identifying the strongest among several closely related channels. To make the exposition manageable, we present only those models in which

¹⁷Personal communication, Monali Zeya Hazra, Center for Science and the Environment, March 13, 2007.

¹⁸Some of the characteristics we use to construct these interaction terms, such as *EFFRIVER*, display some minor variation over time. For these, we calculate the pre-disclosure average and interact it with the post-disclosure dummy to create the corresponding interaction term.

Table 2.3: OLS regression results: Green Ratings Project channels

Model number	5	6	7	8	9	10	11	12
<i>TREND</i>	COD -0.104* [0.044]	TSS -0.146** [0.029]	COD -0.13** [0.034]	TSS -0.162** [0.031]	COD -0.102* [0.048]	TSS -0.162** [0.039]	COD -0.128** [0.038]	TSS -0.17** [0.035]
<i>POSTGRP</i>	0.214 [0.205]	0.384* [0.156]	0.21 [0.164]	0.15 [0.136]	0.252 [0.228]	0.471* [0.169]	0.233 [0.164]	0.192 [0.129]
<i>ONELEAF * TREND</i>					0.014 [0.036]	0.064 [0.046]	0.007 [0.037]	0.042 [0.035]
<i>ONELEAF * POSTGRP</i>					-0.379* [0.182]	-0.44* [0.176]	-0.349 [0.211]	-0.329 [0.211]
<i>SCALE</i>	0.067 [0.249]	-0.336+ [0.173]	0.02 [0.263]	-0.343+ [0.186]	0.03 [0.234]	-0.383* [0.157]	0.035 [0.269]	-0.35+ [0.189]
<i>FINALGOOD</i>	-0.22 [0.334]	0.013 [0.240]	-0.362 [0.354]	-0.224 [0.263]	-0.39 [0.304]	-0.105 [0.223]	-0.475 [0.300]	-0.286 [0.239]
<i>PCTBAMBOO</i>	-0.281 [0.399]	-0.055 [0.319]	-0.462 [0.393]	-0.364 [0.363]	-0.405 [0.354]	-0.175 [0.299]	-0.551 [0.354]	-0.416 [0.351]
<i>PCTGRASSES</i>	0.653 [1.115]	-0.249 [1.331]	0.262 [1.182]	-1.048 [1.363]	0.964 [1.296]	-0.408 [1.388]	0.53 [1.247]	-1.054 [1.386]
<i>PCTRECYCL</i>	-0.292 [0.504]	-0.291 [0.369]	-0.603 [0.484]	-0.977* [0.369]	-0.248 [0.581]	-0.251 [0.343]	-0.501 [0.539]	-0.916* [0.333]
<i>PCTPULP</i>	-1.903 [1.334]	1.461 [3.568*]	-2.1 [1.620]	0.799 [3.948*]	-1.846 [1.632]	1.779* [1.634]	-2.107 [1.634]	0.951 [3.989]
<i>PRICE.WAGES</i>	1.384 [1.262]	3.568* [1.652]	1.473 [1.294]	3.948* [1.827]	1.332 [1.226]	3.562* [1.659]	1.602 [1.262]	3.964* [1.815]
<i>FORSHARE</i>	-0.38 [0.723]	-2.105** [0.573]	-0.365 [0.756]	-2.181** [0.700]	-0.263 [0.621]	-1.962** [0.509]	-0.422 [0.677]	-2.135** [0.671]
<i>PRICE.NAOH</i>	0.021 [0.530]	-0.893* [0.383]	0.005 [0.535]	-0.92* [0.387]	0.05 [0.524]	-0.906* [0.370]	0.028 [0.531]	-0.917* [0.379]
<i>PRICE.CL</i>	-0.169 [0.197]	-0.441+ [0.232]	-0.184 [0.203]	-0.465+ [0.237]	-0.171 [0.204]	-0.462+ [0.228]	-0.184 [0.221]	-0.48+ [0.233]
<i>PERMIT</i>	0.018 [0.072]	-0.052 [0.105]	-0.005 [0.069]	-0.083 [0.108]	-0.041 [0.082]	-0.09 [0.093]	-0.054 [0.084]	-0.113 [0.097]
<i>EFFRIVER</i>	-2.059 [1.212]	-2.412** [0.829]	-3.52* [1.261]	-4.409** [1.207]	-4.014* [1.610]	-3.304** [0.993]	-4.53* [1.657]	-4.857** [0.941]
<i>PRICE.WOOD</i>	-0.209 [0.467]	-1.08** [0.361]	-0.203 [0.469]	-1.092** [0.359]	-0.154 [0.489]	-1.053** [0.360]	-0.174 [0.494]	-1.078** [0.356]
<i>WEALTH * TREND</i>	-0.13 [0.193]	0.058 [0.169]			-0.136 [0.203]	0.053 [0.185]		
<i>WEALTH * POSTGRP</i>	-0.631 [1.033]	-2.607** [0.812]			-0.288 [1.030]	-2.398* [0.928]		
<i>SINGLE * TREND</i>			0.02 [0.038]	0.079+ [0.041]			0.018 [0.043]	0.064+ [0.031]
<i>SINGLE * POSTGRP</i>			-0.292 [0.189]	-0.576** [0.198]			-0.132 [0.198]	-0.446+ [0.229]
Constant	4.919 [3.430]	9.449** [2.373]	6.638+ [3.832]	11.241** [3.095]	6.742+ [3.298]	10.688** [1.858]	7.172+ [4.081]	11.673** [2.886]
Observations	154	152	154	152	154	152	154	152
R-squared	0.54	0.67	0.54	0.65	0.57	0.69	0.57	0.66

+ significant at 10%; * significant at 5%; ** significant at 1%
Clustered standard errors in brackets; all regressions include plant fixed effects

interaction terms are significant.

Table 2.3 presents results from 10 models intended to identify channels through which GRP operates. Model 5 (for *COD*) and Model 6 (for *TSS*) include *WEALTH* interaction terms, and Model 7 (for *COD*) and Model 8 (for *TSS*) include *SINGLE* interaction terms. As discussed below, we include additional models with *ONELEAF* interaction terms to disentangle the effects of baseline environmental performance from other plant and community characteristics: Model 9 (for *COD*) and Model 10 (for *TSS*) include *WEALTH* interaction terms along with *ONELEAF* interaction terms and Model 11 (for *COD*) and Model 12 (for *TSS*) include *SINGLE* interaction terms along with *ONELEAF* interaction terms.

Turning to the results, first note that even with the inclusion of these additional interaction terms, there is virtually no change in the qualitative results on the controls discussed in the previous section.

In both Models 5 and 6, *WEALTH * POSTGRP* is negative, and it is significant in the *TSS* model. This result suggests that plants in wealthier communities were more responsive to disclosure than those in poorer communities. Several explanations are possible. Residents of relatively wealthy communities may have been more likely to pressure plants to improve their performance following a GRP rating. Alternatively, or perhaps as a result, regulators in such communities may have been more ready to crack down on poorly performing plants following disclosure.

Turning to Models 7 and 8, the interaction term *SINGLE * POSTGRP* is negative and significant in the *TSS* model, implying that plants that are part of a conglomerate or multi-plant firm were less responsive to GRP ratings than stand-alone plants. Again, several explanations are possible. Plants that are part of a conglomerate may have better access to the human capital needed for environmental management, be better informed because they share best practices with other plants in the same firm, and/or have better access to the financial capital needed for pollution control.

Though those explanations appear plausible, it is also possible that plants in wealthier communities and plants that are not part of a conglomerate were more responsive to disclosure because they were dirtier to begin with. Simple correlation coefficients suggest that this may be true in the case of stand-alone plants; the evidence is less clear that dirty plants were located in wealthier communities. In Models 9 through 12, we add the *ONELEAF* interaction terms (*ONELEAF * TREND* and *ONELEAF * POSTGRP*) to disentangle the effects of baseline environmental performance from other plant and community characteristics. In these regressions the *WEALTH* and *SINGLE* interaction terms remain significant, although levels of significance are attenuated. These results suggest that the channels proxied for by *WEALTH* and *SINGLE* interaction terms facilitate environmental improvement above and beyond that driven by the simple fact that plants in wealthy communities and those that are not part of conglomerates tend to be dirtier.

2.5.3 Robustness checks

In this subsection we address some possible robustness issues.¹⁹ First, with an unbalanced panel covering at most 22 plants over eight years, we have a limited number of observations. Therefore, we need to make sure that our regression results are not being driven by outliers. To that end we performed an outlier check. We repeated each regression in Tables 2.2 and 2.3 22 times, omitting one plant in each regression. The *ONELEAF*, *WEALTH*, and *SINGLE* interaction terms are significant in each regression. A second concern is that several of our regressors, including the composition of fiber inputs, as well as *SCALE* and *FINALGOOD*, could be endogenous if abatement decisions and production decisions are made simultaneously. However, these variables display only minor temporal variation, so any endogeneity should be minimal. As a robustness check, we re-estimated all regressions presented in

¹⁹The results discussed in this section are available from the authors upon request.

Tables 2.2 and 2.3, omitting the potentially endogenous regressors *PCTBAMBOO*, *PCTGRASSES*, *PCTRECYCL*, *PCTPULP*, *SCALE*, and *FINALGOOD*. In all cases, our results are qualitatively unchanged. We conclude that endogeneity is not an important practical concern in this context.

Finally, note that in the interest of preserving degrees of freedom and concise exposition, we have omitted regressors whose inclusion had no effect: real price of coal, vintage of the plant, other types of regulatory permits, share of sales spent on R&D, and diversity of the product mix produced by each plant.

2.6 Conclusion

We have used eight years of exceptionally detailed survey data on 22 of India's largest pulp and paper plants to evaluate the Green Rating Program, an Indian environmental performance public disclosure program. We sought to determine whether a 1999 GRP rating caused plants to reduce their water pollution loadings. We have also attempted to shed light on the mechanism by which the rating may have done this. We found that the GRP drove significant reductions in pollution loadings among dirty plants but not among cleaner ones. This result comports with the finding by Dasgupta et al. (2007) that performance ratings programs in Indonesia, Philippines, China, and Vietnam all led to improvements among plants with moderately poor performance records, but not among those with either very bad or good records.

We also found that pulp and paper plants located in wealthier communities were more responsive to GRP ratings, as were stand-alone plants. We hypothesized that the former result suggests that environmental performance ratings programs may have an effect by mobilizing local communities and/or regulators to exert pressure for reductions in discharges. We hypothesized that the latter result implies that performance rating programs are more effective when targeted at plants with better access to human and financial capital for pollution abatement.

This study adds to a thin but fast-growing body of evidence that public disclosure programs can be an effective environmental management tool, even in developing countries where weak regulatory institutions, limited political will, and other problems hamstringing conventional pollution control policies. Although an analysis of the costs of administering the GRP is beyond the scope of our study, we suspect it has been less expensive than conventional policies that involve standard setting and enforcement. If that is indeed the case, then our findings indicate that public disclosure programs may be an efficient as well as effective environmental management strategy.

Finally, we note that whereas virtually all national-level performance ratings programs in developing countries are administered by state environmental regulatory agencies, the GRP is run by a nongovernmental organization. To the extent the GRP is replicable, it suggests that even in countries where institutional and political constraints preclude state-run initiatives, public disclosure may offer a means of making progress.

Appendix: Analytical model

This appendix presents a simple analytical version of the graphical model of public disclosure in Section 3. To keep the model as simple as possible and focus attention on pollution abatement, we assume that the firm makes production and abatement decisions sequentially. First it chooses a level of output, q , and a vector of levels of financial and human capital, k . Subsequently, it chooses a level of abatement, α , treating both q and k as fixed. We model the firms second-stage abatement decision only. Note that abatement here may also include pollution prevention. The firm chooses α to maximize profit, Π , given by

$$\Pi = P[g(\alpha, d)]q - C[\alpha, t(d)] - W(\alpha, d)k - H(\alpha, d) \quad (2.3)$$

where

$$H(\alpha, d) = r(\alpha, d) + c(\alpha, d) + j(\alpha, d) \quad (2.4)$$

and

- $P(\cdot)$ is the equilibrium price of output;
- g is an index of green consumerism – the sensitivity of P to the plants discharges;
- d is a measure of the public disclosure of information about the plants discharges;
- q is the quantity of output;
- $C(\cdot)$ is the cost of abatement;
- $t(\cdot)$ is the plants information about abatement technologies and its own discharges;
- $W(\cdot)$ is a vector of the costs of two types of capital, financial and human;
- k is a vector of two types of capital, financial and human;

- $H(\cdot)$ is the total cost of the plants discharges generated by external agents;
- $r(\cdot)$ is costs generated by formal regulatory authorities;
- $c(\cdot)$ is costs generated by communities; and
- $j(\cdot)$ is costs generated by courts.

Following the literature discussed in Section 2, we make the following assumptions about the price and cost functions:

- the stronger is green consumerism, the lower is the equilibrium price the plant receives for its output (P is decreasing in g);²⁰
- the less the plant abates and the more the public knows about its discharges, the stronger is green consumerism (g is decreasing in α and increasing in d), the higher are the costs of financial and human capital (W is decreasing in α and is increasing in d), and the greater are the costs imposed on the plant by external agents (r , c , and j are all decreasing in α and increasing in d); and
- the less the plant abates and the more information it has about its discharges and abatement technologies, the lower is the marginal cost of abatement (C is increasing in α and decreasing in t).

Finally, we make the reasonable assumptions that

- abatement has a diminishing marginal impact on green consumerism, capital costs, and costs imposed by external agents; and an increasing marginal impact on abatement costs (g , W , H , and C are all convex in abatement).

²⁰To keep the exposition simple, we implicitly assume that the plant is an inherently dirty one – for example, an aged coal-fired power plant – so that regardless of its choice of α , green consumerism always reduces equilibrium price. We could just as easily assume that the plant is an inherently clean one whose equilibrium price is always increased by green consumerism. Allowing green consumerism to increase or decrease equilibrium price depending on the plant’s choice of α makes the model needlessly complex given our limited goal of illustrating how various channels discussed in the literature operate.

The first-order condition for the choice of the optimal level of discharges, α^* , is²¹

$$\left(\frac{dP}{dg} \frac{\partial g}{\partial \alpha} q - \frac{\partial W}{\partial \alpha} k - \frac{\partial H}{\partial \alpha} \right) - \frac{\partial C}{\partial \alpha} = 0 \quad (2.5)$$

The term in parenthesis represents the marginal benefit of abatement due to an increase in the equilibrium price of output (the first term in braces); a reduction in the costs of labor and capital (the second term); and a reduction in costs imposed by formal regulatory authorities, communities, and the courts (the third term). We will refer to the sum of these three terms as the marginal abatement benefit (MAB). The last term in (2.5) is the marginal abatement cost (MAC). The plant chooses α^* such that MAB is equal to MAC.

Using (2.5), it is straightforward to show that the total derivative of α^* with respect to d is unambiguously negative. Therefore, public disclosure will increase abatement. Figure 1 makes this point graphically. Given our assumptions on $P(\cdot)$, $C(\cdot)$, $W(\cdot)$, and $H(\cdot)$, the MAC schedule is increasing in α and the MAB schedule is decreasing in α . The plant chooses the level of discharges where these schedules intersect. An increase in d will cause $t(\cdot)$ to increase and the MAC schedule to shift down. It will also cause $g(\cdot)$, $W(\cdot)$, and $H(\cdot)$ to increase and the MAB schedule to shift up. Each of these shifts will cause α^* to increase.

²¹The convexity of $g(\cdot)$, $C(\cdot)$, $W(\cdot)$, and $H(\cdot)$ guarantee that the second-order condition is met.

CHAPTER III

Do State Renewable Portfolio Standards Promote In-State Investment in Renewable Capacity?¹

3.1 Introduction

In the United States, power plants are responsible for approximately 40 percent of the nation's carbon dioxide emissions, leading environmental and other interest groups to target this sector as they seek to reduce emissions in response to concerns of global climate change. The lack of action by the federal government has led some state and local governments to fill this void with a variety of policy approaches (Engel and Orbach, 2008). One of the most common state-level policy instruments, and the object of significant attention, is known as a renewable portfolio standard (hereafter, RPS). An RPS is a policy that ensures that a minimum amount of renewable energy (such as wind, solar, biomass, or geothermal energy) is included in the portfolio of electric generating resources serving a state. RPS regulations generally impose obligations that increase over time. The stated intent of these policy measures is usually some combination of increasing the diversity, reliability, public health and

¹The material in this chapter represents joint work with Haitao Yin

environmental benefits of the energy mix.² As of April 2009, 30 states³ and the District of Columbia have passed renewable portfolio standards.

While RPS policies all share several key features, they vary dramatically in design across states. These design differences have been carefully detailed by Berry and Jaccard (2001); Wiser et al. (2005); Wiser et al. (2007); and Wiser and Barbose (2008). However, econometric analyses of the effectiveness of RPS policies have largely ignored this heterogeneity in RPS design. For methodological convenience, previous empirical analyses have treated RPS policies as identical or have characterized the differences among them in an overly simplistic manner. A primary argument in the present study is that without properly accounting for the wide heterogeneity we see in RPS policies, empirical studies of their effectiveness may result in very misleading conclusions. Moreover, careful analysis of these differences and their influence on RPS effectiveness can afford policymakers an opportunity to improve the effectiveness of RPS policies through their redesign.

In this paper, we develop a measure for the strength of an RPS. This measure explicitly considers some key design features that could potentially affect the magnitude of the incentives for developing new renewable capacity. This new measure suggests that some seemingly aggressive RPS policies in fact provide only weak incentives, while some seemingly moderate RPS policies are in fact relatively ambitious. It follows that in any analysis addressing the question of whether RPS policies are effective drivers of investment in renewable electricity capacity, imposing uniformity

²Some proponents of RPS policies have also claimed that they can provide employment benefits (the often-discussed “green jobs”), or generate learning economies important for the development of renewable technologies, though these questions are not examined in the present paper.

³The 30 states are AZ, CA, CO, CT, DE, HI, IA, IL, MA, MD, ME, MI, MN, MO, MT, NC, NH, NJ, NM, NV, NY, OH, OR, PA, RI, TX, VA, VT, WA, and WI. Virginia and Vermont have passed unconventional policies that have been deemed by some to be “optional”. However, unlike the voluntary policy, of say, North Dakota, the policies in both Virginia and Vermont contain credible commitments that provide real incentives for renewable development. Nonetheless, we have also performed the empirical analysis presented in this paper with alternative classifications of the Virginia and Vermont policies, and the results are qualitatively identical. These results are available upon request.

on the policy is inappropriate.

Based on the new measure, we present the most rigorous statistical analysis of RPS policies to date. For the purposes of this paper, we define effectiveness of an RPS policy as the extent to which it increases investment in in-state renewable electricity capacity. We find that RPS policies do in fact lead to a statistically significant increase in in-state renewable electricity development. This result stands in sharp contrast to those when the differences in RPS stringency are ignored or measured in an overly simplistic manner, as has been done in previous studies.

The remainder of the article is organized as follows. In the next section, we provide a brief overview of the research done on RPS policies to date. In section 3, we present some background on RPS policies, key dimensions of heterogeneity in these policies, and propose a new measure for the stringency of such policies. In section 4, we present an empirical framework for estimation of the effectiveness of an RPS in promoting renewable energy development. Section 5 describes the data and compares different measures for RPS stringency. In section 6, we present the results of our estimations. Finally, in section 7, we conclude.

3.2 Literature Review

Rigorous study of renewable portfolio standards has been rare. In this section, we outline the existing literature on these relatively new policies. In this paper, our primary concern is not the efficiency of RPS policies, but rather their effectiveness – which is a necessary (though not sufficient) condition for efficiency. Furthermore, while first-best efficiency is obviously ideal, the effectiveness criterion may ultimately be more useful when other more efficient policies being considered (usually cap-and-trade or a carbon tax) are politically infeasible. Nonetheless, we review the literature on both questions in this section.

State-level RPS policies are often seen by economists as a suboptimal policy for

two key reasons. First, since the most significant environmental benefits from RPS legislation come from the internalization of global externalities, a regional approach runs the risk of inducing welfare losses in the regulated region without incurring environmental benefits, as the damaging pollutants can continue to be produced elsewhere. For example, Bushnell et al. (2008) argued that the effectiveness of such local initiatives is limited due to a leakage or reshuffling problem and claimed that RPS policies are “largely symbolic unless they facilitate change beyond their local regions”. Second, since many states require that the renewable electricity be generated in-state, the principle of allowing the cost of pollutant reduction to be minimized by allowing these reductions to occur where they are least costly is violated.

Two recent papers have developed theoretical models to examine the cost-effectiveness of RPS policies. Palmer and Burtraw (2005) perform numerical simulations to predict the impacts of a national RPS, and find that it raises electricity prices, but that it is not as cost-effective as a cap-and-trade policy for reducing carbon emissions. Fischer and Newell (2008) develop and calibrate a model that predicts that an RPS-like policy is a sub-optimal policy whether the goal is reducing carbon emissions or promoting technological progress. A third paper, (Michaels, 2007), though lacking a formal model, is also extremely critical of the oft-proposed federal RPS, arguing that it is an inefficient way to achieve any of the policy objectives often attributed to an RPS. Further, the article’s author argues that the record of state implementation of RPS policies has, thus far, been largely symbolic.

Nonetheless, and perhaps in part because they are perceived to address several political goals at once, state-level RPS policies have proliferated. Lyon and Yin (2009) examine the political economy behind these policies, and find that renewable energy potentials (measures of the strength of the wind and solar resources in a state), Democratic majorities in the state legislature, and organization of the renewable industry are all significant antecedents of a state-level RPS. Interestingly, they do not

find that high levels of existing renewable development make a state more likely to adopt an RPS.

Distinct from, though not orthogonal to, the question of efficiency is the question of effectiveness. Namely, do RPS policies achieve their primary stated goal of increasing renewable capacity and generation in the states in which they are passed? The most prominent studies have not included complete statistical analyses and have concluded that the policies are either ineffective (Bushnell et al., 2008) or “largely symbolic” (Michaels, 2007).

As far as we know, only three (Menz and Vachon, 2006; Adelaja and Hailu, 2008; Kneifel, 2008) studies have employed econometric analysis to empirically identify the impacts of RPS on renewable energy development. These studies, however, have taken a rather blunt approach. Using cross-sectional data, Menz and Vachon (2006) and Adelaja and Hailu (2008) both establish a positive correlation between existence of an RPS and renewable development, but the use of cross-sectional data precludes any conclusions regarding causality. Furthermore, these studies treat RPS’s as being uniform across states. Menz and Vachon (2006) and Adelaja and Hailu (2008) both use a binary RPS variable, which abstracts away from the very real heterogeneity across RPS’s.

Kneifel (2008), on the other hand, uses a panel data approach and finds that RPS policies do not lead to an increase in renewable capacity in a state if the requirement is based on generation, as is common in most states⁴. His study does in fact differentiate among RPSs by using their nominal requirements, which are the RPS goals that are written into the laws, as his measure of the strength of the policies. However, as evidenced in this paper, using nominal requirement as the measure of an RPS fails to capture some important design features that are decisive for RPS effectiveness and

⁴Given that most of the relatively few capacity-based policies are enforced on the basis of generation, using capacity conversion factors that are usually publicly-known, we argue that Kneifel’s differentiation between capacity- and generation-based policies is a false dichotomy.

therefore may also lead to misleading conclusions.

The current study attempts to further the literature in two ways. First, we construct a measure that incorporates the most important design features of an RPS and therefore more accurately assesses the strength of an RPS; secondly, we perform a panel data analysis and demonstrate how misleading conclusions might result when the heterogeneity of design is ignored or over-simplified as has been done in previous studies.

3.3 Background

RPS policies, which are the central focus of this paper, differ from other policies designed to incentivize renewable energy installation and generation, in that they are essentially minimum quantity mandates, though with varying degrees of flexibility. All strive to ensure that a minimum amount of renewable energy is included in the portfolio of electric generating resources serving the state. Furthermore, all the RPS policies we examine here clearly specify the path of the requirement over time, and generally build to a final standard at some distant point in the future (for example, Michigan's RPS, passed in October 2008, has an ultimate target of 10 percent by 2015, with intermediate requirements of 2 percent by 2012, 3.3 percent by 2013, and 5 percent by 2014).

The fact that these requirements are written into the laws themselves provides researchers with an obvious measure for the strength of an RPS; we refer to this as the nominal requirement.⁵ Although straightforward, using nominal requirement as

⁵This, to a certain extent, is the approach taken by Kneifel (2008). Kneifel (2008) takes the target requirement in a number of years after enactment, and linearly interpolates backwards to the enactment date of the policy to obtain the requirement for each year after enactment. For example, a policy enacted in 1996 with a sales requirement of 1.0% beginning in 2000 would be linearly interpolated to be 0.2% in 1996 and increase by 0.2% each year until it reaches 1.0% in 2000. We argue that this is a strong and ultimately unnecessary assumption. First, all states stipulate the requirement to be enforced in every year until the ultimate goal is reached, and these do not generally follow linear patterns. Second, given the large fixed costs associated with renewable energy generation and the learning curves often thought to accompany renewable energy development, it's

a measure neglects a sizeable amount of policy heterogeneity that could potentially have significant impact on the strength of an RPS. Previous research on RPS design (Berry and Jaccard, 2001; Wiser et al., 2005, 2007; Wiser and Barbose, 2008) suggests that the heterogeneity in three distinct dimensions have the greatest importance. We examine each in turn, and discuss how we account for them in our analysis.

3.3.1 Coverage

Wiser et al. (2005) have argued that a well-designed RPS should ideally apply equally and fairly to all load-serving entities in a state. However, in practice, there are vast differences in coverage, as different types of utilities are treated differently by some of the policies. For example, in Maryland and five other states (Iowa, Texas, Hawaii, Minnesota and Wisconsin), all utilities, including investor-owned utilities, power marketers, rural cooperatives, and municipal cooperatives, are required to comply. However, other states have provided partial exemptions in meeting RPS requirements, either to entire classes of utilities, or in some cases, to individual utilities. In Montana, the RPS applies only to investor-owned utilities, which generate only 45 percent of the electricity that is sold in the state. Five other states (Connecticut, Pennsylvania, Arizona, Illinois and Colorado) have exemptions of a similar magnitude, such that less than 60 percent of the electricity market in those states is covered by the respective RPS policy.

3.3.2 Existing Capacity

Another design feature that could affect the effectiveness of RPS is whether the RPS allows generation from existing renewable resources to fulfill the requirements, or whether the standard must be filled with generation from new investments in

not clear why we should expect producers to develop renewable capacity in a linear manner, even in years when the time path of the requirement is not explicitly laid out. In this paper, we start from the actual scheduled requirements in each state.

renewable resources. These obviously have different effects - requiring new resources creates a stronger incentive for new development. Allowing generation from existing assets to “count” will weaken the incentive, and furthermore may allow windfall profits to accrue to those utilities that own existing renewable generating capacity.

States vary greatly in this aspect. While some states, including Arizona, Massachusetts, Montana, and Vermont, only allow generation from new assets to count towards the policy, most states allow generation from all units that existed at the time the legislation was passed. Many states, including Delaware, Maine, North Carolina, New Hampshire, New Mexico, New York, Oregon, Virginia, and Washington, allow generation from some, but not all, existing units. For example, Washington’s RPS states, “New renewable generation resources are defined as having first gone into commercial operation after 12/31/97. Renewable generation units that entered service before that date may not account for more than 1 percent of total retail electricity sales in any compliance year.” When eligible existing capacity is large compared to RPS requirements, the strength of an RPS would be significantly overstated if we used the nominal requirement as the main measure, because the policy-induced incentive to install new renewable generating capacity is relatively small.

In order to take into account the heterogeneity in coverage and existing capacity, and thus more accurately capture the size of the new incentive generated by these policies, we propose a new variable, *INCRQMTSHARE*:

$$INCRQMTSHARE_{it} = \frac{NOMINAL_{it} * COVERAGE_{it} * SALES_{it} - EXISTING_{iT}}{SALES_{it}} \quad (3.1)$$

where $NOMINAL_{it}$ is the nominal requirement in state i in year t , $COVERAGE_{it}$ is the proportion of sales of the utility industry in state i covered by the RPS at time t , $SALES_{it}$ is the total retail sales in state i in year t , and $EXISTING_{iT}$ is the renewable generation in year T that, if generated in later years, would be eligible to

fulfill the RPS requirement in state i . T is the date the RPS legislation or mandate is enacted.⁶ This new variable, *INCRQMTSHARE*, thus represents the “incremental percentage requirement”, or the mandated increase in renewable generation in terms of the percentage of all generation. For the remainder of the paper we refer to it as the incremental requirement.

One key challenge in deriving *INCRQMTSHARE* is to calculate *EXISTING* _{iT} , the amount of eligible existing renewable generation. The difficulty is due to the fact that state policies differ not only in whether existing capacity is eligible, but also in which technologies are counted as “renewable”. We calculate this variable state by state based on each state’s definition of “renewable”.⁷ Thus, three cases arise in the calculation of *EXISTING*: (1) when existing capacity/generation is not allowed, *EXISTING* = 0, but (2) when all existing capacity/generation is allowed, *EXISTING* is equal to the renewable generation from existing capacity in period T . However, in the third case, when states allow existing generation only in certain cases, we need to take a case-by-case approach. For example, in the state of Washington (mentioned above), we define existing renewable generation as the renewable generation in year 2006 from plants installed between 1998 and 2006 (the year Washington passed its RPS) plus the minimum of either (a) the 2006 renewable generation from plants installed in or before 1997 or (b) 1% of electricity sales in 2006.⁸

⁶If a policy was passed during the first six months of the year, T is set to the previous year, otherwise T is the year in which the policy was passed. Regardless of how T is set, we always consider the RPS to become “active”, in the sense that it enters into the decision-maker’s calculus, in year $T + 1$.

⁷Note that this state-by-state calculation that take varying technologies into account is only performed for the variable *EXISTING*. For the dependent variable, we use a uniform definition of renewable technologies that is explained below.

⁸We recognize that there is an implicit assumption that the amount of generation from existing capacity will continue to be the same for every year after T . This could be violated if renewable capacity is decommissioned after year T , or if, for example, renewable capacity was “online”, but not operating at full capacity in years prior to T . However, given the relative newness of most renewable generating capacity and the low operating costs associated with most renewable technologies, this assumption should have minimal effects, whether on our proposed measures or our regression results.

3.3.3 REC trading

Most RPS policies are enforced through a credit-trading mechanism. When electricity is generated from a renewable source in states that have a renewable energy credit program, there are two resulting products - the electrons that are fed into the grid, and the environmental attributes associated with producing reduced-carbon or carbon-free electricity. In most states, these environmental attributes are accounted for in the form of renewable energy credits, or REC's. Each REC represents one megawatt hour (MWh) of electricity generated from an eligible renewable energy resource. In some cases, the REC's are bundled with the electricity that they are associated with - this is often the case when an independent power producer has contracted, *ex ante*, to sell the electricity to a given utility. But in some cases, the REC's are instead retained by the independent power producer, and presumably are sold at a later point in time. At the end of a compliance year, the administrator of the program calculates each utility's required amount of renewable generation, based both on the legislated percentage and the share of the state's sales belonging to that utility. A utility then has a specified amount of time to purchase the REC's necessary to meet its requirement if it is "short". Treatment of REC's varies across states, and the way these REC's are treated should result in variation of the impact of an RPS policy.

- Some states, including California, Iowa, Illinois and Hawaii, simply do not allow REC trading or out-of-state REC trading. In these states, the obligated utilities are required to meet a certain standard through their own generation or through power purchase agreements.
- Some states allow out-of-state REC's, but heavily incentivize in-state generation. This is usually done either by creating set-asides, where a certain portion of a utility's obligation must be met with in-state REC's, or multipliers, where

in-state REC's get extra credit. For example, in Arizona, a MWh of electricity generated from in-state renewable capacity is credited as 1.5 REC's, whereas the same amount of electricity generated out-of-state is credited as normal.

- Some states impose very restrictive conditions on the eligibility of out-of-state renewable generation, which in essence disallows out-of-state generation. For example, in Texas, out-of-state resources are technically eligible to generate Texas REC's, but the output of the facility must be readily capable of being physically metered and verified in Texas by the program administrator.
- Finally, some states allow free trade of REC's and provide no preferential treatment to in-state REC's.

RPS policies that fall into the first three categories listed above are presumably adopting such provisions in an effort to ensure that any economic and environmental benefits (for example, avoided emissions or “green jobs”) resulting from RPS passage do not leak across the border to other states. Without these in-state constraints, utilities may purchase either renewable electricity or REC's from out-of-state resources, therefore mitigating the strength of an RPS in promoting in-state renewable development.⁹

3.3.4 Penalty or Alternative Compliance Payment

Another design difference among state RPSs is the treatment of non-compliant energy producers. Some RPS policies, including those in Texas, California, Connecticut, Montana, Washington and Wisconsin, explicitly impose a financial penalty for noncompliance. For example, in Texas, any utility that fails to meet its obligations under the RPS must pay a penalty equal to the lesser of \$50 or 200% of the average

⁹Some analysts (see, e.g., Wiser 2006) have raised the possibility that imposing in-state-requirements may raise the question of violating U.S. Constitution's Interstate Commerce Clause. This is a valid concern but beyond the scope of this paper.

cost of credits traded during the year for each MWh it falls short. In some other states¹⁰, the RPS instead establishes a mechanism called an Alternative Compliance Payment (ACP), which serves a similar function. For example, in Massachusetts, a retail electricity supplier may meet its RPS obligations (in whole or in part) for any compliance year by making an ACP, rather than by investing in renewable capacity or purchasing REC's. The ACP rate was set at \$50 dollars per MWh for 2003, and in each subsequent compliance year, the state's Department of Energy Resources is required to adjusted the rate up or down according to the previous year's Consumer Price Index¹¹. While they are expressed in different terms, explicit penalties and ACP's should, one expects, have similar effects - they both effectively set a cap on the cost of complying with the RPS. In most of the states that have either an explicit penalty or ACP in place, the level of financial incentive for compliance is of a similar magnitude to those of Texas and Massachusetts¹². However, nearly half of the states¹³ with RPS policies have no such financial mechanisms.

The effect of an ACP or penalty on the effectiveness of an RPS in driving investment in in-state renewable capacity is unclear. One might think that the explicit specification of either a penalty or ACP gives the RPS "teeth": it makes explicit the consequences for a utility who fails to meet its RPS obligations, and thus provides strong incentives to comply with the RPS. However, an explicit penalty or ACP also establishes an upper bound to the negative consequences of non-compliance - it renders the regulatory contract more complete, and thus in a sense limits the downside risk to the utility who chooses not to comply.

¹⁰As of February, 2009, these include MA, ME, WI, NJ, RI, PA, MD, DC, DE, and NH.

¹¹The adjusted ACP rate for 2008 is \$58.58 per MWh.

¹²Two states that have this mechanism charge amounts significantly lower than the others, who generally charge around \$50 per MWh. The penalty in Montana is \$10 per MWh, and in Maryland, the ACP is either \$15 or \$20, depending on the 'technology tier'. The findings reported in this paper do not depend on whether or not we treat Montana and Maryland as having a penalty/ACP.

¹³These include IA, MN, NV, AZ, NM, CO, NY, HI, VT, IL, VA, NC, MO, and OR.

3.4 Empirical Framework

The primary objective of this paper is to determine the effectiveness of state-level RPS policies in incentivizing investment in new renewable energy. Alternatively put, to what extent can the recent growth in renewable capacity in the 50 states be attributed to RPS policies?

In order to accurately answer this question, we exploit a lengthy panel of data that allows us to control for unobserved state and year heterogeneity. This is akin to a change-in-changes approach - with state and year fixed effects we control for existing differences among the states as well as exogenous technological progress, giving us consistent coefficient estimates. We estimate several models of the form

$$RENEWSHARECAP_{it} = \alpha_i + \gamma_t + \xi Z_{it} + \delta W_{it} + \beta x_{it} + e_{it} \quad (3.2)$$

where *RENEWSHARECAP* is the percentage of generating capacity in a state that is non-hydro renewable, α_i represents a state-specific intercept, γ_t represents year fixed effects, Z_{it} represents other state policies that are designed to encourage renewable investment, and W_{it} represents various social and economic variables that might have an impact on the development of renewable energy. Finally, x_{it} is a measure for the RPS policy that varies both within and between states. In some specifications, we interact x_{it} with one of three variables thought to impact the effectiveness of an RPS; one is a dummy variable indicating the existence of an in-state requirement, the second is a dummy indicating that the RPS contains a penalty or ACP, and the third is a measure of how dependent a state is on electricity generated in other states. Because the error terms are likely to be correlated across time within a state, and because we expect the variance to differ by state, we estimate standard errors that are clustered at the state level.

We construct our dependent variable, the percentage of generating capacity in a

state that is non-hydro renewable (*RENEWSHARECAP*), using data we discuss in the next section. x_{it} takes different forms in different specifications; in addition to the nominal requirement and incremental requirement measures discussed above, we also use

- *RPS* - a binary variable that equals 1 if a mandatory RPS law is effective in a given year, and 0 otherwise.¹⁴ If the legality of an RPS was in dispute (as was the case in IA until 1997), we set RPS=0. This is similar to the measure used in Menz and Vachon (2006) and Adelaja and Hailu (2008).
- *RPSTREND* - a state-wise cumulative sum of RPS, denoting the number of years that the RPS has been in effect.¹⁵ Menz and Vachon (2006) use an analogous measure in some specifications.

In addition to RPS policies, states have also developed and implemented many other policy instruments to encourage installation of renewable generation. We include the following alternative policies in Z_{it} as controls.

- One popular alternative policy is known as a mandatory green power option, under which each utility in the state is required by law to offer its customers the choice of opting to “buy” green power. Consumers opt to pay a premium on their electricity bills, and then the utility must procure enough generating assets or RECs to provide an amount of renewable electricity equal to the amount purchased by those consumers who have chosen this option. *MGPOPTION* is a binary variable that equals 1 if such a law exists in that state and year. As of April 2009, eight states have a mandatory green power option law.

¹⁴We set RPS=1 if the law became effective on or before June 30 of that year. This is the coding rule we adopted for any policies evaluated in this paper. We also experimented with setting this variable equal to 1 if an RPS had just been passed, rather than in effect, and found qualitatively similar results.

¹⁵To date, no RPS has been repealed.

- Another type of policy designed to encourage development of renewable electricity is known as a public benefits fund. These are state-level funds established and maintained by the state public utility commissions in order to support energy efficiency and renewable energy projects. The funds are collected either by charging consumers a small amount, or by requiring payments from the utilities themselves. *PUBBENFUND* is a binary variable that equals 1 if, in a given year, a state maintains a public benefits fund that has as part of its mandate the support of renewable energy projects, and 0 otherwise. As of late 2008, 19 states maintain a public benefits fund that supports renewable energy.
- A third type of policy designed to encourage development of renewable electricity is called net metering. Net metering allows for the flow of electricity from consumer-sited installations back to the grid, so that excess generation at such installations can defray the cost of a customer's bill. These laws provide an additional incentive for small, customer-sited generation. *NETMETERING* is a binary variable that equals 1 if, in a given year, a state has a net-metering law on the books. As of late 2008, 42 states have such a law in place.
- The last type of policy we control for in this paper is referred to as interconnection standards. These are standards that facilitate the contracting process, making it easier, at both the technical and procedural level, for customer-sited generation to be installed. *INTERCONSTAND* is a binary variable that equals 1 if a state has codified interconnection standards to facilitate customer-sited renewable energy installation. As of late 2008, 37 states had such laws on their books.

Besides these policy variables, we also include some social and economic variables in the regression analysis, as they might be thought to have either a direct impact on the development of renewable energy, or indirectly by making the adoption of RPS

policies more likely.

- **Electricity Price.** Electricity price, *ELECPPRICE*, could influence demand for renewable energy resources. On one hand, high electricity prices may reflect the need for the state to seek out alternative energy sources and ensure a viable long-term energy supply, implying that such states would be more likely to develop renewable energy. On the other hand, it may be more difficult to pass on the extra costs of shifting to renewable energy to customers when electricity prices are already high. This suggests that renewable energy could face more resistance in states with high electricity prices. The predicted sign of the overall effect is ambiguous. We use lagged prices so that this regressor is predetermined with respect to the dependent variable.
- **State Income.** The transition to renewable energy may cause an increase in electricity prices. States with higher incomes will be more capable of affording the increased price, and therefore are presumably more likely to develop renewable resources. To account for this, we include the median income for 4-person families in each state from 1993 to 2006 in the analysis, which is denoted as *STATEINC*.
- **League of Conservation Voters (hereafter, LCV) Scores.** In states where citizens have stronger environmental preferences, there may be higher demand for renewable energy development. Following previous studies (e.g. Maxwell et al. 2000), we use the average LCV scores of Senators and Representatives in each state, *LCVSCORE*, as a proxy for the environmental preferences of the citizens in the state. Each year, the LCV selects environmental issues that exemplify the environmental agenda with the help of a panel comprising the main U.S. environmental groups. The organization then creates an index by counting the number of times each representative or senator in Congress votes favorably for

the “environmental agenda” (e.g., tropical forest conservation or fighting global climate change). The index ranges from 0 to 100, with 100 representing a record of voting with the environmental agenda in all cases.

- Net import of electricity. A state that is heavily dependent on the import of electricity may have stronger incentives to develop renewable energy to increase the diversity of its energy mix and reduce its energy dependence. To take this confounding factor into account, we include *IMPORTRATIO*, a lagged measure of whether states import or export power, in all specifications.

$$IMPORTRATIO_{it} = \frac{SALES_{i,t-1}GENERATION_{i,t-1}}{SALES_{i,t-1}} \quad (3.3)$$

In some specifications, we include the interaction term between *IMPORTRATIO* and *INCRQMTSHARE* to see whether a state’s response to an RPS depends on the relative abundance of its existing electricity supply.

We include *RECFREETRADE* in some regressions. This is a binary variable that equals 1 if two conditions are met: (1) the state allows its RPS obligations to be met with REC’s, and (2) it treats out-of-state REC’s no differently from in-state REC’s. This variable equals 0 either if the RPS does not allow REC trading, or if it allows only or strongly favors in-state REC trading¹⁶. We include this binary variable and its interaction with *INCRQMTSHARE* in order to evaluate the extent to which imposing an in-state-requirement renders an RPS less effective in promoting in-state renewable energy development. In some specifications, we also include *NEIGHBOR*, which is meant to capture the size of the new market for renewables resulting from RPS implementation in neighboring states. This will allow us to control for possible

¹⁶Or, of course, if the state does not have an RPS policy.

spillover effects. The measure is constructed as follows:

$$NEIGHBOR_{it} = \frac{\sum_{a=1}^A INCRQMT_{at} * RECFREETRADE_{at}}{SALES_{it}} \quad (3.4)$$

where A denotes the number of states adjacent to state i , and $INCRQMT_{at}$ is the numerator of equation (3.1) for state a .

Finally, we include $PENALTYACP$ in some regressions. This also is a binary variable which is equal to 1 if either penalty or ACP is to be imposed in case of noncompliance and 0 otherwise. We include this binary variable and its interaction with $INCRQMTSHARE$ in order to evaluate to what extent providing a financial penalty or ACP may render an RPS more or less effective in promoting in-state renewable energy development.

3.5 Data

In this section, we describe the data that is used to create the measures laid out in Section 3.3 and to perform the analysis described in section 3.4.

As discussed above, one key contribution of this paper is the development of a new measure for the strength of an RPS. Once this variable is constructed, we can compare our measure with the more commonly used “nominal” measure. For this purpose, the key task is to calculate $INCRQMTSHARE_{it}$ as defined in equation (1), which requires information on each state’s electricity market and RPS policies.

For variables measuring the relative size of the electricity industry in each state, $SALES_{it}$, we employ publicly-available data on electricity sales (EIA-861) from the Energy Information Administration (EIA). EIA data is also critical in the construction of existing eligible renewable generation, $EXISTING_{iT}$. We obtain annual generation data at the generating unit level from the EIA-906 data files.

We construct an analogous measure based on generation rather than capacity, and refer to it as *RENEWSHAREGEN*. This is used primarily in our interpretation of the coefficients (see the Results section); it is less suitable as a dependent variable because the EIA altered its definition of renewable energy in 2000. To code the RPS policies, we use data from a variety of sources. The Union of Concerned Scientists (hereafter, UCS) maintains a database¹⁷ on the design and implementation of existing state standards. Another excellent database that was very helpful in our data collection efforts was DSIRE, the Database of State Incentives for Renewable Energy.¹⁸ We referred to both databases in our coding of the RPS variables, and when necessary, referred to the actual legislation and/or public utility commission rules to resolve any discrepancies or missing information. The variables *PENALTYACP* and *RECFREETRADE*, described above, are based on information contained in these sources. Other RPS-related variables include:

- *NOMINAL_{it}*, the nominal percentage requirement as written into the law. Every RPS law has an explicitly defined requirement path that evolves over the years. For the handful of states for which the law is coded in absolute capacity terms, rather than as a percentage of generation, we multiply by a constant, called a capacity conversion factor¹⁹, that accounts for the intermittent nature of the most popular renewable technologies, then divide by retail sales in a given year, in order to convert this variable to be in the same units across all of our data.
- *COVERAGE_{it}*, the proportion of retail sales in a state-year that can be at-

¹⁷http://go.ucsusa.org/cgi-bin/RES/state_standards_search.pl?template=main

¹⁸<http://www.dsireusa.org>

¹⁹These state-specific capacity conversion factors are often either explicitly written into the legislation, as is the case in TX, or implicit from the history of the RPS, as is the case in IA, where part of the afore-mentioned legal dispute centered around the question of whether 105 MW of wind would satisfy the RPS, or whether 260 MW – an amount of renewable generation that would displace 105 MW of conventional capacity – was required under the law. When the capacity conversion factor was not available, we used 0.35.

tributed to entities that are required to comply with the RPS. We use EIA-861 data to calculate the proportion of retail sales in each state that are undertaken by each utility or class of utility.²⁰ These weights are then combined with data from the laws themselves in order to obtain this variable.

- $EXISTING_{iT}$, is the amount of renewable generation in the year prior to the enactment of the RPS policy. We decide what types of existing renewable generation are eligible from the UCS and DSIRE database, and then refer to generating unit level data from EIA-906 in order to aggregate generation from existing eligible plants in year T.

We derive $INCRQMTSHARE_{it}$ based on equation 3.1. We finish with a balanced panel of $50 * 14 = 700$ observations, one for each state-year from 1993 to 2006. Table 3.1 shows nominal requirements and incremental requirements in 2006 for the 16 states that had a binding standard in that year. The differences between nominal requirement and incremental requirement are striking. We also rank the states based on nominal requirement and incremental requirement, and the correlation between either the measures themselves or their respective ranks is not statistically different from zero. The two measures are clearly not synonymous.

This table clearly demonstrates the potential pitfalls if the incorrect measure of policy stringency is applied in empirical analysis surrounding questions of RPS impact, regardless of the dependent variable. We argue that our new measure is more accurate, as it explicitly accounts for several key design features that affect the effectiveness of an RPS. It is not hard to imagine that studies on the effectiveness of RPS policies will be highly dependent on the choice of measure employed. Figure 3.1 illustrates this point more vividly. In two states that have the strictest nominal requirements, Maine and California, the passage of an RPS appears to have had little

²⁰We find minimal change in these shares over the 7 years for which we have data, so we use 2006 data to construct these weights.

Table 3.1: Comparison of Measures of RPS Stringency in 2006

State	Nominal Requirement	Incremental Requirement	Rank of Nominal Requirement	Rank of Incremental Requirement
ME	30%	0	1	14
CA	14.31%	0	2	14
HI	8%	1.02	3	8
NV	6%	0.38	4	12
WI	5.69%	1.23	5	7
NM	5%	4.33	6	1
NJ	4.58%	2.77	7	3
MN	4.36%	2.77	8	2
TX	2.04%	1.62	9	6
MA	2%	1.72	11	5
CT	2%	1.8	10	4
PA	1.5%	0	12	14
AZ	1.05%	0.62	13	10
MD	1%	0.05	14	13
IA	0.86%	0.67	15	9
NY	0.81%	0.49	16	11

positive impact on the amount of renewable capacity in the state. After Maine's first RPS (with a 30% requirement) was written into law in September 1997, the renewable capacity decreased. In California, an RPS was passed in September 2002, mandating that 20% of the state's electricity generation be renewables-based by 2010. Yet the growth thereafter appears to be a simple continuation of the pre-existing trend. In contrast, for the two states that rank as No. 1 and No. 2 based on incremental requirement, New Mexico and Minnesota, passage of an RPS appears to have had a strong and positive impact. New Mexico passed an RPS with gradually increasing requirements (starting in 2005) in December 2002, and shortly thereafter we see the first significant installation of renewable capacity in that state. Similarly, in Minnesota, a series of laws mandating installation of renewable capacity was passed starting in 1994. We see jumps in capacity in 1998, 2001, 2002, and 2006, all years in which the binding requirements increased.

In the next section, we study the effectiveness of RPS policies employing these

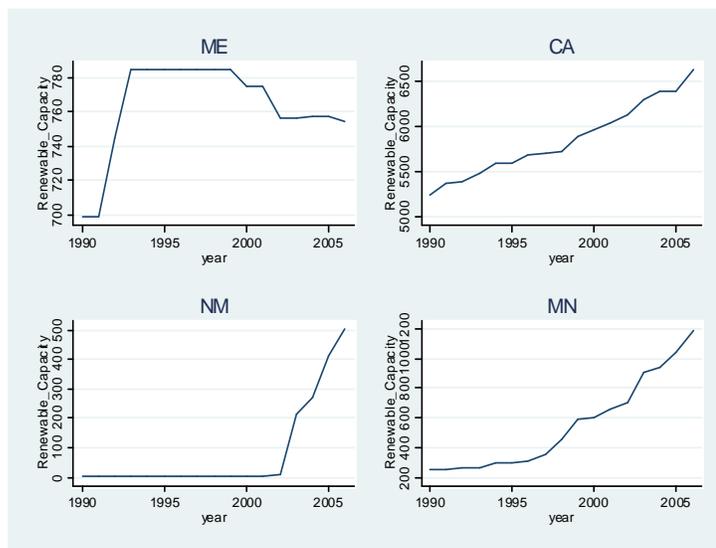


Figure 3.1: Growth of Renewable Capacity in Selected States

and other measures of policy stringency. While Figure 3.1 is certainly illustrative, the econometric analysis in the following section includes all states and controls for other policy changes and factors that could also have an impact on our outcome variable. The dependent variable, *RENEWSHARECAP*, is the share of capacity in a state-year that is based on non-hydro renewable technology. Using EIA-906, we build yearly data files that include all plants that were online in a given year, from 1990 to 2006. We count the capacity of all plants whose primary energy source is classified as non-hydro renewable by EIA. This includes wind, geothermal, and solar generating units, as well as several types of biomass. We then divide by the total capacity in a state for that year to obtain *RENEWSHARECAP*.

Information on policies other than RPS is retrieved from DSIRE. For social and economic control variables, data on electricity prices²¹, generation²², and consumption²³ are obtained from the Energy Information Administration, data on state income is obtained from the US Census Bureau²⁴, and data on LCV scores is collected from

²¹<http://www.eia.doe.gov/cneaf/electricity/epa/epat7p4.html>

²²<http://www.eia.doe.gov/cneaf/electricity/epm/table1.1.html>

²³http://www.eia.doe.gov/cneaf/electricity/epa/sales_state.xls

²⁴<http://www.census.gov/hhes/www/income/4person.html>

the League of Conservation Voters²⁵. Summary statistics of the data used in the regression analysis are provided in Table 3.2.

Table 3.2: Summary statistics

Variable	Mean	Standard Deviation	Minimum	Maximum
<i>RENEWSHARECAP</i>	2.39	3.71	0	27.36
<i>RPS</i>	0.16	0.37	0	1
<i>RPSTREND</i>	0.7	1.97	0	13
<i>NOMINAL</i>	0.57	3.22	0	30
<i>INCRQMTSHARE</i>	0.07	0.32	0	4.33
<i>MGPOPTION</i>	0.03	0.17	0	1
<i>PUBBENFUND</i>	0.16	0.37	0	1
<i>NETMETERING</i>	0.35	0.48	0	1
<i>INTERCONSTAND</i>	0.17	0.38	0	1
<i>ELECPRICE</i>	7.07	2.16	3.43	18.33
<i>LCVSCORE</i>	44.8	27.21	0	100
<i>STATEINC</i>	57.56	11.46	32.59	94.44
<i>POWERIMPORT</i>	-0.26	0.63	-3.04	0.61
<i>RECFREETRADE</i>	0.05	0.21	0	1
<i>PENALTYACP</i>	0.09	0.29	0	1
<i>NEIGHBOR</i>	0.28	1.72	0	19.59

3.6 Estimation Results

Table 3.3 presents results from several estimations of equation 3.2, and highlights how the estimates of the effectiveness of an RPS are highly dependent on the coding scheme chosen. When the RPS is introduced into the estimation model as a simple binary variable that is turned on when “treatment” is administered, or as a cumulative count of the years a given state has been subject to this treatment, we obtain coefficients on RPS that are not significantly different from 0, as shown in the first two columns of Table 3.3. This result suggests that RPS policies are ineffective in accelerating renewable energy, which is consistent with Michaels (2007)’s claim that RPS policies are largely symbolic.

²⁵<http://www.lcv.org>

Table 3.3: Measures of RPS and the Impact of an RPS on Renewable Electricity Investment

	-1	-2	-3	-4
<i>RPS</i>	-0.087 (0.431)			
<i>RPSTREND</i>		-0.036 (0.182)		
<i>NOMINAL</i>			-0.272 (0.044)**	
<i>INCRQMTSHARE</i>				0.558 (0.175)**
<i>MGPOPTION</i>	3.197 (0.582)**	3.254 (0.719)**	2.959 (0.706)**	2.994 (0.504)**
<i>PUBBENFUND</i>	-0.467 (0.535)	-0.437 (0.456)	0.115 (0.258)	-0.547 (0.58)
<i>NETMETERING</i>	-0.713 (0.438)	-0.748 (0.562)	-0.557 (0.219)*	-0.652 (0.43)
<i>INTERCONSTAND</i>	0.47 (0.503)	0.5 (0.609)	0.344 (0.25)	0.404 (0.505)
<i>ELECPRICE</i>	-0.097 (0.108)	-0.091 (0.113)	0.138 (0.104)	-0.116 (0.101)
<i>LCVSCORE</i>	-0.003 (0.005)	-0.003 (0.005)	-0.001 (0.004)	-0.003 (0.005)
<i>STATEINC</i>	0.01 (0.03)	0.012 (0.036)	0.01 (0.026)	-0.001 (0.025)
<i>IMPORTRATIO</i>	2.185 (0.965)*	2.148 (0.800)**	1.376 (0.392)**	2.215 (0.928)*
State Fixed Effects	Yes	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	700	700	700	700
R-squared	0.95	0.95	0.97	0.95

Robust standard errors in parenthesis

+ significant at 10%; * significant at 5%; ** significant at 1%

Fixed effects regressions; in all specifications the dependent variable is the percentage of a state's electricity generating capacity that is based on renewable technology.

In column 3 of Table 3.3, we instead allow the effect of the RPS policy to be a function of the nominal requirement. As discussed above, this method has two shortcomings, in that it ignores exemptions from the RPS and it includes generation expected from existing capacity that may be eligible under the policy. So if a state introduces an RPS that is already being met, and new non-renewable capacity is erected in the state (causing the renewable share of capacity to drop), we could actually see a negative impact. This is in fact what we observe – the coefficient on *NOMINAL* is negative and significant.²⁶

However, in column 4 of Table 3, we instead specify that the renewable share of capacity is a function of the “incremental requirement” associated with an RPS policy. As discussed above, this measure is a much more accurate indicator of the strength of an RPS. Under this specification, we get a much more intuitively appealing result - the coefficient on *INCPCTRQMT* is positive and significant. We interpret this coefficient as follows - an RPS that mandates that the utilities serving a state increase the renewable share of generation by 1 percentage point will result, on average, in a 0.56 percentage point increase in the share of capacity that is classified as non-hydro renewable. We adopt these results as our preferred baseline specification.

In assessing this coefficient, a couple of points bear mentioning. First, because of the intermittent nature of some types of renewable capacity, one might expect that a 1 percentage point increase in generation should actually be associated with a much greater increase in capacity. However, a non-trivial portion of the renewable generation produced in a given year is undertaken at facilities that are not classified by EIA as “renewable”. These include, for example, natural gas plants that occasionally

²⁶This negative result (as well as the comparatively high R-squared measure) is driven by Maine, where the situation described above actually occurred. In 1997, Maine passed an RPS whose nominal requirement (which took effect in 2000) was already easily met by its existing eligible resources. In 2000-2001, Maine brought several new natural gas plants online, while two biomass plants were retired. Estimating the regression in column 3 without Maine results in a coefficient on *NOMINAL* that is statistically indistinguishable from 0. The rest of the results presented in the paper are insensitive to the exclusion of Maine.

use landfill gas as a fuel, or coal plants that occasionally use biomass. A demeaned regression of Percentage of Renewable Generation on Percentage of Renewable Capacity yields a coefficient of about 0.68, suggesting that if an RPS was the sole force in driving renewable development, was perfectly enforced, and was met with only in-state capacity, we would expect a coefficient of about 1.47.

There are a number of possible explanations for why the coefficient we observe is significantly less than 1.47. The most obvious are (1) that the policies are not being fully enforced, (2) that utilities find it in their interest to comply through the payment of penalties rather than by installing new capacity or acquiring RECs, (3) that most states are complying with the RPS by purchasing out-of-state RECs, or (4) that much of the renewable development we see is being driven either by other policies or by changes in technology that are affecting both RPS and non-RPS states. Unfortunately, our analysis does not allow us to address precisely why the coefficient is less than 1.47.

With regards to other policy variables, it is interesting to note that on average, the mandatory green power option has both an immediate and persistent impact on the renewable share of capacity in a state. The coefficient for *MGPOPTION* is positive and significant regardless of how RPS is measured. A more surprising set of results is the negative and occasionally significant coefficients on the variables associated with both the public benefits fund and the net metering policy alternatives. The only non-policy variable that has a significant coefficient is *IMPORTRATIO*, a finding which suggests that states that experience an increase in the import of electricity have acted more aggressively in developing renewable energy in subsequent years.

In column 1 of Table 3.4, we add the interaction term between *IMPORTRATIO* and *INCRQMTSHARE* to our baseline specification, and obtain a coefficient that is not statistically different from 0. This suggests that RPS policy success, as defined for the purpose of this paper, does not depend on the size of a state's electricity

supply, relative to its demand.

Table 3.4: RPS Design and the Impact of an RPS on Renewable Investment

	-1	-2	-3	-4	-5
<i>INCRQMTSHARE</i>	0.492 (0.219)*	1.011 (0.307)**	0.677 (0.155)**	0.557 (0.188)**	0.931 (0.251)**
<i>MGPOPTION</i>	2.983 (0.504)**	2.896 (0.481)**	2.876 (0.531)**	2.994 (0.518)**	2.685 (0.463)**
<i>PUBBENFUND</i>	-0.525 (0.594)	-0.566 (0.608)	-0.34 (0.476)	-0.547 (0.609)	-0.242 (0.452)
<i>NETMETERING</i>	-0.664 (0.425)	-0.625 (0.459)	-0.612 (0.397)	-0.651 (0.437)	-0.693 (0.426)
<i>INTERCONSTAND</i>	0.422 (0.499)	0.36 (0.554)	0.366 (0.467)	0.404 (0.504)	0.447 (0.511)
<i>ELECPRIICE</i>	-0.113 (0.101)	-0.131 (0.101)	-0.085 (0.102)	-0.116 (0.102)	-0.06 (0.116)
<i>LCVSCORE</i>	-0.003 (0.005)	-0.002 (0.005)	-0.002 (0.004)	-0.003 (0.005)	-0.002 (0.004)
<i>STATEINC</i>	0.002 (0.024)	-0.003 (0.023)	0.013 (0.031)	-0.001 (0.023)	0.016 (0.026)
<i>IMPORTRATIO</i>	2.206 (0.936)*	2.201 (0.901)*	2.238 (0.975)*	2.216 (0.911)*	2.145 (0.888)*
<i>IMPORTRATIO * INCRQMTSHARE</i>	-0.318 (0.312)				-1.339 (0.503)*
<i>RECFREETRADE</i>		0.301 (0.442)			1.678 (1.111)
<i>RECFREETRADE * INCRQMTSHARE</i>		-0.71 (0.357)+			-1.529 (0.503)**
<i>PENALTYACP</i>			-0.937 (0.729)		-1.744 (1.177)
<i>PENALTYACP * INCRQMTSHARE</i>			-0.026 (0.65)		0.773 (0.736)
<i>NEIGHBOR</i>				-0.001 -(0.038)	-0.003 (0.038)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes
Observations	700	700	700	700	700
R-squared	0.95	0.95	0.95	0.95	0.95
Wald test p-value		0.11	0.12		0.04

Robust standard errors in parenthesis

+ significant at 10%; * significant at 5%; ** significant at 1%

Fixed effects regressions; in all specifications the dependent variable is the percentage of a state's electricity generating capacity that is based on renewable technology. The Wald test p-value row gives the p-values on a test that the coefficients on the variables not included in the baseline specification (Table 3.3, Column 4) are jointly equal to 0.

The impact of two RPS design features are examined in column 2 and 3 of Table 3.4. In column 2, we investigate the effect of how an RPS policy treats out-of-state REC's. We introduce *RECFREETRADE* and its associated interaction term into our baseline specification. As one might expect, allowing out-of-state REC's mitigates the effectiveness of the RPS significantly. The results suggest that a mandated increase of 1 percentage point of the renewable share of generation will result, on average, in a 1.01 percentage point increase in states that either prohibit or otherwise discourage out-of-state RECs, and in a 0.30 percentage point increase in states that

place no restrictions on out-of-state RECs. A Wald test of this latter effect reveals that the resulting impact is no longer significantly different from 0.²⁷

In column 3, we investigate whether an RPS policy is rendered more or less effective by the existence of a penalty or ACP. We introduce *PENALTYACP* and its interaction term with *INCRQMTSHARE* into our baseline specification. Both *PENALTYACP* and its associated interaction term have a coefficient that is not statistically different from 0. A Wald test of the joint significance of coefficients for *PENALTYACP* and *PENALTYACP * INCRQMTSHARE* yields a p-value of 0.12, slightly above conventional thresholds for statistical significance.

Column 4 of Table 3.4 introduces *NEIGHBOR* into the baseline specification; the estimated coefficient is small and essentially no different from zero. This suggests that the growth of renewable capacity in a state cannot be explained by increases in the relative size of the renewable electricity market resulting from RPS implementation in neighboring states.

Column 5 of Table 3.4 presents the results from a specification that includes all variables used in the previous specifications of Table 3.4. The key insights are unchanged, with the exception that the coefficient on the interaction between *IMPORTRATIO* and *INCRQMTSHARE* is now significant. This suggests that compared to states that are more energy self-sufficient, the impact of RPS policies is smaller in states that rely on imported electricity. This finding is consistent with the idea that RPS policy outcomes are dependent on the transmission infrastructure that exists in a state at the time the RPS takes effect - states with a higher *IMPORTRATIO* will, more often than not, also have in-state transmission grids that are constrained, meaning that the addition of new in-state renewable resources will be more difficult.²⁸ It is also interesting to note that the baseline effect of *INCRQMT* is now 0.93, notably closer

²⁷This interpretation assumes that a state's RPS is already in place and is not altering its treatment of out-of-state REC's.

²⁸Vajjhala et al (2008) explores the interdependence between transmission and renewable policy success with a series of simulations.

to the coefficient of 1.47 that we would expect if the RPS were perfectly enforced, met with only in-state capacity, and perfectly binding.

Our analysis in this article treats all policies as exogenous, which is clearly a very strong assumption that is unlikely to hold in practice. We acknowledge that endogeneity is a valid concern. Accordingly, our analysis has included social and economic variables thought to impact RPS adoption. However, we recognize that this does not fully address endogeneity concerns. Work in progress by the authors more thoroughly addresses issues of endogeneity and also investigates whether RPS policies to date have led to an increase in electricity prices.

3.7 Conclusion

Existing empirical research on the impact of state-level RPS policies in the United States has often taken a naive approach. This has usually included a cross-sectional approach or the use of very blunt proxies for policies that are in fact very heterogeneous.

In this paper, we have introduced a new way to measure the stringency of renewable portfolio standards (RPS). We argue that it is a much better indicator of the magnitude of the incentive provided by an RPS because it explicitly accounts for some RPS design features that may have a significant impact on the strength of an RPS. The difference between this new measure and other more commonly used measures is striking; some seemingly aggressive RPS policies in fact provide only weak incentives, while some seemingly moderate RPS policies are in fact relatively ambitious.

We also investigate the impacts of renewable portfolio standards on the renewable electricity development in a state using our new measure of RPS stringency, and compare the results with those when alternative measures are used. The difference in the estimates is again striking. Using our new measure, we confirm that, on average, RPS policies have had a significant and positive effect on in-state renewable energy

development. These results cast doubt on the argument that the passage of RPS policies has been purely symbolic, or that they have otherwise not been implemented. These findings are masked when differences among RPS policies are ignored. We also find evidence that another important design feature – allowing “free trade” of RECs – can significantly weaken the impact of an RPS, and that the effectiveness of an RPS is in part dependent on a state’s existing ‘balance of trade’ in electricity. It should come as no surprise that the importance of digging into policy design details is crucial when assessing policy effectiveness. These results should prove instructive to policy makers, whether considering the development of a federal-level RPS or the development or redesign of a state-level RPS.

CHAPTER IV

Measuring the Impact of the Toxics Release Inventory: Evidence from Manufacturing Plant Births

4.1 Introduction

Pollution disclosure has often been called the third wave¹ of environmental regulation. After earlier emphases on command and control regulation and market-based approaches, an increasing number of policies in an increasing number of jurisdictions are focusing on information disclosure as a primary tool. This approach has gained popularity partly because it doesn't force policy-makers to pick technology winners or impose uniform standards on firms and industries who face different costs of being "clean" (as can be the case in command and control regulation). Nor does it rely on a need to choose the "optimal" amount of pollution or optimal tax rates, as is required of the most common market-based solutions. Instead, it relies on a Coasian bargaining argument; if stakeholders and the general public value environmental attributes and have adequate information as to the polluting activities of firms, then the public can process this information via various channels, and impose costs or otherwise give incentives for firms to achieve the right balance between "clean" and

¹Tietenberg (1998)

“dirty”. Incidentally, information provision is also generally less costly to administer than the methods employed in the two previous waves of regulation, another factor which undoubtedly plays a role in its proliferation as a policy tool.

The flagship example of a disclosure-based environmental policy is the Toxics Release Inventory (TRI), a program which has existed for more than 20 years. Created by the October 1986 passage of the Emergency Planning and Community Right-to-Know Act (EPCRA), the TRI is a publicly available database maintained by the U.S. Environmental Protection Agency (EPA) that contains information on toxic chemical releases and waste management activities reported annually by certain industries as well as federal facilities. Industrial or federal facilities that either produce more than 25,000 pounds or handle more than 10,000 pounds of any of the more than 600 listed toxic chemicals must provide detailed information on the treatment, recycling, or release of these substances.² Every year, the TRI typically contains approximately 80,000 facility-chemical reports from more than 20,000 different facilities.

Since the first TRI data became publicly available in June of 1989, it has given rise to a vast body of anecdotal evidence suggesting that media, investors, workers, industry, the government, and the general public have used TRI data to learn about environmental risks and facility-level industrial performance. The EPA has documented more than 100 uses of the TRI data by government, businesses, and citizens (U.S. Environmental Protection Agency, 2003), and several of these documented uses have in turn led to increased efforts by companies to improve their environmental performance (Hamilton, 2005).

However, statistical evidence that the introduction of a disclosure-based approach led to systematic change in pollution outcomes is weak, due to the simple fact that the data covered in the TRI were not recorded in any widespread way prior to the implementation of the program. Rather, the existing research on the TRI (e.g. Hamilton

²See Bennear (2008) for a thorough explanation of the factors that determine whether a facility is subject to the TRI disclosure requirements.

1995, Konar and Cohen 1996, Khanna et al. 1998, Bui and Mayer 2003, Oberholzer-Gee and Mitsunari 2006) has focused either on the reaction of different stakeholders to the release of information contained in the TRI, or the heterogeneity in improvements in environmental performance during the early years of the TRI. However, none have addressed the question of whether firms or plants were actually doing anything differently in response to the existence of the TRI. This lack of an appropriate counterfactual has made it very difficult to evaluate the impact of a disclosure-based approach to environmental regulation.

Traditional environmental regulation is thought to have a direct effect on manufacturing outcomes (in established industries) in three ways:

- it can force incumbents in the affected industries to close;
- it can induce incumbent plants in the affected industries to operate in a more environmentally-friendly way, either through changes in the production process or through improvements in pollution abatement; or
- it can prevent, or otherwise dissuade, would-be establishments from entry, especially in the dirtiest industries.

This paper investigates the third pathway to affect manufacturing, entry deterrence. Accordingly, my empirical analysis investigates whether we observe a shift in the patterns of new plant creation, or plant ‘births’ in the industrial sectors most likely to be affected following the implementation of the TRI. Previous literature (Becker and Henderson, 2000) has established a clear reduction in plant births in counties affected by traditional command-and-control regulation, but the question of whether a disclosure-based policy - which represents a very different approach - has similar effects has not been answered. However, this benchmark for comparison is not the only advantage to focusing on plant births; while from an environmental standpoint one might think that the gains would be higher by closing an older, less efficient plant

or changing the way these older plants operate, prevention of new plants from opening in the region may be more realistic. Incumbent plants often have well-entrenched political interests and also tend to be large employers within the community, both of which can render political action against them more costly than action against potential entrants.

I use unique establishment-level micro-data from the U.S. Census to address this question. I find that in the counties that had the highest levels of toxic releases, this distinction triggered a slight decrease in manufacturing plant births in high-TRI sectors, and a simultaneous increase in the number of births in the cleanest industries. Furthermore, I find that these average effects mask a significant amount of heterogeneity in county-level ‘responses’ to the TRI, and I provide evidence that the magnitude of these shifts are highly correlated with the prevailing income levels in the county. The implications of these results are discussed in the Conclusion.

The remainder of this paper will be structured as follows. In section 2, I present a brief background on the relevant institutions of environmental regulation in the United States, while in section 3 I review the literature on the TRI and pollution disclosure programs more broadly. In section 4, I provide more detail on the Census data used here, while section 5 presents my empirical strategy to identify measurable effects of the TRI on plant births. Section 6 presents the results and Section 7 concludes the paper.

4.2 Background

The institutions surrounding the regulation of pollution in the United States are complicated. In order to provide a better understanding of the context under which this disclosure-based approach to regulation was introduced, some aspects of the relevant institutional landscape are useful to highlight.

I begin by presenting a brief history of the environmental regulation of industrial

pollution in the U.S. in the period leading up to the passage of EPCRA and the TRI. The Clean Air Act Amendments of 1977 were the fifth major piece of federal legislation relating to the reduction of air pollution since the original Air Pollution Control Act of 1955. The 1977 Amendments led to major changes in the way air pollution (though *not* toxic pollution³) was regulated in the United States. Under this regulatory regime (which continues, with minor modifications, today), counties whose ambient air quality is below federal standards for any of 6 “criteria” air pollutants can be designated as “non-attainment” counties. The states where these counties are located are required to develop State Implementation Plans that detail how these counties will attain federal air quality standards within 10 years. The Act granted the federal government the power to withhold federal highway funds or to impose moratoria on new plant construction in recalcitrant states. The bill also gave the federal government the power to impose civil penalties directly on polluters. The key regulatory instrument targeting industrial facilities was technological controls on equipment, under which both new and existing plants were subject to tighter standards in non-attainment areas. In attainment areas, only new large plants were subject to regulations.

The focus of EPCRA was very different. Motivation for increased federal regulation of toxic chemicals was triggered by a tragic chemical accident in Bhopal, India, in December 1984, and subsequent accidents in the U.S. that, while smaller in scale than Bhopal, nonetheless suggested vulnerability to similar accidents. EPCRA’s purpose was to encourage and support emergency planning at state and local levels and to provide the public and government with information regarding possible risks from toxic emissions. The portion of the Act relevant to this study is Section 313, which requires industrial facilities that use, manufacture, or process above a threshold amount

³Toxic, or hazardous, pollutants, as opposed to conventional pollutants, are those pollutants that are known or suspected to cause cancer or other serious health effects, such as reproductive effects or birth defects.

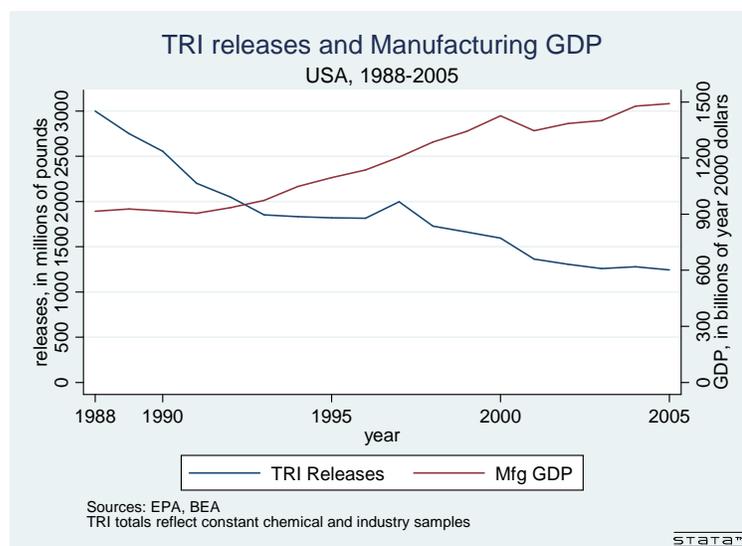


Figure 4.1: TRI Releases and Manufacturing GDP: USA, 1988-2005

of any of the listed toxic chemicals in a given year to provide detailed information on the use and disposal of these chemicals to both the EPA and state officials by July 1 of the following year. This information is subsequently made available to the public, and is known as the Toxics Release Inventory. Data from the first reporting year, 1987, was released on June 19, 1989. This reporting lag has persisted throughout the period of the current study, with a given calendar year's data being released in the spring two years later.

The first two decades of the Toxics Release Inventory have coincided with a noticeable decrease in the amount of reported TRI releases; total releases have decreased by 58.5% between 1988 and 2005.⁴ As Figure 4-1 indicates, this decrease has happened despite an increase in real manufacturing GDP during the same period.

A typical year's inventory contains approximately 80,000 facility-chemical reports

⁴While 1987 was the first year for which TRI data were collected, the data are generally regarded as problematic in some respects (U.S. General Accounting Office, 1991). As a result, analysis that examines decreases in TRI releases (e.g. Hamilton 1999) generally uses 1988 data as the baseline. Benneer (2008) points out that the method of aggregation used by EPA in calculating these figures assumes that a facility's failure to report a given chemical means that the facility's emissions of the chemical were 0, where in several cases failure to report will be positive, but under the threshold. She finds that up to 40% of the observed decline in reported toxic releases in Massachusetts may be attributed to non-reporting due to the reporting thresholds.

from over 20,000 facilities. These data are often aggregated to identify the most highly-polluting facilities in various industries or geographic regions, or to benchmark corporate environmental performance, either within an industry or within a firm over time. The annual EPA reports on TRI⁵ also typically contain similar lists for the “dirtiest” counties. The existence of these lists suggest that facilities face varying degrees of public pressure to reduce toxic releases. These lists motivate the variation I exploit for identification in my empirical analysis.

The TRI is comprised of self-reported estimated emissions, and previous researchers have raised questions about its validity (see, e.g. Toffel and Marshall 2004). While the degree to which the TRI data are accurate remains an open question, there is a fair amount of quality control and verification before the data is released to the public, a fact which should allay some of these concerns. I provide some more detail on the data collection and verification process here. Facilities must file annual reports on releases and waste treatment with EPA by July 1st of the following year. The U.S. EPA then checks the data for consistency, ‘echoes’ the information back to the facilities, allows facilities to make corrections or clarifications of errors, and will refer the issue to an EPA Regional Office for further investigation if necessary.⁶ Each EPA region also has a TRI enforcement program, which conducts a limited number of data quality inspections (of reporting facilities) and non-reporting inspections (of facilities that are in TRI industries but did not report) each year. Violations, whether stemming from late reporting, failure to report, or data quality issues, can lead to penalties of \$25,000 per day, per chemical, per violation, and possibly criminal charges as well. The EPA routinely takes enforcement actions against facilities that either fail to report or report data inaccurately,⁷ suggesting that the incentive to

⁵See, for example, the 1987 TRI Public Data Release (EPA 1989). The TRI data were initially made available in paper reports, but eventually were also disseminated via CD-ROM’s and the EPA website.

⁶EPA’s Region 5, which contains roughly one quarter of the facilities reporting to TRI, receives “several hundred” such referrals per year (Codina, 26 October 2009).

⁷For example, in October 2009 EPA’s Region 9 levied a \$194,000 fine in October 2009 against a

provide truthful information is real. In addition, EPCRA allows citizens to file suit for specified violations of the law, including reporting requirements.⁸

EPA has also conducted TRI data quality surveys, most recently in 1996, which sent surveyors to several facilities to perform their own estimates of releases and other toxic waste management activities. These reports conclude that the information provided by respondents is generally fairly accurate. While facilities in the three sectors assessed in 1996 underestimated releases by 28% on average, three of the four previous surveys found that facility estimates were within 2.2%, on average, of the site surveyor estimates (and that over-reporting is nearly as frequent as under-reporting). These same surveys found that facilities correctly determined whether or not a chemical used at the facility was above the reporting threshold between 93 and 95% of the time (where, again, over-reporting is nearly as common as under-reporting).⁹

There are several anecdotal examples of the TRI data having been used by citizen groups, non-governmental organizations, or local regulators to spur some of the highest toxic emitters into reducing their toxic pollution, whether through citizen-led information campaigns, guides produced by national environmental organizations, or direct negotiation. After Calhoun County, Texas was highlighted by the initial TRI report as the county with the highest level of toxic releases in the country, a community group there used TRI data to build community awareness about local pollution, eventually obtaining a commitment from Alcoa for aggressive pollution reduction initiatives at two local plants. Similarly, a group in Butler County, PA, which was among the highest TRI counties for several years in the mid-to-late 1990's, eventually pro-

California facility that failed to report TRI chemicals over a five-year period (see U.S. Environmental Protection Agency 2009).

⁸See Gray (2002), p.55-59.

⁹It is also worth noting that despite any potential problems with TRI data, it has been widely used in both the economics and management literature as a measure of environmental performance (e.g. see Hamilton 1995, Konar and Cohen 1996, Khanna and Damon 1999, King and Lenox 2000, and Maxwell et al. 2000).

cured a commitment from the state to restrict the level of nitrates that a major steel plant in the area was allowed to release into Connoquenessing Creek. The section 313 reporting requirements have also been cited as directly influencing firms that produce and use TRI-designated chemicals, leading them to implement new emissions reduction initiatives (Baram et al. 1992, p.40; Hamilton 2005), sometimes even without external pressure (Hamilton 2005, p.4).

The existence of the TRI is also credited with leading to further changes in federal law on toxic pollution. The first, known as the Pollution Prevention Act of 1990, simply increased the reporting requirements of the TRI, mandating more information on the steps taken by the companies to reduce their generation of toxic materials. The second, passed in the same year is known as the Clean Air Act Amendments of 1990 (CAAA90). These Amendments included provisions related to the regulation of toxics, centered on requirements that the EPA develop maximum achievable control technology (MACT) standards that all new and existing major source facilities would eventually need to meet. The Water Quality Act of 1987 also established stricter control of the release of toxic pollutants into bodies of water. None of the subsequent federal regulations appear to provide for any geographic heterogeneity in the regulatory pressures faced by either new or incumbent manufacturing plants.

4.3 Literature Review

There is a growing literature on the effects of TRI and similar pollution disclosure programs. I highlight those studies, as well as relevant studies from other strands of the environmental economics literature, in this section.

4.3.1 The Toxics Release Inventory

The literature on the effects of the Toxic Release Inventory is sizeable, although the focus has not been on manufacturing outcomes *per se*. Three noteworthy papers

focus on the response of the financial markets, and whether that in turn had an effect on firm environmental performance. Hamilton (1995) examines whether or not the initial release of the TRI in 1989 was “news” to the media and investors, and finds that (1) firms with higher releases of toxic chemicals were more likely to have those releases covered by print journalists at some point that year and (2) stockholders in firms that reported TRI pollution figures experienced negative and statistically significant abnormal returns upon release of the information. Konar and Cohen (1996) extend this line of research and find evidence that the 40 firms who experienced the largest abnormal returns in 1989 saw weakly significant decreases in releases per dollar of revenue between 1989 and 1992 while the average firm did not, suggesting that the financial markets may provide incentives for firms to change their environmental behavior. Khanna et al. (1998) also examine the effect of TRI releases on financial market outcomes, and in most years find negative abnormal 1-day returns for those firms reporting TRI releases. They also find that these losses are associated with subsequent decreases in on-site releases.

There are also two noteworthy studies that examine to what extent the TRI affected housing prices in the areas surrounding facilities that release toxic wastes. Bui and Mayer (2003) find that declines in toxic releases are not related to any political economy variables and that housing prices did not respond either to the initial disclosure of toxic releases or subsequent decreases in reported toxic releases over time. They suggest that this is evidence that the public may be unable to process the complex information contained in the TRI, and that right-to-know laws may not be the most effective form of regulation. Oberholzer-Gee and Mitsunari (2006) perform a very similar study, though they focus on a different region (Pennsylvania instead of Massachusetts) and look at a finer geographic level, using geo-coded data. They find that home buyers did adjust their risk perceptions following the release of the TRI, and that the amount of air releases near the home had an additional effect

on prices after the pollution information became public.

4.3.2 Pollution Disclosure

Despite the fact that the effects of the TRI on firm behavior and pollution outcomes are not fully understood, disclosure programs have continued to proliferate as alternatives to traditional environmental regulation in both developed and developing countries. The theoretical basis for this type of program is derived from Coase (1960), who author argues that with well-defined property rights, coupled with tradeability of the externality and no transaction costs, efficient pollution levels should be reached. Resolution of the information asymmetry between polluter and stakeholder (whether that stakeholder is represented by the government or the affected public) should significantly lower one of the key transaction costs, implying that in certain cases a disclosure program may result in pollution levels that are closer to the efficient level. Accordingly, if the information asymmetry plays an important role in determining pollution outcomes, we should see some shift in these outcomes once the information asymmetry is reduced. Tietenberg (1998) identifies channels through which public disclosure may motivate improved environmental performance. These include output market pressures, input market pressures, judicial pressures, and regulatory pressures.

Most of the other empirical studies on the impacts of disclosure-based programs have focused on the developing world. Blackman et al. (2004) surveys managers of plants participating in an Indonesian disclosure and rating program; their findings suggest that an important means by which the program spurs abatement is through improving managerial information. García et al. (2009) identify characteristics of Indonesian plants that were more responsive to these same ratings and find that foreign-owned plants, those in more densely populated areas, and those with low initial ratings were more responsive, all other things equal. In Chapter 2 of this

dissertation, I analyze a similar ratings program focusing on pulp and paper mills in India and find that stand-alone plants, those in wealthier communities, and those with low initial ratings were more responsive.

One relatively unexplored pathway through which environmental disclosure policies may affect firm decisions is through more formal regulatory channels. Harrington (1999) points out that under regulatory programs that feature relatively small maximum penalties, low inspection probabilities, and high costs of compliance, compliance may not be rational. However, Harrington also notes that the firm-regulator relationship is dynamic and shows that when inspection probabilities vary based on previous compliance records, compliance can be rational. Decker (2003) continues this line of inquiry by suggesting that the relationship with a regulator is complex and multi-dimensional, and consequently, so is the compliance decision. He finds that waiting times for environmental permits required for plant expansion increase when a plant's recent compliance records are poor. Even a brief permitting delay can result in foregone profits from a plant sitting idle that are a much larger 'penalty' than the administrative fines imposed by the regulator when non-compliance is first found. There is vast evidence¹⁰ that state and federal regulators alike use TRI data for a variety of reasons, even if there are no explicit penalties associated with the level or types of releases.

4.3.3 Community Characteristics and Environmental Outcomes

Several studies have uncovered relationships between community characteristics and environmental outcomes. Pargal and Wheeler (1996) uncover strong support for their 'informal regulation' hypothesis, finding that despite the lack of formal environ-

¹⁰For example, U.S. Environmental Protection Agency (2003) documents more than 40 uses of TRI data by state and federal regulators and legislators for the purpose of enforcement targeting, environmental risk assessment, crafting new legislation, and other uses. Decker (2003) also finds evidence that the time until permit approval under certain environmental statutes is a function of TRI releases in other facilities owned by that applying firm.

mental regulation (and prior to the implementation of the disclosure program mentioned above), firm-level environmental performance in Indonesia is highly negatively correlated with income per capita in the surrounding community, *ceteris paribus*. Hamilton (1993) finds that existing commercial hazardous waste processing facilities' likelihood of having planned capacity expansion decreases with county-level voter turnout measures (a proxy for a county's propensity for collective action), and that the probability of planned capacity reductions increases with this same explanatory variable. Arora and Cason (1999) examine whether community characteristics influence TRI releases in the early years of the program, and find evidence in support of a U-shaped environmental Kuznets' curve, though their results also suggest that any variation in environmental outcomes due to community characteristics are driven by Southern states.

This informal regulation hypothesis has been extended in attempts to explain heterogeneity in the plant-level response to environmental policies and other policies that result in environmental outcomes, through the inclusion of interaction variables between the policy variable and variables that reflect community or plant/firm characteristics. These include the second chapter of this dissertation and García et al. (2009), discussed in the previous subsection. Delmas et al. (2007) examine the effect of electricity deregulation, and find that deregulation resulted in a bigger increase in renewable generation for utilities located in states with higher environmental preferences (as measured by Congressional voting records of the representatives from those states). Delmas et al. (2010) look at the effect of fuel mix disclosure policies, which require electric utilities to disclose the fuel mix of their electricity generation to their customers. They find that the effects of these programs vary according to how much of the utility's customer base is residential; the shift from nuclear to fossil fuels and clean fuels is more pronounced for those utilities with heavier reliance on residential customers.

4.3.4 The 1977 Clean Air Act Amendments

Establishment-level micro-data from the Census has previously been used to measure the effects of more conventional regulation. In particular, a series of papers has exploited the county-level variation in regulatory status under the 1977 Clean Air Act Amendments to identify its effects on county-level manufacturing outcomes. Becker and Henderson (2000) document a shift away from plant births in non-attainment counties for industries likely to be affected by the non-attainment designation, documenting reductions in plant birth rates of 26-45% in polluting industries. They also find evidence that regulatory status affected the size of new plants and the timing of investment. Greenstone (2002) uses Census of Manufactures data to estimate the effects of regulatory status on employment growth, capital formation, and output growth in pollution-intensive industries, and finds sizeable negative (though not always significant) effects on each. List et al. (2003) reinforce the findings in Becker and Henderson (2000), using a New York State dataset and semi-parametric matching methods. Becker (2005) shows that heavy emitters of the criteria air pollutants that were located in non-attainment counties generally had higher air pollution abatement expenditures, with estimates that imply hundreds of thousands of dollars of additional annual costs for the average affected plant.

My study furthers the literature on these topics in two key complementary ways. First, it contributes to the TRI literature by being the first to examine its effects on manufacturing outcomes, specifically new plant births. Second, it allows for direct comparisons of this relatively new regulatory approach with previous papers that have established the effects of more traditional environmental regulations.

4.4 Data

In my empirical analysis I employ confidential establishment-level micro-data accessed via the Michigan Research Data Center of the Census Bureau's Center for Economic Studies (CES). The primary data sets in use will be the Longitudinal Business Database and the Census of Manufactures. I discuss each of these in turn.

4.4.1 The Longitudinal Business Database

The primary data set I employ in this paper is the Longitudinal Business Database (LBD), built and maintained by researchers at the Center for Economic Studies of the U.S. Census. The LBD provides longitudinally linked data for all employer (i.e., those with paid employees) establishments contained in the Census Bureau's business register, the Standard Statistical Establishment List (SSEL), with data dating back to 1975. This allows researchers to observe all establishment births (and deaths) beginning in 1976, as well as data on employment, location, industrial activity, and firm affiliation.

The LBD represents a significant improvement over previous longitudinal datasets compiled by Census, which suffered from broken longitudinal linkages that led to spurious establishment births and deaths (Jarmin and Miranda, 2002). Previous studies (Becker and Henderson, 2000; Levinson, 1996) using Census data to look at the effects of environmental regulation on plant births have focused on the Census of Manufactures (CMF), which provides data on the entire population of U.S. manufacturing establishments every 5 years. List et al. (2003) have pointed out that this 5-year window will fail to capture plant births that are born and die within that span of time. They estimate that this may cause researchers to miss as much as 25% of new manufacturing plant births.

4.4.2 The Census of Manufactures

I complement my baseline regressions using data from the Census of Manufactures, despite this issue with the data. While the CMF is conducted with lower frequency, the data contained therein is more comprehensive in that it also provides wage and output data that is not contained in the LBD. In addition to using this dataset to check the robustness of my results, I also use CMF data (along with data from its annual sub-sample the Annual Survey of Manufactures) to construct proxies for real wage rates in the counties being analyzed.

4.4.3 Supplementary Data Sets

I use Toxics Release Inventory Data to construct a county-level treatment variable. Using TRI data, I replicate the “Top 25” county lists by ranking counties in terms of their unweighted¹¹ total of on-site land, water, and air releases of all chemicals. I use these sums to construct an indicator variable that equals 1 if counties are among the “top 25” in on-site releases. There are thus 400 county-year observations (25 counties x 16 years); their geographic distribution is given in Figure 4.2. This figure suggests that there is significant movement in and out of the “top 25” over time, as only a handful of counties appear in the list 12 times or more. The figure also indicates that there is a significant amount of geographic heterogeneity among the treated counties; there are several treated counties in most regions of the country.

I also experiment with using an indicator variable for counties that were in the top 50 and top 100 for on-site releases, as well as a continuous 0 to 1 measure where

¹¹Toffel and Marshall (2004) suggest that when using TRI data as a measure of organizations’ environmental performance, using one of several databases to weight the TRI chemicals by toxicity is more appropriate than summing un-weighted releases. However, in my analysis, I argue that the un-weighted sums are most appropriate, as this is how the data were presented in EPA reports. As a robustness check, I estimate alternative specifications where the treatment variable is based on sums of releases weighted by the inverse of the “reportable quantities” established under the Resource Conservation and Recovery Act, and find (unreported) results similar to those presented in later sections of the paper.

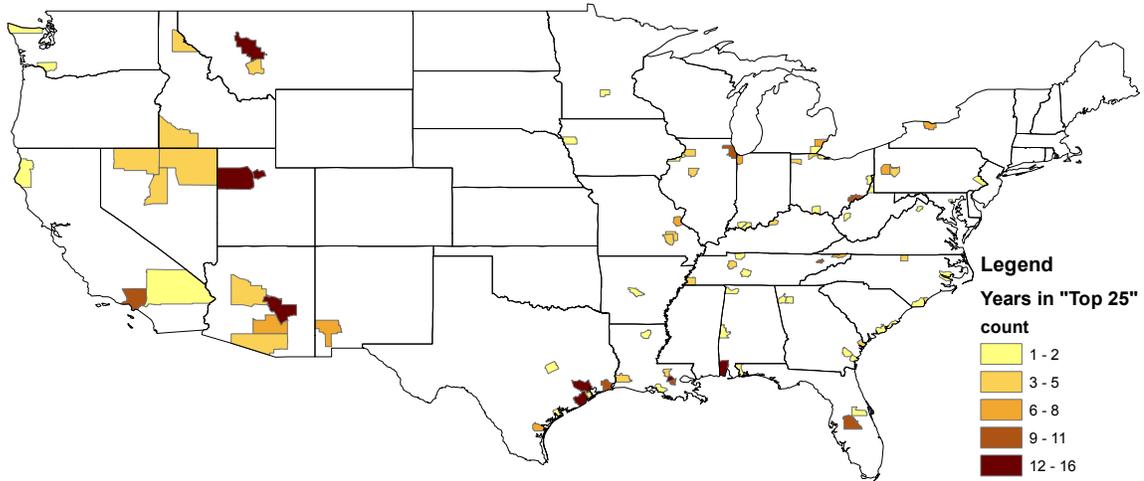


Figure 4.2: Geographic Distribution of ‘Top 25 TRI’ treatment

the county with the highest releases in each year is assigned a value of 1 and the county-level sums are scaled accordingly.¹² Note that the Toxics Release Inventory is generally made available to the public in the spring 2 years after the relevant year; for example, 1987 data first became available in June 1989. I thus allow my treatment measure based on year t data to affect births in year $t + 3$.¹³

I also use TRI data to construct sector-level measures of toxic emissions per employee, where employment data are obtained from the LBD. I use designations from the EPA Sector Notebook Project to define sectors with similar pollution profiles.¹⁴ I use historical nonattainment area designation data from the EPA Green Book in order to control for traditional regulatory pressure as instituted by the 1977 CAAA. Finally, I employ data from the Census Bureau’s County and City Data Book series for county-level demographic data.

I use the EPA Sector Notebook Series to classify manufacturing births into one

¹²Scaling these measures is necessary because reporting requirements were not constant over time. Regardless of the measure used, all yield qualitatively similar results.

¹³I have experimented with allowing the lag to only be two periods instead of 3. The results are weaker, but generally consistent with those presented here.

¹⁴The SIC and NAICS designations, which are commonly used as the boundaries defining an industry or sector, are typically based on product definitions. The EPA Sector Notebooks combine 4-digit SIC codes whose industrial facilities and environmental issues are similar.

of 21 sectors. I classify these 21 sectors into two groups on the basis of average TRI emissions per employee over the period 1988 to 2005,¹⁵ which are summarized in Table 4.1. The 8 dirtiest sectors (non-ferrous metals, agricultural chemicals, inorganic chemicals, organic chemicals, plastic resins, oil, pulp and paper, and steel) are grouped in the High-TRI (or “dirty”) group, while the next 13 sectors are grouped together in a Middle-TRI group.¹⁶ I also follow Becker and Henderson (2000) in defining a Control (or “clean”) group, comprised of several 4-digit SIC industries which are both (1) non-polluting manufacturing industries that are generally regarded as below the radar of the EPA and (2) not major suppliers to polluting industries. These include all apparel industries (all of SIC code 23), mattresses and bedsprings (SIC 2515), and leather gloves, luggage, and handbags (SIC 315-317).

4.5 Empirical Approach

I perform empirical analysis to estimate the determinants of the pattern of plant “births” across geographic units. As described above, both the LBD and the CMF allow researchers to observe how many manufacturing facilities are opened in each industry in a given geographic location over time. I am interested in estimating a model of the general form

$$B_{jt} = B(X_{jt}, f_j + e_{jt}) \quad (4.1)$$

where B_{jt} is the number of births in county j in time t , $B(\cdot)$ is a generic function, X_{jt} is a vector of county characteristics including regulatory variables, f_j is a county fixed effect, and e_{jt} is an i.i.d. error term.

¹⁵I omit 1987 for reasons described above, though the results are insensitive to its exclusion. I also check for movement in the relative rankings of the industries over the years, and there is very little movement - the 8 sectors I classify as dirty are the 8 dirtiest, by TRI releases per employee, in nearly every year since the TRI was created.

¹⁶It could also be argued that there are natural breakpoints between the 4th and 5th sectors, or between the 7th and 8th sectors. I re-constitute the High-TRI group with 4 sectors and 7 sectors and report these results in the appropriate section.

Table 4.1: Sector-level averages of TRI releases per employee

Sector Name	SIC codes	TRI releases per employee
Non-ferrous metals	333-334	5072.1
Agricultural Chemicals	2873-2879	4361.9
Inorganic Chemicals	2812-2819	3154.9
Organic Chemicals	2861-2869	2648.1
Plastic Resins	2821, 2823-24	1443.0
Petroleum Refineries	2911	1273.9
Pulp and Paper	2611, 2621, 2631	1150.5
Iron and Steel	331	472.8
Pharmaceuticals	2833-34	192.2
Rubber	30	127.0
Metal casting	332, 336	99.5
Automobile manufacturing	371	98.4
Wood Furniture	2511-12, 2517-19, 2521, 2531-2541	89.8
Stone, Clay, and Glass	32	73.0
Fabricated metals	34	56.2
Lumber	24	44.6
Aerospace manufacturing	3721-3728	42.4
Textiles	22	35.3
Shipbuilding and repair	3731	29.5
Electronics	36	27.6
Printing	2711-2782	21.4
Control	23, 2515, 315-317	1.8

Source: author's calculations from TRI and LBD data. Releases are in pounds per employee. Sector definitions are from the EPA's Sector Notebook Series. Based on this data, I define high-TRI, or dirty, sectors as the first 8 in this table.

I follow Becker and Henderson (2000) in estimating the conditional poisson model developed by Hausman et al. (1984). The basic poisson model has a single parameter, λ , which enters the argument for the probability of observing B_{jt} births in county j at time t as follows:

$$prob(B_{jt}) = \frac{e^{-\lambda_{jt}} \lambda_{jt}^{B_{jt}}}{B_{jt!}}. \quad (4.2)$$

The conditional poisson model allows λ to vary as a function of X_{jt} and f_j :

$$\lambda_{jt} = exp(X_{jt}\alpha + f_j) \quad (4.3)$$

where α is the parameter vector of interest.

Combining equations 4.2 and 4.3 and taking the product over $t = 1, \dots, T$, the probability of a sequence of births in a county over time is thus

$$prob(B_{j1}, \dots, B_{jt}) = \frac{exp\left(-\sum_{t=1}^T \lambda_{jt}\right) \prod_{t=1}^T \lambda_{jt}^{B_{jt}}}{\prod_{t=1}^T (B_{jt!})}, \quad (4.4)$$

while the total births for that county over time can be expressed as

$$prob\left(\sum_{t=1}^T B_{jt}\right) = \frac{exp\left(-\sum_{t=1}^T \lambda_{jt}\right) \left(\sum_{t=1}^T \lambda_{jt}\right)^{\sum_{t=1}^T B_{jt}}}{\left(\sum_{t=1}^T B_{jt}\right)!}. \quad (4.5)$$

Rearranging terms, the probability of observing a particular sequence of births over time within a county, conditional on the total number of births observed in that

county over T periods, can be expressed as

$$prob \left(B_{j1}, \dots, B_{jt} \left| \sum_{t=1}^T B_{jt} \right. \right) = \prod_{t=1}^T \left[\frac{\exp(X_{jt}\alpha)}{\sum_{s=1}^T \exp(X_{js}\alpha)} \right]^{B_{jt}} \cdot \frac{\left(\sum_{t=1}^T B_{jt} \right)!}{\prod_{t=1}^T (B_{jt}!)}. \quad (4.6)$$

The fixed effect is thus conditioned out of the likelihood, and the estimates are robust to any time-invariant unobserved heterogeneity. The estimation method is thus inherently robust to a primary source of omitted variable bias, and not needing to identify a large number of county fixed effects allows for fairly quick convergence. Counties that have no births over the entire length of the panel in the sector(s) being analyzed are dropped from the sample, since they contribute no information to the likelihood. Following Wooldridge (1999), I calculate standard errors that are robust to violations of the poisson assumption that the conditional mean equals the conditional variance.¹⁷

After constructing the samples discussed in the previous section, I estimate the conditional poisson model described above. For each specification, I estimate the model twice; once for the group of High-TRI sectors and once for the control sector (which forms its own group, of 1 sector). The X vector includes several controls, which I summarize here.

- *MFGEMP* is the log of manufacturing employment within the county but *outside* the group, lagged one year (I construct this from the LBD). While the estimation method conditions out all time-invariant observables, it is reasonable to expect that some of the unobservables will change over a 29-year-panel. This variable is meant to capture the effect of any time-varying unobservables (such as market growth, infrastructure improvements, or tax law changes) that would

¹⁷In unreported results, I also re-estimate my baseline specifications using conditional negative binomial models and obtain quantitatively similar results. For the conditional poisson models, I have also estimated bootstrapped standard errors, which are generally very similar to those reported here.

make the county more or less attractive to *all* manufacturing;

- *REALWAGE* is the log of the real wage rate within the county but *outside* the group, lagged one year. I deflate wages using the output price index for each 4-digit SIC, using the NBER-CES Manufacturing Industry Database by Bartelsman, Becker, and Gray (available at <http://www.nber.org>). I then take a weighted average within each county of the prevailing real wage rate outside the group being analyzed, and take the natural log.
- NA_{co} , NA_{o3} , NA_{so2} , and NA_{tsp} are indicator variables for non-attainment status for carbon monoxide, ozone, sulfur dioxide, and total suspended particulate matter, respectively. Each equals 1 if the county (in part or in whole) was in non-attainment for the respective pollutant as of January 1st of that year, and 0 otherwise.¹⁸ This allows me to control for any changes in traditional regulatory status that might have affected plant births in polluting industries.¹⁹
- *OWNEMP*, which I also construct from the LBD, is the log of manufacturing employment inside the county and *inside* the group, lagged one year. Plant births will generally not be independent over time, and may be influenced by recent growth or expansion in the same sector - we may see agglomeration effects that encourage entry, or we may see local competitive effects that discourage entry. High levels of employment (or analogously, a high number of facilities) in the group of dirty sectors may also dissuade future entry even in the absence of disclosure, through a “smokestack effect”, as stakeholders will have some noisy

¹⁸Non-attainment status is actually decided on July 1st of every year, but I assume that this has no effect on plant births until the following year. I have experimented without this ‘lag’, and the results, while weaker, are consistent with those presented here.

¹⁹There are two EPA air quality standards that I do not implicitly control for; lead and nitrogen dioxide. First, only 12 counties have ever exceeded the lead quality standards. Secondly, only four counties have ever exceeded NO_2 standards, and any county-year where the NO_2 standards are not met, the ozone standards are not met either. There are also two standards (total suspended particulate matter, in 1991 and ozone, in 2004) whose definition changed slightly during the period under consideration. In both cases, I assume that non-attainment status had the same effect under the new definition as it did under the old.

signal of the level of toxic pollution even in the absence of disclosure. This variable is meant to control for these effects.

- Every regression also includes year dummies, in order to capture any possible unobserved changes that affect all counties in the estimation, such as macro-economic shocks or changes in trade policy.
- Several county-level demographic variables are also included in some specifications, all from the City and County Data Book Series. *INCOME* is per-capita income in thousands of year 2000 dollars, *UNEMP* is the prevailing unemployment rate (in percentage terms), *HIGHSCHOOL* is the percentage of the county's population over 25 years of age that has a high school diploma, *POVERTY* is the percentage of the county's families below the poverty level, *WHITE* is the percentage of the county's population that is white, and *TURNOUT* is the percentage of residents who are of voting age that voted in the most recent presidential election.

Because some of the explanatory variables are based on LBD data (which begins in 1976) and are lagged one year, my panel begins in 1977. It ends in 2005, the most recent year of available LBD data. I lose a handful of observations because of missing county characteristics or wage data, though the vast majority of counties have a full panel of 29 observations. The number of counties included in each estimation varies based on the sectors defining that sample.

The Toxics Release Inventory differs from previous forms of environmental regulation, and these differences provide several reasons to suspect that we may not see the same shifts in plant births. First and foremost, the designation of counties as attainment or non-attainment under the 1977 Amendments was a bright line treatment - new plants were subject to much stricter controls, in the form of minimum technology standards, in non-attainment counties. Consequently the cost of entry rose

much faster in those counties than in others. However, under the TRI, there was no such clear distinction. The county-level designation arising from the TRI is instead a listing of the counties with the highest toxic releases.²⁰ Second, even in those counties that were highlighted as the ‘dirtiest’, local and state regulators were given neither a mandate to improve this status nor an explicit incentive to do so (such as the withholding of federal highway funds, as is the case under the standards established by the 1977 Amendments to the Clean Air Act). Third, counties are a fairly blunt measure of the area that might be affected by toxic releases; most of the anecdotal evidence of TRI data being used by stakeholders to generate pressure suggests that the spotlight is typically focused on companies or facilities, rather than all the potential polluters in a county.²¹ In fact, Oberholzer-Gee and Mitsunari (2006) suggest that economic effects of the TRI may be limited to geographic levels much finer than the county. In short, the change in incentives brought about by the TRI will depend not just on the county borders, but also on the preferences and reactions of the stakeholders at more micro-levels within the county - how likely they are to learn from the newly-disclosed information, and how likely they are to act on it, conditional on having learned of it.

At the same time, the EPA’s regular inclusion of lists of the top U.S. counties for TRI total releases in its annual public data releases does suggest that, on average, different counties face differing levels of pressure. These lists could conceivably affect the decisions of local regulators, in terms of permitting for new entrants. In addition, any public pressure resulting from the TRI would be expected to be higher, on average, in counties that appear on this list. Furthermore, this method of developing the treatment variable has other advantages; it is straightforward to calculate and is analogous to more traditional regulation and would thus provide estimates that are directly comparable to those in other studies. At the same time, drilling down to finer

²⁰See, for example, U.S. Environmental Protection Agency (1989), or subsequent public data releases.

²¹This is exacerbated by the fact that counties vary in size significantly.

levels of geographic detail is not feasible, as the estimation method I choose requires aggregation at some level, and very few zip codes or even cities will have witnessed multiple births within my sectors, or even sector groupings, over time.

The limitations of using the county-level treatment variable should bias my estimates of the county-level effects of TRI towards 0. Consequently, using the county-level treatment measure is a fairly conservative approach.

As described in the literature review above, several studies have suggested that response to pollution disclosure programs, and environmental outcomes more broadly, vary with community characteristics, such as income levels, education levels, and voter turnout. Thus, in some specifications I allow the treatment to vary with the prevailing levels of certain community characteristics that proxy for a community's ability to leverage the new information into pressure on the plant. In any specification where I include one or more community characteristics interacted with the treatment variable, I also include the community characteristic by itself to capture its baseline effect. However, the community characteristics tend to move very slowly, implying that most of the differences in plant births due to cross-sectional variation in community characteristics will be captured by the (unidentified) fixed effect - the coefficients on the baseline community characteristics will thus capture the effect of changes in these characteristics within a county.

The data set consists of panel data spanning the period between 1977 and 2005. Summary statistics of the data I employ are provided in Table 4.2, while correlations between the variables used in the analysis are presented in Table 4.3.

In Table 4.4, I also provide summary statistics on the mean of plant births in the two samples. It suggests that, in both samples, the treatment group and the control group are quite different. The High-TRI counties tend to have higher levels of births in both clean and dirty sectors. However, the empirical analysis helps to identify how much of this variation is due to other factors, whether observed or unobserved, in

Table 4.2: Summary Statistics

Variable	<i>Dirty sector sample</i>		<i>Control sector sample</i>	
	Mean	Std. Dev.	Mean	Std. Dev.
<i>NA_{co}</i>	0.055	0.229	0.046	0.209
<i>NA_{o3}</i>	0.173	0.379	0.150	0.357
<i>NA_{so2}</i>	0.023	0.150	0.020	0.139
<i>NA_{tsp}</i>	0.089	0.285	0.076	0.265
<i>MFGEMP</i>	7.927	1.483	7.657	1.580
<i>OWNEMP</i>	3.307	2.867	3.785	2.570
<i>REALWAGE</i>	2.221	0.371	2.179	0.380
<i>UNEMP</i>	6.797	3.490	6.942	3.597
<i>INCOME</i>	13.771	4.198	13.437	4.222
<i>HIGHSCHOOL</i>	66.451	13.995	65.202	14.662
<i>POVERTY</i>	11.438	6.115	12.203	6.695
<i>WHITE</i>	86.739	14.710	86.431	15.485
<i>TURNOUT</i>	55.854	9.875	55.931	9.935
observations	53906		65599	
births	13535		89148	
total counties	1927		2382	
years	1977-2005		1977-2005	

Refer to text for variable definitions. The dirty sector sample is comprised of counties that had at least one new plant in any of the 8 high-TRI sectors between 1977 and 2005.

Table 4.3: Correlations Between Variables Used in the Analysis

	<i>NA_{co}</i>	<i>NA_{o3}</i>	<i>NA_{so2}</i>	<i>NA_{isp}</i>	$\ln(\text{mfgemp})$	$\ln(\text{ownemp})$	$\ln(\text{rwagerate})$	<i>unemp</i>	<i>income</i>	<i>highschool</i>	<i>poverty</i>	<i>white</i>	<i>turnout</i>
<i>dirty births</i>													
<i>1</i>													
0.2364	1												
0.2323	0.3649	1											
0.0581	0.1465	0.0763	1										
0.1966	0.405	0.321	0.2699	1									
0.3902	0.3636	0.4313	0.09	0.2851	1								
0.264	0.1621	0.2557	0.1526	0.2409	0.3624	1							
0.1574	0.1243	0.2024	0.0579	0.1143	0.3419	0.1673	1						
-0.059	-0.0134	0.0298	0.0468	-0.1412	0.0669	0.0669	0.087	1					
0.1953	0.1744	0.2824	0.0054	0.0019	0.3498	-0.1459	0.5179	-0.3511	1				
0.1068	0.1547	0.1816	0.0421	0.0536	0.2063	0.0423	0.495	-0.3057	0.7895	1			
-0.0832	-0.1133	-0.2705	-0.0727	-0.1142	-0.334	-0.0659	-0.3854	0.2846	-0.5802	-0.6202	1		
-0.1116	-0.0578	0.004	0.0612	0.0471	-0.061	-0.0988	0.0663	-0.1153	0.0607	0.2253	-0.5819	1	
-0.0447	0.0326	0.02	0.0517	-0.0053	-0.1079	-0.0347	0.1455	-0.1252	0.193	0.3513	-0.3115	0.4081	1

Table 4.4: Plant Birth Means

	Mean	Treatment county mean	Control county mean
New dirty plants	0.251 (0.851)	1.908 (3.844)	0.240 (0.784)
New clean plants	1.359 (17.925)	25.795 (138.890)	1.235 (14.912)

addition to the treatment. One advantage of the conditional poisson routine is that it estimates relative, rather than absolute, effects of the treatment, so this difference in means should be unimportant as long as the treatment has a similar proportional effect across countries.

4.6 Results

In this section, I present the results of the conditional poisson estimations as described above.

4.6.1 Main Results

The baseline results are presented in columns 1 of Tables 4.5 and 4.6. First, I focus on Table 4.5, Column 1, which estimates the effect of being a known high-TRI county on the number of births in the eight dirtiest sectors in the year where the TRI is expected to have an effect. The coefficient on $HighTRI_{25}$ indicates that a ‘treated’ county could expect a 9 percent reduction in the number of dirty-plant births. However, the coefficient is only significant at a p-value of .15. Recall that conditional poisson estimation conditions out county-level fixed effects and thus effectively removes time-invariant county-level heterogeneity from the estimates, and that I also include (unreported) year fixed effects in all estimations.²² Thus, the results presented here are akin to those from a differences-in-differences type procedure - the 9 percent reduction is net of any macro-level variation, and reflects changes within treated counties that did not occur in non-treated counties.

Nonetheless, it’s possible that the treatment may have coincided with a decrease

²²Theoretical results have shown that inclusion of indicator-type fixed effects in non-linear estimation routines can lead to bias in the resulting coefficients, in what is called the incidental parameters problem (see e.g. Neyman and Scott 1948. However, Monte Carlo studies (see Heckman 1981) have suggested that the bias is minimal for panel data with 8 or more observations per unit. With as many as 29 observations per unit, any bias in my estimates due to an incidental parameters problem should be minimal.

Table 4.5: Conditional Poisson Estimations of Plant Births in the 8 Dirtiest Sectors

	1	2	3	4	5	6
<i>NA_{co}</i>	0.0148 [0.0462]	0.0040 [0.0447]	0.0158 [0.0463]	0.0093 [0.0453]	0.0153 [0.0463]	0.0088 [0.0459]
<i>NA_{o3}</i>	0.0019 [0.0411]	0.0128 [0.0412]	0.0003 [0.0413]	0.0053 [0.0412]	-0.0010 [0.0410]	0.0028 [0.0407]
<i>NA_{so2}</i>	0.0682 [0.0869]	0.0719 [0.0857]	0.0673 [0.0872]	0.0693 [0.0871]	0.0690 [0.0866]	0.0713 [0.0868]
<i>NA_{tsp}</i>	-0.0125 [0.0449]	-0.0201 [0.0417]	-0.0115 [0.0462]	-0.0158 [0.0453]	-0.0100 [0.0454]	-0.0123 [0.0448]
<i>MFGEMP</i>	0.309*** [0.0468]	0.306*** [0.0469]	0.309*** [0.0468]	0.306*** [0.0468]	0.304*** [0.0467]	0.301*** [0.0466]
<i>REALWAGE</i>	-0.0011 [0.0385]	0.0055 [0.0392]	-0.0018 [0.0385]	0.0037 [0.0392]	-0.0042 [0.0383]	-0.0003 [0.0389]
<i>OWNEMP</i>	-0.105*** [0.0129]	-0.106*** [0.0128]	-0.105*** [0.0129]	-0.106*** [0.0128]	-0.106*** [0.0129]	-0.107*** [0.0129]
<i>HighTRI₂₅</i>	-0.0897 [0.0611]	1.250*** [0.334]				
<i>INCOME</i>		-0.0075 [0.00792]		-0.0070 [0.00800]		-0.0068 [0.00804]
<i>HighTRI₂₅ * INCOME</i>		-0.0723*** [0.0173]				
<i>HighTRI₅₀</i>			-0.0561 [0.0590]	0.3430 [0.342]		
<i>HighTRI₅₀ * INCOME</i>				-0.0214 [0.0181]		
<i>HighTRI₁₀₀</i>					-0.0867* [0.0444]	0.3130 [0.227]
<i>HighTRI₁₀₀ * INCOME</i>						-0.0211* [0.0117]
Observations	53906	53906	53906	53906	53906	53906
Number of counties	1927	1927	1927	1927	1927	1927
pseudo R-squared	0.087	0.094	0.085	0.086	0.088	0.090

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Year fixed effects are included in all specifications. Sample includes a total of 13535 births.

Table 4.6: Conditional Poisson Estimations of Plant Births in the Control Industries

	1	2	3	4	5	6
<i>NA_{co}</i>	0.0971 [0.0636]	0.0718 [0.0533]	0.0966 [0.0624]	0.0696 [0.0522]	0.0817 [0.0579]	0.0616 [0.0513]
<i>NA_{o3}</i>	0.0552 [0.0831]	0.0633 [0.0868]	0.0548 [0.0824]	0.0636 [0.0859]	0.0502 [0.0792]	0.0562 [0.0807]
<i>NA_{so2}</i>	-0.0960** [0.0388]	-0.0774* [0.0411]	-0.0900** [0.0385]	-0.0764* [0.0404]	-0.0762* [0.0394]	-0.0727* [0.0398]
<i>NA_{tsp}</i>	-0.0543 [0.0801]	-0.0295 [0.0725]	-0.0582 [0.0801]	-0.0323 [0.0727]	-0.0714 [0.0760]	-0.0535 [0.0681]
<i>MFGEMP</i>	0.248*** [0.0831]	0.200** [0.0869]	0.248*** [0.0827]	0.201** [0.0874]	0.255*** [0.0774]	0.217** [0.0868]
<i>REALWAGE</i>	-0.0115 [0.0385]	-0.00922 [0.0375]	-0.0104 [0.0383]	-0.00821 [0.0373]	-0.00699 [0.0382]	-0.00415 [0.0373]
<i>OWNEMP</i>	0.107*** [0.0273]	0.0991*** [0.0240]	0.105*** [0.0260]	0.0970*** [0.0231]	0.0949*** [0.0203]	0.0874*** [0.0197]
<i>HighTRI₂₅</i>	0.171*** [0.0345]	-1.002*** [0.319]				
<i>INCOME</i>		-0.0158* [0.0088]		-0.0152* [0.0086]		-0.0117 [0.0082]
<i>HighTRI₂₅ * INCOME</i>		0.0588*** [0.0157]				
<i>HighTRI₅₀</i>			0.180*** [0.0262]	-0.682** [0.271]		
<i>HighTRI₅₀ * INCOME</i>				0.0420*** [0.0128]		
<i>HighTRI₁₀₀</i>					0.235*** [0.0413]	-0.671*** [0.187]
<i>HighTRI₁₀₀ * INCOME</i>						0.0439*** [0.00810]
Observations	65599	65599	65599	65599	65599	65599
Number of counties	2382	2382	2382	2382	2382	2382
pseudo R-squared	0.639	0.678	0.647	0.677	0.680	0.692

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Year fixed effects are included in all specifications. Sample comprises 89148 births.

in general attractiveness of the affected areas to any manufacturing activity. In order to examine this possibility, I run similar estimations using my control sample of clean industries. In these new estimations, the dependent variable is the number of births in any of several industries that have very little pollution (toxic or otherwise) as described above. The baseline results, presented in Column 1 of Table 4.6, are not consistent with a general downturn in manufacturing in the treated counties. More specifically, a county receiving the *HighTRI*₂₅ treatment could expect a 17% *increase* in the number of clean-plant births, a finding that is significant at the 1% level. This suggests that counties that were in the top 25 in the country for toxic releases saw a shift in their new manufacturing activity, away from the dirtiest sectors and towards cleaner industries.

Applying these coefficients to the means in Table 4.4, this suggests that High-TRI counties would have seen 0.18 more dirty plant births per year and 3.77 fewer clean plant births per year in the absence of the treatment.

These baseline results mask a significant amount of heterogeneity in the county-level response to the high-TRI treatment. In column 2 of both Tables 4.5 and 4.6, I allow the effects of the High-TRI treatment to vary with county levels of per-capita income. The results are striking. First, I focus on the results in the high-TRI sectors; the coefficient of -.0723 (which is significant at the 1% level) in Table 4.5 on the interaction between per capita income and the treatment variable suggests that for each \$1,000 increase in per capita income of the affected counties, the number of plant births decreases by more than 7%. At the same time, the coefficient of .0588 on the same variable in Table 4.6, also significant at the 1% level, indicates that per capita income is also an important determinant of the magnitude of this aspect of the response to the TRI treatment.

Readers may notice that the coefficients on *HighTRI*₂₅ have reversed signs in both of the augmented models. However, this is less alarming than it initially appears; the

proper interpretation of this coefficient is now the expected effect of the treatment in a county whose per capita income is 0, which is essentially meaningless. But it does suggest that the shift induced by the High-TRI treatment may be *reversed* in poorer counties. Dividing the coefficient on $HighTRI_{25}$ by the coefficient on the interaction term suggests that, in fact, the shift in industrial activity is in the other direction (from clean to dirty) in counties where per capita income levels are below about \$17,000.²³ Equally important, this suggests that in richer counties the response to this High-TRI classification is much stronger than the expected effects reported in columns 1 and 2 of Table 7.

This reversal may appear to be a curious result; it is not immediately clear why the revelation of this information would cause more dirty plants and fewer clean plants to open in dirty counties that are also poor. However, if we consider general equilibrium effects, there is an intuitive explanation for this result. Presumably, the counties that earn this dubious honor have some inherent advantages that have led to the arrival of dirty industries in the first place. The knowledge that a county is particularly dirty does not change this. However, richer counties that access and leverage this information become less willing to host (or less attractive to) dirty industry, so new dirty plants are more likely to land in poorer counties that are (1) still attractive to dirty industry, and (2) can not afford to be as ‘picky’ about new sources of income.

Above, I provide reasons why defining treatment as being among the top 25 counties in toxic releases is the most appropriate approach, but one might still argue that it is a fairly arbitrary measure. Thus, in columns 3-6 of Table 4.5, I repeat the analysis in columns 1 and 2, except that I use two alternate measures of the treatment: being among the 50 or 100 dirtiest counties in terms of TRI emissions. The results are consistent with those discussed above; on average, there is a modest decrease in dirty plant births, and the magnitude of this decrease depends on per capita income.

²³This turning point is \$17,289 in Table 4.5 and \$17,041 in Table 4.6. All per capita income levels are expressed in year 2000 dollars.

For both the “Top 50” and “Top 100” definitions of the treatment, the results are weaker than for the “Top 25”, and the significance of the results is generally lower, but this is to be expected if the effects are stronger for those counties with the highest emissions levels. Similarly, in columns 3-6 of Table 4.6, I repeat the analysis for clean births, but using these alternate definitions of the treatment. The results are again very comparable. In separate, unreported regressions I have also used a continuous measure, and I find qualitatively similar results.

The coefficients of the control variables also yield insights. First, in Table 4.5, note that none of the coefficients on the non-attainment status variables are significant. This reflects the fact that the sectors that are the highest polluting in terms of toxic releases are not always the highest polluting in the criteria pollutants,²⁴ and suggests that non-attainment status does not in general deter plant entry in highly toxic industries. The positive and significant coefficient on *MFGEMP* suggests that plant births in the dirty sectors are responsive to the same time-varying county traits that spur growth in other manufacturing sectors. Wages do not play a consistent role in determining plant births, which is consistent with the fact that most of the sectors classified as dirty here are capital-intensive and perhaps less sensitive to labor conditions than other manufacturing. Finally, the negative and significant coefficient on *OWNEMP* suggests that there are not aggregation effects in this sector grouping, and in fact that lagged growth in this group of industries decreases plant births in the current period. In Table 4.6, the results are fairly similar. One exception is that non-attainment status in sulfur dioxide is correlated with a decrease in clean plant births. While never significant, the coefficients on *REALWAGE* are uniformly negative, which is consistent with the notion that these sectors tend to be more responsive

²⁴For example, Greenstone (2002), using the same EPA Sector Notebook definition of sectors, classifies the nonferrous metals sector as a high emitter of carbon monoxide and sulfur dioxide, but not of total suspended particulates or of the antecedents of ozone, while the organic chemicals sector is classified as a high emitter only of the antecedents of ozone. As indicated in Table 4.1, these are two of the dirtiest sectors in terms of toxic releases.

to wages. Finally, I find evidence of aggregation economies in the clean sector, with a positive and highly significant coefficient on *OWNEMP*.

4.6.2 Robustness Checks

Upon examination of Table 4.2, some concern arises that the samples used in estimation of the results presented in Tables 4.5 and 4.6 are systematically different, and that these differences may be driving the results. To ensure that this is not the case, I re-estimate the baseline conditional poisson models, for both dirty and clean plant births, on a uniform restricted sample. This sample is limited to counties that had at least one plant birth in each of the three groups - High-TRI, Middle-TRI, and Control. This limits the sample to 49575 observations in 1747 counties. These results are presented in Table 4.7, and are virtually identical to the results presented in the first two columns of Tables 4.5 and 4.6.

As mentioned above, I also re-estimate the models using CMF data. These results are possibly biased, due to the fact that the CMF misses between 22% and 24% of plant births, according to the data used here. These results, presented in Table 4.8, are qualitatively very similar to what I find using the LBD data. The dirty-sector birth estimations are presented in the first two columns, with the control sector births in columns 3 and 4. The primary difference is that the baseline coefficient on the treatment variable (column 1) is smaller in magnitude and less precisely estimated in the high-TRI sectors, while the baseline coefficient is much higher in the clean sector estimations (column 3). The income interactions are of the same sign and similar magnitude.

Examination of Table 4.1 suggests possible alternate cutpoints for the definition of “dirty” sectors. In particular, the pulp and paper sector has a much higher average emissions per employee ratio than the iron and steel sector, and the gap between the organic chemicals and resins sectors is fairly large. In the first two columns of Table

Table 4.7: Conditional Poisson Estimations of Plant Births, Balanced Sample

	dirty births		clean births	
	1	2	3	4
<i>NA_{co}</i>	0.0133 [0.0462]	0.00357 [0.0447]	0.0978 [0.0645]	0.0736 [0.0544]
<i>NA_{co}</i>	-0.00115 [0.0411]	0.00941 [0.0412]	0.0696 [0.0837]	0.0767 [0.0875]
<i>NA_{co}</i>	0.0679 [0.0869]	0.0712 [0.0857]	-0.0979** [0.0393]	-0.0803* [0.0425]
<i>NA_{co}</i>	-0.0121 [0.0449]	-0.0191 [0.0417]	-0.0521 [0.0806]	-0.0276 [0.0733]
<i>MFGEMP</i>	0.315*** [0.0486]	0.312*** [0.0486]	0.275*** [0.0866]	0.228** [0.0948]
<i>REALWAGE</i>	0.000251 [0.0392]	0.0058 [0.0398]	-0.00289 [0.0394]	4.96E-06 [0.0382]
<i>OWNEMP</i>	-0.0959*** [0.0131]	-0.0967*** [0.0130]	0.130*** [0.0307]	0.121*** [0.0272]
<i>INCOME</i>		-0.00613 [0.00796]		-0.0150* [0.00908]
<i>HighTRI₂₅</i>	-0.0846 [0.0619]	1.335*** [0.343]	0.161*** [0.0359]	-0.996*** [0.323]
<i>HighTRI₂₅ * INCOME</i>		-0.0763*** [0.0177]		0.0580*** [0.0159]
Observations	49575	49575	49575	49575
Number of counties	1747	1747	1747	1747
pseudo R-Squared	0.090	0.096	0.656	0.691

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Year fixed effects are included in all specifications. Columns 1 and 2 include 13202 (high-TRI) births, while columns 3 and 4 include 86459 (control sector) births.

Table 4.8: Conditional Poisson Estimations Using Census of Manufactures Data

	High-TRI sector births		Control sector births	
	1	2	3	4
NA_{co}	-0.128*** [0.0496]	-0.138*** [0.046]	0.0771 [0.0561]	0.0741 [0.0551]
NA_{o3}	0.0175 [0.0443]	0.0325 [0.0444]	0.101 [0.0878]	0.102 [0.0873]
NA_{so2}	0.061 [0.0523]	0.0481 [0.0531]	-0.100*** [0.0373]	-0.0853** [0.0405]
NA_{tsp}	0.0511 [0.0428]	0.0568 [0.0393]	-0.0686 [0.0688]	-0.0664 [0.06]
$MFGEMP$	0.231*** [0.0422]	0.224*** [0.0419]	0.356*** [0.0938]	0.356*** [0.0955]
$OWNEMP$	-0.0856*** [0.0131]	-0.0862*** [0.0131]	0.0929** [0.0402]	0.0851** [0.0362]
$REALWAGE$	0.0562 [0.0512]	0.0591 [0.0516]	-0.0494*** [0.0164]	-0.0475*** [0.0158]
$INCOME$		-0.00877 [0.00908]		-0.00695 [0.00979]
$HighTRI_{25}$	-0.0394 [0.0829]	0.984** [0.473]	0.340*** [0.054]	-0.565* [0.312]
$HighTRI_{25} * INCOME$		-0.0610** [0.0258]		0.0465*** [0.0162]
observations	10651	10651	14435	14435
counties	1777	1777	2408	2408
pseudo R-squared	0.196	0.220	0.730	0.740

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Year fixed effects are included in all specifications. Columns 1 and 2 include 10121 (high-TRI) births, while columns 3 and 4 include 67981 (control sector) births.

4.9, I present the coefficients of interest from conditional poisson estimations where the outcome variable is births in high-TRI sectors, under the respective alternate definitions. The samples have also been adjusted accordingly, omitting counties that saw no births in the relevant sectors over the 29 years of the panel. The results are very similar to the baseline results presented above.

While the baseline results presented in Tables 4.5 and 4.6 provide an overview of the effects of this High-TRI treatment on the affected manufacturing sectors, it doesn't tell us anything about how the TRI impacted individual sectors. Thus, in the remaining columns of Table 4.9, I present the key coefficients from analogous estimations, but where the count of births is sector-specific, and the samples are redefined accordingly. First, note that the general pattern identified in the main results (a decrease in births, where the magnitude of this decrease is correlated with county income levels, in sectors with high levels of toxic pollution) is reflected in the individual sectors. In 5 of the 8 sectors, the expected impact (panel 1) is negative, though only one coefficient is significant. Furthermore, in 7 of the 8 sectors, the negative impact is increasing in income levels, and the coefficient is significant in 4 of the 7 (see panel 2). However, the estimates are generally less precise, making it difficult to make any strong conclusions about impacts on specific sectors. There are exceptions - the impact was fairly pronounced on inorganic chemicals, and to a certain extent, on the nonferrous metals, plastic resins, and iron and steel sectors. However, it is worth noting that the treatment did not have the expected effect on the pulp and paper industry - the signs on both coefficients of interest are reversed, though neither is significant.

I also run two other robustness checks, though I do not report the results here due to space considerations. First, I estimate conditional negative binomial models²⁵

²⁵The negative binomial distribution is not subject to the Poisson restriction that the conditional mean equals the conditional variance. While my use of robust standard errors should address this over-dispersion concern, I estimate conditional negative binomial specifications as a robustness check. However, the conditional negative binomial is prone to its own problems, in that it is not a true

Table 4.9: Alternate samples

	7 dirtiest sectors	4 dirtiest sectors	Nonferrous Metals	Agricultural Chemicals	Inorganic Chemicals	Organic Chemicals	Plastic Resins	Petroleum Refineries	Pulp and Paper	Iron and Steel
<i>HighTRI₂₅</i>	-0.08 [0.0758]	-0.118 [0.0810]	-0.25 [0.175]	0.0422 [0.169]	-0.226* [0.126]	-0.0948 [0.0998]	-0.15 [0.153]	0.169 [0.211]	0.247 [0.171]	-0.0403 [0.0913]
	<i>Panel 1: Restricted model</i>									
<i>HighTRI₂₅</i>	1.136*** [0.382]	1.418** [0.576]	0.758 [1.475]	0.75 [0.766]	1.287 [0.812]	1.697* [0.880]	0.811 [1.222]	2.059** [0.887]	0.0156 [1.264]	1.597*** [0.511]
<i>HighTRI₂₅ * INCOME</i>	-0.0663*** [0.0198]	-0.0852*** [0.0294]	-0.0559 [0.0755]	-0.0382 [0.0408]	-0.0879** [0.0431]	-0.0966** [0.0446]	-0.0537 [0.0630]	-0.0978** [0.0495]	0.0122 [0.0631]	-0.0859*** [0.0251]
	<i>Panel 2: Expanded Model w/ Income and Interaction</i>									
	<i>Panel 3: Sample Statistics</i>									
Observations	47895	41318	14130	23336	20049	18675	14896	10738	13282	30772
Counties	1708	1467	494	821	703	657	518	380	463	1078
Total births	9388	6363	1241	1658	2000	1464	1270	868	887	4147

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Each column presents the coefficients of interest from two estimations, analogous to columns 1 and 2 of Table 6. Year fixed effects are included in all specifications.

that follow the main specification, and the results are qualitatively unchanged. I also interact the Clean Air Act non-attainment status indicators with the treatment of interest to test whether there are ‘spillovers’ in the sense that the decrease in births in the affected industries and counties following the introduction of the TRI is driven by the same counties being in non-attainment. I find no such evidence, and the coefficients of interest are largely unaffected. The results from these two robustness checks are available upon request.

4.6.3 Other community characteristics

So far, I have focused on the role of county income levels in explaining the variation in the effects of the TRI on dirty sector births in the dirtiest counties. However, as discussed in the literature review, other studies have found significant impacts of other community characteristics in determining environmental outcomes, both in the absence and presence of environmental regulation. In this subsection, I discuss the results from an expanded analysis intended to investigate the importance of other community characteristics in this particular setting. The estimation results are presented in Table 4.10.

In column 1, I have repeated the results from column 2 of Table 4.5, for ease of comparison. In the second column, I present results from a specification that includes the most recent county-level unemployment figure as well as its interaction with my treatment variable. The results suggest that any decrease in dirty plant births in High-TRI counties is mitigated by higher unemployment levels. Column 3 provides the results from an estimation where the specification in column 1 has been augmented by the percentage of the county’s population that has received a high school diploma, as well as its interaction with the treatment variable. The results indicate that any decrease in dirty plant births following High-TRI designation is also associated with

fixed-effect method. See Allison and Waterman (2002) for details.

Table 4.10: Effects of Other Community Characteristics

	-1	-2	-3	-4	-5	-6
<i>NA_{co}</i>	0.00399 [0.0447]	0.000914 [0.0448]	0.000219 [0.0434]	0.00455 [0.0438]	-0.0044 [0.0430]	-0.00188 [0.0451]
<i>NA_{o3}</i>	0.0128 [0.0412]	0.0167 [0.0410]	0.0144 [0.0411]	0.0306 [0.0417]	0.04 [0.0410]	0.0163 [0.0407]
<i>NA_{so2}</i>	0.0719 [0.0857]	0.0775 [0.0868]	0.0686 [0.0829]	0.0609 [0.0821]	0.0542 [0.0788]	0.0729 [0.0857]
<i>NA_{tsp}</i>	-0.0201 [0.0417]	-0.0205 [0.0418]	-0.0163 [0.0409]	-0.0179 [0.0410]	-0.00818 [0.0412]	-0.0163 [0.0420]
<i>MFGEMP</i>	0.306*** [0.0469]	0.301*** [0.0479]	0.307*** [0.0467]	0.272*** [0.0467]	0.288*** [0.0462]	0.302*** [0.0470]
<i>REALWAGE</i>	0.00545 [0.0392]	0.00939 [0.0396]	0.0199 [0.0407]	0.00936 [0.0393]	0.0235 [0.0406]	0.00706 [0.0392]
<i>OWNEMP</i>	-0.106*** [0.0128]	-0.106*** [0.0128]	-0.108*** [0.0128]	-0.111*** [0.0130]	-0.111*** [0.0128]	-0.106*** [0.0128]
<i>HighTRI₂₅</i>	1.250*** [0.334]	0.946** [0.369]	2.249*** [0.700]	1.081** [0.448]	1.472** [0.645]	1.888** [0.747]
<i>INCOME</i>	-0.00748 [0.00792]	-0.00711 [0.00795]	-0.00899 [0.00768]	-0.0165** [0.00812]	-0.00856 [0.00771]	-0.00847 [0.00799]
<i>HighTRI₂₅ * INCOME</i>	-0.0723*** [0.0173]	-0.0694*** [0.0169]	-0.0575*** [0.0163]	-0.0681*** [0.0167]	-0.0712*** [0.0207]	-0.0857*** [0.0232]
<i>UNEMP</i>		-0.00671 [0.00655]				
<i>HighTRI₂₅ * UNEMP</i>		0.0409* [0.0248]				
<i>HIGHSCHOOL</i>			0.00942*** [0.00305]			
<i>HighTRI₂₅ * HIGHSCHOOL</i>			-0.0169** [0.00734]			
<i>POVERTY</i>				-0.0226*** [0.00667]		
<i>HighTRI₂₅ * POVERTY</i>				0.00928 [0.0204]		
<i>WHITE</i>					0.0141*** [0.00283]	
<i>HighTRI₂₅ * WHITE</i>					-0.00317 [0.00523]	
<i>TURNOUT</i>						0.00319 [0.00319]
<i>HighTRI₂₅ * TURNOUT</i>						-0.00768 [0.00766]
Observations	53906	53906	53906	53906	53906	53906
Number of counties	1927	1927	1927	1927	1927	1927

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Robust standard errors in brackets. Year fixed effects are included in all specifications.

Sample includes a total of 13535 births.

the education levels of the affected population.

Columns 4 and 5 present results that are designed to address questions of environmental justice. The finding that counties with higher levels of income experience a decrease in dirty-plant births after high pollution levels are revealed, while those with lower levels of income experience an *increase*, is consistent with the classic environmental justice story - that poor and minority populations bear a disproportionate burden of pollution²⁶. In column 4, I present the results from a conditional poisson estimation that includes the percentage of the county's population that is below the poverty line and its interaction with the treatment variable, while the results in column 5 come from a specification that includes the percentage of the population that is white, along with the respective interaction. The baseline effects most likely are reflective of the correlations with economic development. The coefficients on the interaction variables, while having signs that are consistent with the environmental justice story, are insignificant.

Finally, in the last column, I present results from a specification that includes voter turnout in the most recent presidential election and its interaction with the treatment indicator. As discussed earlier, some studies (see e.g. Hamilton 1993) have suggested that location of polluting industrial facilities can be affected by the county's propensity for collective action. The resulting coefficient is negative, but not significant.

4.7 Discussion and Conclusion

Disclosure-based approaches to environmental regulation have proliferated since the creation and apparent success of the Toxics Release Inventory in the late 1980's. In this paper I investigate whether the increased transparency and access to pollution information engendered by the TRI was adequate to induce polluting entities to

²⁶See for example Brulle and Pellow (2006).

change their behavior. I find that in counties with high-levels of TRI emissions, the revelation of this information precipitated a modest decrease in plant births in high-TRI industries and an increase in plant births in clean industries. This effect varies significantly across counties, with the effect being much stronger in higher-income counties, and reversed in low-income counties.

The results of the analysis suggest several lessons relevant to understanding the effects of disclosure programs. First, it appears that we should only expect county-wide impacts in those areas that are at the very top of the distribution. The results are generally mitigated as we move past the “top 25” definition of the treatment. This of course does not mean that stakeholders outside of the dirtiest counties are unable to leverage the information in the TRI.

Second, the finding that the response to the treatment is highly variable and largely context-dependent has a variety of interesting implications. From an efficiency standpoint, this may be a very good thing. The debate over environmental federalism²⁷ has centered on the fact that the marginal costs of pollution and the marginal benefits from the industrial activity that generates that pollution are probably very heterogeneous across (and even within) jurisdictions, and imposition of uniform standards (as is done under the Clean Air Act Amendments) probably leads to inefficiencies. A disclosure program, on the other hand, decentralizes the pollution ‘allocation’ process, putting more control into the hands of local regulators, stakeholders, and firms. Dirty counties with lower employment or income levels are probably more willing to play host to additional polluting plants than those dirty counties who are already wealthy and near full employment. Of course, this is the other side of the environmental justice coin; some would argue that more pollution in poorer communities reflects injustice, not simply the economist’s efficiency gains.

Third, the county-level effects are generally smaller in magnitude than those found

²⁷For example, see Oates (2001).

in Becker and Henderson (2000). The highest effects I find, for the nonferrous metals and inorganic chemical sectors, are both smaller and less significant than those they find in all four of the industries they analyze. Of course, the levels of aggregation are different, but this comparison is consistent with the intuition that the information-based approach to environmental regulation is less onerous than the command and control approach embodied in the Clean Air Act Amendments. To the extent that the decreased growth of high-pollution manufacturing in affected counties activity represent ‘costs’ of traditional environmental regulation, the comparison of these studies suggests that disclosure is less costly.

Unfortunately, the analysis performed does not allow me to make any conclusions about the process through which the TRI had its main impacts. Local regulators could be using the information in their permitting decisions, potential entrepreneurs may be responding to an increased threat of public scrutiny in already-dirty locales, or local stakeholders could be leveraging the information in order to influence these two groups. Clearer identification of these channels presents an obvious opportunity for future research – one that could inform our understanding of firm self-regulation, firm location decisions, collective action processes, and the firm-regulator relationship.

This paper has focused only on plant births. While, as previously discussed, this may well be the most likely industrial outcome to be impacted by environmental regulation, the effect of disclosure on outcomes relevant to incumbents, such as investment, changes in the production process, productivity, and plant closings, is also key to any evaluation of the relative merits of disclosure-based policies, and presents another opportunity for further study.

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